Can Corporate Sociopolitical Stances Move Employee Politics? Evidence from Campaign Contributions

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Abstract

Firms increasingly take public partisan positions on contested social issues, yet the political spillovers of corporate stance-taking among employees remain poorly understood. This paper studies whether stances reshape employee campaign contributions, distinguishing firm-linked giving through corporate PACs from external partisan giving. A stacked difference-in-differences design across 44 stance events at 33 firms (2018–2022), linking 28 million employee-week observations across LinkedIn employment records, voter registration, and itemized contributions, shows that stances raise weekly giving by 22 percent, concentrated in the firm-linked PAC channel and in Democratic-directed giving. A pre-registered survey experiment randomizing employer stance direction reveals an alignment-conditional bifurcation: aligned employees mobilize toward the PAC, while misaligned employees withhold and counter-mobilize toward their own partisan side. The behavioral signature distinguishes identity activation from belief updating. The findings reposition the firm as an intermediary political institution that shapes where employee political money flows.

1 Introduction

Corporate political engagement has long operated behind closed doors - through lobbying, regulatory bargaining, and quietly directed political spending (Culpepper 2010; Drutman 2015). Increasingly, however, firms engage in politics publicly: advertising campaigns spotlighting racial-justice protests, statements on reproductive rights, speech on legislation related to sexual orientation and gender identity. These create high-salience, time-stamped moments of corporate political communication (Fisch and Schwartz 2024; Saracevic and Schlegelmilch 2026), and a growing literature documents that they carry real economic stakes - shaping consumer demand, recruitment, retention, and investor responses (Burbano 2019; Bhagwat et al. 2020; Conway and Boxell 2024; Hong and Li 2021; Adrjan et al. 2026).

However, the political stakes of this phenomenon are at least as real, and considerably less understood. A growing interdisciplinary literature documents what shapes corporate political behavior from the inside: the leftward shift in the ideology of corporate leaders (Steel 2026), the influence of executive ideology on corporate political action (Chin, Hambrick, and Treviño 2013; Cohen et al. 2019; Gupta, Fung, and Murphy 2021; Bizjak et al. 2022), and the role of institutional investors in corporate political giving (DesJardine, Shi, and Westphal 2024; Bertrand et al. 2026). The reverse question - how the firm's political behavior shapes the politics of those embedded within it - has received much less attention, even as partisan identity has come to structure workplace relationships (McConnell et al. 2018; Mason 2018; Hurst and Lee 2024) and as politically patterned sorting has yielded ideologically distinctive workplaces (Chinoy and Koenen 2024; Frake, Hurst, and Kagan 2025; Kagan, Frake, and Hurst 2026; Adrjan et al. 2026).

Corporate stance taking during 2018-2022 was overwhelmingly tilted toward liberal positions, rather than symmetric movement toward both parties (Cassidy and Kempf 2025; Steel 2026). The observational analysis in this paper therefore characterizes employee responses to one directional slice of corporate partisanship. A followup survey experiment, by design, addresses this asymmetry: it randomizes both liberal and conservative employer stances across three issue domains, enabling direct comparison of responses across stance direction and extending external validity beyond the realized stance mix of the field period.

This paper studies one proximate channel of those political spillovers: employee political donations. Employees are uniquely positioned as both agents of the firm and civic actors, repeatedly exposed to employer signals in a workplace from which exit is costly (Hertel-Fernandez 2016; White, Hurst, and Kagan 2026). Political donations are consequential, directionally interpretable, and measured with high precision in administrative data (Bonica 2016). The analysis distinguishes two giving channels: external donations to candidates, parties, and interest groups, and firm-linked giving through the corporate political action committee (PAC), which is organizationally branded as the firm's political vehicle and therefore signals identification with the firm's posture. Among aligned employees, the framework predicts mobilization toward the outside organizations and approval-driven giving through the PAC; among misaligned employees, it predicts withholding from the PAC paired with backlash - heightened giving to the employee's own partisan side. Persuasion, by contrast, would predict misaligned employees moving toward the firm's position. This is the empirical signature that adjudicates among mechanisms.

I first estimate the average effect of corporate stances on realized political giving using administrative records linked to 44 hand-collected stance events from 2018 to 2022 across 33 treated firms. In a stacked difference-in-differences design with industry- and year-matched controls across roughly 28 million employee-week observations, corporate stances raise the probability that an employee makes any donation in a given week by 8.9 percent and the probability of making a corporate PAC donation by 29.2 percent relative to the pre-period control mean. Total weekly donation amounts rise 21.5 percent, with effects concentrated in giving to Democratic organizations (21.8 percent) and in the firm-linked PAC channel (25.1 percent); Republican-directed amounts are statistically indistinguishable from zero. The effects are most pronounced among ideologically aligned employees.

I complement the field evidence with a survey experiment that standardizes exposure and solicitation across liberal and conservative employer stances. Respondents allocate a one-dollar bonus across partisan organizations, issue-aligned interest groups, the employer's corporate PAC, and the option to keep any portion for themselves. Experimental results mirror the field evidence: aligned respondents increase total giving, concentrated in the firm-linked PAC channel, while misaligned respondents withhold from the PAC and counter-mobilize through increased giving to their own partisan side. The behavioral signature is what misaligned employees do with the dissonant cue - they direct heightened giving toward their own partisan side, not toward the firm's, exactly what identity activation predicts and the opposite of persuasion.

The paper's primary contribution is to an interdisciplinary literature that looks inside the firm to study business and politics. That literature has primarily documented who within the firm shapes its political behavior (Steel 2026; Cohen et al. 2019; Chin, Hambrick, and Treviño 2013; Bertrand et al. 2026); I document the reverse channel - how that political behavior, once produced, shapes the politics of others within the firm. Read together, the two strands describe a pipeline from corporate leadership composition through public stance-taking to employee response. Methodologically, the analysis pairs employee-level partisan registration from L2 with itemized donations from the Database on Ideology, Money in Politics, and Elections (DIME; Bonica 2024) via a custom crosswalk - the employee-level analog to the BoardEx-DIME crosswalk Steel (2026) uses for corporate leadership. Two secondary contributions follow: an alignment-conditional bifurcation in which a single stance produces simultaneous mobilization and counter-mobilization, and a channel-conditional bifurcation in which organizational political infrastructure reshapes *where* political money flows rather than only its level.

The analysis speaks to three active conversations in political science. First, work on workplace political behavior has documented partisan sorting in labor markets and the workplace as a site of political socialization (Gift and Gift 2015; McConnell et al. 2018; Hertel-Fernandez 2018; Stuckatz 2022; Chinoy and Koenen 2024); the present paper isolates a behavioral mechanism through which the workplace exerts that influence. Second, work on donor behavior and campaign finance has emphasized ideological and access-related motivations of donors and the role of candidate characteristics in shaping giving (Bonica 2014; 2016; Barber 2016; Barber, Canes-Wrone, and Thrower 2017; Hill and Huber 2017; Kalla and Broockman 2016); I add organizational triggers of donor activation, including the activation of first-time donors among employees of stance-taking firms. Third, work on partisan cue-taking and affective polarization has shown that partisan cues from political elites activate identity-protective responses in mass publics (Bullock 2011; Iyengar, Sood, and Lelkes 2012; Mason 2018; Iyengar et al. 2019; Kalla and Broockman 2018); the paper extends this to cues from a non-traditional source - the firm - and finds the same behavioral signature, suggesting the partisan content of the cue matters more than its institutional origin.

The paper sits closest to Hertel-Fernandez (2024), who show that with inclusive participation norms, local union leaders can mobilize cross-pressured conservative members into the union's PAC. The present paper is the inverse organizational case: when firms take public partisan stances without comparable participatory infrastructure, cross-pressured employees withdraw from the firm-linked PAC and counter-mobilize externally. Read together, the two cases clarify that organizational signal type and participatory culture jointly determine whether the cross-pressured stakeholders mobilize through, withdraw from, or counter-mobilize against a politically active organization.

Substantively, these findings reposition the firm as a selective participation-shaping institution - analogous to but distinct from unions, churches, and civic associations (Verba, Schlozman, and Brady 1995; Hertel-Fernandez 2016) by virtue of repeated exposure, imperfect exit, and an institutionalized giving vehicle that can absorb approval responses. As corporate political speech becomes more common and more explicitly partisan, the firm warrants greater attention as a political institution in its own right - one whose public speech shapes not only its market environment but the political behavior of those who work within it.

2 Corporate Stances, Employees, and Political Donations

Public corporate stances on sociopolitical issues provide a tractable setting for studying political spillovers from firms to stakeholders. Two features make them useful. First, stances are typically time-stamped, attributable, and widely disseminated, creating a clear before/after moment that can be aligned with stakeholder outcomes (Conway and Boxell 2024; Fisch and Schwartz 2024). This event-like structure isolates a discrete communication shock rather than relying on diffuse, ongoing political activity whose timing and audience are harder to pin down. Second, stances plausibly function as partisan cues: because they are public, they become common knowledge, encouraging observers to interpret the firm through a partisan lens (Bullock 2011). In polarized environments such cues are especially powerful, compressing complex political meaning into a simple signal whose behavioral effects are strongest when the stance maps cleanly onto existing partisan divisions (Cohen 2003; Mason 2018).

The scale of the phenomenon warrants attention. S&P 500 firms alone employ roughly 18 percent of the U.S. workforce - a population larger than the total currently covered by U.S. union contracts.[1] As the institution long theorized as the dominant workplace political intermediary has shrunk in coverage (Verba, Schlozman, and Brady 1995; Schlozman, Verba, and Brady 2012), large firms have grown in employment reach and in political vocality.

This vocality emerged from a particular alignment of corporate composition and political moment. Over the prior two decades, the average ideology of corporate directors and executives shifted leftward - from modestly conservative in 2001 to roughly centrist by 2022, driven largely by turnover in the corporate-leadership labor market (Steel 2026). This shift coincided with a broader leftward shift in racial attitudes among educated professionals - the "Great Awokening" (Tesler 2016; Chudy 2024; Mikkelborg 2025)[2] - which raised expectations that elite institutions take public positions on issues of race, gender, and political legitimacy. By 2018-2022, the resulting wave of corporate stance-taking was overwhelmingly liberal identified (Cassidy and Kempf 2025), reflecting both the leftward composition of corporate leadership and the broader political environment to which that leadership was responding. The observational sample's liberal-only restriction is therefore not a design choice but a reflection of the actual stance-taking environment during this window.[3]

Employees are a natural population in which to study these spillovers. They are not politically neutral inputs into the firm - they are partisan citizens embedded in a workplace where exposure to employer signals is repeated, socially reinforced, and difficult to escape on short notice. Party identification is a durable social identity that shapes how people interpret information and evaluate institutions (Green, Palmquist, and Schickler 2004); when a firm takes a partisan stance, employees can read it as a signal about whose values are recognized as legitimate within the organization. This makes employee responses conceptually distinct from consumer or investor responses, which are mediated primarily through market exchange rather than identity and membership. The employee response is therefore the most demanding test of whether corporate partisan signals translate into stakeholder political behavior.

Workplaces also amplify partisan cues in ways that other stakeholder environments do not. Labor markets exhibit political sorting, so stances often arrive in organizations that are already politically structured: partisan cues affect hiring and job choice (Gift and Gift 2015), workers value co-partisan employers (McConnell et al. 2018), and politically patterned sorting yields ideologically distinctive workplaces over time (Chinoy and Koenen 2024; Colonnelli et al. 2023; Frake, Hurst, and Kagan 2025). Employees also shape the constraints firms face when engaging politically and can themselves be turned into political agents through workplace mobilization (Hertel-Fernandez 2016, 2018; Li 2018; Li and DiSalvo 2023). Together, these features make ideological alignment - whether the employee agrees or disagrees with the stance - the central dimension of response.

[1] S&P 500 firms collectively employed approximately 28 million U.S. workers in 2024, against a U.S. civilian labor force of approximately 168 million; private-sector union coverage was approximately 6 percent (Bureau of Labor Statistics 2025).

[2] The term is contested in the cultural-criticism literature; I use it descriptively to denote the documented leftward shift among college-educated white Americans on questions of race and identity during the 2010s and early 2020s.

[3] Conservative-leaning corporate stance-taking - including the 2024-2025 wave of corporate DEI rollbacks - is a distinct empirical question reserved for a companion paper. The survey experiment in Section 5 partially addresses this asymmetry by randomizing both liberal and conservative employer stances under controlled conditions.

Political donations provide a particularly useful behavioral lens. They are consequential, directionally interpretable, and measured with high precision in administrative data (Bonica 2016; Schlozman, Verba, and Brady 2012). Crucially, contribution data allow a clean decomposition of where political engagement flows: to the employer's corporate PAC versus to outside candidates, parties, and interest groups (Sorauf 1984). This decomposition separates a workplace-adjacent, firm-linked channel from broader political reallocation in the wider political marketplace, and it is precisely this distinction that the paper's theoretical framework, developed in Section 3, is built around. Babenko, Fedaseyeu, and Zhang (2020) show that employees donate significantly more to CEO-supported candidates than to otherwise similar candidates, with effects persisting around CEO departures - direct evidence that organizational political signals shape employee donation behavior even through informal channels.

3 Testable Hypotheses

Public corporate stances can change employee political giving through several mechanisms, but whether giving rises or falls - and where it flows - depends on the channel and the employee's ideological alignment with the stance. I organize the framework around two observable channels: contributions to the employer's corporate PAC, which is a firm-linked vehicle that functions as an approval or affiliation signal, and co-partisan non-PAC giving - donations to the party-side aligned with the employee's own views - which captures broader political spillovers beyond the organization. Together, these channels distinguish within-firm political affiliation from external partisan mobilization.

Table 1 organizes the paper's directional predictions. Rows correspond to the two alignment conditions (aligned vs. misaligned employees), columns to the two giving channels (corporate PAC vs. co-partisan non-PAC), and each cell contains a primary predicted mechanism - the one that theory most directly implies - alongside the main competing mechanism that the empirical design is built to adjudicate. I develop the logic for each cell in turn.

Table 1: Directional predictions by ideological alignment and channel

Employee–firm alignment	Corporate PAC giving	Co-partisan non-PAC giving
Aligned	**↑ Affiliation/Approval** *vs. ↓ Substitution*	**↑ Mobilization** *vs. ↓ Substitution*
Misaligned	**↓ Withholding** *vs. ↑ Persuasion / Pressure*	**↑ Backlash** *vs. → Persuasion/demobilization*

Note: Each cell lists the primary predicted direction (bold) and the main competing mechanism(s) (italicized). Co-partisan non-PAC giving is restricted to recipients aligned with the employee's own party; cross-partisan giving is excluded as it reflects a distinct set of mechanisms (persuasion, identity conversion) rather than the approval, mobilization, withholding, or backlash predictions organized here.

Aligned employees, PAC channel: affiliation vs. substitution. The corporate PAC is not merely a generic political vehicle. It is organizationally branded and directly associated with the firm (Li 2018), which gives it a meaning that contributing to an outside organization does not carry: contributing to the PAC is a visible act of identification with the employer's political posture. For an aligned employee whose views are congruent with the stance, this creates a strong expressive opportunity. The stance signals that the employer recognizes and shares the employee's political values, making the PAC a natural vehicle for expressing approval in return. The theoretical expectation is that aligned employees increase PAC giving following an aligned stance—not because they become more politically engaged in the abstract, but because the organizational channel becomes newly meaningful as an expression of agreement. This prediction should manifest more strongly on the participation margin than on the amount margin: PAC enrollment is a discrete institutional act, while amount changes require active reconfiguration of payroll deductions and remain bounded by contribution caps.

The competing prediction is substitution: if the public stance itself satisfies the expressive motives that would otherwise drive personal donations, employees may feel their own PAC contribution is redundant (Yildirim et al. 2024). On this account, the firm has already spoken on the employee's behalf, and a PAC contribution adds little. Substitution would predict no increase—or a decrease—in PAC giving among aligned employees.

Aligned employees, co-partisan non-PAC channel: mobilization vs. substitution. For external giving, the primary prediction is mobilization. An aligned employer cue raises the salience of the issue, validates the employee's existing partisan identity, and makes political participation feel timely and normatively appropriate (Zaller 1992; Bullock 2011; Mason 2018). The mechanism is not informational—the stance does not teach employees something new about the issue—but identity-expressive: it signals that the employee's political values are shared and legitimate within the workplace, lowering the psychological cost of acting on them. Mobilization predicts increased giving to co-partisan candidates, parties, and organizations.

The competing mechanism is again substitution, now operating in the external channel. If the firm's public stance satisfies the expressive demand that would otherwise be met through outside donations, aligned employees may reduce external giving even as their attitudes become more supportive—the firm has, in effect, donated on their behalf. If approval flows through the PAC and mobilization flows through co-partisan giving, both should increase among aligned employees; substitution in the external channel would instead predict a decrease in co-partisan amounts that offsets any PAC increase.

Misaligned employees, PAC channel: withholding vs. persuasion vs. pressure. For misaligned employees, the primary prediction is withholding: contributing to the firm's PAC following a dissonant stance would read as endorsement of a political posture the employee rejects. The same affiliation logic that makes PAC giving expressive for aligned employees makes it costly for misaligned ones. A misaligned employee who continues to give to the PAC after a stance they disagree with is, in effect, financing a political signal that opposes their own values—and because the PAC is organizationally branded, the action is legible to others in the workplace as political acquiescence (Hirschman 1970). Withholding therefore has both expressive and social logic. As with the aligned case, the prediction is more likely to manifest on the participation margin—declining to enroll, lapsing existing enrollment—than on amounts among continuing givers.

Two competing mechanisms warrant attention. The first is persuasion: if the stance is credible and the employee's prior attachment to the opposing view is weak, the employer's signal could shift underlying beliefs, reducing perceived dissonance and leaving PAC giving stable or increased. Persuasion requires strong conditions—the employer must be perceived as a credible political authority and the employee must be genuinely movable rather than merely cross-pressured (Kalla and Broockman 2018)—and these conditions are less likely to be met in a highly polarized environment. The second is pressure: if employees perceive that PAC participation signals organizational loyalty, or that withholding carries professional risk, misaligned employees might *increase* PAC giving following a stance—not because they have updated their beliefs, but because they are signaling continued alignment with management. Pressure produces an observable pattern directionally similar to persuasion but motivationally distinct.

Misaligned employees, co-partisan non-PAC channel: backlash vs. persuasion. The primary prediction for misaligned employees in the external channel is backlash. Interpreting the employer's stance as an identity threat—as evidence that the workplace privileges a political identity that is not their own—misaligned employees respond by increasing donations to causes that reflect their own partisan beliefs (Iyengar, Sood, and Lelkes 2012; Iyengar et al. 2019). This is active counter-mobilization: the dissonant cue does not suppress political engagement but redirects it, pushing misaligned employees toward heightened investment in their own partisan side. The affective polarization literature provides direct support for this mechanism: partisan identity is most strongly activated not by congruent cues but by perceived threat from the opposing side (Mason 2018). A firm publicly aligned with a position the employee rejects is precisely such a threat, arriving in an environment from which exit is costly and exposure is repeated.

The competing mechanism is again persuasion, now operating in the external channel. A sufficiently compelling stance could shift misaligned employees toward the firm's position rather than driving them away, reducing rather than increasing co-partisan giving. Critically, persuasion predicts a distinctive empirical signature: not merely a null effect on co-partisan giving, but an increase in *cross*-partisan giving—misaligned employees shifting toward the opposing party. This is the empirical test that separates persuasion from backlash and from withholding.

The two channels together. Read jointly, the four cells generate a two-part theoretical claim. The alignment of the employee with the stance determines the valence of the response: aligned employees express approval and mobilize, while misaligned employees withhold and counter-mobilize. The channel determines what form that response takes: approval flows through the organizationally branded PAC, while mobilization and backlash both flow through co-partisan giving. The prediction is not only that stances move the total level of giving but that they reshape *where* it flows, and the direction of that reshaping depends on the ideological relationship between employee and employer.

The framework also generates a clean adjudication structure. Identity activation and persuasion predict opposite patterns in the misaligned co-partisan-versus-cross-partisan split: identity activation predicts that misaligned employees direct heightened giving toward their own partisan side; persuasion predicts cross-partisan shifts toward the firm's side. The co-partisan-vs-cross-partisan ratio is therefore the test of identity activation vs. persuasion. The PAC channel, in turn, distinguishes withholding from pressure: withholding predicts misaligned employees give less to the firm-linked PAC; pressure predicts they give more. Together, the two channels adjudicate the underlying mechanism in a way no single outcome could—a property the empirical analysis in Sections 4 and 5 exploits directly.

4 Study 1: Differences-in-Difference Analysis of Employee Political Contributions

Study 1 uses administrative data on realized political giving to estimate the average effect of corporate stances on employee contributions in the field. The design has high external validity: it captures actual donation behavior at scale in the institutional environment in which stances occur. Its limitation is that it is less well-suited for isolating the channels and mechanisms identified in Section 3. Corporate PAC contributions are rare and institutionally mediated by firm solicitation and timing, which can attenuate observed responses and limit power for detecting moderating by alignment. Study 2 addresses these limitations by standardizing exposure and solicitation under controlled conditions, and by randomizing both liberal and conservative stances to extend external validity beyond only the liberal stances studied in the field.

4.1 Data

For this study, I use individual-level data on employment history, political party registration (as a proxy for alignment), and contributions to campaign finance recipients (including candidates, parties, interest groups, and corporate PACs). I also create a novel dataset of companies that took public, liberal-leaning political stances during 2018-2022. Using these linked data, I observe the donation behavior of employees who work at a company that takes a public stance and compare it to the donation behavior of employees at relevant control companies that did not take an equivalent stance.

Raw datasets. I use nationally comprehensive LinkedIn data scraped between 2008 and 2025, which includes 705 million profiles across 20 million companies globally. The data contain only information that individuals share publicly on their LinkedIn pages - analogous to a resume. Relevant to this study, individuals report their geographic location, education history, and employment history.

I also use nationally comprehensive voter file snapshots from 2025 from the vendor L2. These files contain 185 million records nationwide. While the voter file does not record for whom an individual voted, it does record whether they voted and the party with which they are registered. Thirty states and the District of Columbia record party affiliation in their voter rolls; for the remaining states, L2 imputes party registration using primary voting participation and other behavioral indicators. The main difference-in-differences estimates in Section 4.3 use the inclusive measure that combines formal registration in coverage states with L2's imputation elsewhere; partisan registration is highly predictive of vote choice and ideological placement (Abramowitz and Saunders 1998; Bartels 2000; Levendusky 2009).[4]

I collect information on campaign finance from the Database on Ideology and Money in Politics (DIME; Bonica 2024). I merge DIME records to the voter file using a proprietary crosswalk. The DIME data include contributions through 2024 to local, state, and federal elections, as well as to political action committees and interest groups. A question relevant to the weekly estimation strategy used below is whether the contribution dates recorded in DIME are sufficiently precise to support the event-time analysis at the weekly frequency. The answer varies by contribution type. For contributions processed through online platforms such as ActBlue - the dominant conduit for small-dollar Democratic giving during the 2018-2022 study window - transaction dates reflect credit-card processing timestamps and are accurate to the day. In the analytical sample, 83.8% of Democratic-directed transactions by count and 50.7% by dollar value are routed through ActBlue (identified via FEC committee ID C00401224), confirming that precise date information underlies the majority of the Democratic-directed giving that drives the main results. For itemized contributions submitted directly to campaigns by check, reporting is less precise: campaigns are required to file quarterly, and the recorded contribution date may reflect when a check was received and logged rather than when it was written. This introduces classical measurement error in the timing of a subset of contributions, which attenuates the weekly treatment effect estimates by blurring the post-period signal rather than generating spurious effects. Estimates of whether any donation occurs in a given week are less sensitive to this issue than estimates of dollar amounts, since mis-assignment of a contribution by a few days affects the dollar-amount aggregation within a week more than it affects whether a donation is recorded at all.

Merging the data sources. I match LinkedIn profiles to voter file records on name, location, and inferred year of birth across 180 increasingly general rounds. Rounds proceed from the most granular geography (Census place) to the broadest (state), and from the most restrictive name criterion (exact first and last name with middle initial) to the most permissive (nickname-normalized matches). I match birth years within a ten-year window and retain only profiles matched to exactly one voter file record. The procedure yields a 32.5 percent match rate (44 million individuals), comparable to other LinkedIn–voter-file linkages.[5]

Match quality varies across rounds. Appendix Table A.2 reports the distribution: 72.6 percent

[4] Party registration is a coarse proxy for issue-specific preferences (Baldassarri and Gelman 2008; Therriault 2015). On abortion, for example, a substantial minority of Republicans report that abortion should be legal in most cases (Pew Research Center 2024). Section 5 addresses this limitation by directly eliciting issue-specific policy views. Appendix A.14 reports a parallel-specification robustness check restricting the panel to employees in the 30 coverage states plus DC where partisan registration is recorded formally; the headline results preserve sign and significance.

[5] Studies that perform a similar match yield rates between 24 and 44 percent (Kim 2025; Chinoy and Koenen 2024).

of matched individuals in the analytical sample are recovered in Tier 1 (exact full name), 11.0 percent in Tier 2 (first four characters of first name plus exact last name), 7.8 percent in Tier 3 (first four characters of both names), and 8.5 percent in Tier 4 (nickname-normalized exact matching). Appendix Table A.3 re-estimates the main specification on the Tier 1 subsample alone; estimates are qualitatively unchanged.

Corporate stance dataset. I construct a novel dataset of corporate political stances issued by S&P 500 firms during 2018-2022, hand-curated from firm communications and contemporaneous secondary sources. The dataset covers five issue domains: corporate responses to the January 6, 2021 insurrection; statements supporting racial-justice movements following the murder of George Floyd; responses to state-level abortion restrictions and *Dobbs*; responses following the 2018 Parkland shooting; and responses to Georgia SB 202 in 2021.

The event-study design requires a well-defined treatment date, and this requirement imposes a binding constraint on event inclusion. I restrict the sample to corporate stances for which I can identify verifiable, time-stamped public-facing communication attributable to the firm - press releases, official social-media posts, executive announcements made in a managerial capacity, or contemporaneous reporting that attributes a specific date to the firm's own statement. Stances that were reported or inferred without a traceable originating communication are excluded, even when the underlying corporate action is documented elsewhere. The criterion is conservative by design: it excludes events for which corporate political activity is documented but imprecisely time-stamped, and skews the sample toward firms with sufficient communications infrastructure to issue clear statements. As noted in Section 2, I further restrict attention to liberal-leaning stances and require that treated firms maintain a corporate PAC, the firm-linked giving channel central to the theoretical framework.[6]

Sample construction and balance. For each treatment event, I construct a comparison group of control firms drawn from the same four-digit NAICS industry. I begin with the universe of publicly traded firms in the WRDS Compustat database that share the treated firm's industry and are observed in the event year, then restrict candidate controls to firms that maintain a corporate PAC and have no record of taking an equivalent stance on the focal issue during the relevant period.

[6] The motivation for this restriction is partly measurement. While partisan registration is highly predictive of broad ideological orientation, it remains a coarse proxy for issue-specific preferences, and misclassification is most likely when individuals are cross-pressured - when party identity does not map cleanly onto positions on the specific issues implicated by a stance. Direct evidence from the survey experiment in Section 5 illustrates how serious this cross-pressure problem is for the issue domains where corporate stance-taking has been concentrated. Among Republican respondents in the survey sample, 48.9% express at least some support for legal abortion access and 49.6% express at least some support for DEI in hiring, compared with 38.1% expressing support for transgender equality. On abortion and DEI in particular, Republican registration captures only about half of the position implied by the stance: roughly half of Republicans hold non-opposed views. Using Republican registration as a proxy for opposition to liberal corporate stances on these issues would substantially attenuate the alignment-based comparisons that the paper's framework requires. In contrast, for the liberal stances studied here, Democratic registration more reliably captures alignment with the stance's implied issue position, reducing attenuation from ideological misclassification in the alignment-based comparisons.

I standardize employer names in the LinkedIn data and use the cleaned identifier to link workers to treated and control firms.

For each stance event, I construct a stacked event-time panel spanning 12 weeks before and 12 weeks after the stance date, restricting to individuals employed at either the treated firm or an eligible control firm on the stance date itself. Treatment is assigned at the firm-event level; treated firms cannot serve as controls for other events, and treated employees cannot serve as controls for other events. Event time is indexed in weeks relative to the stance week.

Figure 1 summarizes the construction pipeline. To ensure sufficient statistical power within each event, I retain events that meet three minimum thresholds: at least 250 treated employees, at least 50 treated employee-donors, and at least 50 control employee-donors. After these restrictions, the analytical sample contains 44 stance events at 33 treated firms, yielding 1,120,622 employee-by-event observations and a stacked panel of 28,015,550 employee-week observations. Some employees appear in multiple stacks as controls, accounting for the difference between the employee-event count and the distinct-employee count in Table 2. A complete list of events appears in Appendix Table A.1.

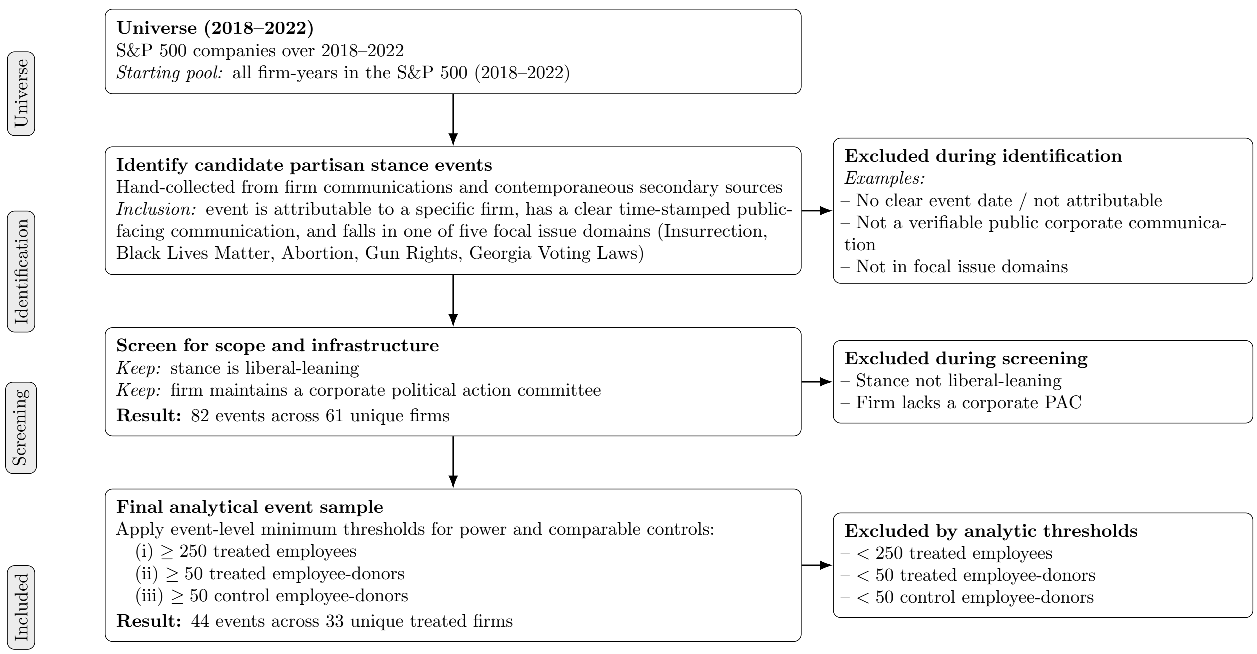


Figure 1: Event-sample construction pipeline for the observational analysis. The screening stage applies the substantive scope and infrastructure criteria; the included stage applies the three minimum quantitative thresholds described in Section 4.1.

Table 2 reports employee-level balance across the analytical sample. Treated-firm employees are modestly more Democratic-registered (0.447 vs. 0.387) and less Republican-registered (0.251 vs. 0.339); the prior-donor rate is essentially identical (0.033 vs. 0.031). Democratic shares show modest differences: treated employees are slightly more male (0.557 vs. 0.522), more graduate-educated (0.174 vs. 0.151), and less White (0.688 vs. 0.756). All standardized mean differences fall well within the conventional moderate-imbalance threshold of 0.20. These patterns motivate the within-stack fixed-effects framework developed in Section 4.2.

Table 2: User-level summary statistics: balance measures (Treated vs. Control)

Variable	Control	Treated	Diff (T–C)	SMD
Share Dem	0.387	0.447	0.060	0.122
Share Rep	0.339	0.251	-0.088	-0.194
Share Donors	0.029	0.029	-0.001	-0.003
Share Prev Donor	0.031	0.033	0.003	0.015
Share Dem Donors	0.017	0.019	0.003	0.021
Share Rep Donors	0.009	0.006	-0.004	-0.041
Male	0.522	0.557	0.036	0.072
White	0.756	0.688	-0.068	-0.151
Black	0.107	0.110	0.003	0.011
Hispanic	0.090	0.114	0.023	0.077
API	0.043	0.084	0.042	0.172
HS degree	0.031	0.037	0.006	0.032
Bachelor degree	0.361	0.365	0.003	0.007
Graduate degree	0.151	0.174	0.024	0.064
N	202,204	534,686		

Note: Table reports user-level means for employees at treated firms and at eligible control firms (defined within the treated firm's four-digit NAICS industry and event year). "Diff (T–C)" is the difference in means (treated minus control). "SMD" is the standardized mean difference, computed as the treated–control mean difference divided by the pooled standard deviation. "Donors" indicates whether a user makes any itemized political donation in the outcome data; "Previous Donors" indicates whether a user made an itemized donation prior to the event window. Race/ethnicity and education shares are constructed from the LinkedIn data. Counts in the N row report the number of user observations pooled across stacks.

4.2 Empirical Strategy

I estimate the effects of public corporate stances on employee political giving using a stacked difference-in-differences (DiD) design across 44 instances of corporate stance-taking. Each firm-event defines a separate stack with a treated firm and an event-specific set of control firms matched within four-digit NAICS industry and event-year. The design compares changes in outcomes for employees at treated firms to contemporaneous changes for employees at control firms within the same stack, absorbing time-invariant differences with employee-by-stack fixed effects and common time-varying shocks with calendar-week fixed effects. To account for time-varying geographic shocks, I also estimate a specification with calendar-week-by-congressional-district fixed effects; the main results in Appendix A.6 remain qualitatively unchanged.

The main outcomes are employee-week measures of campaign-finance activity constructed from itemized contribution records linked to individuals. I examine these outcomes on two margins. The extensive margin measures whether an employee makes any donation in a given week and captures the participation decision - whether the employee enters the donating pool at all. The intensive margin measures the total dollar amount donated in that week (winsorized at the 99th percentile) and captures how much employees give, conditional on giving. The distinction matters for the mechanisms developed in Section 3: organizational approval and withholding (the PAC channel predictions) operate primarily on the participation decision, while mobilization and backlash (the co-partisan predictions) can manifest on both margins. I report estimates separately for total giving, and donations to Republican organizations - decomposing both the firm-linked versus external channels (PAC vs. non-PAC) and the partisan direction of external giving.

Let i index individuals, s index stacks (firm-event pairs), and t index event-time weeks relative to the stance date ($t = 0$ in the stance week). Define Treated_{is} as an indicator equal to 1 if individual i is employed at the treated firm in stack s (and 0 if employed at at a control firm in that stack), and let $\text{Post}_t = \mathbb{1}\{t \geq 0\}$. The baseline difference-in-differences specification is:

$$Y_{ist} = \alpha_{is} + \tau_{cw(s,t)} + \beta\,(\mathit{Treated}_{is} \times \mathit{Post}_t) + \varepsilon_{ist}, \tag{1}$$

where α_{is} are individual-by-stack fixed effects and $\tau_{cw(s,t)}$ are calendar-week fixed effects common within stack. The calendar-week fixed effects absorb national shocks and seasonality in giving, while the individual-by-stack fixed effects absorb all time-invariant differences between treated and control individuals within a stack. Standard errors are clustered by stack.[7]

The estimated sample is defined by employment at the stance date: any individual employed at a treated or control firm on the date of the stance is included in the panel for the entire duration, regardless of whether they remain employed throughout the window. The total donation estimates are therefore intent-to-treat (ITT) effects of exposure to a corporate stance, and this is the appropriate estimand given the paper's theoretical mechanism. The treatment of interest is a discrete, high-salience public communication - a cue whose effects on partisan identity activation need not not require continued employment to persist.[8] All outcomes, including PAC contributions, are estimated on the full panel of pre-stance employees, mirroring the ITT framing applied to total-donation outcomes.[9]

Identification. Identification of β relies on within-stack parallel-trends assumption: absent the stance, treated and control employees would have experienced the same counterfactual change in political giving over event time, conditional on the fixed effects. The assumption is made more

[7] Clustering by stack allows the error term to be arbitrarily correlated across employees and weeks within the same event window - for example, due to unobserved firm-level shocks, contemporaneous media attention, or issue-specific news that jointly affect all employees linked to a given stance event and its matched control set. This is the relevant correlation structure for inference in the stacked design: observations within a stack share the same treatment timing, the same control-group construction, and potentially common unobserved determinants of political giving.

[8] Conditioning on sustained employment would risk excluding some of the most politically activated responses: an employee who quits following a misaligned stance is arguably among those most affected by it, and restricting to stayers would selectively drop such cases from the treated group, potentially biasing estimates upward among aligned employees.

[9] Federal election law restricts corporate PAC solicitation to the firm's "restricted class," which extends beyond current employees to include shareholders and family members of restricted-class members. Former employees who hold company stock through ESOP or 401(k) plans, or whose spouses remain restricted-class members, retain legal eligibility to contribute. While such cases are likely rare, this means that conditioning the PAC analysis on active employment understates the population legally eligible to respond to PAC solicitation, and the full-panel specification used here is the more conservative choice.

plausible by three design choices: control pools are constructed within the treated firm's four-digit NAICS industry and event year; firms taking an equivalent stance on the focal issue during the relevant window are excluded from control eligibility; and the estimation sample is restricted to individuals employed at either the treated or a control firm on the stance date itself. In Appendix A.5, I show that estimates obtained from a modified Granger causality test are indeed consistent with the null hypothesis of no differential pretrends (Angrist and Pischke, 2008).

Threats to inference. Four features of the design bear on interpretation. Each represents a distinct threat to identification, and the first three cut in the same direction: toward attenuation. Taken together, they suggest the estimates that follow are best read as conservative lower bounds on the causal effect of employer stance-taking.

The first is a potential violation of the Stable Unit Treatment Value Assumption (SUTVA). Because corporate stances are public events, employees at control firms are not fully unexposed to treatment - they may encounter the same stance through media coverage, social networks, or industry channels. To the extent control employees are themselves mobilized by ambient exposure, the treated-minus-control difference understates the marginal effect of receiving the cue specifically through one's own employer. The DiD estimates therefore capture the organizational channel net of whatever ambient political activation is already present in the control group.

The second is the endogeneity of stance-taking. Corporate stances are not randomly assigned: they reflect strategic decisions by firms whose most politically engaged employees - particularly those in government affairs and senior leadership - may themselves have shaped the decision to take a stance. If the employees are also the most active political donors, they would likely continue giving in the post-period regardless of whether the stance occurs, and may have begun mobilizing in response to internal deliberations before the formal public announcement. This anticipatory behavior would push some of the treatment effect into the pre-period window, attenuating the estimated post-period coefficient.[10]

A third, related concern involves the conceptualization of the stance as a discrete shock from the perspective of employees. For external audiences, the public announcement constitutes a clear, time-stamped informational shock. What makes it theoretically distinctive for employees, however, is not merely the information it conveys but the common knowledge it creates: the firm's position becomes publicly legible to colleagues and the organization, which is precisely what makes PAC giving expressive as an act of approval or affiliation under the frame work developed in Section 3. An employee who privately learns of the firm's political position through internal channels cannot use PAC giving as a visible signal of alignment in the same way as one responding to a publicly known stance. To the extent that internal communications precede the formal announcement, the DiD estimates capture only the increment attributable to the public, common-knowledge announcement.

[10]Appendix A.8 re-estimates the main results separately for employees at the Director level and above versus those below. Both groups exhibit positive post-period effects, indicating that the behavioral response is not confined to senior employees who may have anticipated the stance. The larger effects among senior employees are consistent with their higher baseline donation propensity rather than reverse causation.

A fourth concern is serial autocorrelation within the weekly panel, which can bias cluster-robust standard errors when treatment effects are concentrated in a narrow event-time window (Bertrand, Duflo, and Mullainathan 2004). Appendix A.4 reports a two-period robustness check that collapses the weekly panel to pre- and post-period averages across a range of averaging windows. Estimates are strongest at narrow windows centered on the event and attenuate as the window widens, consistent with a discrete communication shock activating partisan identity in the period of peak stance salience rather than reflecting a persistent shift in employee giving behavior. The ±4-week estimates closely match the main weekly results in sign and magnitude.

Several of these identification concerns are meaningfully attenuated for one substantial portion of the analytical sample. The 21 Insurrection-period stacks - firms responding to the events of January 6, 2021 - are corporate reactions to an external, unanticipated shock that no firm could have planned for, with most statements issued within days of the event. The compressed response window leaves little room for politically engaged employees to have engineered the firm's response in advance (weakening the endogeneity concern), and the discrete-shock structure is unambiguous: there is no plausible reading under which employees encountered the firm's January 6 position through a slow internal process before the public announcement (weakening the common-knowledge concern). The issue itself also generated broader cross-partisan corporate responses than the other focal domains, reducing ideological self-selection in which firms chose to speak. Appendix A.12 re-estimates the main results separately for Insurrection and non-Insurrection subsamples; the headline total-donation effect is positive and significant in both, indicating that the response is not confined to the deliberative stance-taking where these concerns are most acute.

Taken together, these considerations suggest the estimates that follow should be interpreted as a lower bound on the causal effect of employer stance-taking among employees who receive the stance as a genuine communication shock. The survey experiment in Section 5 addresses each of these concerns directly by design: control respondents receive no ambient exposure to the employer stance, the treatment is unanticipated by construction, and all respondents encounter the stance as a single clean public signal with no prior organizational context.

4.3 Results from Empirical Analysis

Exposure to a corporate stance increases employee political giving in the field, and the pattern of effects across channels maps onto the framework developed in Section 3. The estimates are clearest on the participation margin and concentrated among ideologically aligned employees, consistent with a cue-based account in which partisan corporate signals activate existing political identities rather than shifting them.

Table 3 reports the main difference-in-differences estimates. To facilitate interpretation, I report baseline means from the pre-period control group and percent changes relative to that baseline. Panel A reports effects on the extensive margin (whether an employee makes any donation in a given week); Panel B reports effects on the intensive margin (total dollar amount, winsorized at the 99th percentile).

Table 3: Main results: extensive and intensive margins

Panel A: Extensive margin (any/indicator)				
Dependent Variables:	Any donation	Any PAC donation	Any donation to Dem orgs	Any donation to Rep orgs
Model:	(1)	(2)	(3)	(4)
Variables				
Treated × Post	0.0005*	0.0003*	0.0002	0.0002*
	(0.0002)	(0.0001)	(0.0002)	(6×10^{-5})
Baseline mean (pre, control)	0.005991	0.000922	0.003041	0.001400
Percent change from baseline	8.93	29.22	5.52	10.79
Fixed-effects				
Employee-Stack	Yes	Yes	Yes	Yes
Calendar Week	Yes	Yes	Yes	Yes
Fit statistics				
Observations	28,015,550	28,015,550	28,015,550	28,015,550
R^2	0.28770	0.27535	0.25865	0.32294

Panel B: Intensive margin (amounts)				
Dependent Variables:	Total donation amount	PAC amount	Amount to Dem orgs	Amount to Rep orgs
Model:	(1)	(2)	(3)	(4)
Variables				
Treated × Post	0.1301**	0.0229*	0.0572*	-0.0109
	(0.0392)	(0.0103)	(0.0283)	(0.0319)
Baseline mean (pre, control)	0.603811	0.091053	0.262354	0.156061
Percent change from baseline	21.54	25.11	21.81	-6.99
Fixed-effects				
Employee-Stack	Yes	Yes	Yes	Yes
Calendar Week	Yes	Yes	Yes	Yes
Fit statistics				
Observations	28,015,550	28,015,550	28,015,550	28,015,550
R^2	0.15701	0.13743	0.13963	0.09057

Clustered (stack) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.1*

Participation rises across recipient categories. On the extensive margin (Panel A), exposure to a corporate stance raises the probability that an employee makes any donation in a given week by 8.9 percent relative to the pre-period control mean ($p < 0.05$). The probability of making a corporate PAC donation rises by 29.2 percent ($p < 0.05$), and the probability of donating to Republican organizations rises by 10.8 percent ($p < 0.05$). The probability of donating to Democratic organizations rises by 5.5 percent but is not statistically distinguishable from zero ($p \approx 0.32$; wild-cluster-bootstrap $p = 0.37$).[11] The substantive scale of these estimates deserves emphasis. Baseline weekly participation rates in the analytical sample are extremely low—roughly 0.6 percent for any donation and 0.09 percent for corporate PAC contributions—reflecting the right-tail nature of campaign-finance participation. Recovering detectable post-stance increases on these baselines, in a full panel of employees rather than a pre-screened donor subsample, indicates that stances are bringing new participants into political giving rather than only amplifying activity among already-engaged donors.

Amounts rise sharply, concentrated in Democratic and firm-linked channels. On the intensive margin (Panel B), total weekly donation amounts rise by 21.5 percent relative to the pre-period control mean ($p < 0.01$). The increase is concentrated in two channels: donations to Democratic organizations rise by 21.8 percent ($p < 0.05$), and corporate PAC contributions rise by 25.1 percent ($p < 0.05$). Republican-directed amounts are statistically indistinguishable from zero in the pooled specification ($p = 0.73$; WCB $p = 0.82$). The Democratic-channel concentration is consistent with the directional content of the stance set - corporate stances during 2018-2022 were overwhelmingly liberal-identified, so Democratic-aligned giving is the predicted direction of cue-driven mobilization.

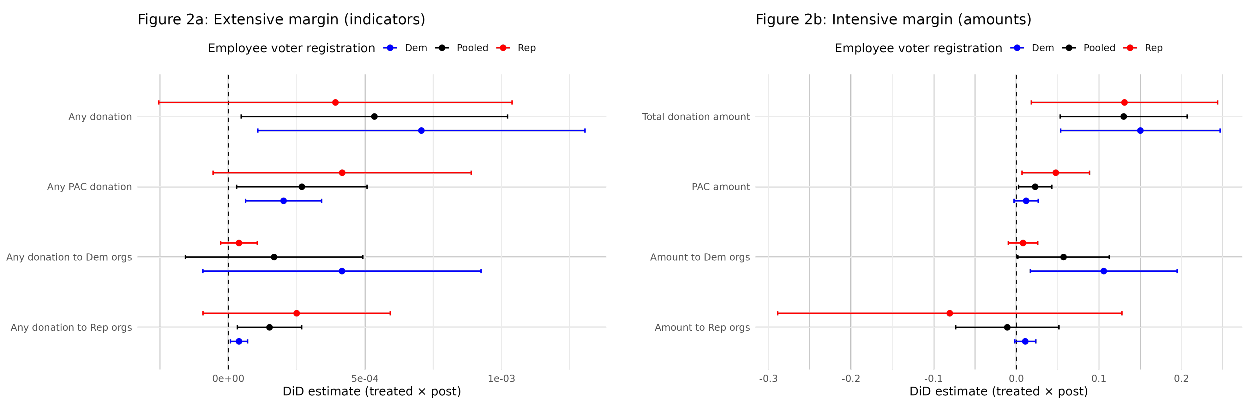
The combination of patterns across margins is informative. The PAC channel shows the largest proportional effect on participation (29.2 percent) but a more modest effect on amounts (25.1 percent), consistent with PAC giving being primarily an institutional-enrollment decision - a discrete act of identification with the firm-linked channel rather than a continuous adjustment of payroll-deduction amounts. The Democratic-channel response, by contrast, shows the reverse pattern: a strong amount effect with a weaker participation signal. This is consistent with mobilization operating through both new donors entering the giving pool and existing donors increasing their contributions to co-partisan recipients.

Robustness. Six sets of supplementary analyses confirm the main pattern. Appendix A.3 re-estimates the headline specification on the Tier 1 subsample of exact-name matches (72.6% of the analytical sample), addressing whether lower-stringency matches introduce systematic bias;

[11] The extensive-margin Democratic-organization cell is the one extensive-margin outcome that does not reach conventional significance under cluster-robust or WCB inference. The intensive-margin Democratic-amount cell is significant ($p < 0.05$), and the non-Insurrection event-type split discussed below recovers a significant extensive-margin Democratic effect in the non-Insurrection subsample. The pooled extensive-margin null is therefore best read as a power limitation on the participation margin for this recipient category rather than as evidence against the substantive Democratic-channel response on amounts.

estimates are qualitatively unchanged on the headline intensive cells. Appendix A.14 re-estimates the headline specification restricting to the 30 states plus DC with formal party registration in voter rolls, addressing potential measurement error from L2's imputation in non-coverage states; headline results preserve sign and significance. Appendix A.6 re-estimates the headline specification with congressional-district-by-calendar-week fixed effects to absorb time-varying geographic shocks; all four positive cells preserve sign and significance. Appendix A.7 reports wild-cluster-bootstrap p-values for each main cell; all conclusions are robust to the small-cluster inference correction. Appendix A.10 reports a fixed-effects and random-effects meta-analysis across the 44 stacks; the headline total-amount effect is positive and significant under both pooling schemes. Appendix A.11 reports a leave-one-out diagnostic that re-estimates the pooled coefficient dropping each stack in turn; no drop reverses the sign of a significant cell.

A direct subsample test re-estimates the headline on the 21 Insurrection stacks and the 23 non-Insurrection stacks separately (Appendix A.12). The total-amount effect generalizes across both event types ($\beta = 0.165$, $p < 0.05$ Insurrection; $\beta = 0.104$, $p < 0.05$ non-Insurrection), but the two channels respond to different stance types: PAC mobilization is concentrated in Insurrection stacks, while Democratic-channel mobilization is concentrated in the non-Insurrection subsample. The Republican-amount cell that appears noisy in the pooled specification conceals strongly offsetting subsample effects (Insurrection +0.092, $p < 0.001$; non-Insurrection -0.079, $p < 0.10$); this and the channel-by-event-type pattern are discussed substantively in Section 6.



Extensive margin

Intensive margin

Figure 2: Heterogeneity results by alignment

Heterogeneity by alignment. Figure 2 decomposes the main effects by registered partisanship across the four headline outcomes. Because corporate stances in the 2018–2022 sample are overwhelmingly liberal-coded, Democratic-registered employees serve as the aligned group and Republican-registered employees as the misaligned group throughout this discussion. Appendix A.9 reports the full numerical decomposition; this paragraph reports the cells that bear directly on the theoretical predictions.

Aligned employees mobilize through both predicted channels. Democratic-registered employees increase corporate-PAC participation by 24.1% ($\hat{\beta} = 2.0 \times 10^{-4}$, $p = 0.007$) and amounts given to Democratic organizations by 17.5% ($\hat{\beta} = 0.106$, $p = 0.024$) — both significant, both in the predicted direction. The Democratic-amount coefficient is 71% of the Democratic-employee total-amount coefficient, indicating that the aligned response is almost entirely routed through the co-partisan channel rather than diffused across recipients.

The misaligned co-partisan cell is not identified in the pooled field design. Republican-registered employees' giving to Republican organizations carries a noisy negative point estimate ($\hat{\beta} = -0.081$, $p = 0.45$) whose confidence interval brackets zero. This pooled null obscures a substantively important event-type asymmetry documented in Appendix A.12: the same cell is strongly positive in the Insurrection subsample ($+0.092$, $p < 0.001$) and weakly negative outside it (-0.079, $p < 0.10$). The pooled field design cannot identify a clean misaligned co-partisan response - the survey experiment in Section 5, by randomizing stance direction, addresses this directly.

The field decomposition is partially complete with respect to the theoretical 2×2: the aligned cells identify cleanly; the misaligned-PAC cell identifies but is interpretively complicated by the firm's channel choice; and the misaligned co-partisan cell is identifiable only in subsample contrast. The survey experiment in Section 5 fills the cells the field design cannot.

5 Study 2: Survey Experiment

The field analysis in Section 4 establishes that corporate stances raise employee political giving, that the response is concentrated in Democratic-aligned and firm-linked channels, and that the channel composition varies systematically by event type. The survey experiment serves three complementary purposes. It extends external validity beyond the realized 2018-2022 stance mix by randomizing both liberal and conservative employer stances. It directly elicits issue-specific policy views, which addresses the coarseness of partisan registration as a measure of stance-alignment (Baldassarri and Gelman 2008; Therriault 2015) and recovers cleanly-identified alignment-conditional estimates that the field analysis can only approximate. And it standardizes PAC solicitation across alignment conditions, which the institutional mediation of corporate PAC giving precludes in administrative data. Together, the two studies yield a tight pairing: the field establishes the response's existence, direction, and scale; the experiment pins down the alignment-conditional mechanism with measurement precision the field cannot offer.

5.1 Sampling and respondent characteristics

I fielded the survey through Verasight in June 2025 to a U.S. adult sample of 1,300 respondents designed to approximate national representativeness. Verasight recruits using a hybrid, multi-modal approach combining probability and non-probability methods with respondent verification and direct recruitment.

To align the experiment with the population observed in the administrative data, I restrict the analytical sample to respondents who are at least 25 years old, registered to vote, and either employed or actively seeking work. This restriction yields 842 respondents. Table B.1 summarizes the full sample and analytical sample; the analytical restriction shifts the age distribution and labor-force attachment by construction while leaving other observables broadly similar.

Pre-treatment issue attitudes are modestly more liberal-leaning than nationally representative benchmarks: supportive views are modal across abortion, DEI in hiring, and transgender equality (Table B.2 provides distributions and Pew comparisons). In all experimental analyses I apply Verasight's raking weights to align marginal distributions on key covariates including party identification, gender, age, education, race, region, and 2024 presidential vote choice. The analytical sample contains meaningful minority shares of respondents opposed to each focal issue, providing the variation in baseline attitudes that the alignment-heterogeneity analyses exploit.

5.2 Experimental design

Respondents evaluate a hypothetical large employer across three policy domains in which firms have engaged in public political speech: abortion and reproductive rights, DEI in hiring, and transgender equality. Before exposure to the employer vignettes, I elicit baseline issue attitudes on a 7-point scale from -3 (very opposed) to 3 (very in favor), which I use to define alignment in heterogeneity analyses.

For each domain, respondents are randomly assigned to one of three vignettes in which the employer publicly supports the policy (liberal stance), publicly opposes the policy (conservative stance), or remains neutral. Vignette text holds salience and format constant across conditions so that treatment variation reflects the direction of the stance rather than differences in presentation. Issue order is randomized; Appendix B.6 confirms no ordering effects.

The primary behavioral outcome is a donation-allocation task with real monetary stakes: respondents allocate a $1 bonus across a menu of political recipients that includes partisan organizations, issue-aligned interest groups, and the employer's corporate PAC, with the option to keep any portion for themselves. Because the budget constraint is binding at $1.00, intensive-margin estimates should be read as proportional reallocation across recipients rather than as proxies for real-world donation magnitudes. The extensive margin—whether respondents donate at all, and to which recipients—is therefore the more directly interpretable outcome, and I give it primary emphasis throughout.[12] For regulatory reasons, respondents' allocations are not transmitted to political organizations, but all respondents receive the full $1.00 bonus regardless of their choices.[13]

The main analysis uses a repeated-measures arm in which key behavioral outcomes are measured

[12]This interpretation is corroborated by an anonymity moderation test reported in Appendix B.9: half of respondents were told donations would be made on their behalf, half were told donations would be anonymous. Intensive-margin effects attenuate in the anonymous condition, consistent with some expressive reallocation within the constrained budget. Extensive-margin effects—whether respondents donate at all and to which channels—are robust across both conditions, supporting the inference that participation decisions reflect genuine preferences.

[13]To preserve incentive compatibility across the three issue modules, respondents are told that one issue module will be randomly selected for payment at the end of the survey and that they will be paid based on their choices in that module.

both before and after vignette exposure. Half of the analytical sample is assigned to this arm; the remaining half receives only post-treatment measures and is used to implement a modified Solomon design diagnosing instrument reactivity (Malhotra, Monin, and Tomz 2019). Appendix B.7 confirms that pre-treatment measurement does not systematically alter post-treatment responses, and full randomization details are in Appendix B.3.

5.3 Hypotheses and empirical strategy

The survey tests three pre-registered hypotheses derived from the framework in Section 3, organized by outcome channel.

H1 (*Any giving*): Partisan stances mobilize employees into political action relative to the neutral control, increasing total giving for both aligned and misaligned respondents.

H2 (*PAC giving*): The approval and withholding predictions operate most directly through the employer's PAC. Aligned employees increase PAC giving as alignment intensifies; misaligned employees reduce PAC giving as misalignment intensifies.

H3 (*Co-partisan non-PAC giving*): Mobilization among aligned employees flows toward their own partisan side as well as the PAC, predicting a positive alignment gradient on co-partisan giving. Misaligned employees counter-mobilize, increasing co-partisan giving as misalignment intensifies—the empirical signature that adjudicates identity activation against persuasion, which would instead predict cross-partisan shifts.

For the repeated-measures arm, let Y_{ijt} denote an outcome for respondent i, issue j, at time $t \in \{0, 1\}$, with $\text{Post}_t = \mathbb{1}\{t = 1\}$. I operationalize ideological congruence as two continuous intensity variables: $\text{AlignedIntensity}_{ij} \in \{0, 1, 2, 3\}$ equals zero for control or misaligned respondents and increases with the strength of agreement between the respondent's pre-treatment views and the employer stance; $\text{MisalignedIntensity}_{ij} \in \{0, 1, 2, 3\}$ is the analogous measure for disagreement.[14] The main specification is:

$$
\begin{aligned}
Y_{tij} = \alpha_i + \gamma_j + \beta_1 \text{Post}_t &+ \beta_2 \left(\text{Post}_t \times \text{AlignedIntensity}_{ij}\right) \\
&+ \beta_3 \left(\text{Post}_t \times \text{MisalignedIntensity}_{ij}\right) + \varepsilon_{tij},
\end{aligned}
\tag{2}
$$

where α_i are respondent fixed effects, γ_j are issue fixed effects, and the standard errors are clustered by respondent. H1 predicts $\beta_2 > 0$ and $\beta_3 > 0$ on any giving. H2 predicts $\beta_2 > 0$ and $\beta_3 < 0$ on PAC giving. H3 predicts $\beta_2 > 0$ and $\beta_3 > 0$ on co-partisan non-PAC giving.

To assess symmetry across stance directions, I estimate a second specification that separates the alignment gradient by stance arm and decomposes outcomes into Democratic- and Republican-directed giving:

[14] Both variables are derived from the pre-treatment *feel* variable on the $\{-3, ..., +3\}$ scale: $\text{AlignedIntensity}_{ij} = \max(\text{feel}_i \times \text{StanceDir}_j, 0)$ and $\text{MisalignedIntensity}_{ij} = \max(-\text{feel}_i \times \text{StanceDir}_j, 0)$ where $\text{StanceDir}_j \in \{-1, +1\}$. The control arm serves as the omitted baseline.

$$
\begin{aligned}
Y_{tij} = \alpha_i + \gamma_j + \beta_1 \text{Post}_t + \beta_2 \left(\text{Post}_t \times \text{Liberal}_j\right) + \beta_3 \left(\text{Post}_t \times \text{Conservative}_j\right) \\
+ \beta_4 \left(\text{Post}_t \times \text{Liberal}_j \times \text{Alignment}_{ij}\right) \\
+ \beta_5 \left(\text{Post}_t \times \text{Conservative}_j \times \text{Alignment}_{ij}\right) + \varepsilon_{tij},
\end{aligned} \tag{3}
$$

where $\text{Alignment}_{ij} \in \{-3, ..., +3\}$ collapses the two intensity variables onto a single scale and $\text{Liberal}_j, \text{Conservative}_j$ index stance arm. Symmetry of the underlying mechanism predicts β_4 and β_5 operate in analogous directions for both PAC and partisan giving regardless of stance direction.

5.4 Results

The results proceed in two steps. Table 4 tests H1-H3 jointly by estimating the alignment gradient across the three outcome channels. Table 5 then asks whether those patterns are symmetric across liberal and conservative stances.

H1: stances increase total giving. The Post × AlignedIntensity and Post × MisalignedIntensity coefficients are both positive and significant on the any-giving outcome (extensive margin: aligned $\hat{\beta} = 0.024, p < 0.001$; misaligned $\hat{\beta} = 0.014, p < 0.05$), confirming that partisan corporate stances mobilize employees into political action regardless of their ideological position relative to the employer. Each unit of alignment intensity raises the probability of making any donation by 2.4 percentage points; each unit of misalignment intensity raises it by 1.4 percentage points.

H2: aligned employees mobilize into PAC giving; misaligned employees withhold. The Post × AlignedIntensity coefficient on PAC giving is positive and precisely estimated: each additional unit of alignment intensity raises the probability of any PAC donation by 5.7 percentage points ($p < 0.001$), a substantial effect relative to the baseline PAC participation rate of 24.3 percent. The Post × MisalignedIntensity coefficient is negative on both margins and reaches conventional significance on the extensive margin $\hat{\beta} = -0.011, p < 0.05$), consistent with the withholding prediction. The alignment gradient on PAC giving - positive at the aligned end, negative at the misaligned end - is the cleanest single result in the experiment and directly confirms the framework's central PAC-channel prediction.

H3: aligned employees mobilize co-partisan giving; misaligned employees counter-mobilize. The Post × AlignedIntensity coefficient on co-partisan non-PAC giving is positive and significant on the extensive margin ($\hat{\beta} = 0.021, p < 0.05$), indicating that alignment brings new donors into co-partisan giving even as it simultaneously pulls budget toward the PAC channel. Among misaligned respondents, the Post × MisalignedIntensity coefficient is positive and significant for co-partisan non-PAC giving on the intensive margin ($\hat{\beta} = 1.710, p < 0.01$) and approaches significance on the extensive margin ($\hat{\beta} = 0.014, p = 0.056$). Misaligned respondents respond to a dissonant employer stance by increasing donations to their own partisan side, with the response scaling in the intensity of misalignment. This is the empirical signature that adjudicates identity

Table 4: Unified results (H1–H3): mobilization, approval, and backlash

Panel A: Extensive margin (any/indicator)			
Dependent Variables:	Any Donation	PAC Donation	Co-partisan non-PAC
	(1)	(2)	(3)
Variables			
Post	-0.044^{**}	-0.042^{**}	-0.047^{**}
	(0.014)	(0.013)	(0.016)
Post × Aligned Intensity	0.024^{***}	0.057^{***}	0.021^{**}
	(0.006)	(0.008)	(0.007)
Post × Misaligned Intensity	0.014^{*}	-0.011^{*}	$0.014^{\dagger}$
	(0.006)	(0.005)	(0.007)
Baseline mean (pre, control)	0.639	0.243	0.645
Fixed-effects			
Respondent FE	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes
Fit statistics			
Observations	2,550	2,550	2,328
R^2	0.761	0.709	0.704

Panel B: Intensive margin (amounts)			
Dependent Variables:	Any Donation	PAC Donation	Co-partisan non-PAC
	(1)	(2)	(3)
Variables			
Post	-3.539^{**}	-0.898	-2.091
	(1.225)	(0.618)	(1.310)
Post × Aligned Intensity	2.198^{***}	2.433^{***}	0.045
	(0.519)	(0.439)	(0.690)
Post × Misaligned Intensity	1.309^{*}	-0.181	1.710^{**}
	(0.539)	(0.256)	(0.643)
Baseline mean (pre, control)	54.568	7.254	43.697
Fixed-effects			
Respondent FE	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes
Fit statistics			
Observations	2,550	2,550	2,328
R^2	0.798	0.532	0.709

Notes: Respondent and issue fixed effects included in all specifications.
Standard errors clustered by respondent in parentheses.
Signif. codes: $^{\dagger}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

activation against persuasion: persuasion would predict misaligned respondents moving toward the firm's position (i.e., increases in cross-partisan giving); the data show the opposite.

Symmetry across stance directions. Table 5 separates the alignment gradient by stance arm and decomposes giving outcomes by partisan recipient. Two patterns stand out. First, PAC giving is strongly increasing in alignment for both stance directions on both margins (extensive: Liberal stance × Alignment $\hat{\beta} = 0.124, p < 0.001$; Conservative stance × Alignment $\hat{\beta} = 0.063, p < 0.001$), reinforcing that the PAC channel reflects organizational approval rather than a phenomenon specific to one ideological direction. Second, partisan giving shifts toward the co-partisan side under each stance: alignment increases Democratic-directed giving under liberal stances ($\hat{\beta} = 0.040, p < 0.01$) and Republican-directed giving under conservative stances ($\hat{\beta} = 0.051, p < 0.01$), while moving in the opposite direction for the out-party in each case. Both patterns are consistent with cue-driven mobilization activating existing partisan identity rather than converting it.

Synthesis. Table 6 synthesizes estimated coefficients and implied mechanisms across all three channels. Among aligned employees, money flows toward the firm: PAC giving rises ($\hat{\beta} = 0.057, p < 0.001$) alongside co-partisan giving ($\hat{\beta} = 0.021, p < 0.05$). The firm-linked channel and the broader partisan channel reinforce each other. Among misaligned employees, the picture inverts: money flows away from the firm linked PAC ($\hat{\beta} = -.0.011, p < 0.05$) and toward their own partisan side. A single corporate stance produces simultaneous mobilization and counter-mobilization, with the direction depending on which side of the stance the employee sits. Aligned employees channel resources toward the firm's preferred political vehicle; misaligned employees channel resources in the opposite direction. These patterns hold symmetrically across liberal and conservative stances (Table 5), indicating that the underlying mechanism reflects a general property of how partisan corporate signals interact with employee political identity rather than a feature of any particular issue or political moment.

The survey results complement the field analysis with measurement precision the field cannot offer. Direct elicitation of issue-specific attitudes recovers alignment-conditional estimates that the coarse-proxy nature of party registration in the field attenuates. The within-respondent design isolates the alignment gradient cleanly, and the randomized assignment of stance direction extends external validity beyond the 2018-2022 liberal-leaning stance mix. Together, the field and the experiment establish that partisan corporate stances mobilize aligned employees toward the firm-linked PAC channel and toward co-partisan giving more broadly, while misaligned employees withdraw from the firm-linked channel and counter-mobilize toward their own partisan side — a bifurcation in the political behavior of the workforce that operates through both organizational and external channels.

Table 5: Heterogeneity by alignment across outcomes

Panel A: Extensive margin (any/indicator)

Dependent Variables:	Any	Any PAC	Any Dem org donation	Any Rep org donation
Model:	(1)	(2)	(3)	(4)
Variables				
Post	-0.0322*	-0.0429**	-0.0224	-0.0585***
	(0.0146)	(0.0147)	(0.0141)	(0.0153)
Liberal stance x Post	0.0175	0.0682***	-0.0049	0.0223
	(0.0190)	(0.0181)	(0.0168)	(0.0183)
Conservative stance x Post	0.0389*	0.0268	0.0075	0.0481*
	(0.0191)	(0.0170)	(0.0172)	(0.0193)
Liberal stance × Alignment	0.0299	0.1236***	0.0402**	-0.0317
	(0.0192)	(0.0204)	(0.0145)	(0.0189)
Conservative stance × Alignment	0.0100	0.0634***	-0.0305*	0.0506**
	(0.0181)	(0.0165)	(0.0132)	(0.0174)
Baseline mean (pre, control)	0.711	0.261	0.545	0.376
Fixed-effects				
Respondent FE	Yes	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	2,328	2,328	2,328	2,328
Respondents	388	388	388	388
R^2	0.76981	0.71304	0.80963	0.77538

Panel B: Intensive margin (amounts)

Dependent Variables:	Any	PAC	Amount to Dem orgs	Amount to Rep orgs
Model:	(1)	(2)	(3)	(4)
Variables				
Post	-2.451	-0.4640	0.2517	-2.239*
	(1.294)	(0.6491)	(1.102)	(1.056)
Liberal stance x Post	1.791	2.751**	-3.260*	2.301
	(1.705)	(0.8518)	(1.382)	(1.466)
Conservative stance x Post	3.390	1.232	0.1260	2.031
	(1.741)	(0.8558)	(1.463)	(1.652)
Liberal stance × Alignment	3.120	5.115***	0.6938	-2.689
	(1.808)	(0.9138)	(1.303)	(1.583)
Conservative stance × Alignment	0.4593	2.044*	-4.243***	2.659
	(1.670)	(0.8210)	(1.207)	(1.545)
Baseline mean (pre, control)	61.292	6.948	35.651	18.693
Fixed-effects				
Respondent FE	Yes	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	2,328	2,328	2,328	2,328
Respondents	388	388	388	388
R^2	0.79798	0.53765	0.80457	0.73933

Notes: Sample excludes neutral respondents (Feeling = 0). Respondent and issue fixed effects included. Standard errors clustered by respondent in parentheses.
*Significance codes: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$.*

Table 6: Estimated effects and implied mechanisms by alignment

Hypothesis		Any Giving H1	PAC Giving H2	Co-partisan non-PAC H3
Aligned	Intensive	2.198***	2.433***	0.045
		(0.519)	(0.439)	(0.690)
	Extensive	0.024***	0.057***	0.021**
		(0.006)	(0.008)	(0.007)
	Mechanism	Mobilization	Approval	Mobilization
Misaligned	Intensive	1.309*	−0.181	1.710**
		(0.539)	(0.256)	(0.643)
	Extensive	0.014*	−0.011*	0.014†
		(0.006)	(0.005)	(0.007)
	Mechanism	Mobilization	Withholding	Backlash

Notes: Coefficients are Post × Aligned Intensity and Post × Misaligned Intensity from Table 4 (Equation 2).
Standard errors in parentheses, clustered by respondent.
All p-values two-tailed. Signif. codes: $^{\dagger}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

6 Discussion

The results of the two studies converge on a clean picture. Corporate partisan stances raise employee political giving on average, and that average effect masks a sharp split by ideological alignment: aligned employees mobilize toward the firm-linked PAC and toward co-partisan giving, while misaligned employees withhold from the PAC and counter-mobilize through heightened giving to their own partisan side. A single stance therefore produces simultaneous mobilization and counter-mobilization within the same workforce, with the direction of response determined by where the employee sits relative to the stance. Table 6 synthesized this pattern across the three outcome channels of the experimental design; the field evidence in Section 4 reproduces its main directional features at scale and adds a non-trivial finding the experiment cannot deliver - that stances activate political participation among employees with no prior history of political giving, including through the firm-linked PAC channel. This section develops five implications: that the mechanism is identity activation rather than information or social pressure; that the PAC functions as a firm-linked approval channel; that stances broaden the political-donor base through that channel; that the channel composition of the response varies systematically with the type of stance; and that, taken together, the evidence repositions the firm as an intermediary political institution.

The pattern of effects is difficult to reconcile with a purely informational account of corporate political speech, in which stances move giving uniformly by raising issue salience or clarifying policy stakes. If salience were the operative mechanism, we would expect increases in giving across the ideological spectrum, not the sharp alignment-conditional response documented here. The data instead match a cue-based account: employees interpret employer stances through the lens of partisan identity, and the behavioral consequences - mobilization, approval, withholding, or backlash - depend on whether the cue is congruent or dissonant with that identity.

Two alternative mechanisms produce distinct empirical signatures that the data rule out. Persuasion would predict that misaligned employees shift toward the firm's position, increasing cross-partisan giving as their beliefs update toward the stance. The survey experiment shows the opposite: misaligned respondents direct heightened giving to their own partisan side in proportion to the intensity of their disagreement ($\hat{\beta}_3 = 0.014$, $p < 0.05$ on the extensive margin of co-partisan giving). Counter-mobilization scaling with disagreement is the behavioral signature of identity activation, not belief updating.[15] Pressure - the alternative that misaligned employees give more to the firm's PAC to signal loyalty even when they disagree with the stance - would predict positive PAC giving among misaligned respondents. The survey experiment shows the opposite: misaligned respondents withhold from the PAC channel, with withholding intensifying in the degree of disagreement ($\hat{\beta}_3 = -0.011$, $p < 0.05$ on the extensive margin of PAC giving).

A distinctive contribution of this paper is the identification of corporate PAC giving as a firm-linked channel through which employees express approval or withhold support in response to employer political signals. Prior work has shown that employee ideology can discipline corporate PAC strategy from the outside (Li 2018; Li and DiSalvo 2023). The present findings suggest a complementary inside channel: when firms take public partisan stances, they actively mobilize aligned employees toward the PAC and induce withdrawal among the misaligned, simultaneously strengthening and reshaping the organizational base for subsequent corporate political activity. The implications for firms depend on workforce composition. A politically homogeneous workforce - increasingly plausible given partisan sorting in labor markets (Chinoy and Koenen 2024; Frake, Hurst, and Kagan 2025; Colonnelli et al. 2023; Bonica et al. 2020) - yields substantial PAC benefit with limited counter-mobilization. A heterogeneous workforce yields the reverse: stances deepen support among the aligned while organizing the misaligned in opposition.

The PAC response is not confined to employees already inside the donor population. Among the 686,713 treated employee-stacks with no prior DIME donation record, stances produce an implied 2,006 first-time PAC donor-weeks in the 13-week post window - substantially larger in absolute terms than the corresponding mobilization among the 23,767 prior-donor employee-stacks (implied 295).[16] The contrast is theoretically informative. External donations can be made through

[15] Hurst and Lee (2024) document a parallel asymmetric pattern in the labor market: a progressive corporate stance produces concentrated repulsion among strong Republicans rather than broad sorting, consistent with identity-protective response among the misaligned.

[16] Appendix Table A.11 reports the full activation breakdown across all eight outcomes. The non-donor PAC coefficient is $\hat{\beta} = 2.25 \times 10^{-4}$ (SE 1.12×10^{-4}, $p = 0.051$), marginal at conventional thresholds; the intensive-margin counterpart is $\hat{\beta} = 0.017$ (SE 0.010, $p = 0.083$), pointing in the same direction. Both estimates are best read as suggestive evidence of first-time PAC activation, with magnitudes large in absolute terms (implied 2,006 first-time donor-weeks and $153,515 in implied contributions).

one-click platforms with no organizational relationship; corporate PAC participation typically requires institutional infrastructure - payroll deduction enrollment, restricted-class membership awareness, familiarity with the firm's vehicle - that is normally inaccessible to non-donors. That stances mobilize substantial first-time PAC participation despite this infrastructure implies they are lowering the activation threshold for a channel that would otherwise be effectively closed. Corporate stance-taking is, on this evidence, expansionary on the firm-linked side: stances broaden the political-donor base through organizational infrastructure that the firm controls.

The pooled estimates conceal meaningful variation in which channel responds to the stance. The Insurrection subsample shows mobilization concentrated in the PAC channel ($\hat{\beta} = 0.058$, $p < 0.05$ Insurrection vs. -0.001 non-Insurrection); the non-Insurrection subsample shows mobilization concentrated in Democratic giving ($\hat{\beta} = 0.086$, $p < 0.05$ non Insurrection vs. 0.013 Insurrection). A coherent reading: the channel through which employees respond tracks the channel through which the firm itself acts. Insurrection-period stances were statements about which candidates the firm would no longer fund - political behavior expressed *through* the PAC. Employees responded in kind. BLM, abortion, gun-rights, and voting-rights stances were communicative rather than PAC-channeled, and employee response routed through external partisan channels accordingly.

The same logic clarifies the Republican-amount cell, where the pooled near-zero ($\hat{\beta} = -0.011$, NS) conceals a strong positive Insurrection coefficient (+0.092, $p < 0.001$) and a marginally negative non-Insurrection coefficient (-0.079, $p < 0.10$). Insurrection stances were directly anti-Republican-coded - public condemnations of January 6 and PAC suspensions targeting Republicans who voted against certifying the 2020 election. The positive Insurrection-subsample coefficient is driven specifically by Republican-registered employees, consistent with the predicted alignment-specific backlash mechanism.[17] The other liberal stances, while ideologically liberal, were not as cleanly anti-Republican-coded and produced no comparable signal. The pooled null is not "no effect"; it is the average of two oppositely-signed responses to event types that mobilize different parts of the firm's political infrastructure.

Taken together, the evidence repositions the firm as an intermediary political institution - analogous in some respects to unions, churches, and civic associations (Verba, Schlozman, and Brady 1995; Hertel-Fernandez 2016), but distinctive in three ways: repeated exposure as a feature of the daily institutional environment, imperfect exit (leaving a politically dissonant employer carries economic costs leaving a church does not), and an institutionalized giving vehicle that can absorb approval responses, channel them into ongoing political activity, and - as the activation evidence shows - recruit new participants. The contrast with unions is sharper still given recent evidence that unions exert less direct political influence on member behavior than commonly assumed under proper causal identification (Yan 2025): the institution long theorized as the workplace political

[17] Decomposing the Insurrection-subsample Republican-amount coefficient by employee party registration yields $\hat{\beta} = 0.209$ ($p < 0.01$) for Republican-registered employees against a baseline weekly mean of \$0.526, versus $\hat{\beta} = 0.013$ ($p = 0.051$) for Democratic-registered employees against a baseline of \$0.061. The Republican-employee coefficient is roughly 2.3 times the pooled subsample estimate and is concentrated among employees whose partisan identity is most directly threatened by the stance content.

intermediary turns out to be weakly effective on its members just as the firm becomes politically vocal with an institutionalized giving vehicle attached.

The closest parallel is Hertel-Fernandez (2024), who shows that with inclusive participation norms, local union leaders can mobilize cross-pressured conservative members into the union's PAC. The present paper documents the inverse case: when firms take public partisan stances without comparable participatory infrastructure, cross-pressured employees withdraw from the PAC and counter-mobilize externally. The two cases together clarify that organizational signal type and participatory culture jointly determine whether cross-pressured stakeholders mobilize through, withdraw from, or counter-mobilize against a politically active organization.

The misaligned co-partisan response is also the behavioral signature affective polarization predicts (Mason 2018; Iyengar et al. 2019): partisan identity activated by perceived threat, producing investment in one's own side rather than belief updating. The firm is not just a stage on which political signals are received; it is an active driver of the polarized engagement these literatures predict.

7 Conclusion

Corporate political speech has undergone a structural shift. Where firms once engaged politics quietly - through lobbying, regulatory bargaining, and behind-the-scenes PAC giving - they increasingly do so publicly, taking overt and often explicitly partisan positions on salient social issues. That shift has attracted substantial attention to the economic consequences of corporate stances: their effects on consumer demand, investor sentiment, and employee recruitment. This paper asks a different question: do public corporate stances move employee politics?

The answer is yes, and in ways that are both theoretically precise and empirically tractable. Aligned employees mobilize - toward the firm-linked PAC and toward their own partisan side - while misaligned employees withhold from the firm-linked channel and counter-mobilize toward their own. The same stance produces simultaneous mobilization and counter-mobilization within a single workforce, with the direction determined by where the employee sits relative to the cue. The behavioral signature - misaligned employees moving toward their own partisan side rather than crossing over - is consistent with identity activation and inconsistent with belief updating or pressure-based loyalty. And the firm-linked channel is not merely a vehicle for the already-engaged: stances activate substantial first-time PAC participation among employees with no prior donation history, broadening the political-donor base through organizational infrastructure firms control.

This reframing carries implications beyond the immediate findings. The corporate-conservative turn - the rollback of DEI commitments,[18] the revision of reproductive health policies, the distancing from prior racial-justice statements - is therefore not only an economic and reputational story but a political one, with downstream consequences for employee mobilization, PAC funding, and the partisan composition of political finance. The survey experiment's symmetry across stance directions

[18] The use of 'DEI' in S&P 500 annual filings dropped 68% between 2024 and 2025 alone (Conference Board 2025).

suggests the mechanisms documented here operate regardless of which side the firm takes, but whether they operate at the same magnitude under conservative stances in the field remains an open empirical question. Understanding how corporate partisanship reshapes democratic participation - and whether those effects persist, sort across employers, or compound through organizational infrastructure - is a research agenda whose importance will only grow as the line between corporate and partisan identity continues to blur.

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A Additional Tables and Figures for Observational Analysis

A.1 Analytical Sample of Firm Events

Table A.1 lists the 44 corporate stance events used in the analytical sample, drawn from 33 distinct publicly traded parent firms. The sample spans five issue domains: corporate responses to the January 6, 2021 insurrection (21 events), statements supporting racial-justice movements following the murder of George Floyd (9 events), responses to state-level abortion restrictions and *Dobbs* (7 events), responses to major gun-policy debates in early 2018 (6 events), and responses to Georgia's Election Integrity Act of 2021 (1 event). The event-construction pipeline that produced this sample is summarized in Section 4.1 and Figure 1.

Table A.1: Firm events and stance dates

Parent	Date	Event
Airbnb	2020-06-01	Black Lives Matter
Airbnb	2021-01-11	Insurrection
Airbnb	2021-09-27	Abortion
Amazon	2021-01-11	Insurrection
Amazon	2022-05-02	Abortion
American Express	2021-01-11	Insurrection
Apple	2020-06-11	Black Lives Matter
AT&T	2021-01-11	Insurrection
Bank of America	2018-04-10	Gun Rights
Bank of America	2020-06-02	Black Lives Matter
Bank of America	2021-01-11	Insurrection
Boeing	2021-01-13	Insurrection
BP	2021-01-11	Insurrection
Capital One	2021-01-11	Insurrection
Cigna	2021-01-12	Insurrection
Cisco Systems	2021-01-11	Insurrection
Citi	2018-03-22	Gun Rights
Citi	2020-09-23	Black Lives Matter
Citi	2022-03-15	Abortion
Comcast	2021-01-11	Insurrection
Dell	2021-01-12	Insurrection
Delta Airlines	2018-02-24	Gun Rights
Delta Airlines	2021-03-31	Georgia Voting Laws
Dow, Inc	2021-01-12	Insurrection
Intel	2021-01-12	Insurrection
JPMorgan	2022-06-24	Abortion
Marathon Petroleum	2021-01-11	Insurrection
Mastercard	2021-01-10	Insurrection
Meta	2022-06-24	Abortion
MetLife	2018-02-23	Gun Rights
Microsoft	2020-06-05	Black Lives Matter

Continued on next page

Table A.1 – continued from previous page

Parent	Date	Event
Microsoft	2022-05-09	Abortion
Netflix	2020-06-15	Black Lives Matter
Paypal	2020-06-11	Black Lives Matter
Pepsi	2020-06-16	Black Lives Matter
Pepsi	2020-10-08	Black Lives Matter
Starbucks	2022-05-16	Abortion
Target	2021-01-11	Insurrection
United	2018-02-24	Gun Rights
Verizon	2021-01-11	Insurrection
Visa	2021-01-11	Insurrection
Walmart	2018-02-28	Gun Rights
Walmart	2021-01-14	Insurrection
Wells Fargo	2021-01-11	Insurrection

Note: This table lists the 44 corporate political stance events used in the analytical sample, drawn from 33 distinct publicly traded parent firms. Each row corresponds to a firm–event pairing, where *Parent* is the publicly traded parent company and *Date* is the event date on which the firm's public stance was first issued or reported. *Event* denotes the issue category (Insurrection = responses to January 6, 2021, with stance dates concentrated between January 10–14, 2021; Georgia Voting Laws = responses to Georgia's Election Integrity Act of 2021; Abortion = abortion and reproductive-rights events in 2021–2022; Black Lives Matter = racial-justice statements in 2020; Gun Rights = responses to major gun-policy debates in February–April 2018). Firms may appear multiple times because some companies took stances on multiple issues and/or at multiple dates within an issue.

A.2 Data Construction and Merging Procedure

The linked dataset used in Study 1 is assembled by merging three sources: Revelio LinkedIn employment data, the L2 voter file, and DIME campaign finance records.

Revelio (LinkedIn) data. I use a nationally comprehensive dataset of LinkedIn profiles scraped between 2008 and 2025, covering 705 million profiles across 20 million companies globally. The data contains only information that individuals share publicly – analogous to a resume – including name, geographic location, education history, and employment history. Because LinkedIn users rarely report their birth year directly, I infer it using the following priority ordering: (1) high school graduation year minus 18; (2) high school start year minus 14; (3) bachelor's degree graduation year minus 23; (4) bachelor's degree start year minus 19; (5) first reported job start year minus 23.

I standardize each user's self-reported location string to a Census geographic identifier using the 2024 Census Gazetteer files for places, counties, core-based statistical areas (CBSAs), and states. Each string is cleaned and matched to the nearest Census place name within the reported state using Jaro-Winkler string distance (threshold < 0.10), falling back sequentially to county and then CBSA if no place-level match is found. Only profiles with a non-missing name, non-missing location, and a successfully assigned geographic level are eligible for matching, yielding a pool of 135,949,177 profiles.

L2 voter file. I use a 2025 snapshot of the nationwide L2 voter file, containing 185 million records with each registrant's full name, residential address, party registration (where recorded), and date of birth. Party registration is formally recorded in 30 states and the District of Columbia; in the remaining 20 states, L2 imputes party registration from primary-voting participation and other behavioral indicators. The main difference-in-differences specification uses the inclusive measure that combines formal registration in coverage states with L2's imputation elsewhere; Appendix A.14 reports a parallel-specification robustness check restricting the panel to employees in the 30 coverage states plus DC, with headline results preserving sign and significance. I standardize L2 residential cities to Census geographic identifiers using the same Gazetteer-based fuzzy matching procedure used for the LinkedIn data, producing a match-level variable (place, county, or CBSA) that is used during the merge to ensure Revelio and L2 records are compared only within the same type of geographic unit.

Merging Revelio and L2. I merge Revelio profiles to L2 voter records using an iterative procedure of 180 rounds organized into four name-matching tiers, proceeding from most to least restrictive. I block on state throughout, so records are never matched across state lines. A profile is eligible for a given round only if it has not been matched in any prior round.

Tier 1 matches on exact full name. *Tier 2* truncates the first name to its first four characters and requires exact agreement on last name, recovering profiles where a nickname or abbreviation appears on LinkedIn (e.g., "Alex" vs. "Alexander"). *Tier 3* truncates both first and last name to four characters. *Tier 4* applies nickname normalization – mapping common nicknames to canonical forms – before running the same exact-name rounds as Tier 1; two passes are run using different nickname libraries, with the deeper-search pass preferred in case of conflict.

Within each tier, rounds proceed from most to least restrictive: matches requiring both birth year agreement (±10 years) and middle initial consistency are attempted first, followed by birth year only, middle initial only, and finally name and geography alone. Across all tiers and rounds, a Revelio profile is retained only if it maps to exactly one L2 voter record; profiles that cumulate two or more candidate matches are excluded from the final crosswalk. Of the 135,949,177 eligible Revelio profiles, 32.5% are matched to exactly one L2 voter record, a rate comparable to similar iterative procedures in the literature (Chinoy and Koenen 2024; Kim 2025). Appendix Table A.2 reports the distribution of matches by tier in the analytical sample; the main difference-in-differences estimates are qualitatively unchanged when restricted to the Tier 1 subsample (Appendix Table A.3).

Campaign finance records (DIME). Individual-level campaign finance data come from DIME (Bonica 2024), which contains itemized contribution records through 2024 to local, state, and federal elections, as well as to political action committees (PACs) and interest groups. I link DIME contributor records to L2 voter IDs using a pre-built name-and-address crosswalk provided by Adam Bonica, which leverages full residential address information available in both datasets. The crosswalk is joined to the Revelio-L2 matched sample via the shared voter ID, so that each matched Revelio profile inherits the campaign finance history of the corresponding voter file record.

A.3 Match Quality and Tier 1 Robustness

The iterative LinkedIn-to-voter-file merge procedure described in Section 4 produces matches of varying confidence across rounds. Table A.2 reports the distribution of matched individuals in the analytical sample across the four tiers, ordered by stringency. The majority of matches (72.6%) are recovered in Tier 1 using exact first and last names. A further 18.8% are recovered through truncated-name matching (Tiers 2 and 3), which uses the first four characters of one or both names, and the remaining 8.5% are recovered through Tier 4 nickname normalization. Two features limit false-positive risk across all tiers: matches are enforced to be globally unique (any individual linked to more than one voter-file record across any round is excluded), and within each tier the most restrictive sub-tiers (requiring both birth year ±10 years and middle initial) are attempted before relaxing to name and geography alone.

Table A.2: Match-quality distribution (N=44 canonical sample)

	Upstream crosswalk (all matches)			N=44 panel employees		
Tier	N matched	Share	Cum. N	N matched	Share	Cum. N
Tier1 (ExactName)	399,937	72.6%	399,937	378,188	73.3%	378,188
Tier2 (First4-FullLast)	60,817	11.0%	460,754	57,025	11.1%	435,213
Tier3 (First4-Last4)	43,188	7.8%	503,942	39,309	7.6%	474,522
Tier4 (Nickname)	46,811	8.5%	550,753	41,412	8.0%	515,934
Total	550,753	100.0%	—	515,934	100.0%	—

Notes: Tier 1 = exact-name match; Tier 2 = first-four-character + full-last-name; Tier 3 = first-four-character + last-four-character; Tier 4 = nickname-normalized. Upstream-crosswalk column counts every user the merge considered; N=44-panel-employees column restricts to users present in at least one of the 44 main analytical-sample stacks.

To assess whether the main results are sensitive to lower-confidence matches, Table A.3 re-estimates the headline difference-in-differences specification on the Tier 1 subsample alone - the 72.6% of the analytical sample recovered via exact full-name matching. All other specification choices are identical to Table 3: employee-stack and calendar-week fixed effects, SEs clustered by `stack`. The Tier 1 subsample contains 378,188 unique users across 15.7 million employee-week observations (51.3% of the full-sample row count).

Coefficient estimates are qualitatively unchanged across all reported cells: signs are preserved on the headline outcomes, and the headline intensive-margin cells (total amount, PAC amount, Democratic amount) retain their pooled significance levels under Tier 1 restriction. The PAC-participation extensive-margin cell loses significance under Tier 1 (β shrinks roughly threefold), reflecting the smaller subsample and the rarity of PAC participation events; sign is preserved. The Republican-amount cell is omitted from Table A.3 because it is not statistically significant in the pooled specification (Table 3 cell-by-cell $p = 0.73$) and is volatile across every diagnostic restriction documented elsewhere in this appendix; Appendix A.12 establishes that the pooled near-zero is the average of oppositely-signed Insurrection and non-Insurrection subsample effects, which makes it uninformative for assessing match-quality sensitivity. The overall pattern is consistent with lower-stringency matches introducing attenuation noise rather than systematic bias on the headline

Table A.3: Table 3 robustness — Tier 1 exact-name matches only (N=44)

	Full N=44 sample			Tier 1 only		
Outcome	$\widehat{\beta}$ (SE)	Baseline	%Δ	$\widehat{\beta}$ (SE)	Baseline	%Δ
Panel A: Extensive margin (any/indicator)						
Any donation	$5.35\times10^{-4*}$ (2.49×10^{-4})	0.005991	8.9%	$7.70\times10^{-4**}$ (2.70×10^{-4})	0.004925	15.6%
Any PAC donation	$2.69\times10^{-4*}$ (1.22×10^{-4})	9.22×10^{-4}	29.2%	9.51×10^{-5} (1.04×10^{-4})	6.10×10^{-4}	15.6%
Any donation to Dem orgs	1.68×10^{-4} (1.66×10^{-4})	0.003041	5.5%	3.16×10^{-4} (1.89×10^{-4})	0.002670	11.8%
Any donation to Rep orgs	$1.51\times10^{-4*}$ (6.00×10^{-5})	0.001400	10.8%	$1.87\times10^{-4**}$ (6.53×10^{-5})	8.80×10^{-4}	21.3%
Panel B: Intensive margin (winsorized amounts)						
Total donation amount	0.1301^{**} (0.0392)	0.603811	21.5%	0.1316^{**} (0.0419)	0.444534	29.6%
PAC amount	0.0229^{*} (0.0103)	0.091053	25.1%	$9.84\times10^{-3*}$ (4.66×10^{-3})	0.053753	18.3%
Amount to Dem orgs	0.0572^{*} (0.0283)	0.262354	21.8%	$0.0654^{\dagger}$ (0.0364)	0.195802	33.4%
Amount to Rep orgs	-0.0109 (0.0319)	0.156061	-7.0%	0.0331^{*} (0.0131)	0.108834	30.4%
Unique users	736,566			378,188		
Observations	28,015,550			15,703,575		

Notes: N=44 main analytical sample. Tier 1 = users matched via exact full name; excludes users recovered via truncated-name (Tiers 2–3) or nickname-normalized (Tier 4) matches. Specification matches Table 3: $y \sim$ did | user_id^stack_id + cal_week, SEs clustered by `stack` (G=44). Baseline is the within-subsample pre-period control mean averaged across stacks. Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.10.

outcomes.

A.4 Two-Period Robustness Check

The headline difference-in-differences estimates in Section 4.3 are recovered from a weekly panel with calendar-week fixed effects. A standard concern with weekly DiD specifications is that within-panel serial autocorrelation can bias cluster-robust standard errors when treatment effects are concentrated in a narrow event-time window (Bertrand, Duflo, and Mullainathan 2004). To assess whether the headline conclusions are robust to this concern, I collapse the weekly panel to a two-period pre/post structure across a range of averaging windows and re-estimate the DiD coefficient at each window.

For a window of size w, the panel is restricted to weeks $[-w, -1]$ in the pre-period and weeks $[0, w-1]$ in the post-period (so each $\pm w$ specification uses w weeks on each side of the stance, with week 0 assigned to the post-period). Each user-stack's pre-period and post-period outcomes are averaged into single observations, yielding two rows per (user, stack) combination. I then estimate

$$Y_{is,p} = \alpha_{is} + \pi_p + \beta\,(\mathit{Treated}_{is} \times \mathit{Post}_p) + \varepsilon_{is,p}, \tag{4}$$

where $p \in \{0, 1\}$ indexes the pre/post period, α_{is} are employee-by-stack fixed effects, π_p is a period fixed effect, and standard errors are clustered by stack. The collapsed-panel structure eliminates the autocorrelation concern by construction.

Figure A.1 reports the resulting DiD coefficients across averaging windows from ± 2 to ± 12 weeks for six outcomes: total donation amount, PAC amount, and Democratic-amount on the intensive margin, and the corresponding any-donation indicators on the extensive margin.

Three patterns are visible. First, all six outcomes are positive at every displayed window, matching the sign of the pooled weekly estimates in Table 3 across the entire window range. Second, the intensive-margin headline (total donation amount) hovers between 0.08 and 0.14 across all displayed windows, against a pooled weekly estimate of 0.130 — a reconciliation within roughly 10–40% at every window. The PAC intensive coefficient is largest at ± 2 ($\hat{\beta} = 0.103$), substantially exceeds the pooled weekly estimate of 0.023 at narrow windows, and attenuates toward 0.02 at ± 12. The PAC extensive coefficient shows the same shape on the indicator margin. Third, the temporal concentration of PAC effects at narrow windows is consistent with a discrete communication shock that activates the firm-linked giving channel in the period of peak stance salience, rather than reflecting a persistent shift in PAC giving behavior over the full post-window.

The ± 4-week estimates merit specific mention because they match the pooled weekly results closely in sign and rough magnitude: total amount $\hat{\beta} = 0.114$ ($p < 0.05$) vs. pooled 0.130; PAC amount $\hat{\beta} = 0.058$ ($p < 0.05$) vs. pooled 0.023; Democratic amount $\hat{\beta} = 0.040$ ($p < 0.10$) vs. pooled 0.057. All three intensive headline outcomes preserve sign and reach at least marginal significance at the ± 4 window under the two-period collapse. The extensive-margin headlines are positive across the displayed window range but reach conventional significance only at intermediate windows (± 2 for total participation, ± 4 for PAC participation), reflecting the lower power of binary outcomes after period collapse.

The two-period results therefore corroborate the headline weekly estimates: the sign and rough

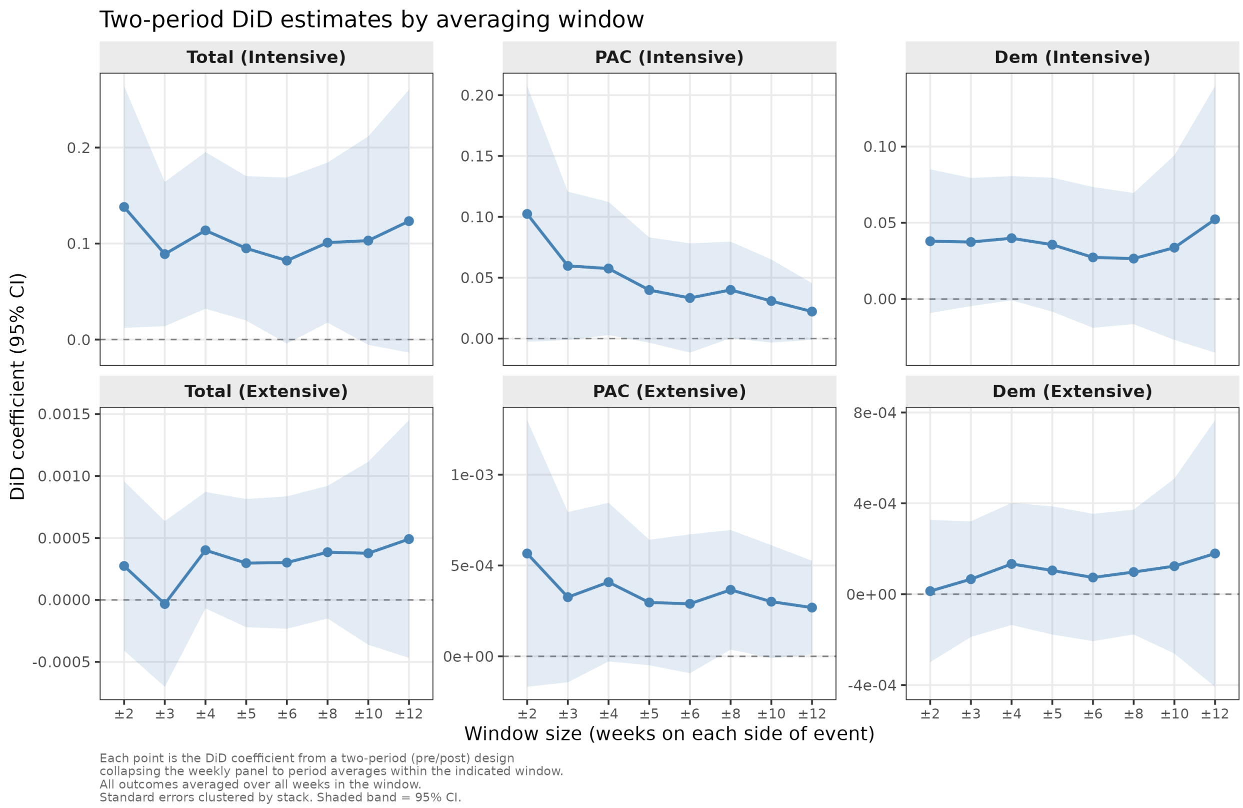


Figure A.1: Two-period DiD estimates by averaging window. Each point is the DiD coefficient from a two-period (pre/post) design collapsing the weekly panel to period averages within the indicated window. Outcomes averaged over all weeks in the window. Standard errors clustered by stack. Shaded band = 95% CI.

magnitude of the pooled DiD are preserved under a specification that eliminates serial autocorrelation by construction, with the temporal profile of PAC effects consistent with a discrete event-time shock. The two-period collapse necessarily absorbs less calendar-time variation than the weekly specification with calendar-week fixed effects, so the point estimates differ in magnitude across windows - the reconciliation is intended to confirm direction and rough scale, not exact replication.

A.5 Pre-Trends Assessment

A standard concern in difference-in-differences event studies is that treatment timing may be correlated with unobserved shocks that also move the outcome. To assess whether such confounding dynamics are present, I take two complementary approaches. First, I implement a modified Granger-style falsification test (Angrist and Pischke 2008; Roth 2022) that collapses the pre-trend assessment onto a small number of lead coefficients and a joint Wald test – an approach well powered for detecting differential pre-trends on sparse weekly donation outcomes (Li 2018). Second, I report full week-by-week event-study estimates as a visual complement that displays the entire dynamic pattern, including the post-period response. Both approaches reach the same conclusion: there is no statistically meaningful evidence of differential pre-trends prior to the stance week, while treated employees exhibit a clear contemporaneous shift in donation behavior at the stance week.

Modified Granger Causality Test

If the identifying parallel-trends assumption holds, then "future treatment" (i.e., event-time leads) should not predict current outcomes after conditioning on the fixed-effects structure. Concretely, for each outcome Y_{ist} I estimate:

$$\begin{aligned} Y_{ist} = \alpha_{is} + \gamma_{cw(s,t)} + \beta_0\big(\mathit{Treated}_{is} \times \mathbb{1}\{t=0\}\big) \\ + \sum_{m=1}^{2} \gamma_m\big(\mathit{Treated}_{is} \times \mathbb{1}\{t=m\}\big) + \sum_{q=2}^{4} \lambda_q\big(\mathit{Treated}_{is} \times \mathbb{1}\{t=-q\}\big) + \varepsilon_{ist}, \end{aligned} \tag{5}$$

where α_{is} are employee-stack fixed effects, $\gamma_{cw(s,t)}$ are calendar-week fixed effects, and t indexes event-time weeks relative to the stance week ($t = 0$). The identifying implication is that the lead coefficients should be zero; significant λ_q would indicate outcomes begin shifting before the stance week in a way that differentially affects treated relative to control employees. I report both the individual lead estimates and a joint Wald χ^2 test of $H_0 : \lambda_{-2} = \lambda_{-3} = \lambda_{-4} = 0$.

Table A.4 reports the corresponding estimates. The joint null of no pre-trends is not rejected for total donation amount ($p = 0.456$) or for Democratic-directed amounts ($p = 0.455$). Two tests sit close to the conventional 0.10 threshold: PAC amount ($p = 0.104$) and Republican-directed amounts ($p = 0.097$).

PAC marginal pre-trend. The PAC marginal is broadly distributed across stacks rather than driven by any single event. Leave-one-out diagnostics indicate that the top five candidate drops move the joint p-value into the 0.105–0.157 range; no single stack drives the result below the conventional threshold. The signal is concentrated at the immediate-pre-stance week (Lead −2, coefficient −0.110), which is consistent with the anticipation-window framing developed in Section 4.2: politically engaged employees at treated firms may begin mobilizing in response to internal deliberations before the formal public announcement, producing a small pre-period upward shift in PAC giving. To the extent anticipation operates, it pushes some of the treatment effect into the pre-period window and

Table A.4: Granger Causality Test

Dependent Variables:	Total Amount	PAC Amount	Dem Amount	Rep Amount
Model:	(1)	(2)	(3)	(4)
Variables				
Lag 2 (week +2)	-0.0015	-0.0122	0.0124	-0.0422†
	(0.0332)	(0.0108)	(0.0277)	(0.0218)
Lag 1 (week +1)	-0.0052	0.0022	0.0049	-0.0519
	(0.0296)	(0.0096)	(0.0240)	(0.0359)
Lag 0 (week 0)	0.1156**	0.0293†	0.0796*	-0.0428†
	(0.0393)	(0.0155)	(0.0312)	(0.0254)
Lead 2 (week -2)	-0.1057	-0.1099†	0.0089	-0.0238
	(0.0783)	(0.0558)	(0.0367)	(0.0251)
Lead 3 (week -3)	-0.0310	-0.0108	-0.0220	0.0062
	(0.0316)	(0.0120)	(0.0249)	(0.0231)
Lead 4 (week -4)	-0.0530	-0.0104	-0.0244	-0.0830†
	(0.0426)	(0.0158)	(0.0275)	(0.0453)
Lead test p-value (H0: Lead -2 = Lead -3 = Lead -4 = 0)	0.4560	0.1041	0.4545	0.0970
Fixed-effects				
Employee-Stack	Yes	Yes	Yes	Yes
Calendar Week	Yes	Yes	Yes	Yes
Fit statistics				
Observations	28,015,550	28,015,550	28,015,550	28,015,550
R^2	0.15700	0.13744	0.13963	0.09057

Clustered (stack) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.1*

attenuates the post-period coefficient – so the headline DiD is best read as a conservative lower bound on the true firm-level treatment effect rather than as evidence of a violation of parallel trends.

Rep-org marginal pre-trend. The Republican-organization marginal is driven by a single stack – Walmart × Gun Rights 2018-02-28 – whose pre-period contains firearms-retailer exposure unrelated to the firm's stance. Dropping that stack alone moves the joint p-value from 0.097 to 0.295. The headline Republican-amount cell in Table 3 is itself statistically indistinguishable from zero (cluster-robust $p = 0.73$; WCB $p = 0.82$), so the marginal pre-trend does not jeopardize a substantive finding. Following Roth (2022), I note that failure to reject the null of no pre-trends should be interpreted cautiously given the limited power of conventional pre-test procedures on sparse outcomes; the parallel-trends assumption is ultimately made more credible by the design choices described in Section 4.2 – particularly within-NAICS control matching and event-year alignment – rather than by the pre-test alone.

Full Event-Study Dynamics

As a visual complement to the Granger-style test above, I estimate the full event-study generalization of the baseline specification:

$$Y_{ist} = \alpha_{is} + \tau_{cw(s,t)} + \sum_{\substack{k=-12 \\ k\neq -1}}^{12} \beta_k \big(\mathit{Treated}_{is} \times \mathbb{1}\{t = k\} \big) + \varepsilon_{ist}, \tag{6}$$

where the omitted reference period is the week immediately preceding the stance ($k = -1$), α_{is} are employee-by-stack fixed effects, and $\tau_{cw(s,t)}$ are calendar-week fixed effects. Standard errors are clustered by stack. Each coefficient $\hat{\beta}_k$ recovers the treated-control difference in the outcome in event-time week k, relative to the week before the stance.

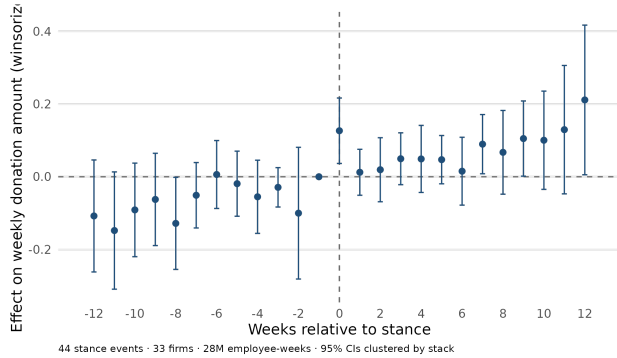
Figure A.2 reports the estimated $\{\hat{\beta}_k\}$ for the two headline pooled outcomes: total winsorized donation amount (Panel A, intensive margin) and any donation (Panel B, extensive margin). Three features of the figure are worth noting.

First, every pre-period coefficient on both outcomes has a 95% confidence interval that contains zero. On the extensive margin, the pre-period point estimates hug the zero line throughout the window. On the intensive margin, the pre-period coefficients sit modestly below zero in the earliest weeks ($k \in \{-12, -8\}$, point estimates in the range -0.10 to -0.15) before tightening toward zero by $k = -2$. Both patterns are statistically indistinguishable from a null of parallel trends, consistent with the joint test reported in Table A.4 ($p = 0.456$ for total amount).

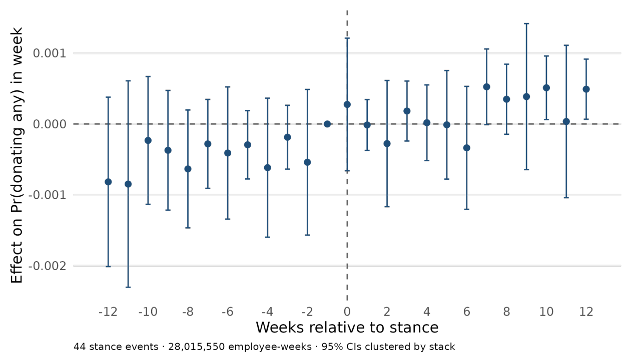
Second, the event-time coefficient at $k = 0$ – the week of the stance itself – jumps to $\hat{\beta}_0 \approx 0.13$ on the intensive margin, matching the pooled *Treated* $\times$ *Post* coefficient of 0.130 from Table 3 almost exactly. The contemporaneous timing is consistent with a discrete public-communication shock activating donation behavior in the week of peak stance salience, rather than reflecting gradual exposure to a slow-moving treatment.

Third, the post-period coefficients remain positive throughout the twelve-week post-window on both margins, with point estimates that drift upward over event time. This dynamic pattern is consistent with the front-loaded-but-persistent effect documented in the two-period robustness check (Appendix A.4): the largest week-on-week response is concentrated near the stance date, but the effect does not mean-revert within the analytical window.

Interpretive caveats. The early pre-period coefficients on the intensive margin, while statistically indistinguishable from zero, sit modestly below the reference line. One reading is sampling noise at the boundaries of the estimation window, where fewer stacks contribute observations to the most distant event-time weeks. A second reading is mild attenuation in the donation trajectory of treated employees in the months leading up to the stance – which, if anything, biases the post-period estimate downward and is consistent with the anticipation-window discussion in Section 4.2 and the Lead -2 pattern noted above. In either case, the absence of any individually-distinguishable pre-period coefficient, combined with the failure to reject the joint null in Table A.4, supports the parallel-trends identifying assumption.



(a) Total donation amount, winsorized (intensive margin)



(b) Any donation indicator (extensive margin)

Figure A.2: Event-study estimates of the treatment effect of corporate stance-taking on employee political giving, by event-time week. Each point reports the coefficient $\hat{\beta}_k$ from Equation 6 on the indicated outcome; whiskers are 95% confidence intervals constructed from stack-clustered standard errors. The omitted reference period is $k = -1$, the week immediately preceding the stance. Panel (a) reports the intensive margin (winsorized total donation amount); Panel (b) reports the extensive margin (any-donation indicator). The vertical dashed line marks the stance week ($k = 0$).

A.6 Calendar-Week × Congressional-District Fixed Effects

A potential concern in the baseline specification is that contemporaneous shocks may differentially affect political giving across geographic areas – for example, local political campaigns, district-specific news coverage, or mobilization efforts that vary over time and space. If such shocks are correlated with the timing of corporate stances or the composition of treated and control employees across geographies, they could confound the estimated treatment effects. I address this concern by augmenting the baseline stacked DiD specification with calendar-week × congressional-district fixed effects, which absorb any time-varying shocks common to individuals residing in the same congressional district in a given week. Formally:

$$Y_{ist} = \alpha_{is} + \delta_{d(i)\times cw(s,t)} + \beta \left(\mathit{Treated}_{is} \times \mathit{Post}_t \right) + \varepsilon_{ist}, \tag{7}$$

where α_{is} are employee-stack fixed effects and $\delta_{d(i)\times cw(s,t)}$ is the full set of congressional-district-by-calendar-week fixed effects. SEs are clustered by `stack`. Because congressional district identifiers are missing for a nontrivial share of observations, including $\delta_{d(i)\times cw(s,t)}$ mechanically reduces the estimation sample size relative to the baseline – 168,452 rows (0.6% of the panel) are absorbed as singletons.

Table A.5: Main results with calendar-week × congressional-district fixed effects: intensive and extensive margins

Panel A: Extensive margin (any/indicator)				
Dependent Variables:	Any donation	Any PAC donation	Any donation to Dem orgs	Any donation to Rep orgs
Model:	(1)	(2)	(3)	(4)
Variables				
Treated × Post	0.0005*	0.0003*	0.0002	0.0002*
	(0.0002)	(0.0001)	(0.0002)	(6.03×10^{-5})
Baseline mean (pre, control)	0.005991	0.000922	0.003041	0.001400
Percent change from baseline	9.01	30.89	5.33	10.86
Fixed-effects				
Employee-Stack	Yes	Yes	Yes	Yes
Congressional District - Calendar Week	Yes	Yes	Yes	Yes
Fit statistics				
Observations	27,847,098	27,847,098	27,847,098	27,847,098
R^2	0.28849	0.27490	0.25968	0.32426

Panel B: Intensive margin (amounts)				
Dependent Variables:	Total donation amount	PAC amount	Amount to Dem orgs	Amount to Rep orgs
Model:	(1)	(2)	(3)	(4)
Variables				
Treated × Post	0.1226***	0.0222*	0.0502*	-0.0169
	(0.0328)	(0.0100)	(0.0206)	(0.0453)
Baseline mean (pre, control)	0.603811	0.091053	0.262354	0.156061
Percent change from baseline	20.30	24.39	19.14	-10.84
Fixed-effects				
Employee-Stack	Yes	Yes	Yes	Yes
Congressional District - Calendar Week	Yes	Yes	Yes	Yes
Fit statistics				
Observations	27,847,098	27,847,098	27,847,098	27,847,098
R^2	0.14992	0.13621	0.13007	0.09053

Clustered (stack) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.1*

Table A.5 reports the corresponding estimates. The results track Table 3 cell-for-cell: same signs and same significance levels on every cell except the binary indicator for Democratic donations (Panel A column 3), which loses its star in this more demanding specification but retains its sign. Exposure to a corporate stance continues to be associated with positive and statistically significant effects on total donation amounts and on PAC contributions. The stability of the headline findings under congressional-district-by-week absorption indicates that the main results are not driven by coincident geographic shocks.

A.7 Wild Cluster Bootstrap

A potential concern with the main results is that the stacked design yields a relatively small number of clusters – 44 stacks – which can render asymptotic cluster-robust standard errors unreliable (Cameron, Gelbach, and Miller 2008). To assess whether inference is sensitive to this, Table A.6 reports wild cluster bootstrap (WCB) p-values for each outcome in Table 3, computed using the Rademacher score bootstrap with $B = 9{,}999$ replications following Cameron, Gelbach, and Miller (2008).

Table A.6: Main results: intensive and extensive margins

Panel A: Extensive margin (any/indicator)				
Dependent Variables:	Any donation	Any PAC donation	Any donation to Dem orgs	Any donation to Rep orgs
Model:	(1)	(2)	(3)	(4)
Variables				
Treated × Post	0.0005*	0.0003*	0.0002	0.0002*
	(0.0002)	(0.0001)	(0.0002)	(6×10^{-5})
Baseline mean (pre, control)	0.005991	0.000922	0.003041	0.001400
Percent change from baseline	8.93	29.22	5.52	10.79
WCB p-value (Rademacher, B=9999)	0.013	0.022	0.374	0.003
Fixed-effects				
Employee-Stack	Yes	Yes	Yes	Yes
Calendar Week	Yes	Yes	Yes	Yes
Fit statistics				
Observations	28,015,550	28,015,550	28,015,550	28,015,550
R^2	0.28770	0.27535	0.25865	0.32294

Panel B: Intensive margin (amounts)				
Dependent Variables:	Total donation amount	PAC amount	Amount to Dem orgs	Amount to Rep orgs
Model:	(1)	(2)	(3)	(4)
Variables				
Treated × Post	0.1301**	0.0229*	0.0572*	-0.0109
	(0.0392)	(0.0103)	(0.0283)	(0.0319)
Baseline mean (pre, control)	0.603811	0.091053	0.262354	0.156061
Percent change from baseline	21.54	25.11	21.81	-6.99
WCB p-value (Rademacher, B=9999)	<0.001	0.020	0.040	0.817
Fixed-effects				
Employee-Stack	Yes	Yes	Yes	Yes
Calendar Week	Yes	Yes	Yes	Yes
Fit statistics				
Observations	28,015,550	28,015,550	28,015,550	28,015,550
R^2	0.15701	0.13743	0.13963	0.09057

Clustered (stack) standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.1*

The WCB results confirm the main findings. All outcomes that are statistically significant under asymptotic clustering remain significant under the wild cluster bootstrap, and the two non-significant cells – the extensive-margin Democratic-organization indicator and the intensive-margin Republican-amount cell – are preserved as non-significant under WCB ($p = 0.374$ and $p = 0.817$ respectively). The PAC outcomes, central to the paper's theoretical claims, are robust on both margins (intensive $p = 0.020$; extensive $p = 0.022$). These results indicate that the main conclusions are not an artifact of the small-cluster approximation. The two-period robustness check in Appendix A.4 provides a complementary perspective by collapsing the weekly panel and eliminating the autocorrelation structure that motivates the WCB correction.

A.8 Heterogeneity by Employee Seniority

A potential concern with the observational design is that senior employees – particularly those at the Director level and above – are the most likely to have been involved in or aware of the stance-taking decision before its public announcement. If politically active executives who helped engineer the stance are also the ones driving the post-period donation effects, the DiD estimates would partly reflect the behavior of decision-makers rather than a genuine response to an employer cue among the broader workforce.

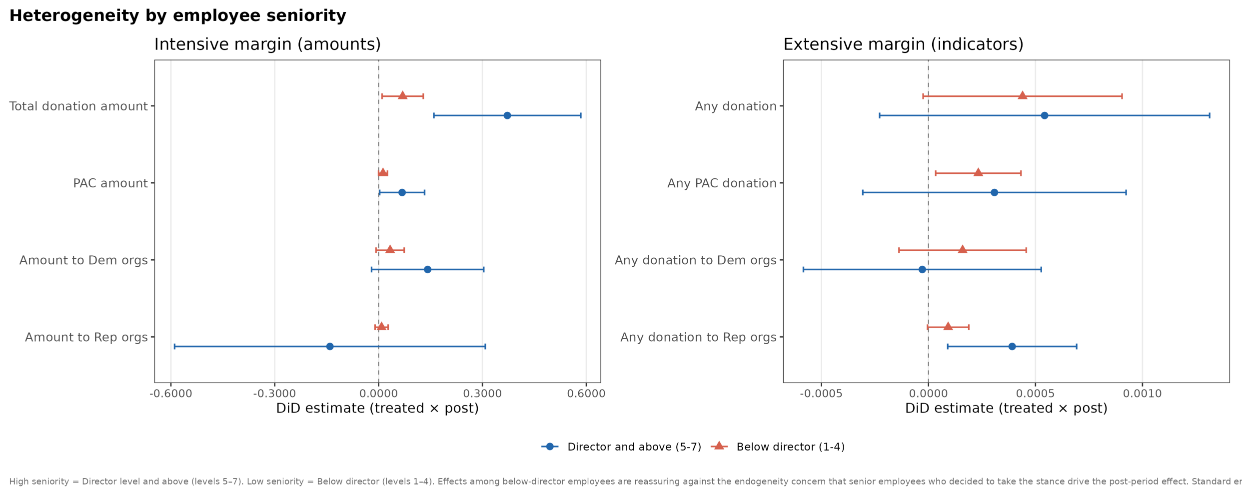


Figure A.3: Seniority Heterogeneity

Figure A.3 addresses this concern by re-estimating the main DiD separately for employees at or above the Director level (108,253 treated user-stacks) and those below (602,227 treated user-stacks). Seniority is measured using Revelio Labs' ensemble-based ordinal scale, which constructs seniority from three components: a title-based model that uses an individual's current job title, company, and industry; a career-history model that incorporates the duration and seniority of prior positions; and an age-based model. The three component scores are averaged and mapped to a seven-point ordinal scale by anchoring the continuous metric to recognizable title keywords – from Entry Level (e.g., accounting intern, software engineer trainee) through Junior, Associate, and Manager levels, up to Director Level (e.g., VP of engineering, chief of accountants), Executive Level (e.g., managing director, director of engineering), and Senior Executive Level (e.g., CFO, COO, CEO). The analysis defines high seniority as levels 5–7 (Director and above) and low seniority as levels 1–4 (below Director).

Two patterns stand out. First, both groups exhibit positive post-period effects on total donation amounts, with below-Director employees responding at $\hat{\beta} = 0.069$ ($p < 0.05$) and Director-and-above at $\hat{\beta} = 0.372$ ($p < 0.001$). The behavioral response is not confined to senior employees who may have anticipated the stance. Second, the effects are larger among the Director-and-above subsample – roughly 5.3× the below-Director coefficient. This gradient is consistent with the well-documented relationship between income, education, and political donation propensity rather than with reverse causation: higher-seniority employees are wealthier, more educated, and more likely to donate at baseline, giving the treatment more margin-movers to work with. If the seniority gradient were driven by endogeneity, we would expect the Director-and-above effect to be anomalously large relative to baseline propensity differences alone, but the pattern is exactly what a propensity-based account would generate. More importantly, the finding that below-Director employees – who had no role in the stance decision and face substantially lower baseline donation rates – also shift their political behavior is reassuring evidence that the main results reflect a genuine organizational cue mechanism operating across the employee hierarchy rather than a narrow effect concentrated among decision-makers.

A.9 Heterogeneity by Employee Partisanship

Section 4.3 reports that the Study 1 response is heterogeneous by employee party registration; the headline cells of the decomposition appear in the main text paragraph on Figure 2. This appendix documents the specification used to construct those estimates and reports the full numerical decomposition.

Subsample construction. Employee party registration is taken from the L2 voter file at the stance date. The Democratic-registered subsample restricts the canonical N=44 stacked panel to employees coded `DEM` at the stance date; the Republican-registered subsample restricts to employees coded `REP`. Employees with any other registration code (independent, third-party, unknown, unregistered) are dropped from both subsamples and are not pooled. The two subsamples therefore do not exhaust the full Table 3 estimation sample, and their row counts sum to less than the canonical 28,015,550 observations.[19] Each subsample is estimated separately with the canonical specification: two-way fixed effects (employee-stack $\times$ calendar-week), standard errors clustered by stack, w99 winsorization on intensive outcomes. Baseline means are computed within-subsample using only same-party control employees in the pre-period.

Reading the table. Table A.7 reports five outcomes by employee partisanship subsample. The first four rows replicate the canonical Table 3 outcomes (any donation, any PAC donation, total amount, PAC amount). The fifth row reports the substantively important co-partisan amount cell, defined as giving to organizations on the employee's own party side: amount to Democratic organizations for Democratic-registered employees, amount to Republican organizations for Republican-registered employees. Within-subsample baseline means appear in italics beneath each coefficient block.

The aligned coefficients reproduce the inline estimates discussed in Section 4.3: Democratic-amount $\hat{\beta} = 0.106$ ($p = 0.024$, 17.5% over a baseline of \$0.605/week) and PAC-participation $\hat{\beta} = 2.0 \times 10^{-4}$ ($p = 0.007$, 24.1% over a baseline of 8.4×10^{-4}). The misaligned co-partisan cell carries a noisy negative point estimate; the percent change is omitted from the table to avoid implying a meaningful decline against a wide confidence interval that brackets zero. The pooled cell is best understood in conjunction with the event-type split in Appendix **??**, where the Republican-amount Insurrection-only and non-Insurrection cells are reported separately.

Cross-partisan giving. Neither subsample exhibits meaningful cross-partisan giving in response to stance exposure. Democratic-registered employees' giving to Republican organizations is small in absolute terms ($\hat{\beta} = 0.011$, $p = 0.10$) against a near-zero baseline of \$0.039/week; Republican-registered employees' giving to Democratic organizations is similarly small and not statistically distinguishable from zero. Cross-partisan cells are omitted from Table A.7 for compactness.[20] This

[19] Dem-registered: 11,913,350 user-stack-weeks. Rep-registered: 7,940,925 user-stack-weeks. The remaining 8,161,275 observations correspond to employees with non-DEM/REP registration codes and are excluded from this decomposition.

[20] Full estimates available on request.

Table A.7: Heterogeneity by employee partisanship: pooled DiD on aligned (Dem-registered) and misaligned (Rep-registered) employee subsamples.

	Aligned *Dem-registered*	**Misaligned** *Rep-registered*
Any donation (extensive)		
Treated × Post	0.0007*	0.0004
	(0.0003)	(0.0003)
Percent change	+8.1%*	+6.7%
Baseline (control, pre)	0.00871	0.00589
Any PAC donation (extensive)		
Treated × Post	0.0002**	0.0004†
	(0.0001)	(0.0002)
Percent change	+24.1%**	+33.7%†
Baseline (control, pre)	0.00084	0.00124
Total donation amount (intensive)		
Treated × Post	0.150**	0.131*
	(0.049)	(0.058)
Percent change	+18.7%**	+19.7%*
Baseline (control, pre)	0.802	0.665
PAC amount (intensive)		
Treated × Post	0.012	0.048*
	(0.007)	(0.021)
Percent change	+14.7%	+36.0%*
Baseline (control, pre)	0.082	0.133
Co-partisan amount (intensive)		
Treated × Post	0.106*	−0.081
	(0.045)	(0.106)
Percent change	+17.5%*	n.s.
Baseline (control, pre)	0.605	0.347

Notes: Each cell re-estimates the canonical Table 3 stacked DiD on the indicated employee subsample, restricting the panel to Dem-registered (Aligned) or Rep-registered (Misaligned) employees. Intensive-margin outcomes are winsorized at the 99th percentile of the full-panel distribution. *Co-partisan amount* measures giving to organizations on the employee's own party side: Dem-org amount for Dem-registered employees, Rep-org amount for Rep-registered employees. The misaligned co-partisan cell's pooled coefficient is too noisy ($p = 0.45$) for a meaningful percent-change interpretation and is reported as n.s.; the underlying split is heavily driven by Insurrection vs. non-Insurrection event types (see Table A.10). Aligned subsample: $N = 11,913,350$ user-stack-weeks; Misaligned subsample: $N = 7,940,925$. Signif. codes: *** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.10$.

is the expected pattern under cue-driven mobilization and inconsistent with persuasion, which would predict aligned employees shifting some giving toward the firm's side.

Connection to other decompositions. The partisan decomposition reported here is complementary to the seniority decomposition in Appendix A.8 and the event-type decomposition in Appendix A.12. The three decompositions answer different questions about heterogeneity in the field response and do not pool: the seniority decomposition rules out an executive-only mechanism; the event-type decomposition documents channel-by-stance-type asymmetry; and the partisan decomposition here documents the alignment-channel asymmetry that the theoretical 2×2 predicts. The activation analysis in Appendix A.13 further documents that a portion of the aligned response operates through employees with no prior DIME donation history.

A.10 Meta-Analysis Across Stacks

Because the empirical design stacks multiple firm-event windows, Table A.8 and Figure A.4 summarize the distribution of stack-specific treatment effects and aggregate them using standard meta-analytic estimators. For each outcome, I first estimate the stack-level $\hat{\beta}_s$ from Equation (1) with within-stack fixed effects and SEs clustered by `employee`, and then combine these estimates across stacks using both a fixed-effects (inverse-variance weighted) meta estimator and a random-effects estimator (REML). The fixed-effects estimator provides the precision-weighted average under the assumption that all stacks share a common true effect; the random-effects estimator allows true effects to vary across stacks.

Table A.8: Meta-analysis summary across stacks (FE inverse-variance and RE REML)

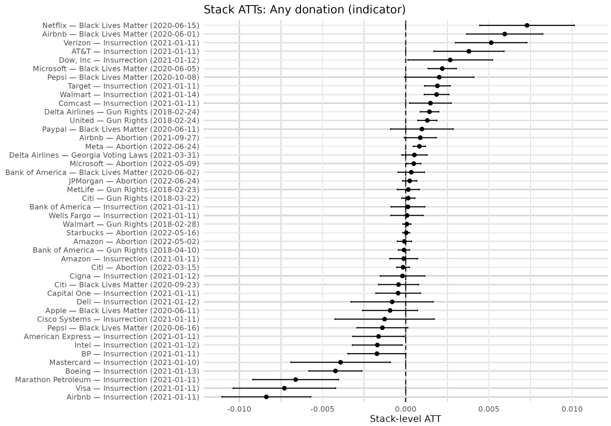
Outcome	k	FE (SE)	FE p	RE (SE)	RE p	Q	Q p	I^2
Total donation amount	44	0.0694*** (0.0125)	< 0.001	0.0995** (0.0312)	0.001	127.96	< 0.001	66.4
Donation amount to Dem orgs	44	0.0178* (0.0075)	0.018	0.0346 (0.0266)	0.194	144.40	< 0.001	70.2
PAC donation amount	44	0.0015† (0.0008)	0.057	0.0126* (0.0062)	0.041	213.20	< 0.001	79.8
Donation amount to Rep orgs	44	0.0083 (0.0050)	0.101	0.0167* (0.0071)	0.020	63.70	0.022	32.5
Any donation (indicator)	44	0.025*** (0.005)	< 0.001	0.007 (0.040)	0.857	363.30	< 0.001	88.2
Any PAC donation (indicator)	44	0.001† (0.001)	0.082	0.007 (0.023)	0.770	349.32	< 0.001	87.7

Note: Fixed-effects (FE) meta estimates are inverse-variance weighted. Random-effects (RE) estimates use REML. Indicator outcomes are reported in percentage points (pp). Q tests the null of effect homogeneity across stacks; I^2 is the share of total variation attributable to between-stack heterogeneity. Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.10.

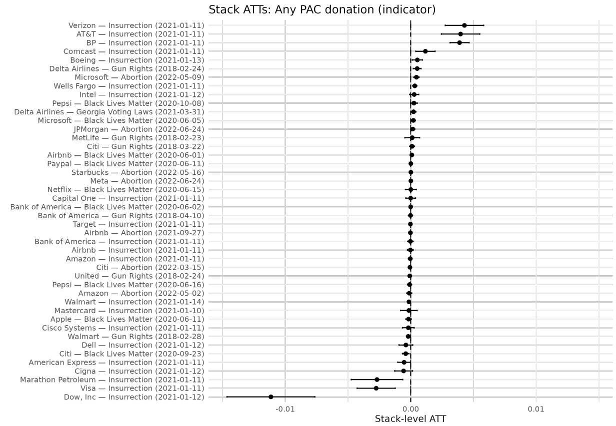
Two patterns stand out. First, the meta-analytic summaries corroborate the main pooled findings on the headline intensive-margin outcomes. For total donation amount, the fixed-effects pooled estimate is positive and precisely estimated (FE = 0.069, $p < 0.001$), and the random-effects estimate moves in the same direction with similar magnitude (RE = 0.100, $p = 0.001$). The Democratic-amount and PAC-amount cells are positive under both pooling schemes, with the Dem-amount RE estimate losing significance ($p = 0.194$) due to substantial cross-stack heterogeneity.

Second, on the indicator outcomes the FE and RE estimators diverge meaningfully. For any-donation participation, the FE estimate is 0.025 ($p < 0.001$) while the RE estimate is 0.007 ($p = 0.857$), with $I^2 = 88\%$. The FE inverse-variance weighting is dominated by a small number of low-SE (typically large) stacks where the indicator-margin effect is detectable; the typical stack-level effect, by contrast, sits closer to zero. The same pattern holds for the PAC indicator. This divergence is not a contradiction with the main pooled DiD - the within-stack effects on the indicator outcomes are themselves heterogeneous, which the leave-one-out diagnostics in Appendix A.11 confirm - but it indicates that on the participation margin, the headline result reflects a smaller number of high-precision firm-events rather than a uniform response across the sample.

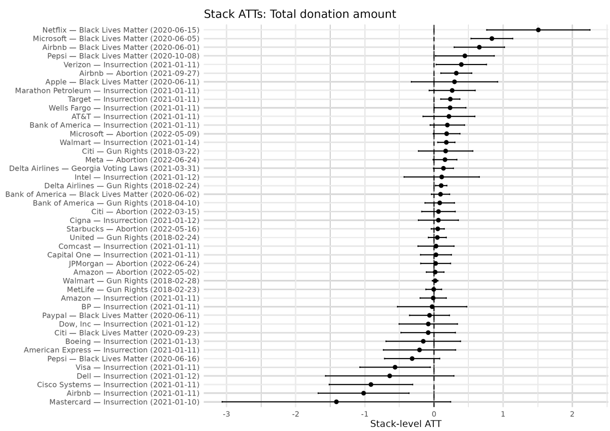
The heterogeneity statistics reinforce this reading. Cochran's Q rejects homogeneity for total donation amount and for the indicator outcomes ($p < 0.001$ in all cases), with I^2 values implying that a sizable share of variation in stack-level estimates reflects between-stack heterogeneity rather than sampling error. For Republican-directed amounts, by contrast, Q is only marginally significant ($p = 0.022$) and I^2 is comparatively low (32.5%), suggesting less cross-stack dispersion in that outcome. The caterpillar plots in Figure A.4 visualize the full cross-stack distribution for each outcome, with FE and RE pooled lines overlaid.



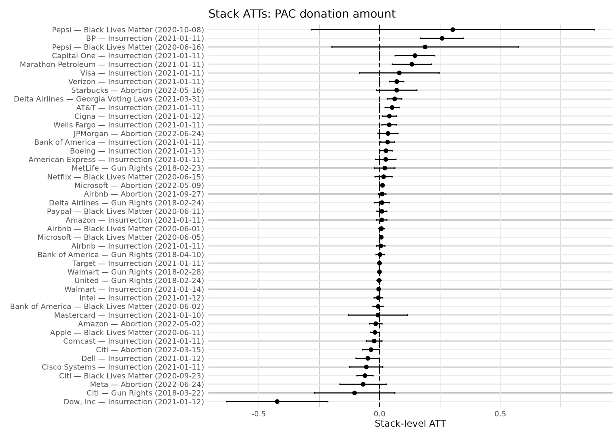
Any donation (indicator)



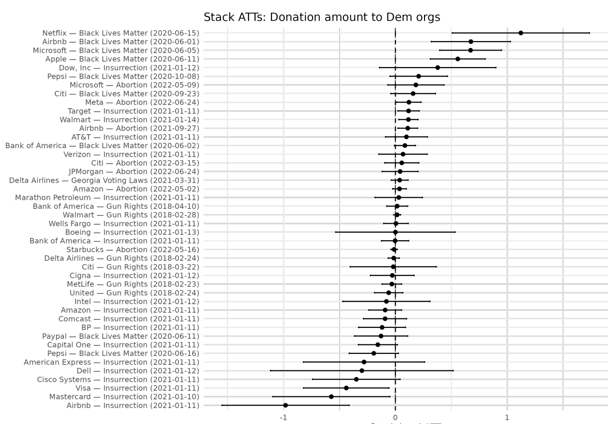
Any PAC donation (indicator)



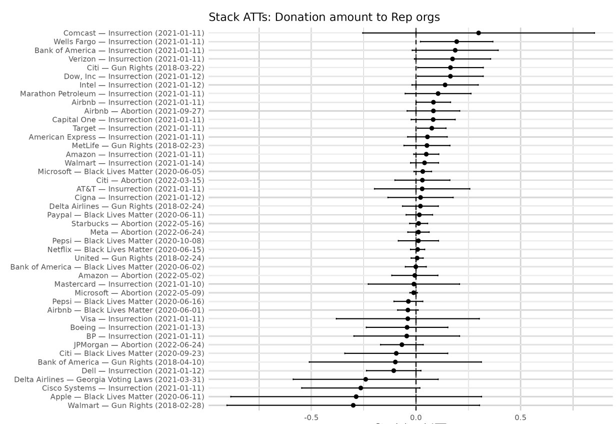
Total amount



PAC amount



Amount to Dem orgs



Amount to Rep orgs

Figure A.4: Per-stack treatment effects with FE and RE pooled lines. Each panel shows the distribution of stack-level $\hat{\beta}_s$ for the indicated outcome, ordered by point estimate, with 95% confidence intervals. Vertical lines mark the fixed-effects (inverse-variance) and random-effects (REML) pooled estimates from Table A.8.

A.11 Leave-One-Out Influence Analysis

A related concern in stacked designs is that pooled estimates may be disproportionately influenced by a small number of stacks. I conduct a leave-one-out (LOO) influence analysis on six headline outcomes by re-estimating the pooled coefficient from Equation (1) 44 times per outcome, each time omitting one stack. Let $\hat{\beta}$ denote the pooled estimate using all stacks and $\hat{\beta}^{(-s)}$ the estimate dropping stack s. I summarize influence by $|\Delta_s| = |\hat{\beta}^{(-s)} - \hat{\beta}|$, the absolute change in the pooled coefficient when stack s is excluded.

Table A.9: Leave-one-out influence analysis (top 3 stacks by absolute change)

Company	Event	Date	Full est. (SE)	LOO est. (SE)	$\lvert\Delta\rvert$
Total donation amount					
Walmart	Insurrection	2021-01-14	0.1301 (0.0392)	0.1073 (0.0362)	0.0228
Microsoft	Black Lives Matter	2020-06-05	0.1301 (0.0392)	0.1143 (0.0362)	0.0158
Amazon	Abortion	2022-05-02	0.1301 (0.0392)	0.1427 (0.0404)	0.0126
Amount to Dem orgs					
Microsoft	Black Lives Matter	2020-06-05	0.0572 (0.0283)	0.0431 (0.0249)	0.0141
Walmart	Insurrection	2021-01-14	0.0572 (0.0283)	0.0435 (0.0274)	0.0137
Apple	Black Lives Matter	2020-06-11	0.0572 (0.0283)	0.0455 (0.0284)	0.0117
PAC donation amount					
BP	Insurrection	2021-01-11	2.29×10^{-2} (1.03×10^{-2})	1.66×10^{-2} (8.41×10^{-3})	6.28×10^{-3}
Marathon Petroleum	Insurrection	2021-01-11	2.29×10^{-2} (1.03×10^{-2})	1.73×10^{-2} (8.93×10^{-3})	5.59×10^{-3}
Meta	Abortion	2022-06-24	2.29×10^{-2} (1.03×10^{-2})	2.57×10^{-2} (1.03×10^{-2})	2.83×10^{-3}
Amount to Rep orgs					
Walmart	Gun Rights	2018-02-28	-0.0109 (0.0319)	0.0245 (0.0169)	0.0354
Bank of America	Gun Rights	2018-04-10	-0.0109 (0.0319)	0.0006 (0.0291)	0.0115
Comcast	Insurrection	2021-01-11	-0.0109 (0.0319)	-0.0172 (0.0316)	0.0062
Any donation (indicator)					
Walmart	Insurrection	2021-01-14	0.0005 (0.0002)	0.0003 (0.0002)	0.0002
Target	Insurrection	2021-01-11	0.0005 (0.0002)	0.0004 (0.0003)	0.0001
Boeing	Insurrection	2021-01-13	0.0005 (0.0002)	0.0006 (0.0002)	0.0001
Any PAC donation (indicator)					
BP	Insurrection	2021-01-11	2.69×10^{-4} (1.22×10^{-4})	1.93×10^{-4} (9.58×10^{-5})	7.58×10^{-5}
Marathon Petroleum	Insurrection	2021-01-11	2.69×10^{-4} (1.22×10^{-4})	2.17×10^{-4} (1.11×10^{-4})	5.20×10^{-5}
Verizon	Insurrection	2021-01-11	2.69×10^{-4} (1.22×10^{-4})	2.35×10^{-4} (1.17×10^{-4})	3.41×10^{-5}

Notes: Each panel reports the three stacks whose omission produces the largest absolute change in the pooled estimate for the indicated outcome. "Full est. (SE)" is the pooled estimate using all stacks; "LOO est. (SE)" drops the listed stack. $|\Delta| = |\widehat{\beta}^{(-s)} - \widehat{\beta}|$.

Table A.9 reports the three most-influential drops per outcome. Insurrection stacks dominate the top-five influence rankings for every outcome. This observation motivated the direct event-type subsample test in Appendix A.12.

One sign flip is worth noting. The Republican-amount cell flips from -0.011 (NS) to $+0.025$ (NS) when Walmart $\times$ Gun Rights 2018-02-28 is dropped. Both pooled and LOO point estimates are statistically indistinguishable from zero, so no statistical claim depends on the sign; the cell's behavior is consistent with the offsetting subsample pattern documented in Appendix A.12, where the pooled near-zero conceals oppositely-signed Insurrection and non-Insurrection coefficients.

Overall, the LOO diagnostics indicate that the headline conclusions are not driven by any single firm-event window. While a small number of stacks measurably shift point estimates, no single omission reverses the sign of any cell that is statistically significant in the pooled specification.

A.12 Event-Type Split: Insurrection vs. Non-Insurrection Stacks

The N=44 analytical sample comprises 21 Insurrection-period stacks (firms responding to the events of January 6, 2021) and 23 non-Insurrection stacks (9 Black Lives Matter, 7 Abortion, 6 Gun Rights, 1 Georgia Voting Laws). The leave-one-out diagnostics in Appendix A.11 surface that several of the most influential single-stack drops are Insurrection-period firms, motivating a direct subsample test of whether the headline results generalize beyond the Insurrection subsample. I re-estimate the main difference-in-differences specification on each subsample separately, dropping the other subsample's stacks from the panel. All modeling choices match the canonical specification: two-way fixed effects (employee-stack × calendar-week), standard errors clustered by `stack`, w99 winsorization on intensive outcomes.

Table A.10: DiD coefficient by event-type subsample (N=44)

Outcome	Pooled $\widehat{\beta}$ (SE)	Insurrection only $\widehat{\beta}$ (SE)	Non-Insurrection $\widehat{\beta}$ (SE)	Baseline mean (pre, control)
Panel A: Extensive margin (any/indicator)				
Any donation	5.35×10^{-4}* (2.49×10^{-4})	6.98×10^{-4} (5.74×10^{-4})	4.17×10^{-4}* (1.51×10^{-4})	0.005991
Any PAC donation	2.69×10^{-4}* (1.22×10^{-4})	6.02×10^{-4}* (2.82×10^{-4})	4.86×10^{-5} (4.47×10^{-5})	9.22×10^{-4}
Any donation to Dem orgs	1.68×10^{-4} (1.66×10^{-4})	-1.11×10^{-5} (3.94×10^{-4})	2.90×10^{-4}* (1.08×10^{-4})	0.003041
Any donation to Rep orgs	1.51×10^{-4}* (6.00×10^{-5})	3.69×10^{-4}** (1.17×10^{-4})	4.34×10^{-6} (1.96×10^{-5})	0.001400
Panel B: Intensive margin (amounts)				
Total donation amount	0.1301** (0.0392)	0.1654* (0.0671)	0.1039* (0.0458)	0.603811
PAC amount	0.0229* (0.0103)	0.0580* (0.0210)	-1.24×10^{-3} (7.17×10^{-3})	0.091053
Amount to Dem orgs	0.0572* (0.0283)	0.0130 (0.0466)	0.0859* (0.0376)	0.262354
Amount to Rep orgs	-0.0109 (0.0319)	0.0918*** (0.0188)	-0.0785† (0.0424)	0.156061
Stacks (G)	44	21	23	
Employee-events	1,120,622	504,599	616,023	
Observations	28,015,550	12,614,975	15,400,575	

Notes: Each column reports the pooled DiD coefficient $\widehat{\beta}$ on Treated × Post for the indicated event-type subsample. *Insurrection only* restricts the stacked panel to the 21 Insurrection-period stacks; *Non-Insurrection* restricts to the 23 BLM / Abortion / Gun Rights / Georgia Voting Laws stacks. All three columns share the canonical specification: two-way fixed effects (employee-stack × calendar-week), standard errors clustered by stack. The pooled column reproduces the corresponding cell in Table 3. Baseline mean is computed on the subsample's own pre-period control cells. Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.10.

Table A.10 reports the pooled, Insurrection-only, and non-Insurrection coefficients side-by-side for all eight outcomes. On the two headline outcomes - total donation amount and any-donation participation - the non-Insurrection subsample is positive and statistically significant on its own (total amount $\hat{\beta} = 0.104$, $p < 0.05$; any donation $\hat{\beta} = 4.17 \times 10^{-4}$, $p < 0.05$). The Insurrection-subsample total-amount coefficient is about 1.6× larger in magnitude but the Insurrection any-donation coefficient is not statistically significant due to a wider standard error. Both subsamples produce same-signed coefficients on both headline outcomes; the result generalizes beyond Insurrection.

The channel composition differs sharply by event type. PAC-channel outcomes are statistically significant only on the Insurrection subsample (PAC amount $\hat{\beta} = 0.058$ Insurrection, $p < 0.05$ vs. -1.24×10^{-3} non-Insurrection, NS; PAC participation 6.02×10^{-4} Insurrection, $p < 0.05$ vs. 4.86×10^{-5} non-Insurrection, NS). Democratic-amount mobilization reverses the pattern: pooled

$\hat{\beta} = 0.057$ ($p < 0.05$) is composed of a near-zero Insurrection coefficient (0.013, NS) and a larger non-Insurrection coefficient (0.086, $p < 0.05$). The Republican-amount cell, the noisiest pooled cell in Table 3 (-0.011, NS), conceals an Insurrection coefficient of $+0.092$ ($p < 0.001$) and a non-Insurrection coefficient of -0.079 ($p < 0.10$). The pooled near-zero is the average of two coherent, oppositely-signed subsample effects; Section 6 develops the substantive reading.

A.13 Activation by Prior Donor Status

The pooled estimates in Section 4.3 do not on their own indicate whether corporate stances mobilize employees who are already politically active or bring new participants into political giving. I re-estimate the canonical Table 3 specification on subsamples defined by employees' prior donation histories. The prior-donor flag is constructed at the user-stack level from a strict date-anchored rule: an employee is classified as a prior donor for a given stack if their first DIME-recorded transaction date precedes the stack's stance date. The flag is built against the full DIME panel rather than the in-window observations, so it cannot leak post-stance information into the classification. Employees who never appear in DIME are classified as non-donors. Each subsample is estimated with the main specification: two-way fixed effects (employee-stack × calendar-week), standard errors clustered by `stack`, w99 winsorization on intensive outcomes. For the extensive-margin cells I additionally report the implied number of post-stance activations in the 13-week post window, computed as the coefficient times the count of treated user-stack-weeks in the post-period; for non-donors this is interpretable as additional first-time donor-weeks.

Table A.11: Activation / gating by baseline donor status (N=44 canonical)

	Non-donor subsample				Prior-donor subsample			
Outcome	$\widehat{\beta}$ (SE)	Baseline	%Δ	Implied	$\widehat{\beta}$ (SE)	Baseline	%Δ	Implied
Panel A: Extensive margin (any/indicator)								
Any donation	0.0002 (0.0002)	0.0031	6.5%	1,824	0.0044* (0.0018)	0.0861	5.1%	1,349
Any PAC donation	0.0002† (0.0001)	0.0005	44.0%	2,006	0.0010 (0.0015)	0.0103	9.2%	295
Any donation to Dem orgs	7.71×10^{-5} (0.0001)	0.0016	4.9%	688	-0.0006 (0.0014)	0.0485	-1.3%	-191
Any donation to Rep orgs	3.52×10^{-5} (3.60×10^{-5})	0.0009	4.1%	314	0.0028* (0.0012)	0.0157	17.9%	865
Panel B: Intensive margin (winsorized amounts)								
Total donation amount	0.0494 (0.0341)	0.2928	16.9%	440,988	1.5771** (0.4467)	11.1648	14.1%	487,291
PAC amount	0.0172† (0.0097)	0.0388	44.3%	153,515	0.1544 (0.1377)	1.5648	9.9%	47,700
Amount to Dem orgs	0.0169 (0.0169)	0.1302	13.0%	150,664	0.6716 (0.4082)	5.6930	11.8%	207,492
Amount to Rep orgs	0.0073 (0.0080)	0.0669	10.9%	65,220	-0.7827 (1.1685)	2.8417	-27.5%	-241,823
Treated user-stacks	686,713				23,767			
Treated user-stack-weeks (post)	8,927,269				308,971			

Notes: N=44 analytical sample. Specification is the main weekly DiD estimated separately on each subsample. SEs clustered by `stack` (G=44). Baseline is the within-subsample pre-period control mean, averaged across stacks (same recipe as Table 3 baseline). Implied = $\widehat{\beta} \times N_{\text{treated employee-stack-weeks, post}}$. For extensive outcomes Implied is interpretable as an implied count of additional donor-weeks; for intensive outcomes it is implied total additional dollars (winsorized at the per-stack 99th percentile). Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.10.

Table A.11 reports the headline finding. Among the 686,713 treated user-stacks of employees with no prior donation history, exposure to a corporate stance produces an implied 2,006 first-time PAC donor-weeks in the 13-week post window, against an implied 295 activations among the 23,767 treated user-stacks of employees with prior donation histories. The non-donor PAC coefficient is marginally significant at the ten percent level ($\hat{\beta} = 2.25 \times 10^{-4}$, SE 1.12×10^{-4}, $p = 0.051$); the intensive-margin counterpart points in the same direction ($\hat{\beta} = 0.017$, SE 0.010, $p = 0.083$; implied \$153,515 in additional contributions). Section 6 develops the substantive interpretation.

A.14 L2-Filtered Robustness: Formally-Coverage States Only

The main difference-in-differences specification uses L2's inclusive partisan-registration measure, which combines formally-recorded registration in 30 states plus the District of Columbia with L2's imputed registration in the remaining 20 states. The imputation is constructed from primary-voting participation and other behavioral indicators and is the standard procedure used by L2 to extend party-registration coverage nationwide. A natural robustness check is to restrict the sample to the 30 coverage states plus DC, where partisan registration is recorded formally and imputation is not required.

Table A.12: Full Sample vs. L2-filtered specification: headline difference-in-differences estimates

	Full sample $\widehat{\beta}$ (SE)	L2-filtered $\widehat{\beta}$ (SE)	Baseline (pre, control) (canonical)
Panel A: Extensive margin (any/indicator)			
Any donation	5.35×10^{-4}* (2.49×10^{-4})	6.38×10^{-4}* (2.80×10^{-4})	5.99×10^{-3}
Any PAC donation	2.69×10^{-4}* (1.22×10^{-4})	2.52×10^{-4}** (8.97×10^{-5})	9.22×10^{-4}
Any donation to Dem orgs	1.68×10^{-4} (1.66×10^{-4})	1.98×10^{-4} (1.66×10^{-4})	3.04×10^{-3}
Any donation to Rep orgs	1.51×10^{-4}* (6.00×10^{-5})	1.56×10^{-4}* (6.68×10^{-5})	1.40×10^{-3}
Panel B: Intensive margin (winsorized amounts)			
Total donation amount	0.1301** (0.0392)	0.1151** (0.0406)	0.6038
PAC amount	0.0229* (0.0103)	0.0149* (0.0070)	0.0911
Amount to Dem orgs	0.0572* (0.0283)	0.0627* (0.0278)	0.2624
Amount to Rep orgs	−0.0109 (0.0319)	+0.0096 (0.0243)	0.1561
Employee-stack FE	Yes	Yes	
Calendar-week FE	Yes	Yes	
Clusters (G)	44	44	
Observations	28,015,550	15,223,725	

Notes: Each pair of columns reports the headline difference-in-differences estimate under two specifications. *Canonical* is the N=44 specification with L2's inclusive partisan-registration measure, combining formally-recorded registration in 30 coverage states + DC with L2's imputed registration in 20 non-coverage states (Eq. 1). *L2-filtered* restricts the panel to employees in the 30 coverage states + DC where registration is formally recorded. Both columns use the same two-way fixed-effects specification (employee-stack × calendar-week) with standard errors clustered by `stack_id`. L2-filtered panel retains 54.3% of full-panel rows. Signif. codes: ***: 0.001, **: 0.01, *: 0.05, †: 0.10.

Table A.12 reports the headline Table 3 estimates on the L2-filtered sample. Restricting to coverage states retains 54.3% of the full panel (15.2M of 28.0M rows). All eight cells preserve their sign relative to the canonical specification, and all cells significant in Table 3 retain their significance tier under L2 restriction. The PAC-participation extensive-margin cell strengthens modestly under L2 restriction, moving from $p < 0.05$ to $p < 0.01$ as the standard error tightens with the more precisely-measured registration variable. The Republican-amount cell flips sign (−0.011 canonical to +0.010 L2-filtered) but is statistically indistinguishable from zero in both specifications.

Table A.13 reports the Figure 2 partisan-decomposition estimates under L2 restriction. The qualitative pattern documented in the main text is preserved: Democratic-registered employees drive the Democratic-channel response, and aligned employees of both parties contribute to the PAC channel. Coefficient magnitudes shift modestly - the intensive-margin Democratic-channel

Table A.13: Canonical vs. L2-filtered specification: partisan-decomposition estimates (intensive margin)

	Canonical		L2-filtered	
Outcome	Dem	Rep	Dem	Rep
Total donation amount	0.1503**	0.1310*	0.1440*	0.1036
	(0.0493)	(0.0575)	(0.0619)	(0.0670)
PAC amount	0.0119	0.0478*	0.0018	0.0358**
	(0.0075)	(0.0209)	(0.0097)	(0.0109)
Amount to Dem orgs	0.1059*	0.0082	0.1032†	0.0171
	(0.0454)	(0.0090)	(0.0559)	(0.0119)
Amount to Rep orgs	0.0109†	−0.0806	0.0151†	−0.0008
	(0.0065)	(0.1064)	(0.0089)	(0.0904)
Employee-stack FE	Yes	Yes	Yes	Yes
Calendar-week FE	Yes	Yes	Yes	Yes
Clusters (G)	44	44	44	44

Notes: Each cell reports the intensive-margin DiD coefficient under the indicated specification and partisan-registration subsample. *Canonical* uses L2's inclusive registration measure; *L2-filtered* restricts to the 30 coverage states + DC. *Dem* and *Rep* columns report DiD coefficients estimated on the Democratic-registered and Republican-registered subsamples respectively. All cells use two-way fixed effects (employee-stack × calendar-week) and standard errors clustered by `stack_id`. The pooled (cross-party) estimates are reported in Table A.12. Signif. codes match Table A.12.

and PAC-channel cells both attenuate by roughly 12-35% under L2 restriction - but no headline qualitative claim is sensitive to the choice of inclusive vs. formal partisan-registration measure.

B Additional Tables and Figures for Survey Experiment

B.1 Sample Characteristics

Table B.1 reports summary statistics for the full survey sample (N = 1,300) and the analytical sample (N = 842). The analytical sample restricts respondents to those age 25+, registered to vote, and employed or actively seeking work, aligning the experiment more closely with the population observed in the administrative employment and donation data. As expected, the analytical sample is older than the full sample by construction, while other demographic and political characteristics are broadly similar.

Table B.1: Summary Statistics for the Survey Experiment

	Full Sample	Analytical Sample
Male	0.49	0.52
Age	51.69	47.92
White	0.80	0.78
Hispanic	0.10	0.11
Black	0.11	0.12
Asian	0.02	0.02
High School	0.37	0.33
College	0.40	0.44
Graduate School	0.21	0.22
Democrat	0.50	0.50
Republican	0.39	0.38
Political Awareness	0.69	0.68
Political Donor	0.32	0.31
PAC Donor	0.07	0.08
N	1300	842

To contextualize baseline issue attitudes, Table B.2 benchmarks the experimental sample against contemporaneous Pew estimates on the same broad issues. Across abortion, DEI, and transgender equality, the experimental sample is more supportive of liberal positions than the Pew benchmark, though the gaps are modest for abortion and transgender equality and larger for DEI.

Figure B.1 visualizes the distribution of baseline issue attitudes in the analytical sample, splitting respondents into pro, neutral, and anti categories for each issue.

Table B.2: Respondents are liberal, but roughly as liberal as the nationally representative respondents in Pew.

	Experimental Sample	Pew Sample
Abortion		
Pro	73%	63%
Anti	20%	36%
DEI		
Pro	68%	52%
Anti	24%	21%
Transgender Equality		
Pro	61%	56%
Anti	28%	26%

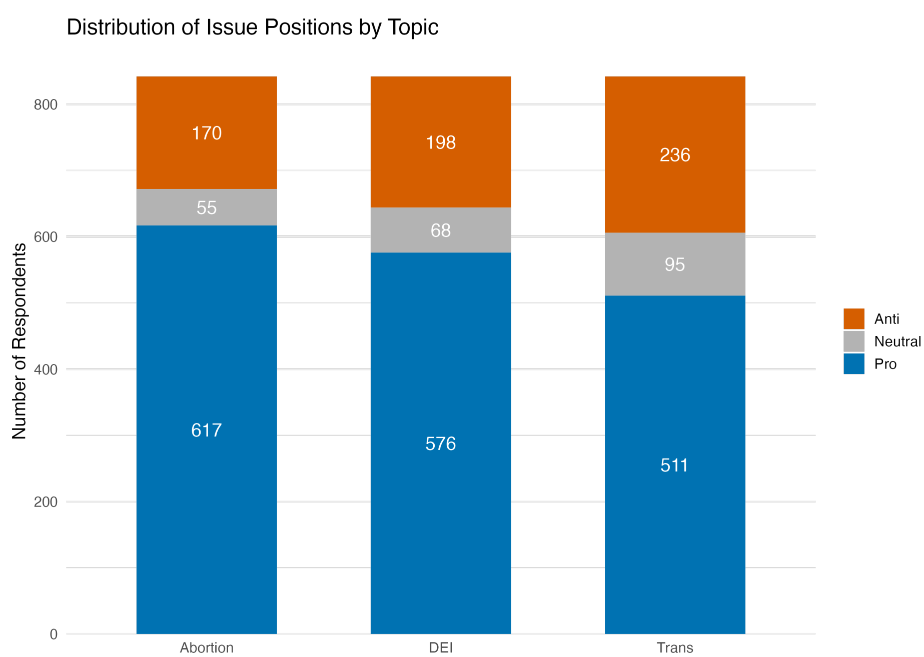


Figure B.1: Distribution of baseline issue attitudes by policy domain

B.2 Sample Weighting

I use sample weights provided by Verasight, the survey provider. Sample weights are used to ensure that the survey data accurately represents the broader population. Verasight uses raking to perform iterative proportion fitting. This adjusts survey weights such that the sample aligns with known population margins (provided by ANES sample statistics). All weights are capped at a maximum value of 5.0 to reduce variance in weights. Verasight weights on the following demographics:

- Gender
- Age
- Education
- Race / Ethnicity
- Region
- Metropolitan Status
- Income
- Party ID
- 2024 Presidential Vote Choice

Weighted sample averages can be found in Table B.3.

Table B.3: Weighted Summary Statistics for the Survey Experiment

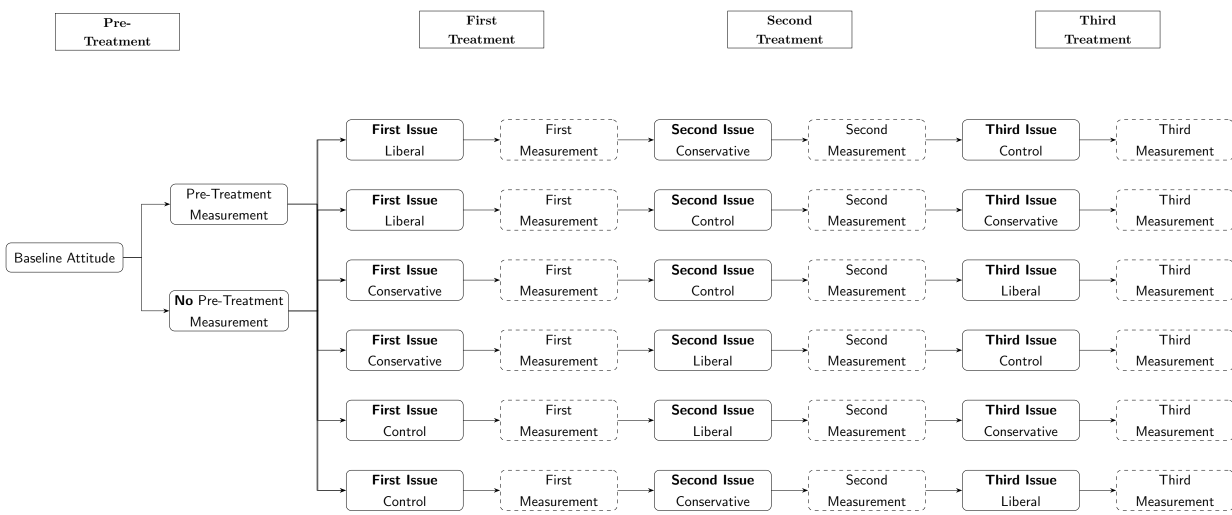
	Full Sample	Analytical Sample	Main Sample
Male	0.49	0.52	0.49
Age	50.05	46.8	47.2
White	0.77	0.75	0.75
Hispanic	0.11	0.13	0.14
Black	0.12	0.13	0.13
Asian	0.03	0.03	0.03
High School	0.47	0.42	0.40
College	0.35	0.38	0.41
Graduate School	0.16	0.18	0.17
Democrat	0.44	0.44	0.47
Republican	0.44	0.43	0.43
N	1300	842	425

B.3 Randomization and experimental protocol

This appendix details the experimental randomization and provides a schematic of the protocol. Respondents are exposed to three issue modules (abortion, DEI hiring, transgender equality). Within each module, they read a vignette describing a hypothetical employer's public response. For each issue, respondents are randomly assigned to one of three stance conditions: a liberal stance (supportive), a conservative stance (oppositional), or a neutral control in which the employer declines to take a stance. Issue order is randomized across respondents.

The primary analysis uses the repeated-measures arm, in which respondents provide pre-treatment measures of the key behavioral outcomes prior to vignette exposure and then report the same outcomes post-treatment. A second arm omits pre-treatment measures and is used to implement a modified Solomon design that diagnoses instrument reactivity (Malhotra, Monin, & Tomz, 2019). Figure B.2 summarizes the resulting protocol and treatment sequencing.

Figure B.2: Respondents are randomized to see one liberal, one conservative, and one control vignette across three issues (abortion, transgender rights, and DEI hiring), with an additional randomization determining whether pre-treatment outcomes are measured.



B.4 Outcome Means

Table B.4 reports raw (unadjusted) pre–post mean allocations, pooled across issues, by treatment arm (Conservative, Liberal, Control) and respondents' baseline orientation (Conservative, Liberal, Neutral). These descriptive means are not causal estimates—they do not condition on issue fixed effects or account for sampling variation—but they provide a transparent summary of the magnitudes and the direction of changes that underlie the regression results in the main text.

Several patterns stand out. First, baseline giving is sharply polarized by respondent type: liberal respondents allocate substantially more to liberal organizations than conservative or neutral respondents across all arms in the pre-treatment period, while conservative respondents allocate substantially more to conservative organizations than liberal or neutral respondents. This stark separation is useful for interpretation because it confirms that the "respondent type" grouping based on pre-treatment feelings corresponds to meaningful differences in baseline giving.

Second, the pre–post changes in partisan giving generally move in a direction consistent with directional responsiveness. For example, PAC giving increases for conservatives in the Conservative treatment (from 4.21 to 6.69), while PAC giving increases for liberals in the Liberal treatment (from 5.08 to 12.28). Meanwhile, changes in the control arm tend to be smaller and less systematically aligned with the stance direction, which is consistent with the control condition capturing background variation and generic pre–post dynamics.

Third, the most pronounced raw movements occur for PAC giving rather than for partisan giving to Democratic or Republican organizations. This is visible in the large increase in liberal respondents' PAC allocations in the Liberal treatment and in the increase for conservative respondents' PAC allocations in the Conservative treatment. In contrast, changes in giving to Democratic and Republican organizations are comparatively modest and sometimes move in the same direction across arms, suggesting that much of the treatment-relevant action in this design may operate through the PAC channel rather than through reallocations between partisan organizations.

Finally, "Kept Money" provides a useful complement: when donation amounts rise in a given cell, retained funds tend to fall (and vice versa), consistent with the budget constraint built into the allocation task. Notably, neutral respondents retain the most in every arm and period, which matches the interpretation that neutral respondents are systematically less willing to allocate funds away from themselves in this task.

Table B.4: Donation Behavior by Treatment and Respondent Alignment

	Conservative Treatment		**Liberal Treatment**		**Control Treatment**	
	Pre-Treatment	Post-Treatment	Pre-Treatment	Post-Treatment	Pre-Treatment	Post-Treatment
Liberal Donations						
Conservative Respondent	4.04	4.66	6.70	7.05	3.99	4.43
Liberal Respondent	45.82	48.43	48.09	43.70	45.26	45.92
Neutral Respondent	11.54	12.91	13.62	10.12	16.76	13.97
Conservative Donations						
Conservative Respondent	41.06	40.09	41.34	40.15	38.56	36.67
Liberal Respondent	12.74	10.92	11.64	10.29	12.67	11.09
Neutral Respondent	13.00	12.49	7.67	6.31	5.29	7.65
PAC Donations						
Conservative Respondent	4.21	6.69	4.52	3.17	7.46	5.43
Liberal Respondent	5.27	5.13	5.08	12.28	6.79	5.33
Neutral Respondent	10.29	6.91	10.24	5.36	3.97	2.35
Kept Money						
Conservative Respondent	50.68	48.55	47.44	49.63	49.99	53.47
Liberal Respondent	36.18	35.52	35.20	33.72	35.29	37.66
Neutral Respondent	65.17	67.69	68.48	78.21	73.97	76.03

Note: Each cell displays the mean outcome before and after treatment. Values represent donation or retention amounts (not percentages), pooled across issues.

B.5 Heterogeneity by Issue

This appendix section examines whether the core treatment effects documented in the main text vary across the three issue domains included in the experiment: abortion, DEI, and trans rights. I estimate a pooled specification that allows the key post-treatment effects to differ by issue via interactions with issue indicators. I then recover and plot the implied issue-specific effects with 95% confidence intervals.

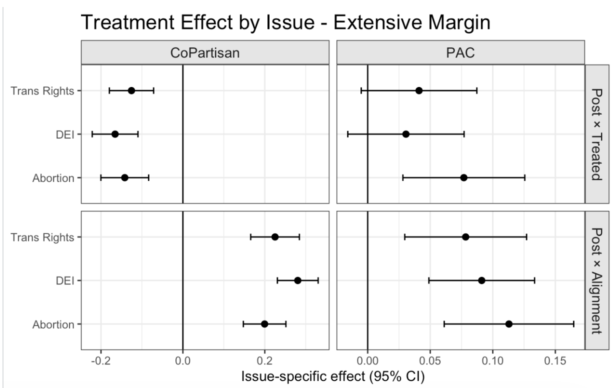
Concretely, I stack the pre- and post-treatment outcome measures and estimate, separately for each outcome family and margin (intensive and extensive), a respondent fixed-effects model of the form:

$$\begin{aligned} Y_{ijt} &= \alpha_i + \gamma_j + \beta_1 \, \mathrm{Post}_t + \beta_{2j} \, (\mathrm{Post}_t \times \mathrm{Treated}_{ij}) \\ &\quad + \beta_{3j} \, (\mathrm{Post}_t \times \mathrm{Alignment}_{ij}) + \varepsilon_{ijt}. \end{aligned} \tag{8}$$

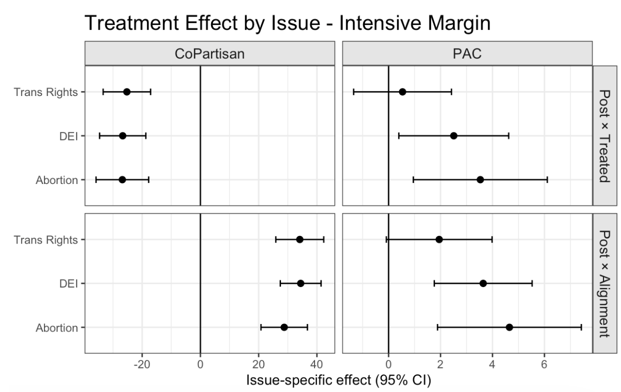
where i indexes respondents, j indexes issues, and $t \in \{0, 1\}$ indexes the pre/post measurement. α_i are respondent fixed effects, γ_j are issue fixed effects, and standard errors are clustered by respondent. The coefficients β_{2j} and β_{3j} are allowed to vary by issue j via interactions with issue indicators in a single pooled regression. The analysis uses survey weights and follows the main text by restricting to non-neutral respondents (Feeling $\neq 0$) when estimating specifications that include the alignment term. Outcomes are (i) co-partisan donations (excluding PAC; defined as donations to the respondent's co-partisan non-PAC organizations) and (ii) PAC donations, each measured on intensive (amount) and extensive (any/indicator) margins. Figures B.3b and B.3a plot the recovered issue-specific effects for Post × Treated and Post × Alignment.

Across all three issues, the issue-specific Post × Alignment coefficients are positive for both co-partisan donations and PAC giving on both the intensive and extensive margins. This mirrors the main findings: employees who are more aligned with the employer's stance exhibit larger post-treatment increases in politically congruent behavior. Importantly, the alignment gradients are not driven by a single issue domain; instead, they appear consistently across abortion, DEI, and trans rights, with broadly overlapping confidence intervals.

The issue-specific Post × Treated coefficients are more mixed across outcomes, which is also consistent with the paper's core interpretation that the average (net) treated effect can mask heterogeneity by alignment. In particular, for co-partisan donations, the Post × Treated effect is negative across issues, while the Post × Alignment effect is strongly positive. This combination implies that treatment increases co-partisan giving for sufficiently aligned respondents, but can reduce it for sufficiently misaligned respondents. By contrast, for PAC giving, both Post × Treated and Post × Alignment are generally positive across issues, indicating that the stance treatment shifts PAC giving upward on average while also increasing the gradient with alignment. Overall, the within-issue evidence supports the main results: the alignment mechanism operates similarly across abortion, DEI, and trans rights.



(a) Extensive margin



(b) Intensive margin

Figure B.3: Heterogeneity by issue

B.6 Vignette Order Effects

To assess whether vignette position (first/second/third) affects responses, we estimate an "order-robust" specification that allows for (i) main effects of order, (ii) order-by-post interactions (capturing differential pre–post shifts by vignette position), and (iii) interactions between order and the key *post* $\times$ *treated* and *post* $\times$ *alignment* terms. We focus on the non-neutral sample (Feeling $\neq 0$), consistent with the alignment-based specifications in the main text. Table B.5 reports the full regression results for Any and PAC outcomes on intensive and extensive margins, and Table B.6 reports joint-test p-values for the order-related coefficient blocks.

Order effects are statistically negligible across the large majority of outcomes: of the sixteen joint tests reported in Table B.6, fifteen fail to reject the null of no order interaction. The one exception is the extensive margin of PAC giving, where the joint test of order-by-treatment interactions reaches conventional significance ($p = 0.039$). Inspection of Table B.5 indicates that this effect is driven by a larger treatment effect when the relevant issue appears third in the vignette sequence ($\hat{\beta} = 0.118$, $p < 0.05$ for Post $\times$ Treated $\times$ Order: 3rd) than when it appears first or second. We interpret this cautiously: the qualitative pattern of alignment-based mobilization holds across all three vignette positions, and the alignment-by-order joint tests fail to reject across all outcomes, indicating that the headline alignment mechanisms are not artifacts of vignette ordering. The position-three effect is consistent with cumulative priming — by the third issue, respondents have been reasoning about employer political stances for two prior modules, which may sharpen the PAC mobilization response.

Table B.5: Order robustness: intensive and extensive margins

	Any		PAC	
	(1) Ext.	**(2) Int.**	**(3) Ext.**	**(4) Int.**
Variables				
Post	-0.0154	-2.486	-0.0257	-0.4017
	(0.0215)	(1.997)	(0.0214)	(0.9776)
Order: 2nd	-0.0098	-2.123	-0.0053	-0.9896
	(0.0166)	(1.527)	(0.0139)	(0.5878)
Order: 3rd	0.0060	-0.0459	-0.0059	-0.8347
	(0.0164)	(1.555)	(0.0154)	(0.6408)
Post × Order: 2nd	-0.0347	-1.263	0.0112	1.353
	(0.0337)	(3.018)	(0.0325)	(1.533)
Post × Order: 3rd	-0.0181	1.229	-0.0651	-1.604
	(0.0331)	(2.904)	(0.0373)	(1.635)
Post × Treated	0.0115	2.737	0.0115	1.018
	(0.0273)	(2.444)	(0.0289)	(1.393)
Post × Alignment	0.0056	0.5330	0.1104***	2.716*
	(0.0205)	(1.851)	(0.0251)	(1.157)
Post × Treated × Order: 2nd	0.0693	4.691	0.0278	1.390
	(0.0446)	(3.849)	(0.0454)	(2.280)
Post × Treated × Order: 3rd	-0.0033	-3.267	0.1182*	3.624†
	(0.0397)	(3.378)	(0.0473)	(2.126)
Post × Alignment × Order: 2nd	-0.0066	0.3130	-0.0053	1.891
	(0.0289)	(2.645)	(0.0344)	(1.685)
Post × Alignment × Order: 3rd	0.0296	1.822	-0.0028	2.080
	(0.0314)	(2.851)	(0.0350)	(1.717)
Fixed-effects				
Respondent FE	Yes	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	2,328	2,328	2,328	2,328
R^2	0.771	0.799	0.713	0.538

Notes: Non-neutral sample (Feeling $\neq 0$). Standard errors clustered by respondent in parentheses. Significance codes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$.

Table B.6: Order robustness: joint tests of order-related coefficient blocks

	Any	**PAC**
Panel A: Extensive margin		
Order main effects (Order: 2nd = Order: 3rd = 0)	0.659	0.910
Post × Order (Post × Order: 2nd = Post × Order: 3rd = 0)	0.584	0.123
Post × Treated × Order (joint)	0.212	0.039*
Post × Alignment × Order (joint)	0.379	0.988
Panel B: Intensive margin		
Order main effects (Order: 2nd = Order: 3rd = 0)	0.280	0.237
Post × Order (Post × Order: 2nd = Post × Order: 3rd = 0)	0.721	0.235
Post × Treated × Order (joint)	0.156	0.230
Post × Alignment × Order (joint)	0.771	0.389

Notes: Entries are p-values from joint Wald χ^2 tests of the indicated coefficient sets. The models are estimated on the non-neutral sample (Feeling $\neq 0$), with respondent and issue fixed effects.

B.7 Modified Solomon Design: Instrument Reactivity

To assess whether the pre-treatment measurement itself alters post-treatment responses (instrument reactivity), I implement a modified Solomon-type design embedded within the repeated-measures experiment. Respondents are randomly assigned either to a *pre/post* condition (in which baseline donation allocations are elicited prior to exposure to the employer-stance vignette) or a *post-only* condition (in which outcomes are elicited only after exposure).[21]

I estimate post-treatment outcomes as a function of stance-arm indicators, stance-by-alignedness interactions (so that treatment effects may vary with alignment), and a set of pretest indicators and interactions that capture instrument reactivity. Concretely, the coefficient on *Had pretest* measures average differences between pre/post and post-only respondents in the control arm, while the interactions *Had pretest* × *Liberal stance*, *Had pretest* × *Conservative stance*, and the corresponding alignedness interactions measure whether treatment effects differ depending on whether a respondent received the baseline instrument.

Table B.7 reports results for Any and PAC outcomes on both intensive and extensive margins. Across all specifications, the pretest-related coefficients are statistically indistinguishable from zero, indicating little evidence that eliciting baseline outcomes systematically alters post-treatment responses. This pattern suggests that the primary treatment effects documented in the main text are unlikely to be driven by measurement-induced reactivity, consistent with the approach in Malhotra, Monin, and Tomz (2019).

[21]The standard Solomon design features four groups; here the design is adapted to accommodate the three stance arms (Liberal, Conservative, Control) and the survey's alignment structure.

Table B.7: Modified Solomon design: post-treatment outcomes with pretest interactions

	Any		PAC	
	(1) Intensive	(2) Extensive	(3) Intensive	(4) Extensive
Variables				
Liberal stance	-3.485	-0.0306	4.320**	0.0653**
	(2.387)	(0.0267)	(1.414)	(0.0231)
Conservative stance	0.6776	0.0004	1.121	-0.0073
	(2.379)	(0.0257)	(1.476)	(0.0229)
Had pretest	3.819	0.0497	-2.476	-0.0394
	(3.468)	(0.0370)	(1.405)	(0.0318)
Liberal stance × alignedness	12.77***	0.1313***	5.647***	0.1518***
	(2.719)	(0.0302)	(1.401)	(0.0251)
Conservative stance × alignedness	-6.659*	-0.0603*	3.277*	0.0276
	(2.840)	(0.0302)	(1.483)	(0.0260)
Had pretest × Liberal stance	3.257	0.0228	-2.302	-0.0124
	(3.185)	(0.0355)	(1.679)	(0.0311)
Had pretest × Conservative stance	-0.7697	0.0077	-0.4652	0.0194
	(3.340)	(0.0363)	(1.735)	(0.0327)
Had pretest × Liberal stance × Alignment	-6.206	-0.0645	-1.559	-0.0662
	(3.816)	(0.0416)	(1.609)	(0.0347)
Had pretest × Conservative stance × Alignment	1.281	0.0173	-2.459	-0.0005
	(3.916)	(0.0421)	(1.743)	(0.0374)
Fixed-effects				
Issue FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	2,308	2,308	2,308	2,308
R^2	0.0226	0.0208	0.0465	0.0427

Notes: Outcomes are post-treatment only. Standard errors clustered by respondent in parentheses.
Significance codes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$.

B.8 Heterogeneity by Political Awareness

In addition to estimating average treatment effects, I explore heterogeneity by political awareness. Following the pre-analysis plan, I estimate the core PAC giving specifications (H1 and H2) separately for respondents with high and low political awareness, defined by a median split of a continuous awareness index constructed from five pre-treatment knowledge items drawn from the ANES: (1) placing the Democratic and Republican parties on a liberal-conservative scale, (2) identifying on which category the U.S. government currently spends the least, (3) identifying which party currently holds the most seats in the U.S. House of Representatives, (4) identifying which party currently holds the most seats in the U.S. Senate, and (5) identifying the term length of a U.S. Senator. Each item is coded as correct or incorrect, and I take the average across all five to form a continuous index, which I then dichotomize at the median to define high and low awareness subgroups.

My pre-registered hypothesis was that lower-awareness respondents would exhibit stronger treatment effects, consistent with theories of cue-taking among less engaged citizens (Kam, 2005). The results in Table B.8 do not support this prediction. Instead, treatment effects on PAC giving are concentrated almost entirely among high-awareness respondents. On the extensive margin, the estimated *Post* $\times$ *Treated* effect for high-awareness respondents is 0.086 ($p < 0.001$), compared to a near-zero and statistically indistinguishable from zero effect of 0.015 among low-awareness respondents. The alignment gradient (*Post* $\times$ *Alignment*) similarly strengthens with awareness, rising from 0.062 among low-awareness respondents to 0.143 among high-awareness respondents.

This pattern is consistent with an alternative interpretation in which political awareness is a prerequisite for recognizing and responding to employer partisan cues: employees must first identify the stance as a politically meaningful signal before it can activate giving behavior. Low-awareness respondents may encounter the same information but fail to connect it to political action. As discussed in Section 6, this finding also speaks to the external validity of the survey experiment more broadly: to the extent the survey sample is less politically engaged than the white-collar workforce in the administrative analysis, the survey estimates are likely conservative.

Table B.8: PAC giving by political awareness subgroup

	Low Awareness		High Awareness	
Panel A: Extensive margin (any PAC donation)	Low H1	Low H2	High H1	High H2
Variables				
Post	-0.053* (0.023)	-0.054* (0.025)	-0.035* (0.016)	-0.035* (0.017)
Post × Treated	0.015 (0.021)	0.012 (0.023)	0.086*** (0.016)	0.102*** (0.019)
Post × Alignment		0.062*** (0.018)		0.143*** (0.019)
Baseline mean (pre, control)	0.318	0.328	0.191	0.206
Fixed-effects				
Respondent FE	Yes	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,170	1,068	1,380	1,260
R^2	0.702	0.710	0.688	0.722
Panel B: Intensive margin (PAC amount)	Low H1	Low H2	High H1	High H2
Variables				
Post	-1.705 (1.149)	-1.289 (1.155)	0.045 (0.641)	0.185 (0.687)
Post × Treated	1.169 (1.024)	1.276 (1.126)	3.326*** (0.893)	3.827*** (0.991)
Post × Alignment		2.308* (0.985)		5.512*** (0.898)
Baseline mean (pre, control)	9.495	9.698	4.348	4.673
Fixed-effects				
Respondent FE	Yes	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,170	1,068	1,380	1,260
R^2	0.506	0.520	0.533	0.568

Notes: Sample split at median pol_aware. Low = at or below median; High = above median. Respondent and issue fixed effects in all specifications. Standard errors clustered by respondent. Signif. codes: $^{\dagger}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

B.9 Anonymity Moderation

The survey randomized respondents into two conditions: half were told that any donations would be made on their behalf, while half were told donations would be made anonymously. The PAP pre-registers this as an additional test, predicting that treatment effects should be weaker for respondents in the anonymous condition — the reasoning being that if responses are partly driven by social desirability or signaling concerns, removing observability should attenuate them. Table B.9 reports the main specification (Equation 2) estimated separately within each condition.

The results offer partial support for the PAP prediction, with an important qualification. On the intensive margin, the alignment gradient is indeed substantially attenuated in the anonymous condition: Post $\times$ Aligned Intensity on any giving drops from 3.422 ($p < 0.001$) in the non-anonymous condition to 0.955 ($p > 0.1$) in the anonymous condition, and the misaligned mobilization effect on any giving similarly disappears (2.387, $p < 0.01$ vs. 0.236, $p > 0.1$). This pattern is consistent with the PAP prediction and suggests that at least some of the intensive margin reallocation reflects social presentation rather than underlying preferences.

The extensive margin tells a different story, however. The aligned PAC gradient remains positive and significant in both conditions (non-anonymous: $\hat{\beta} = 0.072$, $p < 0.001$; anonymous: $\hat{\beta} = 0.042$, $p < 0.001$), and the misaligned PAC withholding effect is, if anything, stronger in the anonymous condition (non-anonymous: $\hat{\beta} = 0.002$, $p > 0.1$; anonymous: $\hat{\beta} = -0.024$, $p < 0.01$). The co-partisan backlash among misaligned respondents also persists in the anonymous condition on the intensive margin ($\hat{\beta} = 1.887$, $p < 0.05$). These results suggest that the core mechanisms identified in the main text — PAC approval among aligned employees and PAC withholding among misaligned employees — reflect genuine behavioral preferences rather than social desirability responses. The attenuation on the intensive margin is more plausibly attributable to the constrained budget design, where the non-anonymous condition may amplify expressive reallocation within the dollar without changing whether respondents donate at all. Taken together, the anonymity results do not undermine the main findings; the extensive margin results, which are the more interpretable signal given the budget constraint, are robust across both conditions.

Table B.9: Anonymity moderation: treatment effects by anonymity condition

Panel A: Extensive margin (any/indicator)	**Non-Anonymous**			**Anonymous**		
Dependent Variables:	Any (1)	PAC (2)	Co-partisan (3)	Any (4)	PAC (5)	Co-partisan (6)
Variables						
Post	-0.056^{*}	-0.068^{***}	$-0.044^{\dagger}$	$-0.030^{\dagger}$	-0.014	-0.050^{*}
	(0.022)	(0.020)	(0.023)	(0.017)	(0.017)	(0.022)
Post × Aligned Intensity	0.028^{***}	0.072^{***}	$0.018^{\dagger}$	0.019^{*}	0.042^{***}	0.025^{*}
	(0.008)	(0.012)	(0.010)	(0.008)	(0.012)	(0.011)
Post × Misaligned Intensity	0.020^{*}	0.002	0.010	0.008	-0.024^{**}	$0.018^{\dagger}$
	(0.009)	(0.008)	(0.010)	(0.008)	(0.008)	(0.011)
Baseline mean (pre, control)	0.620	0.233	0.621	0.661	0.254	0.670
Fixed-effects						
Respondent FE	Yes	Yes	Yes	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	1,338	1,338	1,212	1,212	1,212	1,116
R^2	0.740	0.702	0.729	0.785	0.718	0.677

Panel B: Intensive margin (amounts)	**Non-Anonymous**			**Anonymous**		
Dependent Variables:	Any (1)	PAC (2)	Co-partisan (3)	Any (4)	PAC (5)	Co-partisan (6)
Variables						
Post	-5.129^{**}	-1.650^{*}	-2.036	-1.822	-0.138	-2.205
	(1.821)	(0.704)	(1.865)	(1.601)	(1.059)	(1.863)
Post × Aligned Intensity	3.422^{***}	3.330^{***}	0.101	0.955	1.538^{*}	0.009
	(0.765)	(0.608)	(0.979)	(0.702)	(0.629)	(0.983)
Post × Misaligned Intensity	2.387^{**}	0.439	$1.575^{\dagger}$	0.236	-0.763^{*}	1.887^{*}
	(0.791)	(0.396)	(0.870)	(0.734)	(0.336)	(0.956)
Baseline mean (pre, control)	51.835	6.893	39.938	57.648	7.661	47.755
Fixed-effects						
Respondent FE	Yes	Yes	Yes	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	1,338	1,338	1,212	1,212	1,212	1,116
R^2	0.777	0.548	0.725	0.822	0.522	0.693

Notes: Respondent and issue fixed effects included in all specifications.
Standard errors clustered by respondent in parentheses.
Non-Anonymous: respondents told donations will be made on their behalf.
Anonymous: respondents told donations will be made anonymously.
PAP prediction: treatment effects should be weaker in the anonymous condition.
All p-values two-tailed. Signif. codes: $^{\dagger}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

B.10 PAP H2: Alignment Moderation (Post-Treatment Only)

The pre-analysis plan (PAP) specifies H2 as a within-respondent, within-issue test of whether ideological alignment moderates the association between a corporate political stance and political behavior. Specifically, the PAP hypothesizes that "the association between an employer's corporate stance and political behavior differs across issues according to that individual's level of ideological congruence." The PAP-compliant specification uses post-treatment observations only and exploits within-respondent, across-issue variation in both stance direction and issue-specific feelings to identify the alignment gradient, after absorbing respondent and issue fixed effects.

The main text reports a pre/post within-respondent design (Equation 3, Table 4) that is strictly more efficient than the PAP specification, since it additionally differences out respondent-issue baseline levels of giving.Table B.10reports the PAP-compliant specification for transparency: all respondents in the analytical sample are included, outcomes are measured at post-treatment only, and the right-hand side contains `AlignedIntensity` and `MisalignedIntensity` directly rather than interacted with a post-treatment indicator.

The results strongly confirm PAP H2. On both margins and across all three outcome channels, the alignment gradient operates in the predicted direction and is precisely estimated. Aligned respondents give significantly more in total ($\hat{\beta} = 2.051$, $p < 0.001$ intensive; $\hat{\beta} = 0.022$, $p < 0.001$ extensive) and direct that giving disproportionately toward the employer's PAC ($\hat{\beta} = 3.002$, $p < 0.001$ intensive; $\hat{\beta} = 0.067$, $p < 0.001$ extensive), consistent with an organizational approval channel. Misaligned respondents withhold from the PAC ($\hat{\beta} = -0.648$, $p < 0.01$ intensive; $\hat{\beta} = -0.019$, $p < 0.001$ extensive) while increasing donations to their own partisan side ($\hat{\beta} = 1.968$, $p < 0.001$ intensive; $\hat{\beta} = 0.012$, $p < 0.05$ extensive), consistent with a backlash channel. The coefficients are closely consistent in magnitude and direction with the pre/post estimates in Table 5, confirming that the choice of estimator does not materially affect the substantive conclusions.

Table B.10: PAP-preregistered H2 specification: alignment moderation (post-treatment only, within-respondent)

Panel A: Extensive margin (any/indicator)			
Dependent Variables:	Any Donation	PAC Donation	Co-partisan non-PAC
	(1)	(2)	(3)
Variables			
Aligned Intensity	0.023***	0.063***	0.021***
	(0.004)	(0.006)	(0.006)
Misaligned Intensity	0.009†	-0.020^{***}	0.012*
	(0.005)	(0.004)	(0.005)
Baseline mean (control)	0.637	0.210	0.575
Fixed-effects			
Respondent FE	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes
Fit statistics			
Observations	2,526	2,526	2,271
R^2	0.828	0.737	0.769

Panel B: Intensive margin (amounts)			
Dependent Variables:	Any Donation	PAC Donation	Co-partisan non-PAC
	(1)	(2)	(3)
Variables			
Aligned Intensity	2.140***	2.850***	-0.377
	(0.383)	(0.334)	(0.516)
Misaligned Intensity	1.067**	-0.727^{**}	1.968***
	(0.404)	(0.223)	(0.482)
Baseline mean (control)	54.754	6.302	41.160
Fixed-effects			
Respondent FE	Yes	Yes	Yes
Issue FE	Yes	Yes	Yes
Fit statistics			
Observations	2,526	2,526	2,271
R^2	0.856	0.624	0.752

Notes: Post-treatment observations only. All respondents in analytical sample included.
Respondent and issue fixed effects included in all specifications.
Standard errors clustered by respondent in parentheses.
Signif. codes: $^{\dagger}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

B.11 PAP H3: Between-Respondent Design

The pre-analysis plan (PAP) specifies H3 as a between-respondent, within-issue test of whether ideological congruence moderates the relationship between a corporate political stance and employee political behavior. Specifically, the PAP hypothesizes that "across different individuals facing the same issue, the relationship between an employer's corporate stance and political behavior varies with each respondent's level of ideological congruence." The PAP-compliant specification uses post-treatment observations only, exploits between-respondent variation in alignment and stance direction within issues, and includes issue fixed effects. Unlike the within-respondent specifications in the main text, this design does not absorb respondent fixed effects and does not rely on pre/post changes — it is a purely cross-sectional test of whether alignment moderates post-treatment giving levels across respondents. To parallel the heterogeneity specifications in the main survey analysis, we estimate regressions of the form:

$$\begin{aligned} Y_{ij} &= \beta_0 + \beta_1 \,\text{Liberal stance}_{ij} + \beta_2 \,\text{Conservative stance}_{ij} \\ &\quad + \beta_3 \Big(\text{Liberal stance}_{ij} \times \text{Alignment}_{ij}\Big) + \beta_4 \Big(\text{Conservative stance}_{ij} \times \text{Alignment}_{ij}\Big) \\ &\quad + \gamma_j + \varepsilon_{ij}. \end{aligned}$$

where Y_{ij} is the post outcome for respondent i on issue j, γ_j denotes issue fixed effects, and standard errors are clustered by respondent. Alignment is coded so that the interaction terms can be interpreted as: greater alignment implies a larger treatment effect within each stance arm. Table B.11 reports results for both the intensive margin (amounts) and the extensive margin (any/indicator) across four outcomes: any giving, PAC giving, giving to Democratic organizations, and giving to Republican organizations.

Table B.11 reports results for both the intensive and extensive margins across four behavioral outcomes: any giving, PAC giving, giving to Democratic organizations, and giving to Republican organizations. The results strongly confirm PAP H3. Across both margins, alignment interactions move in the predicted direction: liberal-aligned respondents shift toward Democratic organizations under liberal stances (extensive margin: $\hat{\beta} = 0.280$, $p < 0.001$) and away from Republican organizations, while conservative-aligned respondents shift toward Republican organizations under conservative stances ($\hat{\beta} = 0.163$, $p < 0.001$) and away from Democratic ones. PAC giving is increasing in alignment under both liberal stances ($\hat{\beta} = 0.138$, $p < 0.001$) and conservative stances ($\hat{\beta} = 0.032$), consistent with the organizational approval channel documented in the main text. These patterns are broadly consistent with the within-respondent findings in Table 5, reinforcing the conclusion that ideological alignment is the central dimension along which corporate partisan stances reshape employee political behavior.

Table B.11: Between-respondent post-only heterogeneity by treatment arm and alignment

Panel A: Extensive margin (any/indicator)				
Dependent Variables:	Any	Any PAC	Any Dem	Any Rep
Model:	(1)	(2)	(3)	(4)
Variables				
Liberal stance	-0.021	0.059***	-0.090***	0.046**
	(0.018)	(0.016)	(0.017)	(0.017)
Conservative stance	0.001	0.003	-0.088***	0.070***
	(0.018)	(0.016)	(0.018)	(0.019)
Liberal stance × Alignment	0.117***	0.138***	0.280***	-0.129***
	(0.024)	(0.020)	(0.020)	(0.024)
Conservative stance × Alignment	-0.068**	0.032	-0.270***	0.163***
	(0.024)	(0.021)	(0.020)	(0.023)
Baseline mean (Control)	0.656	0.212	0.499	0.303
Fixed-effects				
Issue FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	2,308	2,308	2,308	2,308
R^2	0.020	0.039	0.129	0.042

Panel B: Intensive margin (amounts)				
Dependent Variables:	Any	PAC	Dem orgs	Rep orgs
Model:	(1)	(2)	(3)	(4)
Variables				
Liberal stance	-1.995	3.136***	-10.592***	5.461***
	(1.630)	(0.847)	(1.319)	(1.354)
Conservative stance	-0.210	0.890	-6.693***	5.592***
	(1.654)	(0.854)	(1.420)	(1.445)
Liberal stance × Alignment	11.516***	5.725***	20.155***	-14.363***
	(2.198)	(0.904)	(1.153)	(1.813)
Conservative stance × Alignment	-8.255***	2.268*	-23.686***	13.163***
	(2.193)	(0.960)	(1.353)	(1.757)
Baseline mean (Control)	56.799	6.321	34.788	15.690
Fixed-effects				
Issue FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	2,308	2,308	2,308	2,308
R^2	0.024	0.038	0.136	0.087

Significance codes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$.

C Pre-Analysis Plan: Clarifications and Deviations

This appendix documents the relationship between the pre-analysis plan (PAP) registered on June 11, 2025 and the analysis reported in the paper. I note any clarifications of ambiguities in the PAP and any cases where the analysis deviates from or extends the registered design.

C.1 Third Issue Domain

The PAP's Motivation and Overview section describes the three political issues as "abortion rights, diversity, equity, and inclusion (DEI) programs in the workplace, and responses to the January 6, 2021 insurrection." This was an editing error in the overview paragraph. The Summary of Treatment section of the same PAP—which specifies the nine vignette conditions—correctly lists transgender rights as the third issue, and the fielded survey used transgender rights throughout. The paper's use of "transgender equality" as the third issue domain is consistent with the actual registered and fielded design.

C.2 Political Awareness

The PAP states that the political awareness index is constructed from "six questions from ANES," describing five standard political knowledge items and a sixth based on respondents' ability to place the Democratic and Republican parties on a left-right spectrum. In the fielded survey, the index was constructed from five items only. The index used in the paper and in Appendix B.8 is therefore based on five items, not six. This was an error in the PAP. The construction of the index is otherwise exactly as registered: each item is coded as correct or incorrect, and I take the average across all five to form a continuous index that is then dichotomized at the median to define high and low awareness subgroups.

The PAP pre-registered a directional prediction that lower-awareness respondents would exhibit stronger responses to corporate political stances, consistent with theories of cue-taking among less engaged citizens (Kam, 2005). As discussed in Appendix B.8, this prediction was not confirmed: treatment effects are concentrated among high-awareness respondents. Although this result contradicts the pre-registered directional prediction, it is theoretically coherent and, in an important sense, reassuring about the validity of the underlying mechanism. This interpretation is also consistent with the external validity of the paper's findings more broadly—the white-collar workers who are the primary subject of Study 1 are, on average, likely to be more politically aware than the general survey population, suggesting the survey estimates may if anything be conservative.

C.3 Pre-Registered Outcomes Not Yet Reported

The PAP pre-registers three sets of outcomes that are not reported in the current version of the paper: (1) petition outcomes (willingness to sign a petition supporting a given political issue, and the alignment-weighted petition variable); (2) attitudinal outcomes (willingness to apply to the firm, motivation to work hard, and likelihood of leaving); and (3) cross-partisan giving (donations to organizations with whom the respondent is politically misaligned). These analyses were conducted as registered and will be reported in a follow-up study.