

Political Consumerism: Ideology or Signaling?

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Abstract

Firms and their executives taking stances on controversial social and political issues such as gun control have been found to affect consumer behavior. This “political consumerism” might be driven by personal ideology or a desire to signal to peers. We run an experiment on 1,198 respondents who know each other to study how ideology and signaling affect a real purchase decision when a CEO engages in activism on gun control. Participants are randomly assigned to see either generic information about a product or the same information plus a CEO statement supporting gun rights. They then make a choice about whether to receive the product or a bonus payment, with half assigned to a public purchase condition in which their choice is observable to someone they know. We find that CEO support for gun rights reduces demand among people who favor stricter gun control laws. This effect does not vary significantly across the private and public purchase conditions, suggesting that personal ideology is a sufficient motive for political consumerism and that CEO activism can impel boycotts even for products whose consumption is not observable to others.

Keywords: political consumerism, CEO activism, partisan, consumer ideology, signaling

1 INTRODUCTION

As firms and their executives increasingly wade into controversial social and political issues unrelated to their core business, there is a need to understand how and why consumers react (Chatterji and Toffel 2019, Bronnenberg and Dubé 2023, Lelkes 2023). “Political consumerism”—the tendency to select products for social, political, or ethical reasons (Newman and Bartels 2010)—could be motivated by personal ideology or by a desire to signal to others (Bronnenberg and Dubé 2023, Lelkes 2023).

Studies confirm that personal ideology and partisanship can affect everyday purchases, and that consumers may “buycott” or boycott products to reward or hold companies accountable for their production methods or political activities (Bennett 1998, Shah et al. 2007, Khan et al. 2013, Stolle and Micheletti 2013, Endres and Panagopoulos 2017, McConnell et al. 2018, Copeland and Boulianne 2020, Panagopoulos et al. 2020, Hambrick and Wowak 2021, Homroy and Gangopadhyay 2023, Hou and Poliquin 2023, Liaukonyte et al. 2023a). Consumers, however, may be especially motivated to purchase or avoid a product when their choice is observable to others and signals an important aspect of their identity, values, or group affiliations (Veblen 1899, Levy 1959, Belk 1988, Wernerfelt 1990, Fournier 1998, Chernev et al. 2011, Yoganarasimhan 2012, Dubé et al. 2017). Such theories of “symbolic consumer behavior” indicate that since partisanship is a social identity (Briley et al. 2019, Iyengar et al. 2019, West and Iyengar 2020), consumers may react to CEO and corporate activism to communicate their beliefs or allegiances, even if doing so has little effect on corporate behavior or public policy

(Wernerfelt 1990, Dubé et al. 2017, Bronnenberg and Dubé 2023, Lelkes 2023). Despite interest in understanding the motivations for political consumerism, distinguishing ideology and signaling is difficult and remains unexplored (Bronnenberg and Dubé 2023).

We study the degree to which political consumerism is motivated by personal beliefs versus a desire to signal. To do so, we create a new coffee company and give 1,198 people who know each other the choice to win either a bag of coffee or a cash bonus payment. In the experiment, we randomly manipulate whether people are exposed to a CEO message supporting gun rights, and whether their choice will be private or public (revealed to someone they know). We find that CEO support for gun rights reduces demand among people who favor stricter gun control laws, and that this effect does not depend significantly on whether a consumer's choice is observable to others.

Our findings contribute to existing literature on political consumerism in two important ways. First, consistent with recent observational research (Homroy and Gangopadhyay 2023, Hou and Poliquin 2023, Schoenmueller et al. 2023, Liaukonyte et al. 2023a), we experimentally confirm that sociopolitical activism affects consumer behavior with an incentive-compatible outcome measure. In doing so, we extend prior studies that have relied on self-reported behavior or measures of purchase intentions (Stolle et al. 2005, Endres and Panagopoulos 2017, Chatterji and Toffel 2019), and we extend prior experiments on political consumerism to explicitly consider controversial CEO activism rather than general corporate political activity (Panagopoulos et al. 2020) or valence issues such as human rights (Sen et al. 2001). Our study design allows us to further

add to this literature not only by measuring changes in consumer behavior but also by quantifying, in dollar terms, consumers' willingness-to-pay for activism.

Second, we are among the first to examine the mechanisms through which corporate and CEO activism affects consumers by empirically distinguishing two primary explanations: personal ideology and signaling. Bronnenberg and Dubé (2023), Lelkes (2023), and Liaukonytė et al. (2023b) underscore the importance and difficulty of understanding how these two distinct mechanisms matter in the context of CEOs and firms engaging in activism. We shed light on this puzzle by showing that personal ideology is a sufficient motive for consumers to adjust their purchases, which implies that activism can prompt boycotts even of products whose consumption is private.

The remainder of the paper is organized as follows. Section 2 discusses theories of how CEO activism affects consumers. Section 3 explains our experimental design and methodology. Sections 4 and 5 describe our data and analysis, respectively. Section 6 presents results of our study, and Section 7 concludes.

2 THEORY AND HYPOTHESES

Political consumerism is the practice of avoiding (boycotting) or selecting (buycotting) products for social, political, or ethical reasons (Newman and Bartels 2010). This tendency is a form of “lifestyle politics”—the propensity to ascribe political meaning to one’s recreational experiences, diet, fashion choices, and purchases (Shah et al. 2007)—that grounds political action in personal identity and sees everyday decisions as opportunities to achieve or express political objectives (Bennett 1998, Bennett 2012).

Surveys indicate that political consumerism is widespread; in recent surveys, 30%–50% of Americans report having boycotted or buycotted a product in the previous year (Endres and Panagopoulos 2017). Several empirical studies lend support to claims that politics affects purchases (Stolle and Micheletti 2013, Copeland and Boulianne 2020). For example, Liaukonyte et al. (2023a) find that consumers in Republican counties purchased more Goya products after the CEO praised then-President Donald Trump; Hou and Poliquin (2023) report that retailers whose CEOs supported gun control after a mass shooting experienced a 5% decrease in store visits in Republican counties; and Schoenmueller et al. (2023) find that consumers demanded more “Democratic brands” after the election of Donald Trump in 2016. Together, these and other studies suggest that “CEO activism”—business leaders speaking out on social and political issues unrelated to their core business (Chatterji and Toffel 2019, Hambrick and Wowak 2021)—considerably affects consumer behavior. We therefore hypothesize that telling consumers about a CEO’s position on a controversial issue will affect purchase decisions:

H1. The rate of purchase for the product when people are informed of the CEO’s activism will differ from the rate of purchase when people are not informed of the CEO’s activism.

Although several studies have explored *whether* CEO activism affects consumers (Chatterji and Toffel 2019, Conway 2023, Homroy and Gangopadhyay 2023, Hou and Poliquin 2023, Schoenmueller et al. 2023, Liaukonyte et al. 2023a), *why* it affects consumers remains under-explored but has implications for the consumption contexts and firms most likely to be affected (Bronnenberg and Dubé 2023, Lelkes 2023).

Two distinct, prominent motivations for consumers to change their behavior are personal ideology and signaling. Research shows that political polarization is an important driver of nonpolitical behavior (Iyengar et al. 2019), and that consumers derive value from consuming products aligned with their political views (Endres and Panagopoulos 2017, Panagopoulos et al. 2020). Consumers who identify with a political party may use political cues to characterize a brand as either the in-group or the out-group and purchase products to reward co-partisans (McConnell et al. 2018, Sun et al. 2021). Additionally, by boycotting products, ideologically motivated consumers may hope to punish egregious behavior and change the beliefs or practices of the targeted firm (Klein et al. 2004, Shah et al. 2007, Stolle and Micheletti 2013, Wang et al. 2018).

Consumers may also react to CEO activism, however, to signal to others their identity, beliefs, or group affiliations (Veblen 1899, Levy 1959, Holbrook and Hirschman 1981, Wernerfelt 1990, Fournier 1998, Sen et al. 2001, Berger and Heath 2007, Chernev et al. 2011). Studies of “symbolic consumer behavior” note that we judge others by their purchases (Levy 1959) and make purchases to communicate desired identities (Wernerfelt 1990, Berger and Heath 2007). Political ideology, or partisanship, is a social identity (Iyengar et al. 2019, West and Iyengar 2020) that forms early in life and typically persists over time (Green et al. 2002). Hence, consumers may react to CEO activism to signal their beliefs, partisan affiliation, or allegiance. If so, the effects of CEO activism for private and public purchases will differ (Sen et al. 2001). Therefore, we test whether the effect of CEO activism on purchases (H1) varies with whether the purchase is observable:

H2. The effect of CEO activism on the rate of purchase will differ for people who are informed that their decision will be made public versus people who are informed that their decision will be kept private.

3 METHODOLOGY

3.1 Experimental Design

To test our hypotheses, we created a coffee company called Bean Brigade that sells single-origin coffee online via a publicly accessible website.¹ In addition to the website, we created a social media presence for the company on Twitter (now known as “X”) several months before the experiment. Figure 1 shows the Bean Brigade website.

[‘Insert Figure 1 Here’]

Using Bean Brigade, we conducted a preregistered experiment on Prolific—an online platform for experiments—with pairs of people who know each other (such as spouses, friends, or acquaintances) and randomly assigned them to one of four (2×2) main study conditions.² The first variable determining study condition was whether the subject saw a CEO statement supporting gun rights. In the non-activism (control) condition, respondents were shown a product page from the Bean Brigade website and told the following:

¹ The website, at beanbrigadecoffee.com, was accessible for the duration of the study.

² See https://osf.io/a5qu3?view_only=5c51b1bb40f349f4bcf6483e87a5bb3e and https://osf.io/bhq75?view_only=f19c97f95378462596e8e0ae07574009 for an anonymous version of the preregistration and pre-analysis plans (there are two pre-registrations because the study included a main survey and a follow-up survey; see Section 3.2). The first pre-registration includes two hypotheses (labelled H3–H4) that we omit from this paper because we received too few responses from the subgroups needed to test the predictions. Hypotheses in the second pre-registration relate to mechanisms and are discussed in Section 6.2 rather than the above theory section.

Bean Brigade is a premium coffee company committed to sourcing the finest beans and roasting them to perfection. Whether you prefer light and fruity blends, bold roasts, or decaf, Bean Brigade offers a premium coffee for you.

In the activism condition, respondents received the same information about Bean Brigade followed by a social media message from the CEO supporting gun rights that read as follows:

Citizens have a right to bear arms and defend their families. Restricting guns infringes the rights of responsible gun owners and won't fix gun violence. It's government overreach and an attack on constitutional rights. We must defend the Second Amendment.

Figure 2 shows the survey for respondents in the activism condition.

[‘Insert Figure 2 Here’]

The second variable determining study condition was whether the subject made a private or public—i.e., observable to their study partner—choice between winning a 12-ounce bag of Bean Brigade coffee or a bonus payment (see Figure 3). The chance of winning was 1 in 12 for both, and the amount of the bonus randomly varied between \$0.25 and \$4. Subjects in the public choice condition were told that their choice would be shared with their study partner:

Your choice here and the information about Bean Brigade coffee will be shared with your [partner]. Your answers to previous questions, however, are private and will not be shared. If you don't want us to share the below choice with your [partner], you may close the survey.³

Subjects in the private condition were given the same choice without seeing a message about the observability of their choice but were told earlier in the survey that responses to

³ The word “[partner]” in the statement was replaced with an appropriate term for the pair of participants (such as “spouse” or “friend”).

questions were private. Although our study design nearly ensures that people in the public choice condition would perceive their decision as observable, it does not guarantee that those in the private condition would not broadcast their choice to others. We therefore conducted a follow-up survey (explained below in Section 3.2) asking subjects how the potential reaction of their partner affected their own choice and whether they discussed their decision. The analyses (in Sections 6.2–6.3) indicate that manipulating the observability of the choice in the survey greatly shifted the probability that the choice was observable and that those in the private choice condition had little desire to signal.

[‘Insert Figure 3 Here’]

As Figure 3 shows, respondents were told that the choice between winning a bag of Bean Brigade coffee or a cash bonus payment was real and that we would pay the cost of the coffee and shipping on their behalf. We gave subjects who chose the coffee and won a coupon code redeemable on the Bean Brigade website. Subjects who chose the cash bonus and won were paid via the Prolific platform. The bonus offered to subjects was randomly set (within participant pair) to one of five values: \$0.25, \$0.50, \$1, \$2, or \$4.

3.2 Subject Recruitment, Study Stages, and Randomization

To recruit subjects who know each other and to make their choices observable to someone in their social network, we divided the study into three stages. In the first stage, we asked people to provide the Prolific identifier of someone with whom they were willing to take studies and to classify their relationship with that person. We then invited the people they had identified to also complete this initial survey. Once two people provided each other’s

Prolific identifiers, they were jointly randomized into a study condition and invited to complete another survey. In this second survey, we gave subjects information about Bean Brigade and let them choose between two prizes: a bag of coffee or a bonus payment, as described in Section 3.1 above. After both members of a participant pair completed this stage, we invited subjects to a third survey, which revealed to each subject their partner's choice in the second stage (for those in the public choice condition) and asked several follow-up questions (of all participants).

Figure 4 illustrates each stage of the study, along with the number of participants moving between stages or exiting the study at each stage. For the main survey (stage 2), we received 1,198 responses; for the follow-up survey (stage 3), we received 1,042 responses.⁴ Attrition was low across all stages; 94% of people invited to complete the main study did so, and 91% of people invited to complete the follow-up did so. Completion of the follow-up study was not affected by treatment assignment ($p = 0.99$).

[‘Insert Figure 4 Here’]

4 DATA AND MEASURES

4.1 Dependent Variables

Our main dependent variable is an indicator for whether a subject chose Bean Brigade coffee over the bonus payment in the second survey. We use this variable to test H1–H2.

⁴ Consistent with our pre-registration, we ceased data collection after 90 days. Hence, the final sample is less than the original target of 2,000 responses. We present a power analysis in the online appendix.

In a follow-up survey, we asked people about the importance of several factors for their choice between the coffee and bonus on a 5-point scale ranging from “Not at all important” to “Extremely important.” Subjects rated the following factors: the amount of the bonus payment, the likely quality of Bean Brigade’s coffee, the values and beliefs of Bean Brigade’s CEO, expressing their own beliefs and values, and how their partner would react to their choice. Additionally, we asked people whether they discussed their choice with their partner either before or after the study. We use these variables to further explore potential mechanisms for the results.

4.2 Independent Variables

We have four study conditions—Private Control, Private Activism, Public Control, and Public Activism—capturing whether a subject’s choice was observable to their partner (private versus public) and whether a subject saw a CEO message supporting gun rights (control versus activism). Additionally, we have five possible bonus amounts—\$0.25, \$0.50, \$1.00, \$2.00, or \$4.00—that we use to measure willingness-to-pay for the coffee.

We condition our analyses on participants’ views about gun policy by asking if they believe gun policy in the United States should be stricter, should be less strict, or is about right. This categorical variable allows us to examine interactions between study condition and participants’ views on gun policy. We also asked participants how often they make coffee at home, the nature of their relationship with their study partner (such as spouse, friend, etc.), and the strength of their relationship with their partner, which we measured on a scale ranging from 1 (very distant) to 5 (very close).

4.3 Summary Statistics

Table 1 reports summary statistics by study condition. Most participants believe federal gun laws should be stricter and say the issue is either very or extremely important to them. Only 8.6% report never making coffee at home, and about 62% participated in the study with a spouse or romantic partner. There is some imbalance across study conditions in views of gun laws, although this difference is not statistically significant at conventional levels ($p = 0.09$) and is accounted for in our analyses, which separately analyze treatment effects by this variable (see Section 5).

[‘Insert Table 1 Here’]

5 ANALYSIS

5.1 Discrete Choice Model

We analyze the results of our experiment using a random utility model of demand:

$$\begin{aligned}
 U_i^C &= V_i^C + \epsilon_i^C \\
 U_i^B &= V_i^B + \epsilon_i^B \\
 V_i^C &= \delta_C + \gamma' X_i + \beta_1' Guns_i + \beta_2' Guns_i \times Activism_i + \beta_3' Guns_i \times Public_i \\
 &\quad + \beta_4' Guns_i \times Activism_i \times Public_i \\
 V_i^B &= \alpha \ln(Price_i)
 \end{aligned} \tag{1}$$

where U_i^C is subject i 's utility of choosing the coffee, and U_i^B is the utility of choosing the bonus payment (which we refer to as $Price_i$ because it is the amount the subject must forgo to choose the coffee). The variable $Guns_i$ is a vector of indicator variables for the subject's views on federal gun laws, X_i is a vector indicating how often the subject makes

coffee at home, and $Activism_i$ and $Public_i$ are indicators for whether the subject was randomly assigned to see the CEO activism message and make a public purchase decision.

We assume the unobserved components of utility are distributed extreme value; the probability of choosing the coffee thus takes the familiar logistic form (McFadden 1974):

$$P^C(Z_i) = \frac{e^{V_i^C(Z_i) - V_i^B(Z_i)}}{1 + e^{V_i^C(Z_i) - V_i^B(Z_i)}} \quad (2)$$

where Z_i denotes the variables described above. We estimate the parameters of the model in Equation (2) via maximum likelihood and cluster the standard errors by subject pair.

5.2 Treatment Effects and Inference

To test our hypotheses, we first use the estimated parameters of the demand curve in Equation (2) to calculate the average marginal effect of CEO activism for each of the six groups of subjects defined by views on gun control and assignment to the public choice condition. Let \mathcal{G}_{sk} be a set of subjects assigned to the private ($s = 0$) or public ($s = 1$) choice condition who hold views on gun laws $k \in \{\text{about right, less strict, more strict}\}$. The estimate of the effect of CEO activism in group \mathcal{G}_{sk} is then as follows:

$$\hat{\tau}_{sk} = \frac{1}{N_{sk}} \sum_{i=1}^N \mathbb{1}(i \in \mathcal{G}_{sk}) [P^C(Activism_i = 1, Z_i) - P^C(Activism_i = 0, Z_i)] \quad (3)$$

where N_{sk} is the number of subjects in group \mathcal{G}_{sk} and $\mathbb{1}(\cdot)$ is an indicator for belonging to the group. Arranging these estimates in a vector, $\hat{\tau}$, we then test H1 using a Wald test of the joint null that CEO activism has no effect in all groups: $\hat{\tau} = \mathbf{0}$.

To test the hypothesis that the effects of CEO activism are the same regardless of whether the choice of coffee is private or public (H2), we take differences in the activism treatment effect across the private/public conditions ($\hat{t}_{1k} - \hat{t}_{0k}$) and perform a Wald test of the joint null that these differences in treatment effects are zero for all three groups of subjects defined by their views of gun laws.

5.3 Willingness-to-Pay for CEO Activism

Although simple differences in means across our randomly assigned study conditions provide consistent estimators of treatment effects, adopting the logit specification for demand in Equation (2) allows us to quantify, in dollar terms, subjects' willingness-to-pay for CEO activism supporting gun rights (which is expected to be negative for subjects favoring stricter gun laws and positive for subjects favoring less strict gun laws). To do so, we calculate the change in price (the bonus amount) that would be required to leave a subject indifferent between accepting their least preferred coffee (for example, the coffee company whose CEO supports gun rights; hereafter, gun rights coffee) and receiving their most preferred coffee (for example, a coffee company with a neutral CEO; hereafter, neutral coffee). To illustrate, the change in price necessary to compensate someone who favors stricter gun laws for accepting the gun rights coffee is the value of E^* that solves

$$\begin{aligned}
 P_k^C(\text{Activism} = 1, \text{Price} = \text{Price} + E^*, Z) &= P_k^C(\text{Activism} = 0, \text{Price}, Z) \\
 \Rightarrow \alpha \ln(\text{Price}) &= \alpha \ln(\text{Price} + E^*) + \gamma \\
 \Rightarrow E^* &= \text{Price} \left(e^{\frac{\beta_2' \text{Guns} + \beta_4' \text{Guns} \times \text{Public}}{-\alpha}} - 1 \right)
 \end{aligned} \tag{4}$$

If we assume that people who support stricter gun laws dislike the gun rights coffee, then changing the price of that coffee by E^* would leave these consumers as well off as changing to the neutral coffee.

6 RESULTS

[‘Insert Figure 5 Here’]

Before discussing estimates of the logit model in Equation (2), we present mean outcomes by study condition and subjects’ views of gun laws in Figure 5. For people who think federal gun laws are “about right,” rates of choosing the coffee are similar across study conditions. People who think gun laws should be less strict, however, are more likely to choose the coffee when told that the company’s CEO supports gun rights. In contrast, people who favor stricter gun laws are less likely to choose the coffee when told that the company’s CEO supports gun rights. When the choice is private (public), people who favor less strict gun laws are 20 (12) percentage points more likely to choose the activist coffee. People who favor stricter gun laws are 18 (6.5) percentage points less likely to choose the activist coffee.

[‘Insert Table 2, Figure 6, and Table 3 Here’]

Table 2 reports estimates of the logit model for demand from Equation (2), and Figure 6 shows the estimated demand curves by study condition and views of gun laws. Table 3 shows estimates for the treatment effect of CEO activism based on Equation (3). The estimates of treatment effects from the logit model in Table 3 indicate that CEO activism supporting gun rights boosts purchases among people who favor less strict gun

laws and decreases purchases among people who favor stricter gun laws. These effects are largest in the private-purchase condition. In the private (public) purchase condition, those favoring less strict gun laws are 25 (9) percentage points more likely to choose the coffee when the CEO is described as supporting gun rights. These differences, however, are not statistically significant owing to the limited number of people in this group ($N = 68$). Subjects in favor of stricter laws, in contrast, are 18 (6) percentage points less likely to choose the coffee in the private (public) purchase condition when the CEO is described as publicly supporting gun rights, with the difference in the private purchase condition being highly statistically significant ($p = 4.77 \times 10^{-5}$). Among those who think gun laws are about right, exposure to CEO activism has little effect on purchase rates in either the private or public purchase condition.

A joint test rejects the null that all treatment effects in Table 3 are zero ($p = 0.002$); thus, the hypothesis that CEO activism affects purchase rates (H1) is supported. A joint test comparing the treatment effects in the private and public purchase conditions, however, fails to reject the null of equal treatment effects across these conditions ($p = 0.31$), indicating a lack of support for the hypothesis that purchase observability moderates the effects of CEO activism (H2). Furthermore, the directional change in the treatment effect estimates between the private and public purchase conditions suggests that observability *attenuates* the effects of CEO activism, a pattern that is inconsistent with the notion that political consumerism is mostly motivated by a desire to signal one's political beliefs or affiliation to others. While our findings cannot rule out all signaling motives, they do support the idea that ideology alone is a sufficient motive for political

consumerism, and we provide further support for these claims below by examining subjects' stated reasons for their choices. This pattern matters for business strategy and marketing because it suggests that the risk of boycotts from engaging in CEO activism is not confined to a subset of companies; people will boycott products for political reasons even when their choices are not observable to others.

6.1 Willingness-to-Pay

[‘Insert Table 4 Here’]

Table 4 shows estimates of willingness-to-pay for (or to avoid) CEO activism; these estimates reflect the price change necessary to compensate someone for not receiving their preferred product. For example, people who favor weaker gun laws prefer the gun rights coffee (Figures 5–6); they would need to be offered a price reduction of about 83% (\$0.83 on the dollar) to compensate for switching to a neutral coffee. In contrast, those who favor stricter gun laws prefer the neutral coffee and would need to be offered a price reduction of 75% to switch from the neutral coffee to the gun rights coffee. Given the small sample of people who favor less strict gun laws, our willingness-to-pay estimates for this group come with a large confidence interval (95% CI = [-0.99, 1.20]) and are not statistically distinguishable from zero. Estimates for those who favor stricter gun laws, however, are more precise and the confidence interval does not include zero (95% CI = [-0.90, -0.49]), indicating a strong negative willingness-to-pay for the gun rights coffee.

6.2 Consumers' Stated Reasons for their Choices

Figure 7 reports subjects' stated reasons for their choice, which were measured in a follow-up survey (see Section 3.2). Consistent with the treatment effect estimates in Table 3, people in the CEO activism conditions ascribe more importance to the CEO's values and to expressing their own values, compared to people in the control conditions. Subjects also report across all study conditions that how their study partner (spouse, friend, etc.) might react to their choice was not an important factor. When the purchase decision is private, the treatment effects of activism (differences in mean importance) for "CEO Values" and "Expressing My Values" are 0.82 (95% CI = [0.58, 1.09]) and 0.35 (95% CI = [0.10, 0.63]), respectively. When the purchase decision is public, the respective differences are 0.86 (95% CI = [0.56, 1.14]) and 0.57 (95% CI = [0.26, 0.84]).⁵ For "Peer Reaction" the treatment effects in the private and public purchase conditions are -0.11 (95% CI = [-0.33, 0.10]) and -0.07 (95% CI = [-0.34, 0.18]), respectively. Neither effect, nor the difference between the public and private effects, differs significantly from zero.

[Insert Figure 7 Here]

While this pattern is inconsistent with signaling to others as an explanation for political consumerism, the fact that "Expressing My Values" is a significantly more important reason for people's choices in the CEO activism conditions—including the

⁵ We report treatment effect estimates and standard errors for each reason in the online appendix. We calculate these differences across conditions while controlling for views of gun laws using methods described in Section 6.1 of Athey and Imbens (2017), and we calculate confidence intervals and standard errors using a non-parametric bootstrap to adjust for clustering by participant pair. In our pre-registration, we planned to also control for price (the bonus amount), but could not do so because some combinations of price and views of gun laws had too few observations per study condition.

private choice conditions—indicates that expressive and self-signaling motives are important (John and Klein 2003, Klein et al. 2004, Dubé et al. 2017).

Finally, the results in Figure 7 show that the perceived quality of the coffee was not more important to people in the treatment conditions (see the online appendix for estimates). This suggests that people are not using the CEO's political beliefs to make inferences about product quality but are instead more influenced by the CEO's values and a desire to express their own values.

6.3 Discussing the Study and Purchase Observability

Our experimental manipulation of purchase observability relies on subjects not choosing to disclose their choice themselves, or at least not doing so in large numbers. If subjects in the private purchase condition regularly discuss their choices, then the private condition is effectively public.⁶ To assess whether people regularly discussed their choices regardless of study condition, in the follow-up survey, we asked if people discussed their choice before and/or after both members of a pair had completed the study. Figure 8 reports the rates at which people say they discussed their choice of coffee or bonus payment by condition.

['Insert Figure 8 Here']

The results indicate that about 30% of all participants discussed the study with their partner at some point. Therefore, the private purchase condition was private as intended

⁶ The public condition is always public regardless of subjects' behavior because we reveal people's choices in that condition during a follow-up survey. It does not matter if people in the public condition discuss their choices before we reveal them.

for most subjects. Furthermore, there are essentially no differences across study conditions in whether people discuss their choice with their partner. If people wished to signal their purchase or non-purchase of the gun rights coffee to their partner, we would expect rates of discussing the study to be higher in the private activism condition than in the private control condition. There is no statistically significant difference, however, across these conditions, and directionally we observe the opposite pattern: people in the private activism condition are less likely (25%) to discuss their choice with their partner than people in the private control condition (30%), a pattern that directionally mirrors the main treatment effects (Table 3), which suggest observability attenuates political consumerism.

6.4 Treatment Effect Heterogeneity

Figure 9 shows estimates of treatment effects by relationship attributes and price. We limit these exploratory analyses to the subsample of 900 subjects who favor stricter gun laws because this is the largest group in the data and the group for which the average treatment effect was statistically significant.⁷ To estimate these treatment effects, we first interact each potential moderator (relationship type, relationship strength, price) with the indicators for study condition in Equation (1), then estimate treatment effects for each level of the moderator as in Equation (3). To classify relationship strength, we split the sample at the median and label relationships of below-median strength as “weak.”

[‘Insert Figure 9 Here’]

⁷ In our preregistration, we proposed examining whether partners have similar beliefs regarding gun laws. We omit this analysis because 83% of subjects favoring stricter gun laws report that their partner holds similar views, and only 2% report their partner favors less strict gun laws, which leaves insufficient variation for analysis (the remaining 15% say their partner thinks gun laws are “about right”).

For all three variables, a Wald test fails to reject the null hypothesis that the interaction terms are zero. The test for the price interaction, however, is marginally significant ($p = 0.087$), and the pattern of estimates is intriguing. As Bronnenberg and Dubé (2023) point out, the value of political consumerism as a signaling device theoretically depends on price. For those favoring stricter gun laws, picking the bonus payment over the gun rights coffee is a better signal when the bonus amount is low. When the bonus offer is high (\$4.00), it is harder for an observer to determine if a consumer declined the coffee for political reasons or simply because the bonus was attractive. Therefore, if signaling is a strong motive for political consumerism, we would expect the treatment effects in Figure 9(c) to be more negative at lower prices (bonus amounts). This is indeed the case, but only in the *private* purchase condition; for those making public decisions, the treatment effects get slightly *more* negative as price increases. Like our main results and subjects' stated reasons for their choices (Figure 7), these patterns are consistent with ideological and self-signaling motives for political consumerism (Dubé et al. 2017, Bronnenberg and Dubé 2023), but they are inconsistent with the notion that political consumerism is primarily driven by a desire to signal one's social identity, beliefs, or group affiliation to others.

7 CONCLUSION

Understanding why consumers adjust their purchasing behaviors is vital to assessing the strategic implications of firms and executives taking stances on controversial social and political issues. Although several studies show that consumers respond to such stances

(Chatterji and Toffel 2019, Conway 2023, Homroy and Gangopadhyay 2023, Hou and Poliquin 2023, Schoenmueller et al. 2023, Liaukonyte et al. 2023a), we know relatively little about *why* CEO activism affects consumers. Two primary drivers of interest are ideology and signaling, but separating the two is difficult (Bronnenberg and Dubé 2023).

We experimentally distinguish ideology and signaling as two potential mechanisms through which CEO activism affects consumers. We find that CEO activism supporting gun rights reduces demand among consumers who favor stricter gun control, and this effect does not significantly depend on whether the purchase is observable to someone else. This indicates that personal ideology is a sufficient motive for political consumerism, implying that CEO activism can impel boycotts even of products consumed privately.

Our results, and the distinction between ideology and signaling, have several implications for firm strategy and marketing. First, while the media often gravitates to popular and highly visible consumer brands when discussing CEO activism—such as The Coca-Cola Company on Georgia’s voting law (Shepardson and Kumar 2021), Chick-fil-A on gay marriage (McGregor 2012), and Disney on LGBTQ+ rights (Schwartzel 2022)—our results suggest that even firms producing commodities and less conspicuous products can be affected by speaking on controversial social and political issues.

Second, theories differ regarding the extent to which consumers care about the credibility of corporate sociopolitical activism. If consumers are ideologically motivated and prefer to purchase from co-partisans, then they will discount messages perceived as inauthentic cheap talk (Melloni et al. 2023). Political consumerism driven mainly by signaling, however, can affect sales even if consumers do not believe firms are credibly

committed to a cause or behaving to further a political goal. As Wernerfelt (1990) points out, if consumers use purchases mainly to signal, then even unverifiable statements can serve as focal points for coordinating purchases. What matters in such an environment is not whether a brand of beer or shoes is committed to liberal or conservative causes but whether consumers collectively agree that drinking or wearing the product identifies membership in a political group. While our results cannot completely rule out signaling, they do suggest that consumer ideology and a CEO's values affect purchases, and thus lend support to theories of marketing and strategy that emphasize authenticity and credibility in political communication (Melloni et al. 2023).

Third, for businesses, understanding whether consumers are driven by mostly ideology or signaling is crucial for formulating an appropriate response. When consumers are ideologically motivated and want to change firm behavior, reversing the consequences of CEO activism may require changes in business practices. If consumers mostly care about signaling, however, brands may find it more fruitful to invest in marketing to change their image and brand association (Wernerfelt 1990).

There are two limitations worth noting. First, because the company we created for this experiment, Bean Brigade, exclusively sells products online, it is possible that the results will not mirror consumer behavior in person. When shopping online, consumers may be more likely to switch brands in response to CEO activism, as they do not need to physically go to a different store. In this case, our results may overestimate the effect of CEO activism relative to the true effect in brick-and-mortar stores. Alternatively, people who shop online may be more loyal than those who shop in person (Danaher et al. 2003),

suggesting an opposite effect. Examining the effects of sales channels, switching costs, and market power on political consumerism may yield valuable insights (Liukonyte et al. 2023a). Second, like most studies of CEO activism, we focus on a single issue (gun policy). Examining multiple issues simultaneously and how issue attributes affect consumer behavior is a potential area for future research (Conway 2023, Mohliver et al. 2023).

Recent research has emphasized CEO activism as a component of business strategy (Bondi et al. 2023, Melloni et al. 2023, Mohliver et al. 2023), indicating that CEOs can affect stakeholders such as consumers (Hou and Poliquin 2023, Liukonyte et al. 2023a), employees (Burbano 2021, Wowak et al. 2022), and investors (Bhagwat et al. 2020, Mohliver and Hawn 2021, Mkrtchyan et al. 2023). We hope our results contrasting two dominant theories of why consumers adjust in response to CEO activism can help scholars better understand the mechanism through which CEOs affect consumers and further illuminate the implications of CEO activism for marketing and firm strategy.

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TABLES

Table 1. Summary statistics by study condition

Characteristic	Private Control N = 298	Private Activism N = 308	Public Control N = 288	Public Activism N = 304	p-value
Views on gun laws					0.093
About right	51 (17%)	45 (15%)	78 (27%)	56 (18%)	
Less strict	12 (4.0%)	15 (4.9%)	19 (6.6%)	22 (7.2%)	
More strict	235 (79%)	248 (81%)	191 (66%)	226 (74%)	
Importance of gun issue					>0.99
Not at all important	2 (0.7%)	5 (1.6%)	5 (1.7%)	2 (0.7%)	
Not too important	23 (7.7%)	15 (4.9%)	19 (6.6%)	23 (7.6%)	
Somewhat important	73 (24%)	76 (25%)	72 (25%)	84 (28%)	
Very important	102 (34%)	119 (39%)	99 (34%)	93 (31%)	
Extremely important	98 (33%)	93 (30%)	93 (32%)	102 (34%)	
Make coffee at home					>0.99
Never	29 (9.7%)	27 (8.8%)	29 (10%)	19 (6.3%)	
Rarely	38 (13%)	37 (12%)	35 (12%)	32 (11%)	
Occasionally	37 (12%)	48 (16%)	40 (14%)	64 (21%)	
Fairly often	61 (20%)	56 (18%)	41 (14%)	46 (15%)	
Very often	133 (45%)	140 (45%)	143 (50%)	143 (47%)	
Partner relationship					>0.99
Spouse/Romantic partner	181 (61%)	187 (61%)	193 (67%)	187 (62%)	
Non-Family	41 (14%)	58 (19%)	34 (12%)	56 (18%)	
Family	76 (26%)	63 (20%)	61 (21%)	61 (20%)	
Relationship strength	4.74 (0.52)	4.71 (0.62)	4.81 (0.48)	4.71 (0.60)	0.77
Price	1.62 (1.46)	1.56 (1.39)	1.60 (1.40)	1.56 (1.33)	>0.99

Note: For categorical variables, cells contain number (percentage) of observations; for *Relationship strength* and *Price* (the bonus amount), cells contain the mean (standard deviation). We calculate *p*-values using a bootstrapped, non-parametric permutation test that accounts for clustering by participant pair and adjust for multiple testing via Holm's correction (Holm 1979).

Table 2. Demand estimates

Coefficient	Estimate	SE	p-value
Log price	-0.686	0.078	<0.001
Views on gun laws × Activism			
About right	-0.040	0.478	0.93
Less strict	1.224	0.948	0.20
More strict	-0.956	0.245	<0.001
Views on gun laws × Public			
About right	-0.629	0.443	0.16
Less strict	0.452	1.010	0.66
More strict	-0.621	0.248	0.012
Views on gun laws × Activism × Public			
About right	-0.051	0.644	0.94
Less strict	-0.782	1.240	0.53
More strict	0.581	0.363	0.11
Views on gun laws			
Less strict	-0.811	0.854	0.34
More strict	-0.365	0.374	0.33
Drinks coffee at home			
Rarely	1.473	0.458	0.001
Occasionally	1.751	0.454	<0.001
Fairly often	2.252	0.452	<0.001
Very often	2.558	0.431	<0.001
Intercept	-2.094	0.509	<0.001
Log-likelihood	-645		
Observations	1,198		

Note: Standard errors are clustered by participant pair.

Table 3. Treatment effect estimates

Views on Gun Laws	$\hat{\tau}_{sk}$	SE	95% CI	p-value	N
<i>Private Purchase</i>					
About right	-0.01	0.10	[-0.20, 0.19]	0.93	96
Less strict	0.25	0.18	[-0.11, 0.61]	0.17	27
More strict	-0.18	0.04	[-0.26, -0.09]	<0.001	483
<i>Public Purchase</i>					
About right	-0.02	0.08	[-0.18, 0.14]	0.83	134
Less strict	0.09	0.16	[-0.22, 0.40]	0.58	41
More strict	-0.06	0.05	[-0.15, 0.03]	0.16	417

Note: Treatment effect estimates ($\hat{\tau}_{sk}$) are calculated using Equation (3) and estimated parameters of the demand model shown in Table 2. Standard errors are clustered by participant pair.

Table 4. Willingness-to-pay estimates

Views on Gun Laws	Parameter Mean	Mean of Draws	Std. Dev. of Draws	Median of Draws	95% CI
Less strict	-0.83	-0.59	0.78	-0.83	[-0.99, 1.20]
About right	-0.06	0.23	1.07	-0.07	[-0.76, 3.03]
More strict	-0.75	-0.74	0.10	-0.75	[-0.90, -0.49]

Note: Estimates are in dollars, refer to willingness-to-pay at a price of \$1.00, and are based on 1,000 draws of the parameters in Table 2 using the method of Krinsky and Robb (1986). Values for *Parameter Mean* refer to willingness-to-pay at the parameter estimates in Table 2. For each group, we report the price change necessary to compensate a consumer for switching from their most preferred to their least preferred product. For those favoring less strict gun laws, the preferred product is the gun rights coffee, while for those favoring stricter gun laws, the preferred product is the neutral (control) coffee. Both groups would require a price reduction to switch to the other product, which is why we report negative price changes for both.

FIGURES

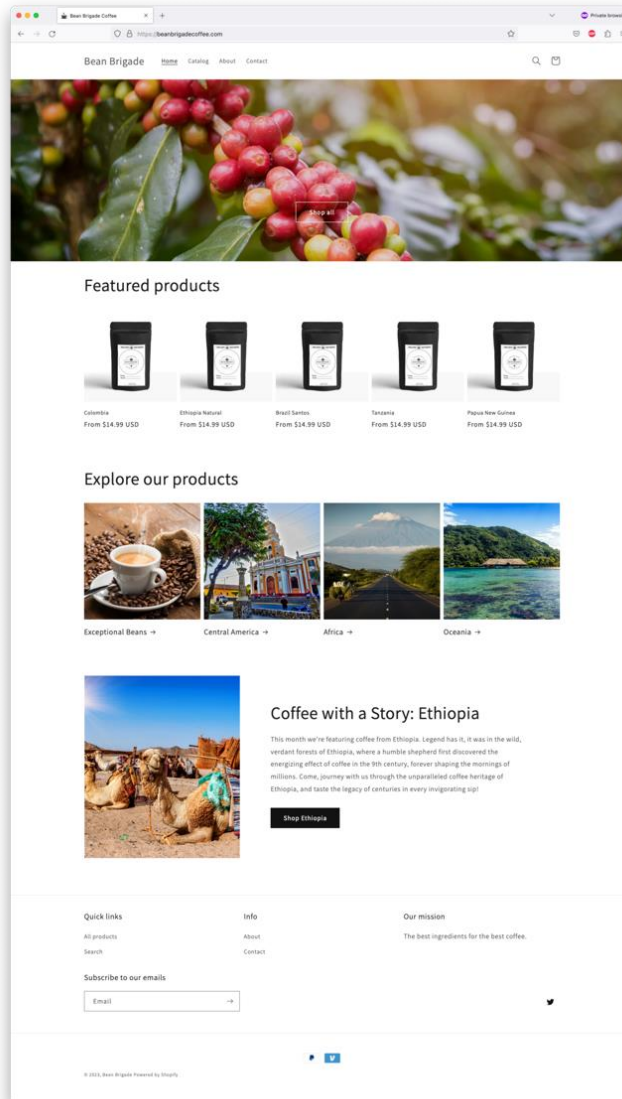


Figure 1. Website for Bean Brigade

POLITICAL CONSUMERISM: IDEOLOGY OR SIGNALING?

We'd like to learn more about your preferences for coffee and your opinion of a coffee company called Bean Brigade.

The following is a sample product page from the Bean Brigade website. Please examine the product page and the following information about the company.



About Bean Brigade

Bean Brigade is a premium coffee company committed to sourcing the finest beans and roasting them to perfection. Whether you prefer light and fruity blends, bold roasts, or decaf, Bean Brigade offers a premium coffee for you.

Recently, Bean Brigade's CEO has been outspoken about the importance of gun rights. The CEO posted the following statement on social media:



Figure 2. CEO activism condition

We'd like to give you the opportunity to win a bag of Bean Brigade coffee!

You can choose between entering to win a **free 12oz bag of coffee and free shipping** or a **bonus payment** of \$0.25. In either case, your chance of winning will be 1 in 12.

The reward is real. If you choose the coffee and win, you will immediately receive a coupon code at the end of this survey along with a link to the Bean Brigade website to redeem your coffee. No credit card is required, and we will pay Bean Brigade on your behalf. When redeeming the coupon, the merchant will not see your responses to this survey, and we will not see shipping information you provide to the merchant. If you choose the bonus payment and win, \$0.25 will be deposited into your Prolific account within 4–7 days of study approval.

Your choice here and the information about Bean Brigade coffee will be shared with your spouse. Your answers to previous questions, however, are private and will not be shared. If you don't want us to share the below choice with your spouse, you may close the survey.



Premium Bean Brigade Coffee	Bonus Payment
 <p>Free 12oz bag of coffee and free shipping. Retail price: \$14.99 Package size: 12oz</p>	 <p>Bonus payment of \$0.25.</p>
Choose premium, Bean Brigade coffee	
Choose bonus payment	

Figure 3. Public choice condition

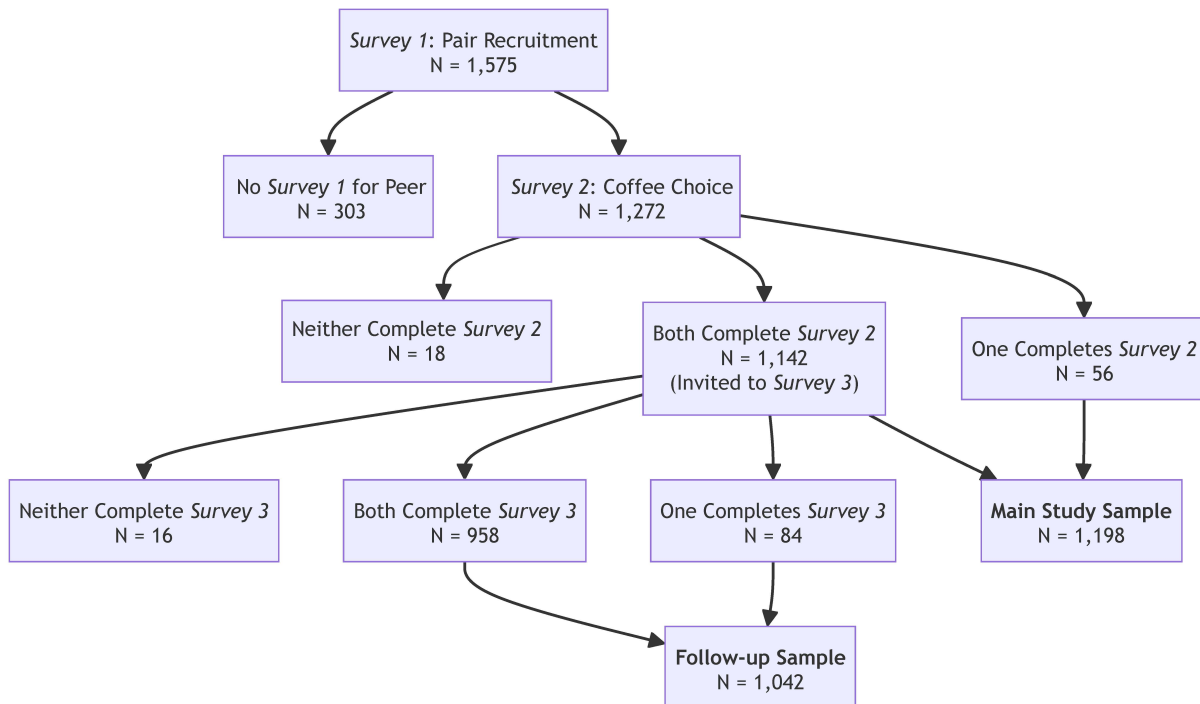
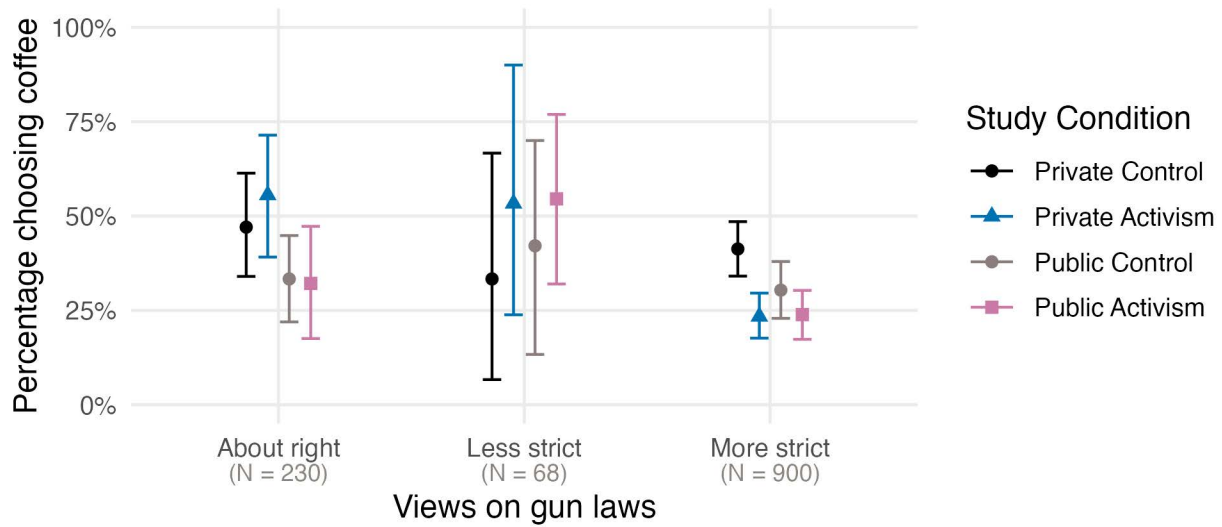
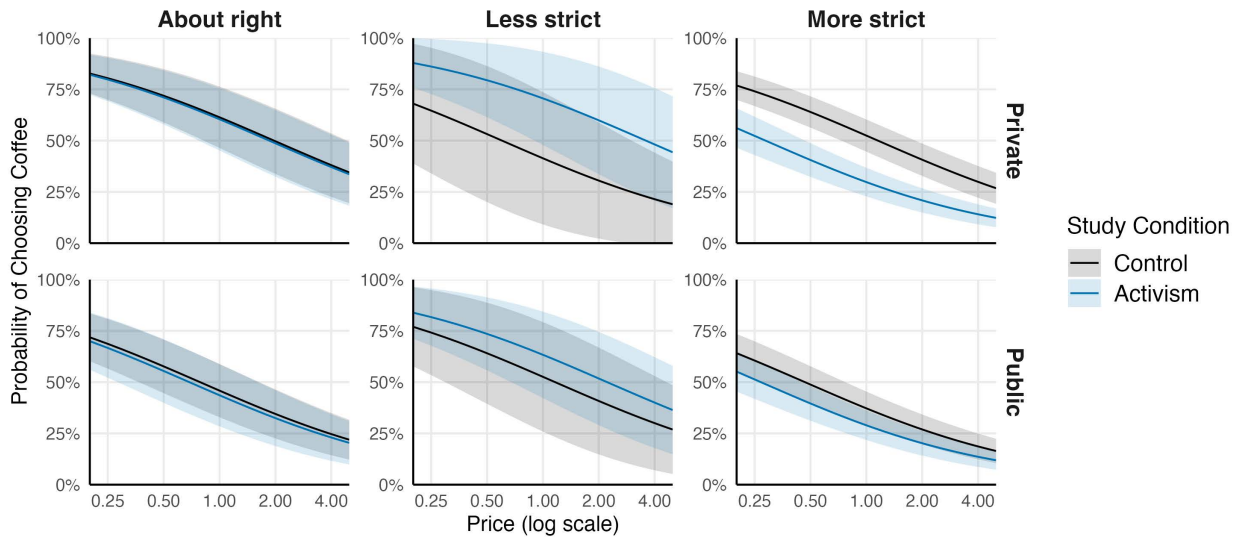


Figure 4. Study stages and subject counts



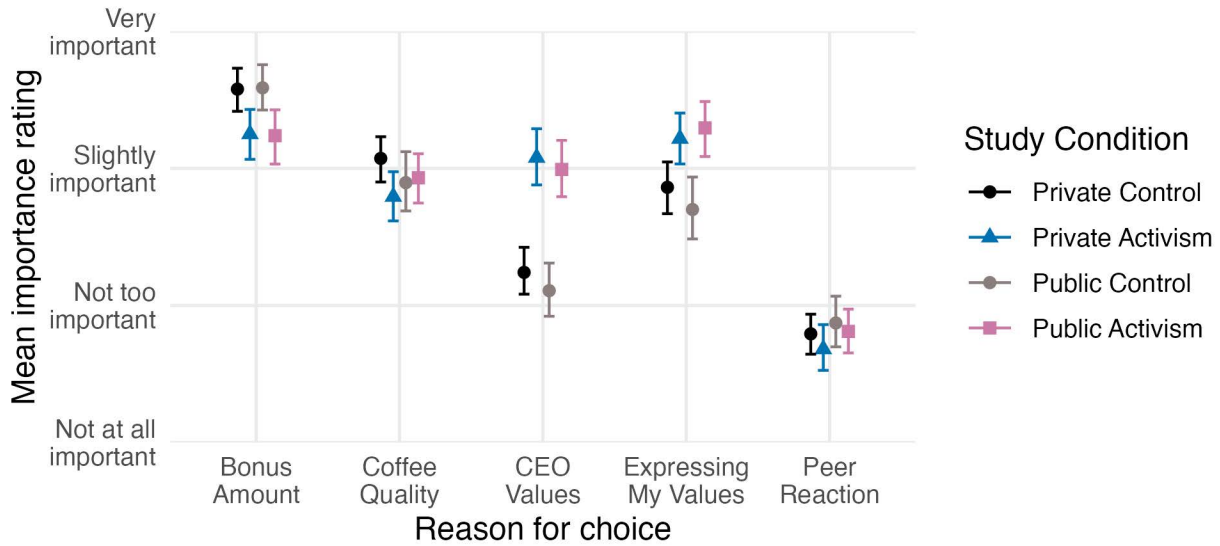
Note: Error bars represent 95% confidence intervals calculated via a non-parametric clustered bootstrap where the clusters are participant pairs.

Figure 5. Mean outcomes by study condition and views of gun laws



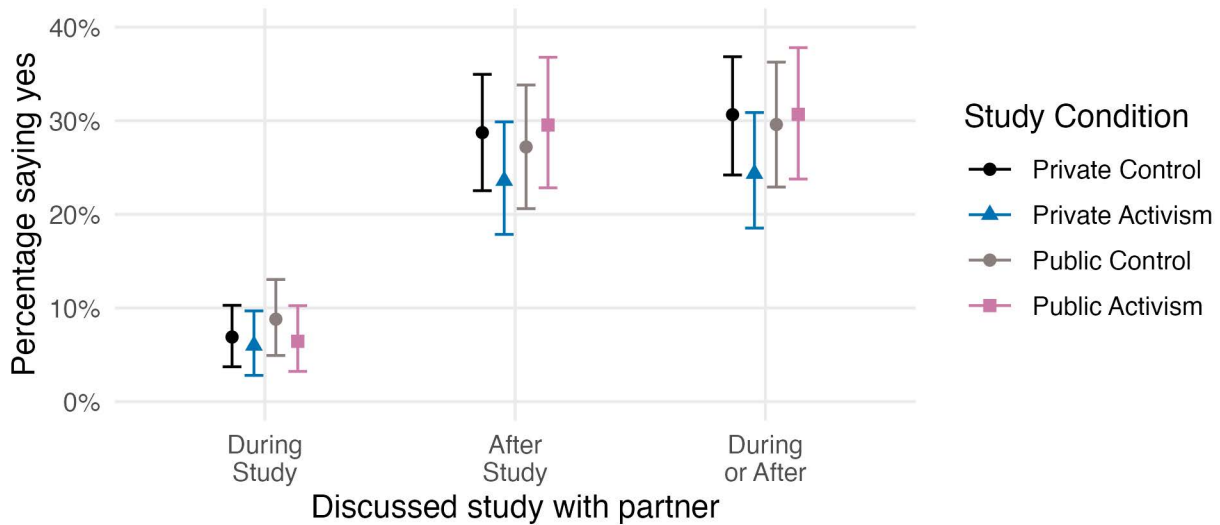
Note: Shaded regions represent 95% confidence intervals.

Figure 6. Estimated demand curves



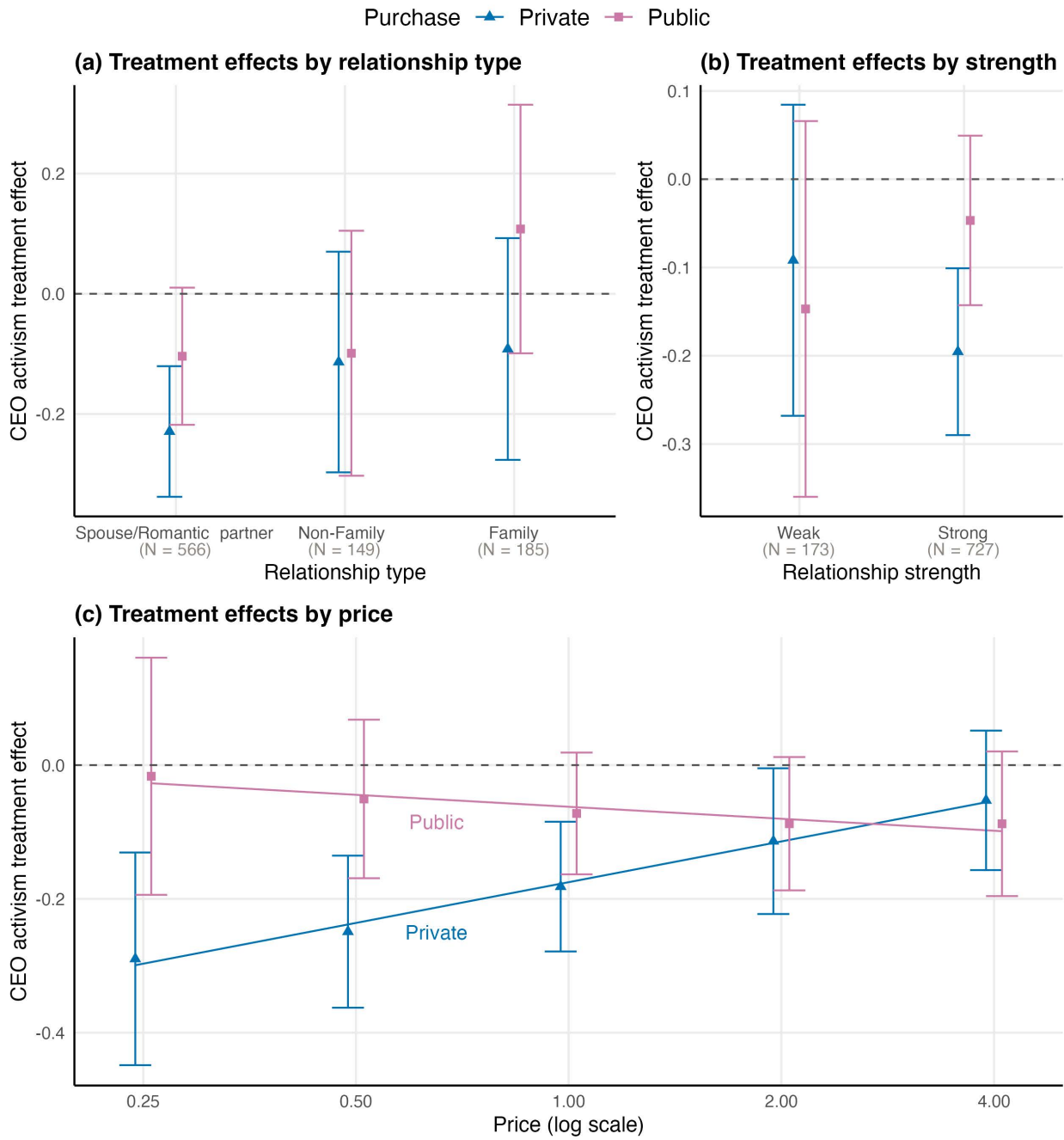
Note: Error bars represent 95% confidence intervals calculated via a non-parametric clustered bootstrap where the clusters are participant pairs.

Figure 7. Stated reasons for choices



Note: Error bars represent 95% confidence intervals calculated via a non-parametric clustered bootstrap where the clusters are participant pairs.

Figure 8. Rates of discussing the study



Note: Sample limited to participants favoring stricter gun laws. Error bars represent 95% confidence intervals and are based on standard errors clustered by participant pair.

Figure 9. Effects of CEO activism by price and relationship attributes

Online Appendix

Political Consumerism: Ideology or Signaling?

A POWER ANALYSIS

To assess the statistical power of our study design, we simulated data from the random utility model of demand (Equations 1–2 in the main text) and then performed our main, pre-registered hypothesis tests (H1–H2) under various assumptions about effect sizes. Simulation is advantageous given the complexity of the model and the need to cluster standard errors to account for correlation within participant pairs.

To simulate the model, we needed data on likely consumer attributes, the price coefficient, and plausible estimates of treatment effects. We used Pew Research Center (2021) data on views of gun laws and assume a large degree of correlation within participant pair in these views. To measure the frequency of drinking coffee and the price coefficient, we gathered 130 responses to a pilot survey and assumed that the distribution of responses and model coefficients would be identical in the main study.¹ Additionally, we assumed a small positive correlation within participant pair in the tendency to make coffee at home.

We calibrated the plausible effect sizes using the results of Liaukonytė et al. (2023), who report a 55.6% increase in purchase probability in heavily Republican counties following conservative CEO activism.² Using pilot data, we set the bonus amounts offered to participants so that about 50% would choose the coffee in the control condition (the true probability ended up being 42%). Based on Liaukonytė et al. (2023), we then assumed that the probability of purchase might be 28 percentage points lower (higher) in the activism conditions for participants favoring stricter (less strict) gun laws. We assumed the effect among those who think gun laws are about right would be about half as large. Using these assumptions, we selected coefficient values for the demand model that would lead to effects similar to or somewhat smaller than those reported in Liaukonytė et al. (2023).

Since Liaukonytė et al. (2023) do not measure the distinct effects of ideology and signaling, we simulated several scenarios. First, we assumed either a medium or large interaction between activism and making a public purchase. Second, we assumed baseline effects of activism that varied from no effect in the private purchase condition to large effects. At one extreme, this means assuming that political consumerism is driven entirely by its observability. At the other extreme, we are assuming that CEO activism has large effects for private purchases and even larger effects for public purchases. Simulation

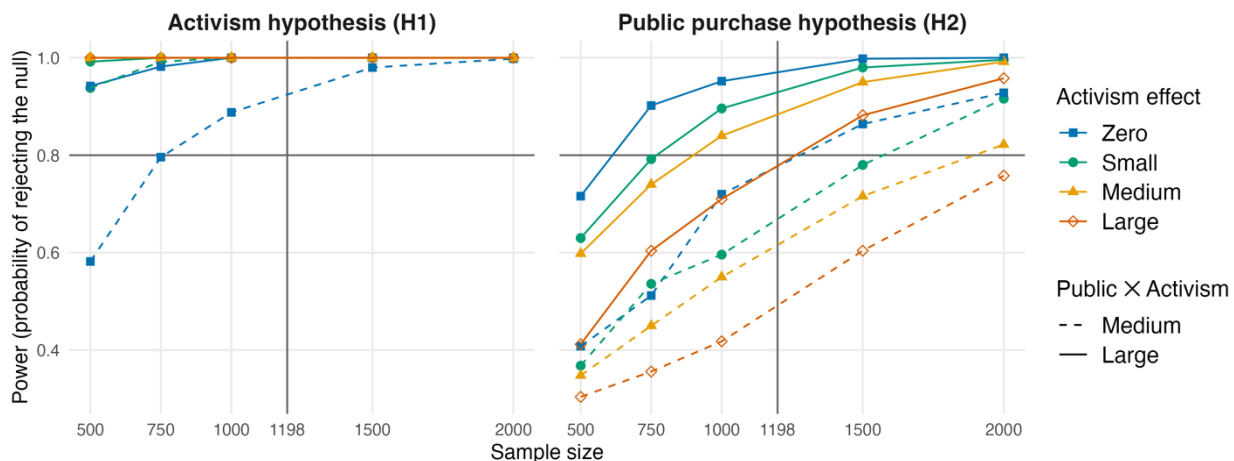
¹ The pilot study did not recruit pairs of participants and did not include any treatments. It was designed solely to measure plausible values for selected model parameters, establish a range of values for bonus amounts, and confirm that people on the Prolific platform would be willing to choose the coffee.

² This estimate likely *understates* the effect for individual Republicans because heavily Republican counties still include many Democrats, who attenuate the average effect for the heavily Republican counties.

suggests that scenarios with a large interaction or large baseline effect produce purchase probabilities consistent with Liaukonytė et al. (2023). Scenarios with a medium interaction effect tend to produce average effect sizes smaller than those in Liaukonytė et al. (2023) unless the baseline effect is also large.

Figure A.1 shows results of the power simulation along with a vertical line at the true sample size ($N = 1,198$). The results suggest that power to detect any effect of CEO activism supporting gun rights in either the private or public purchase conditions (left panel) is above 0.8 for all assumptions about the effect size. Distinguishing between the effects of activism in the private and public conditions (the interaction, right panel), however, is more difficult. Power is at or above the usual threshold of 0.8 for scenarios in which there is a large effect of making an observable purchase.³ Power is more modest for scenarios with only a medium interaction effect but still above the usual power of studies in economics and management research (Askarov et al., Ioannidis et al. 2017).

Overall, our results indicate that our study is well-powered to detect medium to large differences in the effects of CEO activism on private and public purchases, such as those reported in Liaukonytė et al. (2023). Our study, however, would be under-powered to detect small differences or further interaction effects.



Notes: The left plot shows the power for the pre-registered test of the null that CEO activism has no effect on purchases. The right plot shows power for the test of the null that the effect of CEO activism is the same in the private and public purchase conditions. Colors and shape vary in the magnitude of the coefficient on the activism variable, while line type (dashed versus solid) varies in the magnitude of the coefficient on the interaction term between activism and making a public purchase (see Equations 1–2 in main text).

Figure A.1 Power Analysis for Experimental Design

³ At first, the *lower* power for larger main effects in the right panel may seem counterintuitive. The hypothesis, however, concerns the difference between the main and interaction effects. When both are large, it becomes difficult to distinguish them. But when the main effect is zero, and the interaction term is large, it is easy to detect the difference between them. Hence, power decreases as the main effect of activism on private purchases becomes more like the interaction effect for public purchases.

B REASONS FOR CHOICE

Figure B.1 shows subjects' stated reasons for their choices broken down by their views of whether federal gun laws are "about right," should be less strict, or should be more strict. Patterns within each group are generally similar to those shown for the pooled sample in the full text, but reliable separate inferences for the group believing gun laws should be less strict are not possible given the small number of subjects in this group.

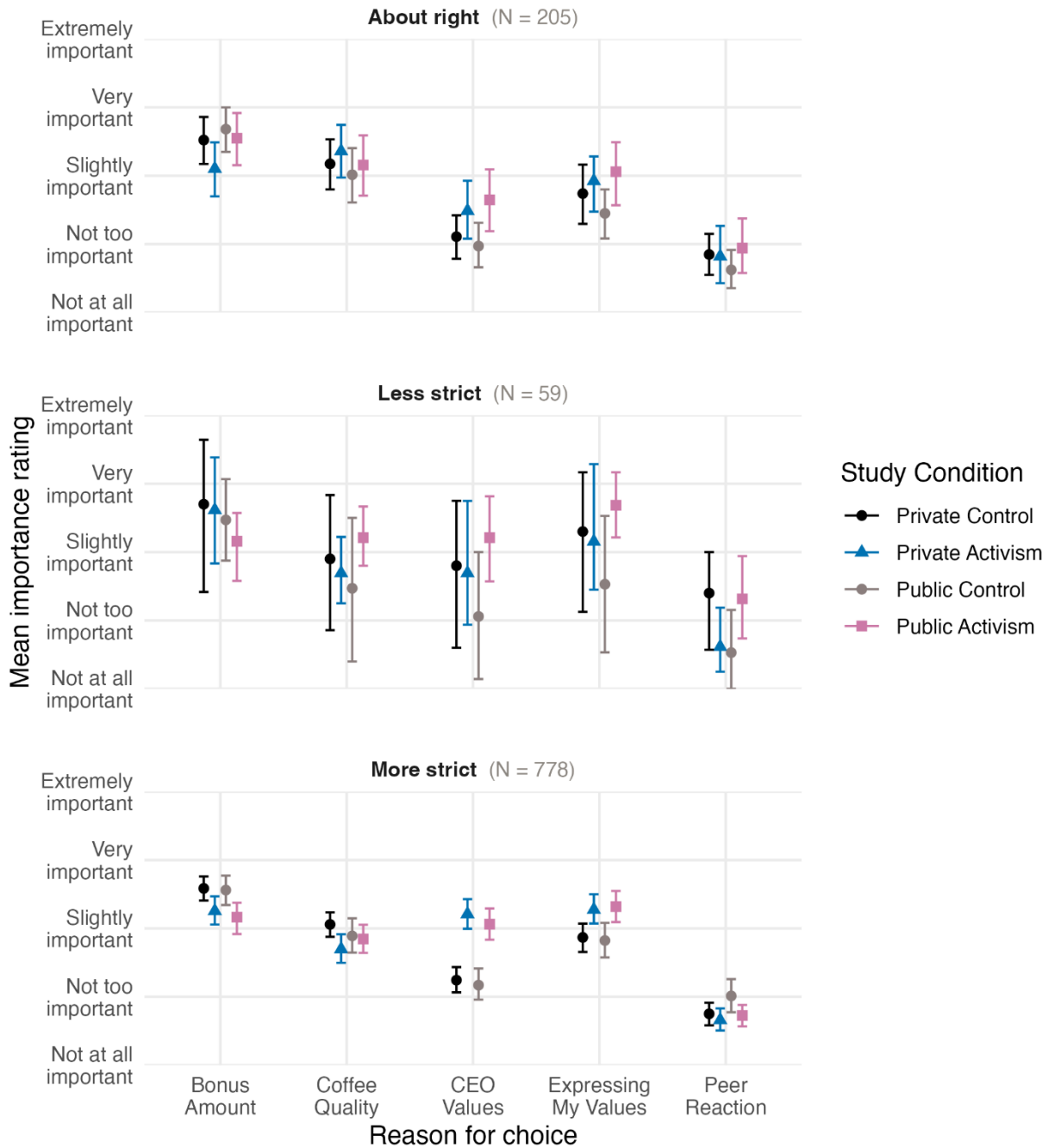
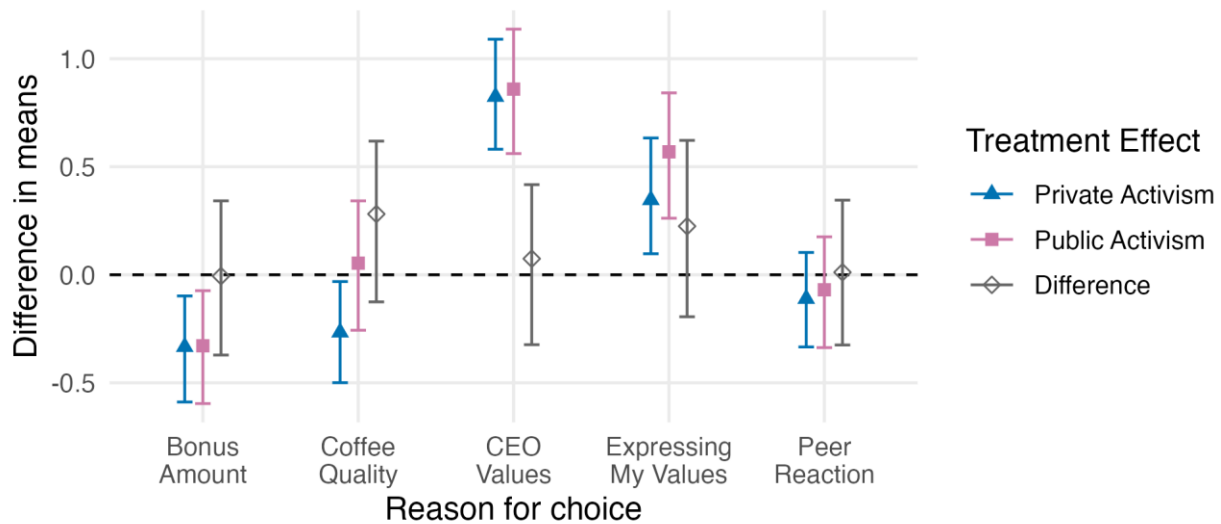


Figure B.1 Stated Reasons for Choice by Views of Gun Laws

Figure B.2 shows estimates for the treatment effects of CEO activism on subjects' stated reasons for their choices. We calculate these effects controlling for views of gun laws with the formulas presented in Sections 4.2, 4.4, and 6.1 of (Athey and Imbens 2017). The large effects for "CEO Values" and "Expressing My Values" are consistent with ideological and self-signaling motives for CEO activism. Meanwhile, the null results for "Peer Reaction" and the lack of a significant difference between the public and private treatment effects suggest that signaling to others is not a prominent motive for political consumerism in our sample.



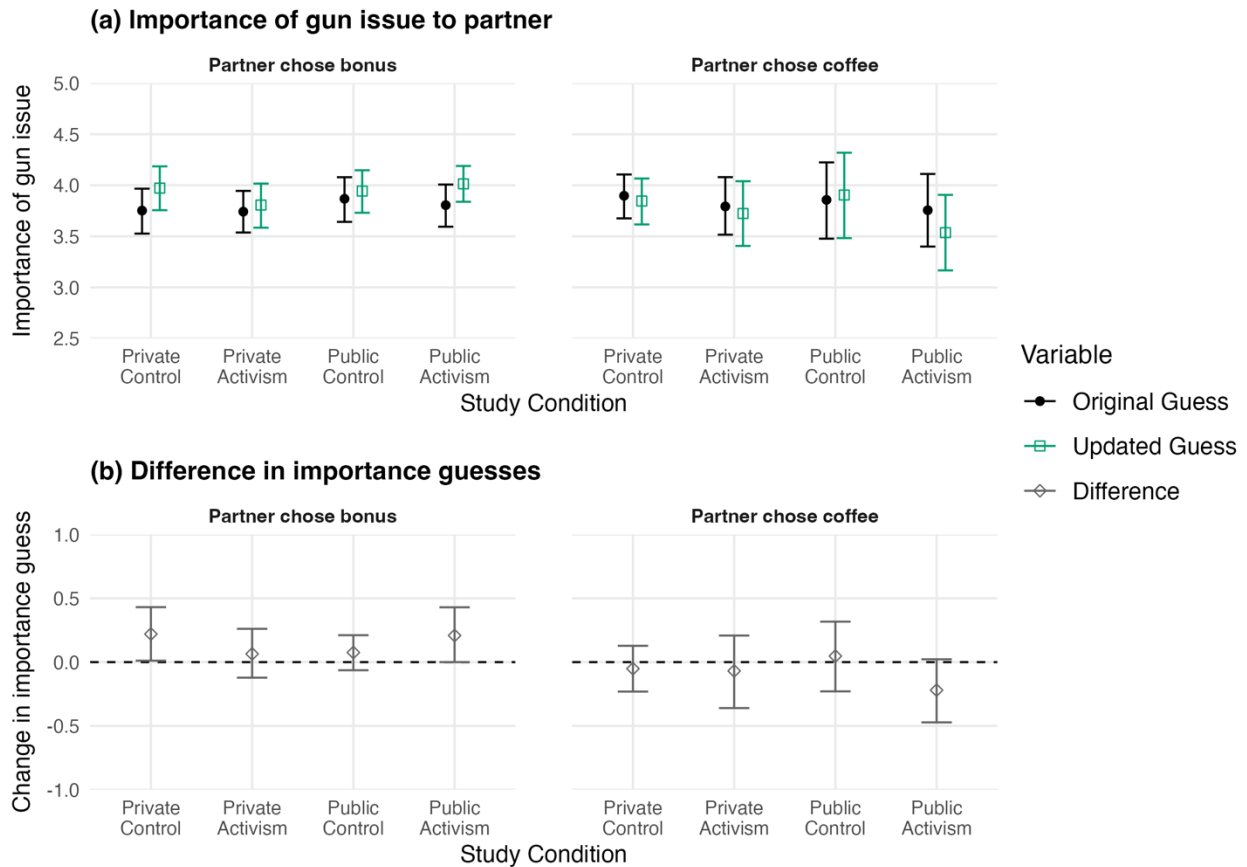
Note: *Difference* refers to the difference between the treatment effects of CEO activism on public and private purchases. Error bars represent 95% confidence intervals calculated via a non-parametric clustered bootstrap where the clusters are participant pairs.

Figure B.2 Treatment Effects of CEO Activism on Reasons for Choice

C DO PEOPLE UPDATE THEIR BELIEFS?

Here, we consider whether people update their beliefs about their partner's views on gun policy after seeing their partner's choice between the bonus and coffee. For subjects not in the public activism condition, seeing a partner's choice should provide little or no information about the person's views on gun policy. In the control conditions, the CEO does not engage in activism supporting gun rights, which makes the coffee neutral. In the private purchase conditions, the partner's decision is not observed. Subjects in the public activism condition, however, could interpret their partner choosing the gun rights coffee as a sign that he or she either supports gun rights or ascribes less importance to the issue of gun control. Similarly, people in this condition who observe their partner choose the bonus payment might assume that gun control is important to their partner or that gun rights are less important.

Although we asked people in both our first-stage recruitment and follow-up surveys to report their partner’s views on gun policy, we unfortunately did not collect enough data to offer a thorough analysis of how people update their beliefs in response to observing their partner’s purchase. In Figure C.1, however, we analyze the subset of the sample whose partners favor stricter gun laws and report not discussing their choice with their partner (for the private activism condition). For this sample, if purchases are effective political signals, we expect people in the public activism condition to say gun policy is more important to their partners after seeing them choose the bonus (the left two plots) and less important after seeing them choose the coffee (the right two plots). This is indeed the case, but the estimates are somewhat imprecise. The positive effect for the public activism condition in the bottom left plot, along with the negative effect in the bottom right plot, are together consistent with the idea that people update their beliefs about other people’s politics based on their purchases. We suggest, however, that future research in larger samples and with better measurement techniques is needed to assess the effectiveness of purchases as signals.



Note: *Difference*, shown in Panel (b), refers to the average within-subject difference between the updated and original guess about how important gun policy is to one’s partner. Error bars represent 95% confidence intervals calculated via a non-parametric clustered bootstrap where the clusters are participant pairs.

Figure C.1 Beliefs about importance of gun issue for partner

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