Mobilizing Internal Constituencies: Political Alignment Between Firms and Employees and Support for Nonmarket Strategy*

Max Kagan
Columbia Business School

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Abstract

Firms use a range of strategies to pursue their nonmarket strategy goals, often simultaneously. One corporate political activity (CPA) strategy involves the mobilization of supportive constituencies to advocate for firms' preferred policy outcomes. While previous research has focused on external constituencies (i.e., consumers), I ask whether firms can be successful at mobilizing internal constituencies (i.e., firms' own employees) in support of firms' political goals. I evaluate this in the context of U.S. elections, and ask whether workers' voting behavior is influenced by their firms' donations. Linking approximately 800,000 employees at 1,623 politically active employers to administrative turnout records across 303 U.S. state and local ballot question campaigns, I find that employees' voting behavior is aligned with firms' policy preferences under certain conditions. Individual employees turn out to vote at higher rates in elections where their employer is politically active. Although vote choices are secret, precinct-level evidence suggests that employees cast votes in line with their employers' preferred outcomes. These turnout effects are larger when firms' policy priorities also affect employees and absent when firms advocate on issues which have little to do with their core business. This suggests that linkages between firms' political activity and employees' voting are driven by recognition of mutual self-interest rather than coercion or pressure and that firms' ability to rely upon support from internal constituencies is likely limited to specific policy issues.

Keywords: constituency building, corporate political activity, nonmarket strategy, employee mobilization, voter file

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1 Introduction

Firms compete in both market and nonmarket arenas. Firms' ability to shape the "rules of the game" through nonmarket strategy can be as consequential as their ability to compete with one another directly (Baron 1995a,b). Research into corporate political activity (CPA) has identified different strategies firms use to influence relevant government policies and shape their institutional environment (Hillman and Hitt 1999; Hillman et al. 2004). Within this literature, firms' use of lobbying and campaign finance to influence policymakers has received significant scholarly attention (Katic and Hillman 2023). However, a third CPA strategy has been comparatively neglected. While building and mobilizing constituencies to support firm policy goals is consistently mentioned as a potentially valuable CPA strategy, we lack evidence on the strategic potential of different firm constituencies.

A small but growing number of studies begin to address this gap. This research highlights the challenges of relying upon external stakeholders such as customers or the general public. In addition to the costs of organizing "grassroots" outreach (Walker 2009; Walker and Rea 2014), the risks of reputational backlash (Walker and Le 2023), and the complexity of managing diverse stakeholder coalitions (Murphy et al. 2024), these strategies also depend upon external constituencies being aligned with firm goals in the first place, which is often not the case (Wen et al. 2024).

Given the challenges of relying upon external constituencies, internal constituencies may show greater promise. Because of the economic, social, and psychological ties between employers and employees, prior researchers have asserted that workers are predisposed to be naturally sympathetic and aligned with firms' nonmarket goals (Lord 2000; Keim and Zeithaml 1986; Keim and Baysinger 1988). Meanwhile, a parallel literature in other social sciences argues that firms can use their influence over employees to persuade (or coerce) them to act in support of firms' political goals (Hertel-Fernandez 2017, 2018; Frye et al. 2025). While both bodies of literature can point to anecdotal cases where employees played a role in supporting firms' broader nonmarket strategy goals, it is not clear the extent to whether employees will consistently take actions in support of firms' nonmarket goals.

I answer this question by asking whether and how employees' political behavior is aligned with firms' nonmarket strategy goals. Alignment is a key determinant of success for constituency mobilization: if constituencies and firms do not share the same policy preferences, firms' efforts to mobilize constituents are unlikely to prove strategically beneficial and may even backfire (Wen et al. 2024). To test the extent of alignment between firms' policy priorities and their employees, I assemble new large-scale evidence covering thousands of firms and hundreds of thousands of workers to examine whether firms' revealed policy preferences are aligned with their employees' behavior on these same issues.

I measure alignment by analyzing employees' voting behavior. Voting is not the only mechanism for mobilizing constituents; constituency building can involve many different actions including attending public meetings, contacting elected officials, and holding rallies or protests. However, elections represent a propitious context for two reasons. First, voting is a meaningful action in and of itself that can directly influence policy outcomes and indirectly influence policymakers by symbolically demonstrating popular support for particular policies (Kollman 1998). Second, voting behavior reflects not only rhetorical alignment but whether this alignment will be translated into meaningful and costly behavioral acts. As such, workers who vote in line with their firms' policy preferences can be counted on not only to vote, but are also more likely to take additional supportive actions.

I focus specifically on ballot questions (referenda) held in U.S. states and local jurisdictions. Ballot questions represent an ideal context for measuring alignment between firms' political priorities and individual workers' actual behavior. Elections in which voters select a party or candidate require multidimensional trade-offs, meaning that (mis-)alignment between workers and firms is hard to attribute to any particular policy goal. Furthermore, strong partisan identities may outweigh specific policy considerations for workers deciding how to vote (Jares and Malhotra 2024). By contrast, ballot questions ask about a single discrete policy proposal where alignment is more straightforward, and ballot questions also lack explicit partisan cues or labels. Additionally, while campaign finance regulation prohibits companies from giving directly to candidates, companies can and do spend directly on ballot question campaigns. As a result, firms' financial contributions can be assumed to be a (literally) costly signal of that firms' policy preferences.

I gather campaign finance data to identify companies that contributed to campaigns advocating for or against state or local ballot measures. I then link administrative voter records with public online employment profiles (i.e., LinkedIn) using recent advances in record linkage techniques (see Frake et al. 2026; Kagan et al. 2026). The resulting sample used in my estimations covers 785,771 treated workers at 1,623 active employers across 303 ballot questions in 39 states.

Because ballots are secret, I cannot directly observe whether employees vote in line with their firms' policy preferences. Instead, I triangulate based on observable measures, which I believe together offer a compelling case for the presence of firm-employee alignment. First, I look at voter turnout as a measure of individual worker mobilization. If employees are aligned with their firms' stance, I expect them to vote at higher rates in elections in which their employer is politically active, as workers will be more motivated to vote than would otherwise be the case. While the marginal increase in turnout may be smaller in elections with high baseline turnout (e.g., quadrennial Presidential elections), it has the potential to be larger in lower-turnout contests.

I find compelling evidence in support of firm-employee alignment. Difference-in-difference analyses show that employees turn out 2.10 percentage points (pp) more in elections in which their firm is politically active relative to comparable matched employees at firms working in the same industry and residing in the same political jurisdiction (95% CI: [1.31, 2.89]). Consistent with these effects operating on the margin—i.e., mobilizing voters who might otherwise have stayed home—the effects are smallest in high-turnout quadrennial Presidential elections (1.0 pp; 95% CI: [0.4, 1.5]) and largest in lower-turnout off-cycle elections (3.5 pp; 95% CI: [0.2, 6.8]). Furthermore—and lending support to the interpretation that increased turnout is due to firm-worker alignment—marginal increases in worker turnout are far larger for ballot questions which directly involve employees' material well-being. In policy issue domains with only low stakes for employees, the increase in voter turnout is only 1.33 pp (95% CI: [0.74, 1.92]), whereas it is 6.61 pp (95% CI: [2.73, 10.49]) in elections with high material stakes for employees.

These increases in turnout are consistent with alignment between firms and employees, especially given that increases are largest where employees' material interests are most aligned with firm stances. However, turnout alone cannot definitively establish whether workers vote in the direction preferred by their employer. While the secret ballot makes it impossible to know with certainty how any individual worker votes, I provide additional evidence from ballot vote shares in the seven states for which fine-grained precinct-level vote share data are available for ballot questions. Although my analyses cannot definitively rule out all forms of geographic confounding, precincts with larger shares of mobilizing-firm employees cast larger vote shares in favor of firms' preferred positions, providing further evidence consistent with firm-employee alignment mattering.

My evidence directly speaks to one potential mechanism of support: employees' own votes (Bombardini and Trebbi 2011). But in addition to directly affecting policy outcomes through election results, the presence of alignment between workers and firms means that firms can rely upon employees more generally to support their nonmarket strategy goals. Given that misalignment between firm goals and external constituencies has been shown to be a key limitation for constituency-based strategies (Wen et al. 2024), this finding affirms the strategic potential of internal stakeholders in supporting firms' non-market strategy. My findings also provide evidence on variation in alignment that can inform managers about when constituency building is most promising. Namely, internal constituency mobilization is most promising when firm policy preferences are clearly aligned with employees' material interest. It also likely holds greater promise in lower-salience "quiet" institutional settings where firms face fewer threats of counter-mobilization from competing interest groups (Culpepper 2010).

This paper makes three contributions. First, I contribute to a growing body of research which evaluates the strategic potential of constituency-based strategies within corporate political activity. CPA is conventionally sorted into information, financial incentives, and constituency building (Hillman and Hitt 1999), but constituency-based tactics have received relatively little attention (Katic and Hillman 2023; Murphy et al. 2024; Wen et al. 2024). I broaden the base of empirical evidence on constituency-based corporate political strategies to encompass internal as well as external constituencies. Second, I join a growing body of research which uses novel individual-level data sources—such as the administrative voter file records upon which I rely here—to provide new perspectives in assessing the role of employees more broadly as key stakeholder for nonmarket strategy. Whereas prior research has often viewed employees as audiences whose reactions must be managed (Burbano 2021; Hurst and Lee 2025; Hou and Poliquin 2025; McKean and King 2024; White et al. 2025), I reaffirm the strategic value of employees as a stakeholder that can positively contribute to firm goals and performance. Third and finally, I contribute to a growing line of management research which bridges nonmarket strategy research with other social science disciplines to better understand the role of organizations in democratic society, including theories and methods from economics (e.g., Bombardini and Trebbi 2011), political science (e.g., Frye et al. 2025; Hertel-Fernandez 2017, 2018) and sociology (e.g., Walker 2012).

2 Constituency Building and Employees

2.1 Corporate Political Activity and the Market for Public Policy

A key tenet of strategy is that firms' ability to shape the institutional environment can help or hinder their performance (Baron 1995a; Hillman et al. 2004; Bonardi et al. 2005). Within the broader nonmarket strategy literature, research into corporate political activity (CPA) argues that firms seek to influence policymakers via a “market for public policy” (Baron 1995b). Hillman and Hitt (1999) identify three main types of strategies used by firms: informational strategies such as lobbying, financial incentive strategies such as campaign contributions, and constituency-building strategies that mobilize supportive stakeholders (for other reviews of CPA, see Bonardi et al. 2005; Dorobantu et al. 2017; Hillman et al. 2004; Katic and Hillman 2023; Lenway et al. 2022; Mellahi et al. 2016).

All three CPA strategies aim to influence policymakers through mutually beneficial exchange. Lobbying has been conceptualized as an informational subsidy wherein firm lobbyists assist policymakers by providing useful information in exchange for access and influence (Hall and Deardorff 2006; Grossman and Helpman 2001), while campaign finance represents a more straightforward exchange of financial resources for access (Kalla and Broockman 2016). Similarly, constituency building is also based around exchange in the market for public policy (Lord 2003; Keim and Zeithaml 1986; Keim and Baysinger 1988). Elected policymakers who seek to remain in power must retain the support of the public (Mayhew 1974). Mobilized constituents can serve as a source of information by demonstrating to politicians that the issue in question is one which is important to their constituents, and thus one it may be valuable for them to address (Kollman 1998; Walker 2009, 2014). Mobilized constituents can also provide a more direct electoral resource in the form of the promise of electoral support if they are able to deliver the goals the constituency seeks.[1]

Given that all measures are forms of exchange, these strategies are often complementary and can operate through mutually reinforcing mechanisms (Garlick et al. 2025). For example, firms make financial contributions to politicians whom they later lobby (Kim and Li 2025). Constituency mobilization via “grassroots” efforts are also often deployed alongside lobbying (Walker 2012). When firms make their case to politicians, being able to point to an organized base of societal support—i.e., a mass of a politicians’ constituents who care about a particular issue—can reinforce firms’ case for their preferred outcome (Kollman 1998).

[1]Another constituency mobilization can theoretically be effective by directly shifting the outcomes delivered by policymaking institutions, rather than indirectly by influencing policymakers. The potential for a firms’ constituents to single handedly alter policy has not been mentioned in management literature, perhaps because many studies of CPA have focused on prominent large-scale contentious policy fights with mobilization and counter-mobilization of many competing interests. While it may not often be the case that a firm can mobilize enough constituents to directly shape outcomes, there may be certain institutional contexts where the direct of effect of constituency mobilization may be enough. For example, studies of California teachers’ suggest that teachers unions are regularly able to achieve their goals (pay rises for teachers) by turning out to vote in extremely low turnout elections, thus ensuring that their own members are elected to serve on school boards (Anzia 2011).

2.2 Constituency Building

In contrast to extensive studies of lobbying and campaign finance, research into constituency building has been far more limited (Katic and Hillman 2023). Most of this research into constituency building has focused around external stakeholders. Walker (2012) and Walker and Rea (2014) examine how firms organize grassroots lobbying campaigns that target the public in general, while Wen et al. (2024) focus on how rideshare companies mobilize their customers (i.e., platform users). Related research highlights other methods companies can use to shape public policy through public-facing strategies, such as the strategic allocation of corporate philanthropy (Luo et al. 2018; Kaul and Luo 2023; Seo et al. 2021) or CEO activism on sociopolitical issues (Chatterji and Toffel 2019; Hou and Poliquin 2023; McKean and King 2024; Poliquin and Hou 2025). Others study firm partnerships with social activist groups and non-governmental organizations (Dorobantu and Odziemkowska 2017; Gama and Gatignon 2025; Odziemkowska and McDonnell 2024). Murphy et al. (2024) offer a rich account of these processes, analyzing how firms construct "bootlegger and Baptist" coalitions with values-driven external stakeholders to build broader political support.

2.3 Employees: An Internal Constituency?

Empirical research into constituency-based strategies have largely focused on external stakeholders, with internal stakeholders largely absent. Bombardini and Trebbi (2011) hypothesize that firms with large, geographically concentrated workforces can use this potential voting bloc as leverage over policymakers (see also Bombardini and Trebbi 2025), but few subsequent studies have sought to deepen our understanding of how firms may harness employees to their strategic advantage in the nonmarket domain. To the extent that workers have featured in studies of corporate political activity at all, the focus has been on activities limited to a small subset of elite or specialized employees that require little participation (or even awareness) from the broader workforce: professional lobbyists (Jia 2018) or "government affairs" staff (Hall and Sun 2026), campaign finance donations by elites (Li 2018; Richter and Werner 2017; Stuckatz 2022; Stuckatz et al. 2025), elites with personal connections to government officials (Haveman et al. 2017; Jia et al. 2022; Wei et al. 2023), or "revolving door" employees formerly employed by government (Egerod et al. 2024; Katic and Kim 2024).

While largely overlooked, ordinary, non-specialized, rank-and-file employees may also be an attractive target for firm mobilization. Unlike external constituents such as the public or platform users which require significant constituency building efforts just to coordinate and contact potential supporters, outreach to employees is straightforward.[2] Instead of spending time, money, and effort on identifying potential constituents or securing alliances with existing organization, firms can move past the constituent building phase and focus on mobilization of employees. Given the manifest advantages and seemingly obvious attraction of internal constituencies, survey research establishes that firm managers use a range of methods to try to actively mobilize workers to become politically active and supportive of their preferred policies (Hertel-Fernandez 2017, 2018).

This survey research establishes that firm managers try to mobilize employees, but we lack evidence on how we might expect employees to respond to these types of messaging and thus if these attempts at constituency-building are likely to be successful.[3] Theoretical treatments have long assumed that workers are a natural constituency that is favorable towards firm mobilization (Keim and Zeithaml 1986; Keim and Baysinger 1988) and as such the principal challenge is to channel this natural alignment to productive use. Whether this alignment actually exists has been largely untested, but matters greatly for understanding the potential for employees to hold promise as a target for constituency mobilization. Alignment between the firm and target constituencies is a key precondition for the success of stakeholder-based strategies; outreach to misaligned constituencies is likely to be ineffective and can even lead to backlash (Wen et al. 2024). Similarly, political outreach to workers urging them to vote for specific political candidates has been shown to breed resentment and lower morale (Frye et al. 2025; Hertel-Fernandez 2018). And while it may be possible to coerce misaligned workers to engage in some actions without genuine enthusiasm, many types of broader constituency mobilization—e.g., speaking in public meetings, engaging with policymakers, or holding rallies—rely upon authenticity and enthusiasm (Lord 2003).

So are employees likely to be aligned with firms' nonmarket strategy goals? As previously noted, a predominant perspective largely treats the presence of firm-employee alignment as axiomatic. On a

[2] It is also legal in the United States: employers can require workers to attend "captive audience" meetings where political topics are discussed (Secunda 2010).

[3] Frye et al. (2025) catalog the use of workplace coercion in competitive authoritarian regimes such as Russia, where public officials press state-dependent employers to mobilize economically dependent workers to vote in support of the incumbent regime. While their methods can undoubtedly be effective at working mobilization—for example, some Russian bosses apparently require their employees to take a cell phone picture of their completed ballot from inside the voting booth—we have no evidence that such heavy-handed tactics are used in advanced industrial democracies such as the United States. Here, I am interested to understand if employees can and will act in support of their firms' policy goals in the absence of these types of coercion.

psychological and sociological basis, workers may incorporate their organizational affiliation into their personal identity, and thus may readily align themselves with their firms' policy goals and priorities (Ashforth and Mael 1989; Dutton et al. 1994; Ashforth et al. 2024). From an economic perspective, firms and their employees share material interests (Rogowski 1987). Research on political behavior also suggests that voters rely upon endorsements as a cognitive heuristics to aid in political decision-making within complex policy domains (Lupia 1994; Lupia and McCubbins 1998). While scholars have typically emphasized the role of advocacy groups as endorsers (e.g., Arceneaux and Kolodny 2009; Broockman et al. 2024), there is no reason to think that a trusted employers' political endorsement might not also influence individual workers' political attitudes and behavior. Especially on questions involving industry regulation, taxes tied to employment, or the continued operation of a local facility, employees might reasonably defer to the recommendations of their employer.

Although there are good reasons to assume that workers are typically aligned with their employer, there are also reasons to question employee-firm alignment. First, survey evidence finds that workers dislike and resent attempts by firms to involve them in politics in general (Frye et al. 2025; Hertel-Fernandez 2018). This parallels experimental research finds that corporate political involvement centered around sociopolitical issues alienates job seekers, demotivates existing workers, and deepens partisan sorting in the labor force (Burbano 2021; Hurst and Lee 2025; White et al. 2025), while also failing to persuade its intended policymaker audience (Hou and Poliquin 2025). Second, employees at most companies are politically diverse and may thus hold many different policy preferences which are not guaranteed to be unified with one another, let alone with the firms' policy priorities. In contrast to earlier research which posited that organizations have a coherent organizational political ideology (Gupta et al. 2017), recent research highlights that the most organizations are politically heterogeneous and include employees across the political spectrum who are unlikely to share unified policy views (Frake et al. 2026; Kagan et al. 2026).

2.4 Elections as an Observable Setting for Constituency Mobilization

Constituency mobilization can encompass the broad range of methods by which citizens can seek to influence policy. For example, constituents can contact their legislators, submit comments to regulatory agencies, attend public hearings or community meetings, and attempt to persuade their neighbors by distributing fliers, knocking on doors, or speaking out using traditional or social media (Kollman 1998; Walker 2014; Murphy et al. 2024).

While all of these actions can be part of a successful campaign, elections—and specifically, ballot questions—represent an ideal case to evaluate alignment between firms and employees. Ballot questions are a form of direct democracy practiced at the state and local level in the United States. Unlike candidate elections where voters delegate power to a representative, ballot questions ask voters to directly approve or reject particular policies.[4] While far less commonly-studied than candidate elections (i.e., Congress), ballot questions are a major feature of the U.S. institutional landscape. During a typical election cycle, there are over 100 state-level ballot questions across 30 or more states. Many questions involve issues which directly weigh upon specific industries or businesses. Common topics include energy policy, insurance regulation, approval for gambling and marijuana, and funding for infrastructure projects. Given the importance of these ballot questions for specific businesses, corporate spending to influence their outcomes can be substantial: Fahlenbrach et al. (2025) document that publicly listed firms alone contributed $1.77 billion to ballot measure campaigns between 2003 and 2018, with total corporate contributions including private firms and trade associations reaching approximately $3.5 billion.

The nature of ballot questions makes them well-suited to understanding employee-firm alignment. Elections for candidates involve contests between involve complex multidimensional trade-offs among potential candidates' personal attributes and distinct policy platforms. Furthermore, strongly entrenched partisan identities (Mason 2018) mean that voters may not change their voting preferences even in the face of policy concerns with major material stakes (Cox 2010; Jares and Malhotra 2024). Given this, it would be hard to attribute workers' voting behavior to (mis-)alignment with their firm, as even workers who are aligned with their firms' policy goals may not vote for candidates who support those policies if they weight other issues—or their own partisan identity—more highly. By contrast, ballot questions ask voters to weigh in on a single discrete issue rather than an overall policy platform. They also are devoid of explicit partisan labels or alignment.[5] As a result, they allow for a much more direct test of worker-firm alignment.

[4]This form of direct democracy is also sometimes referred to as referendums or plebiscites. In addition to the United States, direct democracy is also practiced in countries including Italy, Switzerland, Taiwan, and Uruguay as well as subnational governments in Austria, Australia, and Germany.

[5]This is not to say that ballot questions are necessarily non-ideological or orthogonal to partisan politics. Many ballot questions may be supported by one party and opposed by the other. However, there are no party labels on the ballot: voters opt for "Yes" or "No" rather than "Republican" or "Democrat."

3 Measuring Firm-Employee Alignment

Elections represent an ideal setting to test for firm-employee alignment. First, elections are important policymaking institutions in their own right. Second, alignment on policy views in elections can be seen as not merely indicating that employees alignment can be translated into support when it comes to the ballot box, but also potentially be extended to other methods and tactics of support. Because alignment around election consists of costly signals for both firms (i.e., spending money on campaigning) and employees (taking time to cast a vote), I can treat alignment as not merely rhetoric or cheap talk.

There is one main drawback in using elections, however: the secret ballot. While I can study elections using public administrative records (as I will describe below), these data cannot and do not ever tell us how any individual citizen votes.

I can never fully overcome this shortcoming, but I gather two different sets of data and perform two sets of analysis which I feel are together strongly suggestive that workers and firms are aligned. First, I look at individual voter turnout. Unlike some democracies (e.g., Australia), turnout in the United States is not mandatory, and citizens thus face a choice in deciding whether or not vote. Many choose not to. Even in Presidential elections, more than 40% of potentially eligible voters do not cast a ballot.[6] Many U.S. political campaigns thus focus less on persuading voters but more on persuading voters to actually turn out in support when it matters (Cox 2010; Jares and Malhotra 2024).

Given this, I can treat voting turnout as potentially revealing alignment between workers and their employers. In any given election, some workers will vote regardless, and some will never vote. However, there may exist many voters for whom the decision is marginal. If there an issue that matters to their employer, that could provide a marginal incentive that makes them more likely to turn out to vote. In other words, some workers are Always-Takers (who will vote regardless) and some are Never-Takers (who will never vote); neither group is expected to change their behavior depending upon what issues are on the ballot. I test for the presence of Compliers, whose marginal propensity to vote will increase if "treated" by having their employer take a stance on an election (Imbens and Angrist 1994).[7]

[6] Understanding the voter eligible population is not an exact science, as census data do not track citizenship or potential voter disenfranchisement (i.e., due to felony convictions). One can calculate turnout as a percentage of registered voters, but the full denominator of eligible-but-unregistered voters cannot be exactly known.

[7] While I believe that I am the first to apply measured turnout using population-scale voter file data in this manner, other scholars have theorized that voters are more likely to turn out to vote when issues they care about appear on the ballot or when they feel that their vote is more likely to make a difference, especially in low-turnout elections where few voters would otherwise cast ballots (Anzia 2011, 2012; Bursztyn et al. 2023).

While I cannot state for certain that changes in turnout are driven by firm-employee alignment, the proposed mechanism—i.e., that firm-employee alignment could drive increased voting in elections where firm policy priorities are on the ballot—has several testable implications. First, marginal turnout increases are more likely to be larger in lower-turnout elections. In high-turnout, on-cycle elections, the potential population of Compliers (i.e., voters whose propensity to vote is marginal) is smaller, whereas elections with lower baseline turnout have a greater share of workers whose behavior might be affected. Second, I would also expect voting to be increasing in the extent to which specific firm policy priorities overlap with employees' material interest. Not all firm policy priorities are likely to be shared by employees. In some cases, workers and employees may be clearly aligned. For example, some of the most common ballot question issues concern legalization and regulation of "sin" industries such as gambling and cannabis. In these cases, it is easy to see how casino workers and pot shop employees would be aligned with their employer in favoring more advantageous regulation. Yet in other cases, policy issues may be valued by the firm but lack a clear connection to employees well-being. For example, a change to businesses taxes might be opposed by business, but have limited salience for workers. Finally, some cases may actually pit employees and their employers against one another (e.g., on issues related to labor rights). If alignment is what is driving changes in voting turnout, I expect turnout to vary more for issues where there is clearer alignment between firms' policy priorities and employees' self-interest.

Changes in turnout and variation in timing and employer-firm alignment by issue can all offer evidence consistent with alignment, but cannot definitively establish that increased voting necessarily goes in the direction favored by the firm. Because I cannot bring individual-level data to bear on this question as I can with turnout, I consider more aggregated geographic data. If employees vote in the direction favored by their employer, then it stands to reason that the geographies in which they live and within which voting is tabulated—i.e., their voting precincts—will vote more heavily in the direction favored by the firm. Because workers of any particular firm are unlikely to ever constitute significant portion of a given election precinct (which typically range from 1,000–2,000 voters), the effect of individual workers is likely to be small. However, when one considers the potential for secondary persuasion effects to "spill over" (i.e., from workers to family members and close neighbors), it is possible that detectable effects could be found at the precinct level.

In sum, I overcome the limitations of the secret ballot by combining two different types of evidence with different strengths and weaknesses. My analysis of turnout can allow me to observe individual-level data and can plausibly support a causal interpretation with regard to voter mobilization, but cannot definitively prove that alignment works to a firms' advantage. My analysis of precinct-level returns can provide associational evidence on potential persuasion—i.e., the direction of the vote—but for various reasons, it does not readily lend itself to a plausible causally-identified design. While neither constitutes a silver bullet individually, I believe that both offer novel and compelling evidence on a previously-unaddressed question.

4 Voting Turnout

4.1 Data Construction

4.1.1 Identifying Firm Stances

My challenge is to find clear examples of firm stances on public policy issues that can be linked at scale with employees' actions on the same issue. To identify firm stances, I start with the universe of all state ballot questions which took place between between 2016–2024. For each ballot question, I comprehensively review campaign finance disclosures to capture all firms which donated money (or made in-kind donations) in support of or in opposition to any ballot question. I view these financial contributions as a costly signal of firms' preference on these issues.[8] For statewide ballot measures, I collect campaign finance data from the National Institute on Money in Politics (now OpenSecrets) to identify corporate contributors to ballot measure committees. I supplement my state-level coverage with ballot measures from Washington and Colorado, where municipal campaign finance disclosures are available through state-level portals and were scraped programmatically, and from El Paso, TX and Jackson County, MO, identified through manual searches of Ballotpedia's local ballot measure coverage.[9]

Campaign contribution records are based upon self-reported data—donors provide their name and contact information, and the ballot question campaigns report to relevant state authorities. Unfortunately, campaign finance regulations enforce no standards on reporting which require contributors to produce a consistent name or identifier, which can lead to confusion across name variants, subsidiary-parent relationships, and other sources of ambiguity (Shanor et al. 2022). Additionally, contribution records in

[8] Unlike contributions to political candidates, corporations are free to use their own funds (so-called "treasury dollars") to contribute to ballot measure committees, rather than relying upon voluntary donations by corporate executives through a firm-affiliated Political Action Committee (PAC).

[9] Ballotpedia covers local elections in the 100 largest municipalities.

all states do not always reliably distinguish between individual contributors, for-profit companies, other political campaigns, advocacy groups, and other types of contributors. To produce standardized names focused on only for-profit companies, I use an LLM-assisted pipeline to identify corporate contributors. Full details of this production pipeline, which involves many steps and manual and LLM-assisted cross-checks, are described in Appendix C.

In all, I identify 1,985 companies that contributed to 337 ballot-question campaigns across 131 elections in 40 U.S. states.

4.1.2 Matching to Employees

Having identified firm stances and linked politically active stance-taking firms with their employers, I next gather data measuring whether employees of these firms take action in support of their firms' stance.

We identify workers of these firms by using data on publicly-accessible online employee work profiles (i.e., LinkedIn), collected and standardized by Revelio Labs, a workforce intelligence vendor. Data from Revelio are increasingly used in management and other social science disciplines (e.g., Briscoe and DesJardine 2026; Hurst et al. 2025; Mohliver and Raines 2024; Wang and Zhang 2026). Because campaign finance data use non-standardized names and, since many are private, lack commonly-used identifiers (e.g., gvkey), I implement a four-tier matching cascade involving multiple manual and fuzzy matching techniques. My method relies upon conventional string distance matches to capture minor name variants, semantic embeddings which can capture variants such as abbreviations (Dasanaike 2026; Ornstein 2025), and finally, the knowledge embedded within large language models models (LLMs).[10] I describe the procedure in Appendix C.

Revelio Labs provides information on employment status. To understand these workers' political behavior, I rely upon administrative data records in the form of U.S. voter files. In the United States, individuals' vote turnout are a matter of public record. Voter file data contain details of registered voters'

[10]These methods have different strengths and trade-offs. String distance can retrieve obvious variants and minor typos such as "Microsoft Co." vs. "Microsoft Corp." vs. "Microsotf" [sic]. Semantic similarity relies upon high-dimensional embeddings and so is often able to retrieve far more distant text strings: for example, "BofA" vs. "Bank of America." Finally, LLMs contain massive amounts of knowledge about the world can impressively match subsidiaries and even map companies across M&A or name changes (especially very large commercial models as opposed to smaller local or open weight models). For example, modern LLMs are well aware that that "Instagram," "Meta," "WhatsApp," and "Facebook" all refer to the same corporate parent. As a result, LLMs can effectively evaluate and review the performance of other matching methods. A growing literature now demonstrates that LLMs dramatically outperform human coders on wide range of tasks (Bermejo et al. 2025; Choi et al. 2026).

turnout history, partisan affiliation, and demographic information (Hersh 2015). However, voter files contain no information on employment, which has historically limited their use in management and strategy research. I overcome this limitation by reling upon detailed individual-level microdata from the Politics at Work project (politicsatwork.org), an ongoing research initiative linking employment records to administrative voter files.[11] The voter file I use comes from L2, a nonpartisan data vendor, and contains approximately 180 million registered voters with their complete turnout history across all election types.

Merging the Revelio and L2 datasets presented substantial computational and methodological challenges. There is no unique identifier linking the two sources, and naive fuzzy matching across datasets of this scale would be computationally infeasible. In assembling the Politics at Work data set, the project addressed this through a multi-stage approach. First, both datasets are partitioned by metropolitan statistical area, reducing the scale of required comparisons while preserving theoretically plausible matches. Second, the project applied probabilistic record linkage following Enamorado et al. (2019), which weights multiple matching fields (name, gender, approximate age) by their discriminatory power. Third, it supplemented these with machine learning approaches using large language model embeddings (Ornstein 2025) to capture name variations that string distance measures would miss. The matching procedure is described in detail in Kagan et al. (2026) and Frake et al. (2026) as well as in Frake et al. (2025).

4.1.3 The Firm-Employer Estimation Sample

After matching public campaign data with the Politics at Work microdata (itself comprised of a data merge between Revelio Labs and L2), the fitted sample contains 785,771 treated workers at 1,623 active employers across 303 ballot questions in 39 states. Appendix Table E2 reports the firm-eligibility funnel, and Appendix E reconciles the raw and certified episode manifests.

Linked-worker coverage is highly skewed across company–state cells (Figure 1): the median cell contains 150 linked treated workers, compared with a mean of 1,212 and a maximum of 81,918. These cell counts describe coverage in the linked data rather than the workers entering any one firm–measure episode.

In cases where companies were active on opposing sides of the same ballot question, I consider each

[11] Other Politics at Work research projects include Hurst et al. (2025); Frake et al. (2026); Kagan et al. (2026); White et al. (2025).

measure $\times$ stance occurrence to be a separate episode; I have a total of 344 estimated episodes (Table 1). This definition ensures that each treatment effect reflects a single firm's activity on a specific ballot question rather than an aggregate across heterogeneous campaigns.[12]

4.2 Variables

Treatment I define an individual worker as having been treated if they were employed at a politically active firm (i.e., a firm which made contributions to a ballot question).

Turnout I define *turnout* by whether a worker turned out to vote in a particular election. I distinguish between cases were workers are registered to vote and do not turn out and historical cases where turnout is not recorded because voters were not yet registered in the state. I limit my sample to voters who were registered to vote in the state at the time an election took place and do not include voters who registered later.

Election Timing My sample spans elections held at different points in the electoral calendar. Ballot questions are most commonly held in November during even years, either coinciding with quadrennial Presidential elections or midterm elections. Other elections are held in November of "off-cycle" years (i.e., odd years) and a small number are held at other points throughout the calendar (i.e., not November). Baseline turnout differs dramatically depending upon timing. Presidential-year turnout can exceed 70% in many states, while midterm turnout is somewhat lower and off-cycle elections or those not held in November can be as low as 10–20%. I define four different "election families": namely, Presidential-year Novembers, midterm Novembers, odd-year Novembers, and elections held in other periods.

Material Stake I measure an *employee-livelihood stake* coded at the firm–measure level: how directly the ballot outcome could affect employees' jobs, income, or working conditions. Two language models infer this exposure from the contributor and resolved-employer identity, industry information, the measure text, and the firm's stance, scoring each firm–measure pair on a 1–10 scale; a third model adjudicates category disagreements. The measure is therefore a firm–measure-level proxy for employees' material exposure, not a measure of the firm's revenue stake alone or of individual employees' perceived

[12] Because each episode is a single firm's stance on a single measure, a firm that gave to both sides of the same question would enter as two opposing episodes. In practice this does not arise.

stakes. The categorical specification groups scores into high, medium, and low categories and uses low stake as the reference; the continuous specification enters the score directly. Appendix F provides the full procedure and exact production prompts.

Policy Domain I define a ballot measure's *policy type* as its primary substantive policy domain. I classified each measure from its description using a ten-category codebook. Appendix F provides the full procedure.

4.3 Estimation Strategy

We estimate the effect of employer political activity on voter turnout using a matched episode design that implements conditional parallel trends (Callaway and Sant'Anna 2021) by exact-matching on pre-treatment turnout outcomes and covariates before estimating episode-level treatment effects. Here, I describe details of the estimation strategy.

4.3.1 Aggregation via Stacked Difference-in-Differences

Although I have 344 episodes, they do not follow a common unit-time structure. Each state follows its own election calendar, with variation on when elections are held. (Appendix Table H2 reports the number of ballot measures by state and election year, with separate counts for statewide and local measures.) As a result, it is not possible to simply aggregate across a range of different elections, as the unit time structure is not common across different states and election families.

To pool episode-level estimates, I use the stacked difference-in-differences approach of Cengiz et al. (2019). All episode-level micro-data are stacked into a single dataset with episode-specific relative time indicators, equalized stack weights, and interaction terms for election timing and material stake. The stacked design accommodates the cross-state, cross-year structure of the data: episodes span different states with different election calendars, different treatment dynamics, and different election families, so aggregation requires a framework that defines relative time within each episode rather than imposing a common calendar (Cengiz et al. 2019). A secondary benefit is that the stacked approach avoids the bias from heterogeneous treatment effects that afflicts conventional two-way fixed effects estimation (Borusyak et al. 2024).[13]

[13]The voter $\times$ stack fixed effects absorb all time-invariant voter characteristics, including age at baseline, partisan registration,

I fit the following weighted linear probability model:

$$Y_{is\tau} = \beta\,(D_{is} \times \text{Post}_{s\tau}) + \alpha_{is} + \lambda_{s\tau} + \varepsilon_{is\tau}, \tag{1}$$

where $Y_{is\tau}$ equals one if voter i in episode stack s votes in relative election period τ; D_{is} indicates that the voter belongs to the treated cohort in that stack; and $\text{Post}_{s\tau}$ equals one at the treatment election ($\tau = 0$) in the headline window, $\tau \in \{-3, -2, -1, 0\}$. Because different election families (i.e., timing) have very different levels of turnout (i.e., Presidential years vs. off-cycle), I define each stacks relative unit time only for elections of the same type (for example, a 2022 midterm election would be compared with 2018, 2014, and 2010), The terms α_{is} and $\lambda_{s\tau}$ are voter-by-stack and stack-by-relative-period fixed effects, respectively. I estimate equation (1) by weighted least squares, carrying forward the match weights and rescaling them so that each episode stack receives equal total weight. The coefficient β is therefore the pooled average treatment effect on turnout at the treatment election. Standard errors are two-way clustered by firm and episode stack, allowing the regression errors to be arbitrarily correlated within firms and within campaign episodes.

4.3.2 Matching

Within each episode, I match treated employees to control voters who reside in the same state and whose employers I do not observe mobilizing on any ballot measure in that state at any point in my sample. The control pool thus excludes every firm that mobilizes in the state, so a firm that mobilizes in one episode is ineligible as a control in all others, and no already-mobilized firm ever enters a control group. The design therefore admits none of the already-treated-as-control comparisons that bias conventional staggered difference-in-differences estimators (de Chaisemartin and D'Haultfœuille 2023). My main specification targets up to 10 control workers for each treated worker with similar demographics and employment characteristics. Ten is a ceiling rather than a quota. Matching is without replacement, so a control voter can be used only once within an episode. After the eligible control pool is divided into exact covariate cells, some cells contain fewer than ten distinct controls per treated worker. I use the controls available in that cell; a treated worker with no admissible control does not enter the matched sample.

gender, race, and geographic location. These variables serve as matching covariates that ensure pre-treatment balance, but they are not included as regression controls because they are collinear with the fixed effects.

I deliberately do not match on prior turnout. Conditioning on lagged outcomes moves the design toward a synthetic control without being one, and it invites regression to the mean: matching on noisy pre-period turnout mechanically flattens the pre-trend, so the resulting flat pre-period is an artifact of the matching rather than evidence of comparability. Because I do not match on it, pre-treatment turnout remains an unforced check on the design, and I report it rather than engineering it away (Section B).

I match on the following covariates:

- **Partisanship**. I rely principally on official party registration, which is available in 31 states. For workers in other states as well as those who decline to register with one of the two major political parties (Democratic or Republican) but who nevertheless are likely "leaners" with a great sympathy towards one party, I impute partisanship based upon Bayesian models widely used in political campaigns and by political science researchers (Brown and Enos 2021; Kagan et al. 2026). I match exactly on (imputed) partisanship.
- **Gender.** I match gender exactly. I use L2 for gender rather than L2 as gender comes from administrative records in L2 in the majority of states, whereas Revelio estimates probablistically based upon name.
- **NAICS industry code (2-digit)**. I rely upon Revelio's classification of firms. I match exactly where possible but fall back to cross-industry matching if same-industry controls are insufficient.
- **Age**. I rely upon official dates of birth from the L2 voter file. I match to nearest neighbor(s) based upon age.

Match weights are carried forward into all subsequent estimation. When exact-match cells are sparse, a structured fallback order relaxes covariates: industry is dropped first.

Table 4 describes the individual workers in the estimation sample. As specified above, the matching procedure pairs workers exactly on gender, partisanship, and two-digit NAICS industry, then uses nearest-neighbor matching on age within those exact-match cells.

5 Direction-of-Vote: Precinct-Level Returns

As described in Section 3, I also measure direction-of-vote at the precinct level. Firm-employee alignment can bring employees to the polls who would otherwise have stayed home via mobilization. Firm-employee alignment can also work to shift vote choice—i.e., persuasion. Because I cannot view individual voters' choices, I use precinct-level returns on the ballot measures themselves. I describe my procedure for doing so here.

5.1 Data Construction

From the full set of 344 episodes used in the individual turnout analysis, I identify the subset of 31 ballot questions within seven states that provide election results at the ballot question and precinct level.[14] In this case, the unit is the precinct × ballot-measure cell. I exclude measures where firms mobilize on both sides of an ballot question. Details of case selection and data construction are in Appendix G.

5.2 Variables

Share of Politically Active Workers In each precinct–measure cell, I sign worker counts by firm stance and net them across mobilizing firms. The modeled exposure is the absolute value of this net count divided by official ballots cast on the measure.[15]

Vote Share For each measure and precinct, I define the outcome as the firm-preferred share of ballots cast on the measure. I use the affirmative share when the firm supports the measure and one minus that share when the firm opposes it. Stance therefore orients the outcome, not the nonnegative exposure magnitude or the ordering of an adjacent pair.

5.3 Estimation Strategy

My first specification estimates

$$y_{pm} = \beta x_{pm} + \lambda_m + \varepsilon_{pm} \tag{2}$$

[14] While states provide overall results, it is less common to include precinct-by-precinct results for every question on a given ballot in readily-accessible, machine readable format online.

[15] I normalize by votes cast rather than by the registered electorate for practical reasons. I receive "snapshot" voter files from L2 at irregular periods that vary by state, and I rarely have a voter file that corresponds exactly to the date of an election and its contemporaneous precinct boundaries. For the adjoining-pair design, I repeat the baseline specification using two alternative registration proxies as denominators: the count of surviving year-t registrants in the linked panel and the closest available post-election L2 demographic-roster count. Appendix Table G4 reports both alternatives.

on 169,701 precinct × measure cells, where y_{pm} is the firm-preferred vote share in precinct p on measure m and x_{pm} is the share of ballots in precinct p cast by workers at politically active firms on measure m. Measure effects, λ_m, ensure that the slope is estimated from differences across precincts voting on the same measure rather than from differences in average support across measures. I use Conley spatial-HAC standard errors using a 5 km Bartlett kernel.[16] The model does not absorb stable precinct characteristics. Residential sorting and other differences in precinct composition can therefore generate the association, so I do not interpret it as a causal effect of workplace exposure on vote choice.

This specification asks whether precincts with greater workforce exposure are more supportive of the firm's position than other precincts voting on the same measure. As a complementary spatial comparison, I fit an additional specification that limits comparisons to immediately adjoining precincts. Because precincts are fairly small (typically 1,000 to 2,000 people), adjoining precincts are likely to have similar populations. Figure 2 illustrates the adjoining-precinct design and the variation in workforce exposure across neighboring precincts.

This adjoining precinct specification estimates

$$\Delta y_{ijm} = \alpha_m + \beta \Delta x_{ijm} + \varepsilon_{ijm}, \tag{3}$$

where

$$\Delta y_{ijm} = y_{im} - y_{jm} \qquad \text{and} \qquad \Delta x_{ijm} = x_{im} - x_{jm} \tag{4}$$

and is based on 164,740 nonzero within-measure pair differences. Each pair is oriented so that i is the higher-exposure precinct. Pairs whose members have the same exposure are excluded. Both precincts use the same measure-specific outcome coding. The model estimates one common slope with measure-specific intercepts, α_m. Each pair is weighted by the smaller number of ballots cast across its two precincts.

[16]Appendix Tables G2 and G3 report errors at alternative bandwidths and under a uniform kernel, along with spatial block-bootstrap intervals.

6 Results

6.1 Voter Turnout

Table 5 presents my results. In the first column, I find that across 344 estimated statewide and local firm–ballot-measure episodes spanning 39 states, employees at mobilizing firms turn out to vote at 2.10 percentage points higher rates than matched employees whose observed employers are untreated in the episode (95% CI: [1.31, 2.89]). This constitutes the first evidence consistent with firm-employee alignment by establishing that marginal workers turn out to vote at higher rates when a policy priority of their firm is on the ballot.

Further evidence consistent with this interpretation comes from timing. In Column 2, I find that the marginal increase in turnout is larger in elections with lower turnout, where there are greater number of potential Compliers—i.e., workers for whom the decision to turnout is marginal. Compared to Presidential years where turnout is already higher, the marginal treatment effect is higher in midterm and off-year statewide elections. The midterm interaction is 1.6 pp (95% CI: [0.7, 2.5]) and the off-year interaction is 3.2 pp (95% CI: [−1.2, 7.6]), both relative to presidential-year November elections. The off-year interval includes zero. The effect on "other" elections held outside of November Election Day is also large and positive but has large standard errors and is not significant (4.1, 95% CI: [−0.3, 8.4]).

In addition to differences across timing, I also find that the specific content of the firm stance matters for the extent of firm-employee alignment. Column 3 estimates a positive linear interaction with the firm–measure-level employee-livelihood stake score. Column 4 shows that the difference is concentrated among high-stake firm–measure pairs: workers at firms facing a high-stake measure turn out 5.28 pp more than workers at firms facing a low-stake one (95% CI: [1.37, 9.20]), while the medium- and low-stake estimates do not differ.

6.2 Vote Direction: Precinct Vote-Share

The secret ballot means that the turnout results cannot establish whether the additional voters support their employer's position. Table 6 reports the aggregate vote-direction results, and Figure 2 illustrates the adjoining-precinct comparison.

In the precinct-cell specification, the exposure slope is 4.13 (95% CI [3.202, 5.050]). Column (1) compares precincts voting on the same measure. Its measure effects remove differences in average support across measures, but the model does not absorb stable precinct characteristics. Residential sorting and other differences in precinct composition can therefore generate the association.

The adjoining-pair slope is 0.20 (95% CI [−0.003, 0.406]). The estimate is much smaller than the precinct-cell slope and falls just outside the conventional 5% significance threshold ($p = .054$); its Conley 95% confidence interval narrowly includes zero. The 10 and 25 km spatial block-bootstrap percentile intervals exclude zero, although the corresponding two-sided bootstrap p-values are .054 and .063.

Appendix Tables G4, G2, and G3 report the full robustness analysis: two registration-based exposure denominators for the adjoining-pair design, alternative Conley bandwidths and kernels, spatial block bootstraps, and a restriction to pairs in which both precincts are exposed. The precinct-cell slope remains positive and distinguishable from zero across the applicable inference checks. The adjoining-pair estimates also remain directionally positive, although they are somewhat less precise and some confidence intervals include zero.

6.3 Additional Results

Appendix A reports measure-level analyses of material stake, election scope, policy type, job seniority, and procedural origin, and Appendix F.4 the classification of firms by whether the measure's industry origin runs through their business or at it. Estimates by employee age and workforce partisanship are computed on an earlier analysis stack and are not reported.

Issue Area Figure A1 reports marginal treatment effects by ballot-measure policy area. The regression interacts treatment with a full set of issue-type indicators and omits a reference category, so each coefficient is the estimated treatment effect within that domain. Appendix F documents the coding rules I use to classify ballot measures by issue. Environment and energy measures have the largest point estimate (1.53 pp, $p < 0.001$), followed by housing and land (1.01 pp, $p = 0.03$) and healthcare (0.63 pp, $p = 0.05$); these three are the domains whose intervals exclude zero at conventional levels. Education (0.56 pp, $p = 0.06$) and the residual Other category (0.59 pp, $p = 0.05$) are positive and borderline. Every point estimate is positive, but most domains are individually imprecise, and I read the decomposition as a description of where the pooled effect concentrates rather than as ten separate tests. The pattern is not a clean ranking by presumed shared stake. Industry regulation, despite having more episodes than any other domain, does not separate from zero (0.41 pp, $p = 0.39$), nor do tax and fiscal measures (0.79 pp,

$p = 0.08$) or electoral and governance questions (0.21 pp, $p = 0.44$).

Appendix Table A2 reports a complementary equal-stack-weighted specification over the main estimation window, using labor/employment as the reference category; unlike Figure A1's direct within-domain effects, its interaction coefficients are contrasts against that category. Labor and employment measures—which can place employer and employee interests in tension, as with minimum-wage or workplace-safety regulation—show a near-zero effect (0.08 pp, $p = 0.79$). This result is consistent with a shared-stake interpretation, but labor is not the only null domain: industry regulation and tax and fiscal policy are also statistically indistinguishable from zero. No category has a negative point estimate, so the decomposition provides no evidence of backlash in any domain.

Housing and land is an instructive contrast. A developer campaigning over what it may build has a transparent commercial interest, yet the domain sits near the top of the distribution (1.01 pp, $p = 0.03$). That result is compatible with the material-stake rubric, which asks whether the outcome reaches employees' livelihoods rather than whether the firm's interest is visible. It also cautions against treating a visibly self-interested firm position as necessarily unpersuasive to workers.

Social and civil-rights measures—abortion, same-sex marriage, and similar questions—do not fit naturally on an operational-stake scale. Their estimated effect is 0.42 pp and is statistically indistinguishable from zero ($p = 0.26$). Corporate contributions to such campaigns are rare and do not generally track firms' operational interests (Fahlenbrach et al. 2025).

Election Scope Measure-level analyses include election scope (statewide versus local elections). Appendix Table A1 reports the estimate separately for each; the two are not statistically distinguishable from one another.

6.4 Robustness

The main turnout specification pools the matched firm–measure episodes using a stacked difference-in-differences estimator (Cengiz et al. 2019). Appendix B examines the result using event-study diagnostics, sample restrictions, an alternative aggregation rule, and an assessment of differential registration.

The pooled event study offers limited evidence on parallel trends (Figure B1). The only pooled pre-treatment coefficient not mechanically balanced by exact matching, t_{-3}, is -0.65 pp (95% CI [-1.49, 0.19]). The post-treatment estimates remain positive at t_{+1} (0.8 pp) and t_{+2} (1.3 pp), but these later coefficients are descriptive because increasingly selected episodes and workers contribute at longer horizons. Event studies estimated separately by election timing reproduce the treatment-period gradient in the main results, with larger effects in lower-turnout elections (Figure B2). As in the pooled event study, the timing-specific pre-treatment periods used for matching are not independent tests of parallel trends.

The result remains positive when treated workers are divided according to whether the focal episode occurred in their first observed mobilization year or a later one. The estimated effects are 2.1 pp (95% CI [1.3, 2.9]) for first-year mobilization and 1.8 pp (95% CI [0.7, 3.0]) for later mobilization (Appendix B.3; Figure B3). Both estimates are positive, although this exercise is not a formal test of whether they differ.

Finally, pooling the episode estimates by random-effects meta-analysis yields an effect of 1.26 pp (95% CI [0.94, 1.57]; Appendix B.4). This estimate is smaller than the stacked estimate but remains positive and precisely estimated, although the episode estimates are heterogeneous ($I^2 = 89.1\%$). It is computed on the same episodes as the stacked specification; the comparison is nonetheless not exact, because inverse-variance pooling weights episodes differently and treats their estimates as independent. Appendix B.5 considers selection from conditioning on voter registration. High baseline registration among employed adults and exact matching on prior turnout reduce this concern but cannot eliminate it. If mobilization itself induces registration, the omitted registration margin is more likely to attenuate the estimated total mobilization effect than to account for the positive turnout estimate.

7 Discussion

Constituency building has long been mentioned as a potentially viable corporate political activity (CPA) which firms may use in pursuit of their nonmarket strategy goals (Hillman and Hitt 1999). Employees in particular have often been mentioned as a so-called “natural constituency” who are naturally aligned with firms’ goals and priorities (Keim and Zeithaml 1986; Keim and Baysinger 1988) such that firms can reap nonmarket strategy rewards from their employes (Bombardini and Trebbi 2011). But while other aspects of CPA—i.e., lobbying and campaign finance—have benefited from decades of accumulated empirical evidence, constituency mobilization has received relatively little scholarly attention (Katic and Hillman 2023). To the extent that recent scholars have examined constituency mobilization, they have tended to focus on external stakeholders, such as “grassroots” outreach to the general public (Walker 2012) or strategies which target customers (Wen et al. 2024). While scholars in other disciplines have gathered survey evidence showing that firm managers do attempt to mobilize their employees as supportive internal constituents (Frye et al. 2025; Hertel-Fernandez 2017, 2018), we lack evidence on whether employees and firms are even aligned in the first place in such a way as to make these mobilization attempts likely to succeed.

My assembled evidence here constitutes, to the best of my knowledge, the first ever large-scale evidence on the extent of firm-employee alignment. Prior studies have asserted rather than proven that employees are actually aligned with their employers. This evidence does not just rely upon self-reports or rhetorical alignment, but involves costly signals on the part of both firms and employees. I identify firms' goals from their spending on ballot question campaigns and then ask whether their employees take the time to vote in support of their firms' position on these measures. Because votes themselves are secret and not observable, I present two complementary sources of evidence in support of the claim that firms and employees are aligned. First, I show that employees are, on the margin, more likely to turn out to vote in elections where strategic priorities for their employer are on the ballot.

In addition to my evidence showing effects on marginal voter turnout, I also present evidence on the overall direction of vote at the precinct level. This evidence suggests that precincts with a greater share of employees at politically active firms vote more in favor of the firms' preferred direction. While I cannot definitively rule out potential geographic confounding, an adjoining-precinct design that is likely to control for many types of geographic confounding delivers consistent results.

My results have several implications. For firm manager and organizational leaders my results establish that internal stakeholders—i.e., employees—can hold strategic value beyond economic production and can also contribute to pursuit of nonmarket strategy. My focus on voting is twofold. First, voting—especially on ballot questions—can be an important institutional setting in its own right. While few if any firms may have enough employees to single-handedly influence the results of a national election, it is more plausible that their votes could influence the results of smaller-scale, low-turnout subnational contests (Anzia 2011). Second, in addition to the direct impact of their own votes, workers can contribute to broader constituency mobilization efforts. They can mobilize others to vote, such as their friends, family, neighbors, and other community members. They can also participate in the myriad ways democratic citizens have at their disposal to influence policy, e.g., by contacting public officials, holding rallies or protests, and speaking out on social or traditional media.

In addition to affirming the value of internal constituents for CPA, my results also suggest meaningful scope conditions that build upon other recent studies of constituency building and can guide managers in considering when strategies are most likely to be successful (Wen et al. 2024). First, the difference in effect size between high-salience "on-cycle" and lower-salience "off-cycle" elections is consistent with prior work on "quiet politics" that suggests that firms are more likely to succeed in the nonmarket domain in issue areas and institutions which attract less public scrutiny and potential counter-mobilization (Culpepper 2010). Second, my results indicate that employee-firm alignment is not omnipresent, but varies significantly based upon the material stakes faced by employees. In issue domains where employees and firms share common interests, employees can be powerful and attractive allies. However, in cases where interests alignment is less clear or even divergent, employees are less likely to act in support of firm goals.

While I view this evidence as making important and novel contributions to the study of constituency building, CPA, and nonmarket strategy more broadly, I note several important limits and opportunities for future research. First, my focus on large-scale observational evidence from publicly records means that I lack detailed firm-by-firm details of what deliberate constituency mobilization efforts, if any, accompanied firms' stance-taking by making financial contributions. In some cases, firms may have actively encouraged or solicited participation from employees. In other cases, employees may have discovered firms' stances on their own or acted out of their own self-interest without necessarily even being aware of their firms' position. Future in-depth research can doubtless shed more light on this important question by other means, such as qualitative fieldwork, interviews, or other qualitative studies.

Second, my use of the Politics at Work linked microdata means that I focus on a particular population which is present on LinkedIn. In fact, the Politics at Work data appears surprisingly consistent with the overall labor market on observable covariates (Frake et al. 2026), but it is possible and indeed likely that this population differs from the overall workforce on potentially important dimensions. This relates to a third limitation, which is that I focus—like the overwhelming majority of CPA resarch—on the particular institutional context of the United States (Frye et al. 2025). Future studies can and should consider constituency mobilization in other contexts, especially nondemocratic ones where workers may enjoy far fewer rights and protections (Frye et al. 2025).

I view this research as making several contributions. The first contribution is to literature on nonmarket strategy in general, corporate political activity (CPA), and specifically to constituency mobilization.

Whereas previous empirical literature has largely focused on external stakeholders, this paper brings firm's own workforce into the study of constituency building. Second, I contribute to literatures which consider the value of employees from various perspectives, including human capital and stakeholder theory. While nonmarket strategy literature has treated employees mainly as stakeholders whose reactions to a firm's politics must be managed. Finally, I also contribute to efforts to bridge management scholarship with other research traditions in the social sciences. Political science explains how voters use shortcuts, endorsers, and social networks (Lupia 1994; Campbell et al. 1960; Sinclair 2012), but firms are largely absent from its account of who mobilizes voters. Nonmarket strategy studies how firms pursue political objectives but usually treats that activity as aimed at policymakers. The account here sits at the intersection: political science explains why an employer could be a relevant source on a costly voting decision (Lupia 1994; Grimmer et al. 2026), while management research explains why the employment relationship gives firms repeated access to workers. The 305 episodes across 39 states provide broad evidence across firms, places, and policy questions, although the selected population and institutional setting limit generalizability. More broadly, evidence that employer political activity changes participation contributes to growing research on the societal consequences of organizational behavior (Battilana et al. 2025).

Tables

Table 1: Sample Composition

Unique firms	1,870
Unique ballot measures	303
Statewide	264
Local	39
States with an estimated episode	39
Local jurisdictions (in 4 states)	24
Unique elections (jurisdiction × date)	168
Year range	2016–2024
Firm × measure pairs	3,036
Estimated episodes	344
Statewide	305
Local	39

Table 2: Firm Characteristics

Panel A: Summary Statistics

Characteristic	Unit of observation	Units (N)	Mean	Median	SD	Min	Max
Employment linkage (unit: employer ID)							
Linked workers per employer, all states	employer ID	2,605	6,523	330	26,002	2	408,392
Linked workers per employer, in the measure's state	employer ID × state	3,004	1,322	128	5,179	1	81,918
Treated workers per employer, in one episode	employer ID × episode	4,620	358	19	1,828	1	38,883
Campaign finance (unit: donor firm)							
Total contributions per firm, across its measures ($)	donor firm	1,861	1,246,466	25,000	8,021,394	0	179,955,372
Contributions per firm, per estimated measure ($)	donor firm	1,861	509,523	17,500	2,731,117	0	63,331,461
Estimated measures per firm	donor firm	1,870	1.6	1.0	1.4	1	18

Panel B: Industry Composition

Industry	Firms	%
Construction (23)	233	12.5
Professional Services (54)	220	11.8
Manufacturing (31–33)	213	11.4
Finance & Insurance (52)	185	9.9
Health Care (62)	152	8.1
Real Estate (53)	131	7.0
Accommodation & Food (72)	97	5.2
Retail Trade (44–45)	89	4.8
Agriculture (11)	79	4.2
Wholesale Trade (42)	77	4.1
Mining & Extraction (21)	74	4.0
Utilities (22)	64	3.4
Arts & Entertainment (71)	61	3.3
Information (51)	55	2.9
Educational Services (61)	44	2.4
Transportation (48–49)	38	2.0
Public Administration (92)	32	1.7
Admin & Waste Services (56)	21	1.1
Management of Companies (55)	3	0.2
Other Services (81)	2	0.1

Notes: Each row is a distribution, not a count. The N column gives the number of units named in the second column, and the remaining columns summarize that row's per-unit quantity across those units. The two sub-blocks count different firm populations and are not comparable: a single donor firm can operate under several employer IDs, so the employer-ID counts exceed the donor-firm counts. Panel B counts donor firms. Linked workers are unique workers observed in the employment–voter linkage and therefore a subset of true employment. "All states" sums the employer ID's linked counts across states; the measure-state row uses only the state of the ballot measure; and treated workers are unique voters within an employer-ID–episode cell in the estimation stack. Because a worker can be treated in more than one episode, the employer-ID × episode cells sum to more than the count of distinct treated workers reported in Table 5. Of the 2,607 employer IDs in the estimation stack, 2,605 carry an overall linked-worker count; the remaining 2 have none and are excluded from the first row only, which is why that row's N falls short of the active-firm count in Table 5. These rows do not each inherit a 10-worker minimum: the statewide registry screens the employer's coverage-state count, Colorado screens its linked Colorado count, Washington screens its linked total across states, and the hand-assembled El Paso and Kansas City cases bypass those programmatic crosswalk screens. Contribution rows use the campaign-finance donor firm as the unit. Total contributions sum giving across that firm's estimated measures, and the average divides that total by its number of estimated measures. Contribution amounts are from Follow the Money / OpenSecrets. Oregon Measure 108 is omitted from the dollar rows because its refund and unitemized-donation records cannot be reconciled; the measure remains in all nondollar counts and in estimation.

Table 3: Ballot Measure Characteristics

	N	%
Policy type (all 303 measures)		
Industry Regulation	46	15.2
Electoral & Governance	42	13.9
Education	41	13.5
Tax & Fiscal	37	12.2
Social & Civil Rights	32	10.6
Environment / Energy	27	8.9
Other	22	7.3
Healthcare	21	6.9
Labor & Employment	19	6.3
Housing & Land	15	5.0
Project / Bond	1	0.3
Material stake (2,890 firm × measure pairs)		
High	348	12.0
Medium	1,297	44.9
Low	1,245	43.1
Scope (all 303 measures)		
Statewide	264	87.1
Local	39	12.9
Election timing (all 303 measures)		
Presidential	147	48.5
Midterm	96	31.7
Off-year	39	12.9
Other	21	6.9
Outcome (264 statewide measures)		
Passed	155	58.7
Failed	109	41.3

Notes: Policy type and material stake categories are defined in Appendix F. Policy type is assessed for each ballot measure. Material stake is firm-specific and is defined for each firm × episode pair. Presidential, midterm, and off-year elections occur in November; "Other" includes all non-November elections.

Table 4: Worker Characteristics in the Estimation Sample

	Treated	Control	Std. Diff.[b]	Equiv. p[c]
N (person-episodes[a])	1,660,615	11,050,243		
Unique voters	791,739	4,774,436		
Demographics				
Age	50.4 (14.5)	50.3 (14.9)	0.010	<0.001
Female	0.477	0.485	−0.017	<0.001
Male	0.523	0.515	0.017	<0.001
Partisanship				
Lean Democrat	0.418	0.384	0.069	<0.001
Lean Republican	0.345	0.324	0.045	<0.001
Independent	0.048	0.050	−0.008	<0.001
Employment				
Seniority (1–7)	2.7 (1.6)	2.7 (1.7)	0.008	<0.001

Notes: [a]A single voter can appear in multiple episodes if their employer contributed to more than one ballot measure; person-episodes is the unit of analysis. Statistics computed at the person-episode level, weighted by episode-equalizing stack weights. Seniority is the Revelio Labs 1–7 scale (available for 100% of treated, 100% of control). [b]Difference in means divided by the pooled standard deviation. [c]p-value from two one-sided t-tests (TOST) equivalence procedure (Spenkuch et al. 2023); the reported value is the larger of the two one-sided p-values, under the null that the standardized difference exceeds ± 0.1 standard deviations. $p < 0.05$ indicates that equivalence within the ± 0.1 SD margin can be confirmed.

Table 5: Employee Mobilization and Voter Turnout: Stacked Difference-in-Differences

	Overall	Timing	Stake (continuous)	Stake (categories)
Treated × Post	0.021***	0.008***	0.000	0.013***
	(0.004)	(0.003)	(0.006)	(0.003)
… × Midterm (vs. presidential)		0.016***		
		(0.005)		
… × Off-cycle (vs. presidential)		0.032		
		(0.022)		
… × Other timing (vs. presidential)		0.041*		
		(0.022)		
… × Stake score (1–10)			0.006**	
			(0.002)	
… × Medium stake (vs. low)				0.005
				(0.007)
… × High stake (vs. low)				0.053***
				(0.020)
Num. Obs.	34,957,427	34,957,427	34,957,427	34,957,427
N treated workers	785,771	785,771	785,771	785,771
N control workers	4,708,442	4,708,442	4,708,442	4,708,442
N states	39	39	39	39
N ballot questions	303	303	303	303
N active firms	1623	1623	1623	1623
Voter × Episode FE	Yes	Yes	Yes	Yes
Episode × Period FE	Yes	Yes	Yes	Yes
Clustering	Firm + Episode			

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each column reports a stacked difference-in-differences regression on the combined statewide and local sample. Column 2 uses presidential-year November as the timing reference and retains midterm November, off-cycle November, and all other election dates. Columns 3–4 restrict to episodes with a classified treated firm–measure stake; the continuous interaction is the change per one-point increase on the 1–10 scale, and the categorical specification uses low stake as the reference.

Table 6: Firm Workforce Exposure and Neighborhood Vote Direction

	Levels		Adjacent precinct pairs
	(1)	(2)	(3)
Mobilizing workers / ballots cast	4.13^{***}	0.48^{**}	0.20^{*}
	(0.47)	(0.24)	(0.10)
Fixed effects			
Measure	✓	✓	✓
County × measure	—	✓	—
Ballot measures	31	31	31
States	7	7	7
Observations	169,701	169,701	164,740

Notes: Standard errors in parentheses are Conley spatial-HAC at 5 km with a Bartlett kernel. Columns 1 and 2 are located at each precinct's centroid; column 3 is located at the higher-exposure member's centroid. Column 2 absorbs county-by-measure fixed effects. Its slope is identified by 107,265 cells in 1,833 county–measure groups with within-group exposure variation; the observation count also includes groups with no within-group exposure variation. Column 3 contains 164,740 nonzero within-measure pair differences. Appendix Table G2 reports 10 and 25 km under both kernels for every column. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Figures

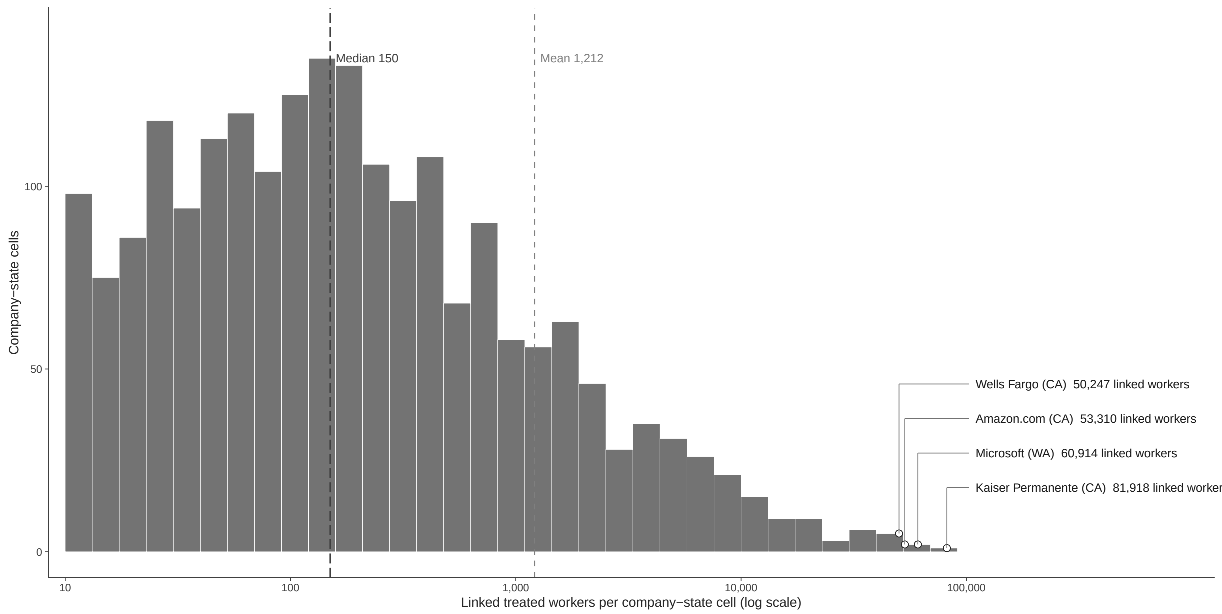

Figure 1: Distribution of linked treated workers across company–state cells.

Notes: The histogram contains 2,083 company–state cells with at least 10 distinct linked treated workers. The horizontal axis is logarithmic. Identical local and statewide cells are counted once. Vertical lines mark the median and mean; labels identify the 4 cells with more than 50,000 linked workers.

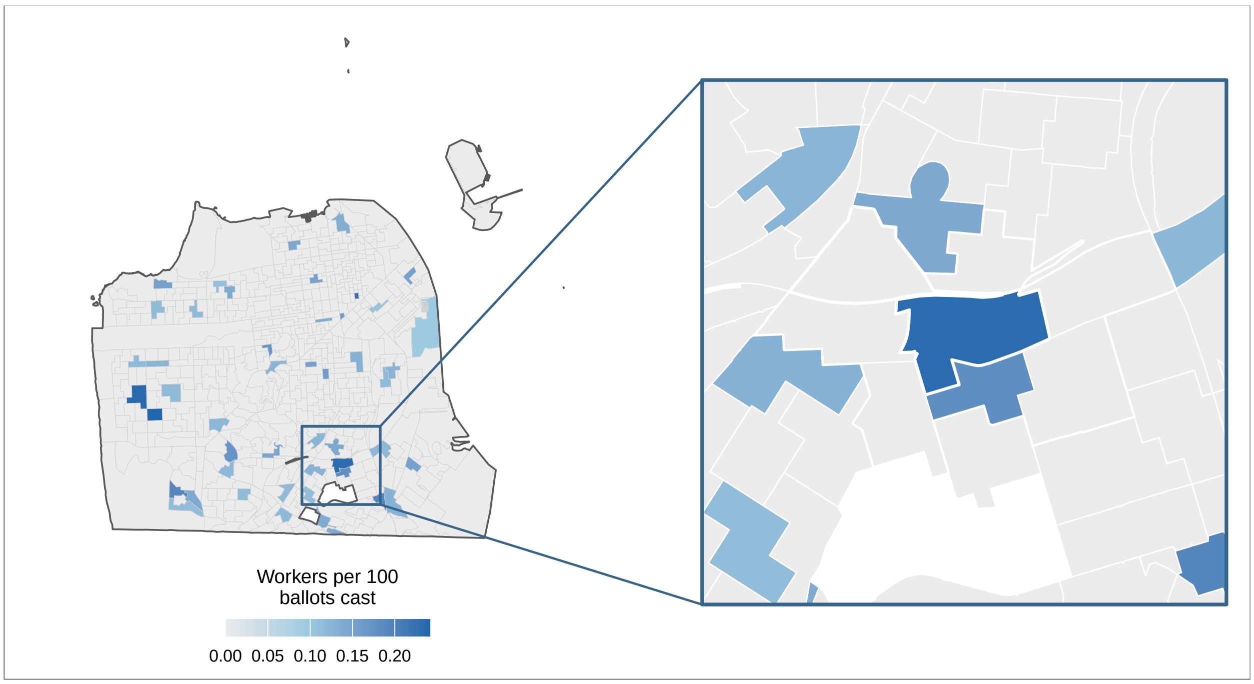

Figure 2: Illustration of the adjoining-precinct design. California's statewide Proposition 23 (2020) would have required dialysis clinics to maintain specified on-site physician coverage, report infection data, obtain state approval before closing or reducing services, and follow additional anti-discrimination rules. The left panel maps San Francisco precincts by mobilizing-firm workers per 100 official ballots cast. The precinct-cell analysis compares precincts voting on the same measure; the adjoining-precinct specification shown at right compares directly adjacent precincts, further reducing concerns about residential sorting.

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A Additional Analyses

A.1 Heterogeneity

This appendix reports supplementary measure-level analyses using the certified 344-stack sample. The specifications include voter × episode and episode × period fixed effects, use equal-stack weights, and cluster standard errors by firm and episode. Material stake and policy type are coded as described in Appendix F; election scope distinguishes statewide contests from the local-jurisdiction sample described in Appendix E. Estimates by employee age and workforce partisanship are computed on an earlier analysis stack and are not reported. The classification of firms by the measure's industry origin is reported separately in Appendix F.4, alongside the coding it derives from.

Tables A1 and A2 report the election-scope and policy-type regressions. Table A3 reports heterogeneity by material stake and Table A4 heterogeneity by job seniority, both estimated on the same certified sample. Figure A1 reports marginal treatment effects by material stake, election scope, and policy type.

Table A5 splits the sample by how the measure reached the ballot: citizen initiative or legislative referral. Procedural form is not a proxy for industry agency. A legislatively referred question can be industry-driven—New Jersey's 2020 cannabis-legalization question reached the ballot by legislative referral and carries an industry-sponsored origin code—and a citizen initiative can be bankrolled by the industry it benefits, so the split describes the route to the ballot, not who set the agenda; the origin coding in Appendix F.4 carries that question. Turnout rises 1.9 percentage points around citizen-initiated measures, and the legislatively referred estimate is 0.5 points lower with a standard error of the same size: the two are not distinguishable.

Table A1: Employee Mobilization and Voter Turnout by Election Scope

	State vs. local	Statewide only	Local only
Treated × Post	0.022***	0.022***	0.018
	(0.004)	(0.004)	(0.012)
… × Local (vs. statewide)	−0.003		
	(0.013)		
Num. Obs.	34,957,427	33,338,264	1,619,163
N treated workers	785,771	778,412	33,057
N control workers	4,708,442	4,654,250	147,409
N states	39	39	4
N ballot questions	303	264	39
N active firms	1623	1579	75
Voter × Episode FE	Yes	Yes	Yes
Episode × Period FE	Yes	Yes	Yes
Clustering		Firm + Episode	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Column 1 is the election-scope specification formerly reported in the main results table, fitted on the eligible combined statewide and local sample: its Treated × Post coefficient is the statewide estimate and the interaction is the local-minus-statewide difference. Columns 2 and 3 refit the headline specification on each scope separately, so each reports its own estimate, standard error and sample counts rather than a linear combination of Column 1's coefficients. The three are a decomposition of one sample; the headline estimate reported in the main results table pools both scopes. Coefficient estimates are followed by firm- and episode-clustered standard errors in parentheses below.

Table A2: Employee Mobilization and Voter Turnout by Policy Type

	Policy type
Treated × Post (labor/employment)	0.007
	(0.009)
… × Tax/fiscal (vs. labor/employment)	0.015
	(0.014)
… × Industry regulation (vs. labor/employment)	0.017
	(0.012)
… × Electoral/governance (vs. labor/employment)	0.006
	(0.009)
… × Social/civil rights (vs. labor/employment)	0.005
	(0.011)
… × Education (vs. labor/employment)	0.006
	(0.009)
… × Healthcare (vs. labor/employment)	0.006
	(0.010)
… × Environment/energy (vs. labor/employment)	0.043**
	(0.020)
… × Housing/land (vs. labor/employment)	0.010
	(0.012)
… × Other policy type (vs. labor/employment)	0.013
	(0.014)
Num. Obs.	34,957,427
N treated workers	785,771
N control workers	4,708,442
N states	39
N ballot questions	303
N active firms	1623
Voter × Episode FE	Yes
Episode × Period FE	Yes
Clustering	Firm + Episode

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This stacked difference-in-differences specification uses the eligible combined statewide and local sample. Coefficient estimates are followed by firm- and episode-clustered standard errors in parentheses below. Labor/employment is the omitted policy category, so the Treated × Post coefficient is its estimated turnout effect and each interaction is the difference from labor/employment. Policy-type contrasts are descriptive heterogeneity estimates. All episodes have dual-rater policy types.

Table A3: Heterogeneous Treatment Effects: Material Stake

	Pooled	+ Material stake	+ Stake (cont.)
Treated × Post	0.021***	0.013***	0.000
	(0.004)	(0.003)	(0.006)
Treated × Post × Medium stake		0.005	
		(0.007)	
Treated × Post × High stake		0.053***	
		(0.020)	
Treated × Post × Stake score (1-10)			0.006**
			(0.002)
Num. Obs.	34,957,427	34,957,427	34,957,427
N treated workers	785,771	785,771	785,771
N control workers	4,708,442	4,708,442	4,708,442
N states	39	39	39
N ballot questions	303	303	303
N active firms	1623	1623	1623
Voter × Episode FE	Yes	Yes	Yes
Episode × Period FE	Yes	Yes	Yes
Clustering		Firm + Episode	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Heterogeneous Treatment Effects: Job Seniority

	Pooled	+ Seniority	+ Seniority (cont.)
Treated × Post	0.021***	0.019***	0.014***
	(0.004)	(0.004)	(0.005)
Treated × Post × Senior (≥ 5)		−0.001	
		(0.005)	
Treated × Post × Seniority (1-7)			0.002
			(0.001)
Num. Obs.	34,957,427	33,964,523	33,964,523
N treated workers	785,771	784,600	784,600
N control workers	4,708,442	4,697,720	4,697,720
N states	39	39	39
N ballot questions	303	290	290
N active firms	1623	1607	1607
Voter × Episode FE	Yes	Yes	Yes
Episode × Period FE	Yes	Yes	Yes
Clustering		Firm + Episode	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each column adds a single interaction to the pooled stacked DiD specification. Seniority columns restrict to workers with Revelio seniority data. Column 2 splits at senior level ≥ 5 on the Revelio 1–7 scale. Column 3 uses the continuous 1–7 scale.

Table A5: Employee Mobilization and Voter Turnout by How the Measure Reached the Ballot

	Procedural origin
Treated × Post (citizen-initiated, 191 episodes)	0.019***
	(0.006)
… × Legislatively referred (96 episodes)	-0.005
	(0.006)
… × Veto referendum (15 episodes)	-0.015
	(0.013)
… × Other origin (3 episodes)	-0.016***
	(0.006)
Num. Obs.	45,624,937
N treated workers	778,412
N control workers	4,654,250
N states	39
N ballot questions	264
N active firms	1579
N episodes coded	305 of 344
Voter × Episode FE	Yes
Episode × Period FE	Yes
Clustering	Firm + Episode

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This stacked difference-in-differences specification uses the statewide portion of the eligible combined sample. Coefficient estimates are followed by firm- and episode-clustered standard errors in parentheses below. Citizen-initiated is the omitted category, so the Treated × Post coefficient is its estimated turnout effect and each interaction is the difference from it. Procedural origin is coded at the measure level from the origin corpus and joined to the stack on the ballot-measure identifier. Local episodes carry no procedural origin because they never enter that corpus and are excluded here; the 39 local episodes of the 344 are not represented. The other-origin arm rests on three episodes and its clustered standard error should not be read as reliable.

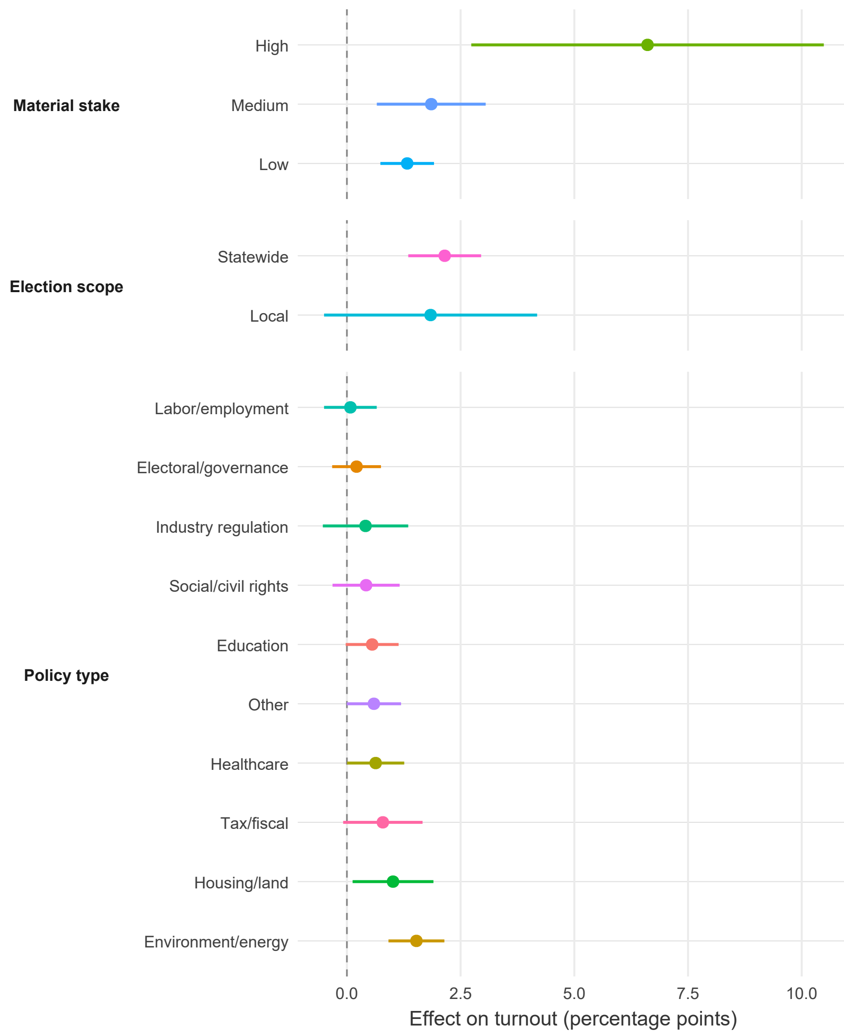

Figure A1: Marginal treatment effects. The three vertically stacked panels report effects by material stake, election scope, and policy type. Policy types remain in a single vertical list. Points are marginal effects on turnout in percentage points; bars report 95% confidence intervals.

B Robustness

This appendix collects the pooled event study, the event study by election timing, and the first-versus-subsequent-mobilization event study. It then reports alternative aggregation and the differential-registration analysis.

B.1 Pooled Event Study and Descriptive Persistence

The pooled event study supplies the principal diagnostic for parallel trends. The specification matches on age, imputed partisanship, gender, and two-digit industry, but *not* on prior turnout, so all three pre-treatment coefficients are free rather than fixed by construction and nothing in the design forces the pre-trends to be parallel. On the eligible combined stack, the t_{-3} coefficient is −0.65 pp (95% CI = [−1.49, 0.19]). Its interval spans zero, but it is negative and larger in magnitude than the treatment-period effect is small, and I treat it as a genuine limitation rather than as a passed test.

At later horizons the estimates are 0.8 pp at t_{+1} and 1.3 pp at t_{+2}. I report these coefficients and decline to interpret them, for four reasons. First, the t_{+1} confidence interval includes zero. Second, they are not robust to departures from parallel trends: under the sensitivity analysis of Rambachan and Roth (2023), the treatment-period effect retains a confidence set excluding zero while both later coefficients admit zero as soon as any trend violation is permitted, including the exactly-linear case. Third, the horizons rest on different samples—a later election in the same family must exist to observe each one—so the coefficients describe successively smaller and more selected sets of campaigns, and t_{+2} exceeds t_{+1} in every sample balanced over the relevant periods, which no account of decaying persistence accommodates. Fourth, roughly one in six treated workers has an employer that campaigns again by the next election, so these coefficients mix any persistence of the original campaign with fresh mobilization by a later one. The employment-voter linkage also uses a single recent voter-file snapshot and therefore cannot recover earlier workers who later moved, died, or were purged. I accordingly make no claim about durability in either direction, and in particular do not identify habit formation (Gerber et al. 2003; Fujiwara et al. 2016). Appendix D describes the panel construction behind this limitation.

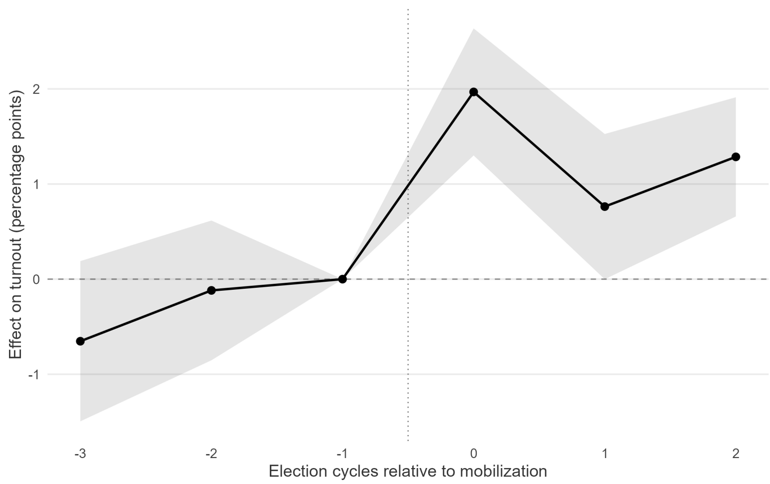

Figure B1: Pooled event study. Each point estimates the turnout difference between matched treated and control employees at the indicated election cycle relative to the treatment election ($t = 0$). The reference period is t_{-1} and is zero by construction; bars report 95% confidence intervals. The shaded band marks the pre-treatment periods, all of which are freely estimated: the specification does not match on prior turnout.

B.2 Event Study by Election Timing

Figure B2 separates the event-study estimates for presidential-year November, midterm November, off-year November, and other election dates. The treatment-period gradient matches the interaction specification in Table 5, Column 2. As with the pooled event study, the matched pre-treatment periods should not be treated as independent tests of parallel trends.

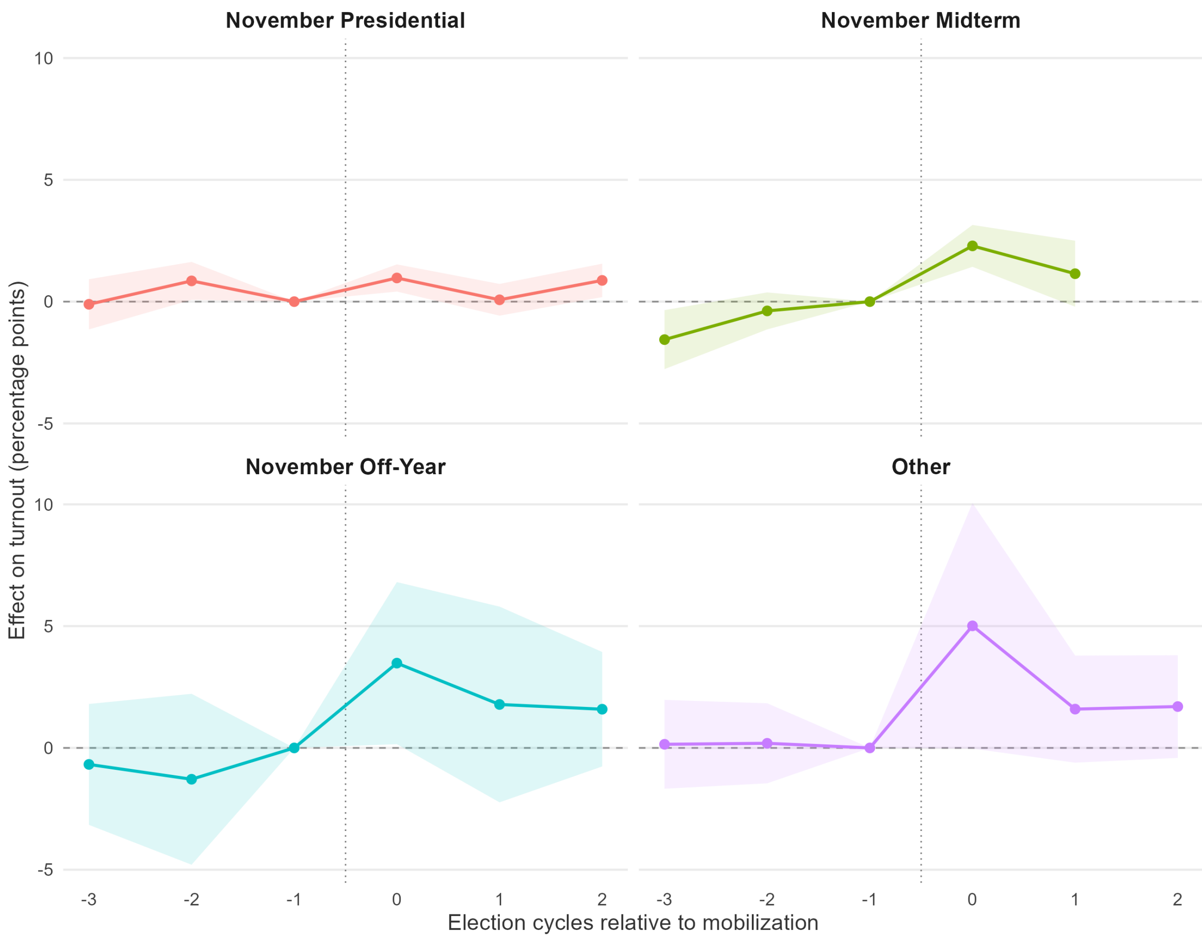

Figure B2: Event study by election timing. The panels report presidential-year November, midterm November, off-year November, and other election dates. Points are turnout effects in percentage points; bars report 95% confidence intervals. The shaded band marks the pre-treatment periods. Note the differing precision across panels: the off-year and other-date panels rest on far fewer episodes than the presidential and midterm panels.

B.3 First versus Subsequent Mobilization

The first-year restriction retains all matched controls and keeps treated workers only when the focal episode occurs in their first observed mobilization year since 2016. Figure B3 shows a positive treatment-period estimate. Its pre-treatment coefficients are not uniformly clean: t_{-2} is essentially zero (0.03 pp), but t_{-3} is -0.81 pp with an interval that excludes zero, so this subgroup inherits the same negative pre-trend as the pooled specification rather than resolving it. The pooled ATT is 2.1 pp (95% CI [1.3, 2.9]); the corresponding estimate among workers observed in a later mobilization year is 1.8 pp (95% CI [0.7, 3.0]). These estimates show that the main result survives in both groups. They should not be read as evidence that the groups differ because the estimation does not provide a covariance-adjusted contrast.

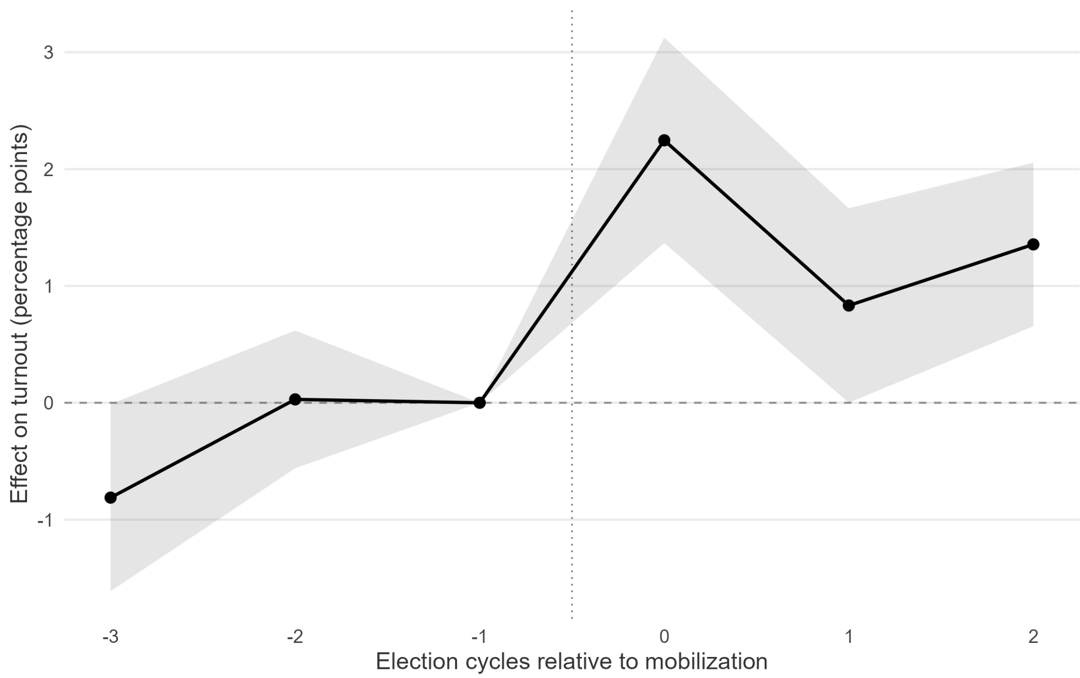

Figure B3: First-year-mobilization event study. The sample retains all matched controls and treated workers whose focal episode occurs in their first observed mobilization year since 2016. Each point estimates the differential turnout between treated and matched control workers at the indicated election cycle relative to the focal election ($t = 0$). The reference period is $t = -1$ and is zero by construction; bars report 95% confidence intervals, and the shaded band marks the pre-treatment periods. "First year" is defined at the worker level, regardless of employer; every episode in that year receives the first-year classification. Mobilization before 2016 is outside the contribution panel and therefore unobserved.

B.4 Alternative Aggregation: Episode-Level Meta-Analysis

The headline analysis stacks the matched episodes in a single regression. As an alternative, I treat each eligible statewide episode as a separate study and pool its matched difference-in-differences estimate by random-effects meta-analysis. Each episode receives a weight equal to the inverse of its total variance,

$$w_i = \frac{1}{\hat{\sigma}_i^2 + \hat{\tau}^2},$$

where $\hat{\sigma}_i^2$ is the sampling variance of episode i and $\hat{\tau}^2$ is the estimated between-episode variance, obtained by restricted maximum likelihood.

Across the 344 episodes of the estimation sample, this procedure yields a pooled effect of 1.26 pp (95% CI [0.94, 1.57]). Table B1 reports that estimate, its statewide and local components, and the corresponding estimates by election family. The statewide component is 1.24 pp (95% CI [0.94, 1.55]) across 305 episodes; the local component is estimated on 39 episodes and its interval spans zero. Between-episode heterogeneity is substantial: $\hat{\tau}^2 = 4.94 \times 10^{-4}$ and $I^2 = 89.1\%$. The REML standard errors treat episode estimates as independent and therefore do not adjust for dependence created when episodes share firms, workers, controls, or ballot measures.

Table B1: Random-effects meta-analysis of episode-level turnout effects

Sample	Episodes	ATT (pp)	SE (pp)	95% CI	I^2 (%)
All episodes (statewide and local)	344	1.26	0.16	[0.94, 1.57]	89.1
Statewide ballot measures	305	1.24	0.15	[0.94, 1.55]	87.1
Local and municipal measures	39	1.45	1.12	[−0.74, 3.64]	97.2
Presidential-cycle November	169	0.63	0.15	[0.33, 0.92]	75.3
Midterm November	110	1.81	0.28	[1.25, 2.37]	86.4
Odd-year November	41	1.63	0.72	[0.23, 3.04]	91.5
Primary	19	1.94	0.87	[0.23, 3.66]	96.6
Other election dates	5	12.09	6.10	[0.13, 24.05]	96.4

Notes: Entries pool episode-level matched difference-in-differences estimates using restricted maximum likelihood, as an aggregation check on the stacked estimator reported in the main results. The analysis covers the 344 episodes of the estimation sample. The two indented rows partition that sample by scope, and the election-family rows partition it again; each set sums to 344. Standard errors and confidence intervals incorporate estimated between-episode variance. I^2 reports the share of total variation attributable to between-episode heterogeneity. The REML covariance calculation treats episode estimates as independent; it does not adjust for dependence created when episodes share firms, workers, controls, or ballot measures.

Weighting. The meta-analysis and the headline stacked specification are estimated on the same 344 episodes, so they differ in aggregation and inference rather than in sample. Inverse-variance weighting gives greater weight to precisely estimated episodes, which are not the same episodes as the largest ones: a treated-headcount-weighted mean of the episode estimates is 0.61 pp. I treat that number as descriptive, since it accounts for neither cross-episode covariance nor sampling variance and supplies no standard error.

B.5 Differential Registration Bias

Because the analysis conditions on voter registration—only individuals who appear in the L2 voter file enter the estimation sample—the treatment effect estimates could be affected by differential registration bias if employer mobilization also shifts the probability of registration (Nyhan et al. 2017). If mobilization induces some employees to register who otherwise would not have, the treated registrant pool differs compositionally from the control registrant pool in ways that matching on observables cannot fully address, because the marginal registrant's control-side counterpart is unregistered and therefore absent from the data entirely.

Two features of the research design limit the scope of this concern. First, the sample is restricted to employed adults with verified employment records, a population with baseline registration rates well above 80%. The share of employees who would register *because of* employer mobilization but not otherwise is small relative to the already-registered employed population. Second, the matching procedure includes exact matching on prior turnout history, which distinguishes voters who were registered and voted in earlier elections from those who were absent. Within the "absent/absent" stratum—where any registration-induced bias would concentrate—treated individuals who registered in response to mobilization are matched to control individuals who also lacked prior registration but registered for other reasons. If anything, these control-side recent registrants are positively selected on political motivation, biasing the within-stratum ATT toward zero.

More broadly, the direction of bias is attenuating: mobilization-induced registrants are by definition low-engagement individuals who would not have registered absent the employer's push. If they register and vote, the full mobilization effect (including the extensive-margin registration channel) exceeds the intensive-margin turnout effect estimated here. If they register but do not vote, they dilute the treated registrant pool with non-voters. Either way, the estimates are conservative with respect to the total mobilization effect.

A direct test These arguments imply something checkable. If the estimate depends on mobilization-induced registrants, removing them should move it. I refit the main specification after dropping every voter whose recorded registration date falls after the episode's preceding election, leaving only voters who were already on the rolls one election earlier. This is a restriction on which observations enter the fit, not a rematch: the matching procedure is unchanged and the matched pairs are identical, so the two estimates differ only in their estimation samples. Every episode contributes to both.

The restriction removes 12.7% of estimation rows and 102,731 treated voters, and the estimate does not move (Table B2). The restricted sample gives 2.10 pp (95% CI [1.31, 2.88]) against 2.10 pp in the full sample, a difference of 0.002 pp. Dropping roughly one treated voter in eight leaves the coefficient and its standard error intact to two decimal places.

More informative than the stability of the point estimate is where the restriction falls. Differential registration requires treated voters to be disproportionately recent registrants, so the filter should bite harder on the treated arm. It bites almost equally: 87.1% of treated rows survive against 87.4% of control rows. The small gap runs in the direction the mechanism predicts, but at a magnitude far too small to generate the estimated effect.

Two limits bound what this establishes. First, L2 supplies one registration date per voter, taken from the latest available snapshot, so a voter who re-registers after moving carries the later date and is dropped despite a long registration history. The restriction therefore discards voters it should keep. That makes the exercise conservative rather than clean: a surviving effect bounds the true one from below, and an attenuated effect would not have shown the headline to be a registration artifact. The asymmetry holds whichever way the number falls. Second, the exercise concerns entry into the sample, not exit from it. Voters removed from the rolls, or who drop out of the linked employment panel, are not recovered by conditioning on earlier registration; if anything the restriction conditions more heavily on survival, since it retains only voters observable at an earlier date. Differential attrition is a separate concern from differential registration, and this test does not speak to it.

Table B2: Direct Test for Differential Registration: Voters Registered Before the Preceding Election

	Full sample	Registered before preceding election
Treated × Post	0.021***	0.021***
	(0.004)	(0.004)
Num. Obs.	33,316,766	33,251,112
Estimation rows	40,805,527	35,635,390
N treated workers	785,771	683,040
N episodes	344	344
Voter × Episode FE	Yes	Yes
Episode × Period FE	Yes	Yes
Clustering	Firm + Episode	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Column 2 refits the headline specification after dropping every voter whose recorded registration date falls after the episode's preceding election; matching is unchanged, so the columns differ only in estimation sample. The restriction retains 87.1% of treated rows and 87.4% of control rows. The voter file supplies a single latest-snapshot registration date per voter, so re-registration resets the date and long-registered voters are dropped; the restriction therefore bites harder than intended and the surviving estimate is a lower bound on the always-registered effect. Coefficient estimates are followed by firm- and episode-clustered standard errors in parentheses below.

B.6 Leave-One-State-Out

The estimation sample is unevenly distributed across states: California, Washington and Colorado together supply 146 of the 344 episodes, so a pooled estimate could in principle rest on one state's campaigns. Table B3 refits the headline specification 39 times, each time dropping every episode in one state, with local episodes dropped alongside their geographic state.

The estimate ranges from 1.70 to 2.34 pp against 2.10 pp in the full sample, and every interval excludes zero. Dropping the three largest contributors moves the estimate up rather than down—to 2.29 pp without California, 2.34 pp without Washington and 2.28 pp without Colorado—so the pooled result is not carried by the states that supply the most episodes. The largest downward movement comes from Maine (1.70 pp), which contributes 13 episodes; the interval there still excludes zero.

Two limits attach. The refits are not independent of one another and share the same matched control pool, so the spread across rows is not a standard error and no test is performed on it. And dropping a state removes its episodes, not its influence on the matching: control workers resident elsewhere are unaffected, so this varies the estimation sample rather than rebuilding the design.

Table B3: Leave-One-State-Out Refits of the Headline Specification

State	Drop	Est.	(SE)	State	Drop	Est.	(SE)	State	Drop	Est.	(SE)
AK	5	2.14	(0.41)	LA	4	2.10	(0.40)	NM	1	2.11	(0.40)
AL	2	2.09	(0.40)	MA	13	2.13	(0.41)	NV	11	2.03	(0.41)
AR	6	2.10	(0.41)	MD	2	2.09	(0.40)	NY	4	2.12	(0.40)
AZ	12	2.12	(0.41)	ME	13	1.70	(0.30)	OH	9	2.07	(0.41)
CA	60	2.29	(0.46)	MI	4	2.14	(0.41)	OK	11	2.10	(0.41)
CO	41	2.28	(0.43)	MN	1	2.10	(0.40)	OR	17	1.94	(0.41)
CT	2	2.10	(0.40)	MO	10	2.04	(0.40)	RI	1	2.11	(0.40)
FL	14	2.13	(0.41)	MS	1	2.10	(0.40)	SD	11	2.09	(0.42)
GA	4	2.12	(0.40)	MT	4	2.11	(0.41)	TX	6	1.95	(0.38)
HI	1	2.09	(0.40)	NC	1	2.09	(0.40)	UT	4	2.11	(0.40)
ID	3	2.10	(0.41)	ND	3	2.10	(0.41)	VA	1	2.10	(0.40)
IL	3	2.11	(0.40)	NE	11	2.20	(0.40)	WA	45	2.34	(0.46)
KY	1	2.09	(0.40)	NJ	1	2.10	(0.40)	WI	1	2.11	(0.40)

Notes: Each row refits the headline specification after dropping every episode in the named state; *Drop* is the number of episodes removed and estimates are in percentage points, with firm- and episode-clustered standard errors in parentheses. Local episodes are dropped with their geographic state. The full-sample estimate is 2.10 pp (SE 0.40); across the 39 refits it ranges from 1.70 to 2.34 pp and every interval excludes zero. No single state's episodes account for the pooled result: dropping the three largest contributors raises the estimate rather than lowering it.

C Case and Company Identification

This appendix describes how I identified the ballot question campaigns and contributing companies that comprise the sample, and how I linked those contributors to employer records in the employment-voter panel.

C.1 Campaign Universe and Case Selection

During a typical election cycle, over 100 state-level ballot questions appear across 30 or more states. I consider all state-level ballot questions from 2016 through 2024, supplemented by selected municipal ballot questions in cities where employment data coverage is available. Using campaign finance data from the National Institute on Money in Politics (now OpenSecrets), I identify ballot question committees that received corporate contributions (OpenSecrets 2026). From the resulting set, I select campaigns where one or more specific companies contributed to a ballot question committee.

C.2 Contributor-to-Employer Matching

A central challenge is linking the names of ballot measure contributors—as they appear in campaign finance records—to the corresponding employer entities in the Revelio Labs employment database (Revelio Labs 2024). Contributor names in campaign finance filings are not standardized: the same company may appear as “BLUE CROSS BLUE SHIELD OF MASSACHUSETTS,” “BLUE CROSS & BLUE SHIELD OF MA,” or “BCBS MASSACHUSETTS.” Meanwhile, Revelio’s employment records use corporate legal names that may differ from the contributor’s filing name (e.g., “Health Care Service Corporation” for Blue Cross Blue Shield of Illinois). The matching procedure must handle these discrepancies systematically.

Starting universe The campaign finance data contains 11,111 unique contributor names across all ballot measures in the sample states and years. Most of these are not employers—the universe includes unions, political action committees, advocacy organizations, political parties, religious organizations, and government entities alongside corporate contributors.

C.2.1 Entity Classification

I first classify each contributor into one of ten entity types using keyword pattern matching on the contributor name. The classifier checks the uppercased name against an ordered list of substring patterns:

- **Union**: AFL-CIO, SEIU, IBEW, Teamsters, Steelworkers, and 40+ additional union identifiers including “WORKERS LOCAL,” “TRADE COUNCIL,” and “BROTHERHOOD OF.”
- **PAC**: “POLITICAL ACTION,” “BALLOT MEASURE COMMITTEE,” “INDEPENDENT EXPENDITURE.”
- **Campaign committee**: “YES ON,” “NO ON,” “COMMITTEE TO ELECT,” “CITIZENS FOR,” “FRIENDS OF,” “CAMPAIGN.”
- **Advocacy**: “PLANNED PARENTHOOD,” “SIERRA CLUB,” “ACLU,” “CONSERVATION,” “CONSERVANCY,” “COALITION,” and 20+ advocacy organization identifiers.
- **Party**: “DEMOCRATIC PARTY,” “REPUBLICAN PARTY,” and state/national party variants.

- **Association**: "CHAMBER OF COMMERCE," "ASSOCIATION OF REALTORS," "FARM BUREAU," "ASSOCIATION," "FOUNDATION."
- **Government**: "CITY OF," "COUNTY OF," "SCHOOL DISTRICT," "DEPARTMENT OF."
- **Religious**: "DIOCESE," "CHURCH OF," "MINISTRY."

The first matching category wins, and pattern order is chosen to resolve ambiguities (e.g., "FRATERNAL ORDER OF POLICE" matches as union before it could match as association). Contributors matching none of these patterns are classified as corporations. This keyword classification identifies 7,156 entities as potential corporate contributors.

Manual curation The keyword classifier is supplemented by a hand-curated exclusion list of 1,389 entities confirmed through manual review to be non-employers despite passing the keyword filter (e.g., "FWD.US," a political advocacy organization; "Represent.Us," a governance reform nonprofit). Most of that list names entities the keyword classifier had already placed outside the corporate pool; 1,036 of the entries remove an entity the classifier had called a corporation. A smaller set of manual overrides preserves entities that contain exclusion keywords but are genuine employers—for example, "Tillamook County Creamery Association" (a dairy cooperative with 77 employees) and "Kaiser Foundation Health Plan" (the operating entity of Kaiser Permanente).

After classification and exclusions, 6,120 corporate entities remain for matching.

C.2.2 Name Normalization

Before matching, I normalize both contributor names and employer names using the following rules, applied in order:

1. Convert to uppercase
2. Strip corporate suffixes: Inc, LLC, Corp, Corporation, Company, Co, Ltd, Limited, LP, LLP, PLC, Holdings, Group, International, Enterprises, Industries, The
3. Replace punctuation (except & and whitespace) with spaces
4. Collapse multiple spaces to a single space

This normalization maps "Procter & Gamble Co." and "PROCTER & GAMBLE COMPANY" to the same key ("PROCTER & GAMBLE"), enabling exact matching across stylistic differences.

C.2.3 Employer Lookup Table

The target for matching is a lookup table of employer names and employee counts by state, extracted from the Revelio employment records. For each of the 52 state partitions in the employment data, I extract all unique company names, their Revelio company identifiers (RCIDs), employee counts (defined as the number of linked voters with active employment at that company), and primary NAICS industry classification. The extraction uses DuckDB for memory-efficient per-state queries over the parquet-formatted employment data. The resulting lookup contains approximately 20 million (company, state) entries.

C.2.4 Four-Tier Matching Cascade

Each contributor entity is matched against the employer lookup using a four-tier cascade. The tiers are applied in order; the first match found is accepted.

Tier 1: Manual aliases A hand-curated mapping of 321 contributor names to their corresponding employer names. These handle cases where automated matching is impossible: corporate renamings ("Fiat Chrysler" → "Stellantis"), DBA names ("Amerco" → "U-Haul"), abbreviations ("BNSF" → "BNSF Railway"), and subsidiary-to-parent mappings ("21st Century Fox America" → "Fox Corp."). The alias file was built iteratively across five review rounds, starting from known difficult cases and expanding as automated tiers revealed gaps.

Tier 2: Exact normalized match The contributor's normalized name is compared to all employer names in the lookup for each state where the contributor is active. If the normalized strings are identical, the match is accepted. This tier captures the majority of matches—companies whose filing name, after suffix stripping, is the same as their employer record name.

Tier 3: Fuzzy matching For short names ($\leq$ 2 tokens after normalization), I apply *containment matching*: the contributor name is accepted as a match if it appears as a substring of an employer name with at least 10 employees. For example, "BASS PRO" matches "BASS PRO SHOPS OUTDOOR WORLD." The employee threshold prevents false positives from small entities with coincidentally similar names.

For longer names ($\geq$ 3 tokens), I use token-level Jaccard similarity:

$$J(A, B) = \frac{|A \cap B|}{|A \cup B|}$$

where A and B are the token sets of the contributor and employer names, respectively. To account for the discrete nature of Jaccard scores at different token counts, I apply graduated thresholds: $J \geq 0.67$ for 3-token names, $J \geq 0.60$ for 4+ token names. I use an inverted token index to efficiently identify candidate pairs sharing at least one token, avoiding exhaustive pairwise comparison.

Tier 4: Semantic similarity Remaining unmatched entities are embedded using OpenAI's `text-embedding-3-small` model and matched via approximate nearest neighbor search using FAISS (Johnson et al. 2021). The employer lookup names are embedded and indexed using FAISS's inner-product index on L2-normalized vectors, which computes cosine similarity. A match is accepted if the cosine similarity exceeds 0.85. This tier captures semantic matches that token-level methods miss—for example, "Progressive Casualty Insurance" matching "Progressive Corp." Queries are restricted to states where the contributor is active to prevent cross-state false matches.

C.2.5 Post-Match Filtering

Three filters are applied after matching:

1. **Match blacklist.** A set of 248 known false-positive (contributor, employer) pairs is removed. These arise when an employer name is superficially similar to the contributor but refers to a different entity (e.g., "WSI US LLC," a staffing company, matched to "Wynn Resorts"; "Horizon Pharma PLC," an Irish biopharmaceutical company, matched to "Horizon Pharmacies, Inc.," a Texas pharmacy chain). The blacklist was compiled through iterative manual review across five matching rounds and a comprehensive LLM-assisted validation of all matches (described below).

2. **Industry filter.** Matches where the employer's NAICS classification indicates a non-employer entity are removed. The filter excludes all six-digit NAICS codes from 813000 through 813999 (NAICS 813xxx).

3. **Employer size threshold.** An employer enters the registry only where the employment data record at least 10 employees for it, assessed on the firm in the state its coverage row was computed in. A separate and larger threshold applies later, at the episode level rather than the firm level: an episode is estimated only if at least 50 employees of the mobilizing firms appear on that state's voter file at that election, pooled across every firm on that side of the measure (Appendix E). No filter at any stage removes an individual employer for having fewer than 50 linked voters.

C.2.6 Iterative Review

The matching pipeline was run five times, with manual review of all matches and high-contribution unmatched entities between rounds. Each review round produced three types of feedback: (a) new manual aliases for entities that should have matched, (b) additions to the entity exclusion list for contributors confirmed as non-employers, and (c) additions to the match blacklist for false-positive matches. The fourth round included a systematic investigation of all 1,743 unmatched entities with contributions of $10,000 or more, using the employer lookup to identify candidate matches for manual verification.

C.2.7 LLM-Assisted Match Validation

After the iterative review, I conduct a comprehensive validation of all matched pairs using a large language model (Claude Opus 4.6, Anthropic). Each of the 2,480 matched contributor–employer pairs is evaluated for two properties: (a) whether the matched employer name plausibly refers to the same entity as the contributor name, considering subsidiaries, DBAs, and corporate renamings; and (b) the three most likely six-digit NAICS 2022 industry codes for the contributor company. The validation is performed by 12 parallel classification agents, each reviewing approximately 200 matches.

This validation identifies 157 false-positive matches (6.3% of the crosswalk), concentrated among Tier 3 fuzzy matches (39% invalid rate) rather than Tier 2 exact matches (1.9%). The invalid matches account for $25.3 million in contribution volume (1.0% of total). All 157 are added to the match blacklist.

The NAICS review reveals that Revelio's industry classifications are unreliable: in a random 50-row spot-check, 34% of Revelio-assigned NAICS codes were incorrect at the six-digit level. Common errors include holding-company codes assigned to operating subsidiaries (e.g., a commercial bank coded as "Corporate Managing Offices"), cannabis companies assigned to pharmaceutical manufacturing (no cannabis-specific NAICS code exists), tribal casino operations classified as amusement parks, and private law firms coded under government legal services. Because the analysis uses industry fixed effects, I replace Revelio's NAICS codes with the LLM's top classification for all companies in the crosswalk, applying manual overrides for known problem categories: cannabis companies are assigned 111998 ("All Other Miscellaneous Crop Farming"), tribal casino operations 713210 ("Casinos"), and tribal governments 921150 ("American Indian and Alaska Native Tribal Governments"). The LLM-assigned codes are mapped to the official Census Bureau NAICS 2022 classification.

C.3 Coverage Validation

For each matched company, I validate the match by counting the number of linked voters with active employment at the matched employer in the relevant state. Employee counts use direct employment at the matched entity, not parent-consolidated headcounts, to avoid inflating treatment groups with employees at sibling subsidiaries. This serves both as a quality check on match accuracy and as the basis for treatment group counts in the analysis.

C.4 Matching Results

The final matching yields 2,819 accepted contributor–employer pairs covering 2,815 unique contributors, from 6,120 candidate corporate entities. A contributor can match more than one employer record, so the pair count exceeds the contributor count. Appendix E reports 2,811 matched contributors rather than 2,815: that funnel is restricted to donors appearing in the contribution records for the sample states and years, which excludes four contributors carrying a match but no contribution row in that window. Table C1 reports the number of matches by tier.

Table C1: Matching Results by Tier

Tier	Method	Matches
1	Manual aliases	249
2	Exact normalized	2,191
3	Fuzzy (Jaccard + containment)	323
4	Embedding similarity (FAISS, cosine ≥ 0.85)	56
	Total	**2,819**

The unmatched entities are predominantly small local businesses below the employment data provider's coverage threshold, entities with no U.S. workforce presence in the contributor's state, or misclassified non-corporate entities that passed the keyword filter.

D Panel Construction

This appendix describes the data infrastructure underlying the analysis. The production backbone is a person-year pipeline that builds state-level November panels and enriches them with treatment metadata, partisanship measures, tenure, and spillover flags. For a subset of states where multiple relevant ballot dates fall in the same calendar year, a supplementary election-level pipeline resolves within-year timing ambiguity by constructing panels at the voter $\times$ election level.

D.1 Data Sources

Two primary datasets are linked to create the analysis panels.

Voter files Voter file data is publicly available from state and/or county officials but is difficult to collect. Like most scholars, I thus rely upon a commercial data vendor, L2 Data. L2 data are widely used in fields including management (Kagan et al. 2026; Frake et al. 2026; Raffiee et al. 2022) as well as other social sciences (Brown and Enos 2021; Chinoy and Koenen 2024). The L2 voter file contains administrative records for approximately 180 million registered voters across all 50 states and Washington, D.C. For each voter, the file includes a complete turnout history across all election types (general, primary, municipal), current and historical residential addresses, party registration where available, and demographic information (age, gender, and race/ethnicity).

Employment records Revelio Labs aggregates employment information from online professional profiles, covering over 100 million U.S. employment records. Each record includes the employer name, job title, start and end dates, seniority level, salary band, and NAICS industry classification. Revelio resolves employer names to a canonical company identifier and maps subsidiary relationships to ultimate parent companies.

Record linkage The two datasets share no common unique identifier. The Politics at Work project links them using a multi-stage approach. First, both datasets are partitioned by metropolitan statistical area (MSA) to reduce the scale of required comparisons while preserving plausible matches. Second, the project applies probabilistic record linkage following Enamorado et al. (2019), which weights multiple matching fields—name, gender, approximate age—by their discriminatory power. Third, it supplements these with machine learning approaches using large language model embeddings (Ornstein 2025) to capture name variations that string distance measures miss (e.g., transliterations, hyphenated surnames, nicknames). The resulting linked dataset covers approximately 40 million Americans with both employment histories and voter turnout records. More complete technical details of this merge, including replication code, are described in Frake et al. (2026) and Kagan et al. (2026).

D.2 Production Person-Year Pipeline

D.2.1 Step 1–2: Preprocessing

The linked employment-voter records are partitioned by state using the state code embedded in L2's voter identifier (LALVOTERID). Separately, the raw L2 voter files are standardized: column names are harmonized across states, and election history columns are extracted for general, consolidated general, and local/municipal elections. Processed voter files are stored as state-level parquet files for efficient downstream access.

D.2.2 Step 3: Base November Panels

For each state, I construct a person-year panel covering November general elections from 2012 through 2024. The panel has one row per registered voter per election year.

Employment status is determined by creating snapshots as of November 1 of each year. A worker is considered employed at a given company if their employment record shows a start date before November 1 and no end date (or an end date after November 1). When a worker holds multiple active positions in the same snapshot, I retain a single primary job using a deterministic hierarchy: highest `pos_seniority` (Revelio's 1–7 measure of job seniority, derived from job title and other fields), then highest `pos_salary` (Revelio's estimated salary based upon geography, job titles, and other fields), then earliest `pos_startdate`, then lowest `pos_pos_number`. This rule is used for the production person-year panels so that each voter-year has at most one attached job record.

Turnout is derived from L2's election history columns. A voter is coded as eligible if their registration date is on or before the election date and they have not been removed from the rolls. Among eligible voters, turnout is coded as 1 if the election history column indicates participation and 0 otherwise. I do not record voters as failing to turnout (i.e., a "0" value) for elections which occurred prior to their recorded registration date.

D.2.3 Treatment Assignment

Treatment is assigned by a flagging stage that reads the base panels of Step 3 directly, together with the firm–ballot-measure crosswalk, and writes a treatment-flagged panel for every state carrying an estimated episode. For each voter-year it records whether the voter was employed at a firm that contributed to a ballot-measure campaign in that state at that election, which measures that firm contributed to, and on which side. Firms are carried as company identifiers rather than as names, and each treated row stores parallel measure-identifier and stance arrays, so that a voter employed by a firm active on several measures is resolved per measure rather than collapsed to a single flag. Two-digit NAICS codes used as an exact matching stratum are constructed at this stage from a NAICS crosswalk rather than inherited from the base panel.

Treatment is evaluated *contemporaneously*: a voter is treated at a given election only if the employment snapshot for that election places them at a mobilizing firm, so workers who joined or left afterwards are not retroactively treated.

D.2.4 Enrichment: Partisanship Scores and Tenure

A separate enrichment chain adds company identifiers, voter-level treatment-group indicators, company partisanship scores, and employee tenure to the base panels. Company-level partisanship scores (VRscores) are merged from Revelio's voter registration–based measures (Kagan et al. 2026). VRscores capture the partisan composition of each company's workforce in a given year, ranging from 0 (entirely Democratic-registered) to 1 (entirely Republican-registered). Scores are loaded only for treatment years to avoid anachronistic measurement. Employee tenure is calculated as the duration of employment (in years) at the affected company as of the election date. I also merge same-MSA employer ideology from an external file by matching employer identifier, metropolitan statistical area, and year. The key Republican-share measure in that merge is `pct_rep_imp`, which captures the imputed two-party Republican share among workers in the same employer-by-MSA cell.

D.2.5 Local Election Panels

The Colorado and Washington local episodes are jurisdiction-restricted catalogs run against the ordinary statewide Colorado and Washington panels, subset to include only jurisdictions with local elections in my sample.

For the El Paso and Kansas City cases, a parallel pipeline constructs panels in person-election format (rather than person-year), with employment snapshots taken as of each local election date. This pipeline follows its own company-identifier and treatment-group steps, adapted for the municipal electoral calendar and geography-specific company lists.

D.3 Election-Level Pipeline for Multidate States

The person-year pipeline is sufficient for most states in the sample because the relevant ballot campaigns map cleanly to one November general-election observation per year. A subset of six states (AZ, CA, ME, MO, OH, OK), however, contains multiple relevant ballot dates in the same calendar year—for example, a May primary ballot measure and a November general-election measure in the same year. For these states, a year-level treatment indicator is too coarse: a voter can be untreated in one election and treated in another within the same calendar year.

The election-level pipeline addresses this by constructing panels at the voter × election level rather than voter × year. For each election family represented in the ballot-measure sample—even-year November general, odd-year November, primary, and residual other election types—the pipeline retains all available same-family elections back to approximately 2010, so that each treated election is anchored by multiple untreated periods for matching and trend assessment. Employment snapshots are taken as of each exact election date rather than November 1, and treatment is assigned using exact election dates and election families rather than `panel_year`.

The election-level pipeline produces treatment timing and matched estimation inputs for the affected states. The estimation code handles both panel grains through an `election_key` abstraction that maps to `election_id` for multidate states and `panel_year` for year-grain states, with sequence-based lag lookups replacing year arithmetic.

E Sample Attrition

Two exhibits report attrition at different units. Table E1 counts episode tasks in the tracked estimation manifests; Table E2 counts firms.

E.1 Episodes

The raw statewide manifest contains 409 tasks: 310 estimated and 99 skipped. Certification removes four raw-only Arkansas tasks: three formerly marked estimated and one formerly marked skipped. The certified statewide manifest therefore contains 405 tasks: 307 estimated and 98 skipped. Removing the 2 tasks belonging to the 2021 California gubernatorial recall, which is a vote on retaining an officeholder rather than a ballot measure, leaves the 305 statewide episodes the analysis estimates. The local manifest is unchanged at 56 tasks: 39 estimated and 17 skipped. The manifests do not record why tasks were skipped.

Table E1: Sample Attrition: From Candidate Episodes to Estimated Episodes

Stage	Episodes	Change	Reason for change
Panel A. Statewide ballot measures			
Frozen candidate universe	417		All statewide measure–stance episodes surviving the classification and crosswalk freeze.
Runtime exclusions		−8	No in-scope treated workers: no contributing firm on that side had linked employees resident in the state at the time of the election.
Episodes sent to estimation	409		
Fewer than 50 linked treated workers (pre-matching floor)		−98	Fewer than 50 employees of the mobilizing firms appear on that state's voter file at that election, counting across all firms on that side of the measure.
Never held		−4	Two Arkansas measures did not qualify for the ballot, removing 4 measure–stance episodes, of which 3 had previously been estimated.
Recall election		−2	The 2021 California gubernatorial recall is a vote on retaining an officeholder rather than a ballot measure, and is out of scope by categorical rule; both stances are removed.
Estimated (analysis sample)	**305**		Episodes contributing to the pooled stacked difference-in-differences estimate.
Panel B. Local and municipal ballot measures			
Candidate local episodes	120		Colorado 55, Washington 63, El Paso TX 1, Kansas City MO 1.
Not restrictable to a jurisdiction		−21	18 Colorado measures from 2016–17, for which no historical voter-file vintage exists that can be linked to individual panel members; 2 Colorado special districts whose membership the voter file does not record well enough to determine eligibility; 1 Washington committee that operated statewide with no single jurisdiction.
Eligible for jurisdiction restriction	99		
Below the treated-worker floor		−43	Fewer than 20 treated workers once both treated and control voters were restricted to residents of the measure's own jurisdiction at the time of that election.
Surviving the restriction	56		54 Colorado and Washington episodes, plus El Paso and Kansas City, which were already restricted to a single jurisdiction.
Fewer than 50 linked treated workers (pre-matching floor)		−17	The common estimator screen requires at least 50 treated voters after the jurisdiction restriction and before matching.
Estimated (analysis sample)	**39**		Episodes contributing to the local pooled stacked estimate: Washington 26, Colorado 11, El Paso and Kansas City 1 each.

Notes: An episode is a ballot-measure × stance cohort in a single election; its treated group is every linked employee of every firm that contributed on that side of that measure. "Estimated" counts episodes that produced a matched sample and an estimate; earlier rows are candidate universes and are not the analysis sample. The two chains are separate eligibility funnels, not separate estimates: each is screened on its own criteria, and the 344 episodes surviving both are then pooled into the combined stacked model. The local chain has an additional preliminary catalog screen of 20 treated voters after the jurisdiction restriction. Episodes that pass that screen still face the same 50-treated-voter pre-matching floor as statewide episodes. Table E2 reports the companion funnel counting firms rather than episodes.

On the 98 statewide episodes that did not reach estimation. All 98 failed the same single screen, recorded under one reason code; none was lost to matching failure, to insufficient turnout history, or to any other cause. They are spread across 29 states, the largest contributions being 10, 9 and 9 episodes, so the drop is not concentrated in one state's data. The screen is evaluated before matching is attempted and reads only the number of treated workers: it never inspects turnout in any period, the availability of controls, or the quality of the match, and so cannot select episodes on the size or sign of the effect. What it does imply is a scope condition. The estimand is the effect of employer mobilization on ballot measures where the mobilizing firms together had at least 50 employees on the state's voter file at that election. That threshold is correlated with firm size and with state size, so the estimated sample tilts toward larger employers and more populous states relative to the candidate universe.

E.2 Firms

Table E2 follows the same path from the firm side, for statewide measures only: the local collection pipeline has no estimated terminus yet, so a local firm chain would end in a blank. Of the 11,111 non-individual donors to statewide ballot-measure committees between 2016–2024, 7,162 are operating companies rather than unions, trade associations, advocacy groups, party organizations or campaign committees. That count is the 7,156 entities the classifier of Appendix C labels corporations, together with six donors it labels otherwise that nonetheless carry an employer record; including them here keeps every stage of this funnel a subset of the stage above it. Of those, 2,811 can be matched to an employer in the employment data, and 2,051 clear the linkage screens; dropping advocacy, religious and civic organizations leaves the candidate universe of 1,985 firms. The two largest losses are upstream and are properties of the data rather than of the design: most ballot-measure donors are not companies, and most of the companies that remain are too small or too obscurely named to be matched to an employer record with confidence.

The candidate universe of 1,985 firms corresponds to 3,065 employer identifiers, of which 1,579 carry at least one treated worker into the pooled regression. The gap between those two numbers, and not the gap between 1,985 and 1,579, is the relevant attrition: a single company routinely appears in the employment data under several employer identifiers, so the firm count and the employer-identifier count are different units and are reported separately rather than compared.

Table E2: Firm Eligibility Funnel: From Campaign-Finance Donors to Estimated Employers

Stage	Firms	Employer IDs	Ballot measures	Elections	States
Donors to ballot-measure committees	11,111		522	163	45
Not an operating company: unions, trade and professional associations, advocacy organizations, campaign committees, party and religious organizations, political action committees, government bodies	−3,949				
Corporate donors carried into employer matching	7,162		495	157	45
No employer in the employment data could be matched to the donor name	−4,351				
Matched to an employer record	2,811		387	140	43
Fewer than ten employees could be linked to the matched employer, or the name match was rejected as unreliable on review	−760				
Accepted employer linkage	2,051		353	135	42
Advocacy, religious and civic organizations (NAICS 813xxx), whose members are not employees	−66				
Candidate universe (frozen registry)	**1,985**	**3,065**	**337**	**131**	**40**
The firm's only measures were not carried into estimation: they did not qualify for the ballot, or were decided at an off-November special election with no comparable prior election	−27	−37			
Firms behind an episode sent to estimation	1,958	3,028	323	130	40
Every episode the firm appears in fell below the fifty-worker minimum treated group	−87	−115			
Every remaining episode the firm appears in is the 2021 California gubernatorial recall, which is out of scope	−9	−12			
Firms behind an estimated episode	1,862	2,901	266	118	39

Continued on next page

Table E2 continued from previous page

Stage	Firms	Employer IDs	Ballot measures	Elections	States
No employee of that employer appears on the state's voter file at that election		−1,322			
Statewide: employers contributing a treated worker to the estimation sample	**1,808**	**1,579**	**266**	**118**	**39**
Local and municipal episodes, screened through the separate eligibility chain described in the notes					
Combined: employers contributing a treated worker to the estimation sample	**1,870**	**1,623**			

Notes: The staged chain above the rule covers statewide measures only. Local and municipal measures are collected, screened, and estimated through a separate eligibility chain whose intermediate stages are not comparable to the statewide ones, so they are not broken out; the combined row below the rule reports the two arms together, and is the population the headline regression estimates on. Its measure, election, and state columns are left blank because the local arm indexes measures by municipality rather than by state × election-year, and the two indexings cannot be added. *Firms* counts distinct standardized donor names in the campaign-finance records. *Employer IDs* counts distinct employer identifiers in the employment data. The two columns are different units, and the mapping between them is many-to-many rather than a hierarchy: most donors carry exactly one employer identifier (the median is one, the mean 3,065/1,985), a minority carry many, and a small number of employer identifiers are claimed by more than one standardized donor name, as when a parent and a subsidiary each donate under their own name (CAESARS ENTERTAINMENT and HARRAHS NORTH KANSAS CITY; DUKE ENERGY and DUKE ENERGY FLORIDA). Neither column therefore nests inside the other, and a row's employer-ID count is not a rescaling of the firm count beside it. *Elections* counts distinct state × election-year pairs. Each stage is a subset of the stage above it within an arm. The two terminal rows report the employers that place at least one treated worker into the estimation sample, which is a narrower population than the eligibility row above them: an employer leaves between those rows when it is linked to a donating firm whose episode estimated but none of its employees appears on the relevant voter file. Their donor counterparts are recovered from the person-level firm list rather than left blank, so both units are reported. Table E1 gives the companion attrition chain counting episodes rather than firms; the fifty-worker minimum that appears in both is a property of the episode, so a firm leaves this table only when every episode it belongs to falls below the floor.

F Ballot Measure Classification

Each ballot measure in my sample receives two classifications: a *policy type* (the substantive domain of the measure) and a *material stake score* (the directness of the link between the measure and the affected firm's employees). Both classifications use multi-model LLM pipelines with inter-rater reliability assessment.

F.1 Policy Type Classification

I classify each of the 370 statewide ballot measures the case-identification pipeline recovered into one of ten policy type categories according to the policy domain the measure is directed at. Classification covers that wider set; the 264 of them that produced matched treated and control workers are the ones entering the estimation sample (Appendix Table H1) and the policy-type decomposition in Table A2. Where a measure is a revenue instrument—a tax, fee, or bond issue—it is typed by the domain its proceeds serve rather than by the fiscal device that raises them, and measures serving no single identifiable domain fall to a residual Other category. The rule and the reason for it are set out under *Mechanism versus purpose* below. The 39 estimated local ballot measures were classified separately under the same codebook and enter the decomposition on the same footing.

- **Industry Regulation**: measures that directly regulate a specific industry's ability to operate, enter, or exit a market (e.g., cannabis legalization, gambling authorization)
- **Tax & Fiscal Policy**: measures that change tax rates, create or repeal taxes, or alter fiscal rules
- **Electoral & Governance**: measures affecting voting systems, redistricting, campaign finance, or government structure
- **Social & Civil Rights**: measures addressing abortion rights, gun control, criminal justice reform, or anti-discrimination
- **Labor & Employment**: measures on minimum wage, working conditions, paid leave, union rules, or occupational licensing
- **Education**: measures on school funding, vouchers, or teacher policy
- **Healthcare**: measures on health insurance, Medicaid, or drug pricing
- **Environment & Energy**: measures on emissions, clean energy mandates, or conservation
- **Housing & Land**: measures on rent control, property rights, or zoning
- **Other**: measures naming a purpose that none of the nine domains above covers—transportation and transit, public safety, veterans' services, libraries, broadband, arts and culture, sports facilities, general infrastructure. A named residual, not a fallback for hard cases, and not a synonym for Tax & Fiscal

Classification procedure. Each of the 370 statewide measures was independently classified by two raters reading a byte-identical prompt: Claude Sonnet 5 (Anthropic) and an OpenAI Codex model (`gpt-5.6-sol`), the latter at high reasoning effort. The 39 local episodes were independently classified under the same codebook by Claude Opus 5 and the same Codex model. Neither rater saw the other's label or the measure's earlier classification; showing a rater the prior answer could inflate agreement without improving it. The prompt states the category definitions, a worked purpose rule (below), and the measure's description. For statewide disagreements, Claude Opus 5 applied the prompt's decision procedure to the two written rationales rather than scoring the measure afresh. The clause that settled each call is recorded alongside it.

Inter-rater reliability. Table F1 reports agreement before statewide adjudication. The statewide pair agree on 94.6% of measures (Cohen's $\kappa = 0.939$), an "almost perfect" result on the Landis and Koch (1977) scale. The local pair agree on 100.0% ($\kappa = 1.000$). The pooled figures combine the two workflows (95.1%, $\kappa = 0.945$); they are descriptive rather than the reliability estimate for a single fixed rater pair. I do not read the local figure as evidence that local classification is more reliable: 39 cases is too few to distinguish a genuinely cleaner classification problem from a run of easy ones, and a κ of one has no room above it to detect the difference. The statewide figure, over 370 measures, is the reliability estimate I would defend. Of the 20 statewide disagreements, 16 were resolved toward the Codex rater and 4 toward the Claude rater.

Two limits are worth stating plainly. First, agreement measures whether two models reading the *same* description reach the same label; it cannot establish that the description was adequate. Where a measure's description is thin, both raters can agree for the same wrong reason, and inter-rater agreement will not reveal it. Second, the adjudicator is a Claude model and so is one of the two raters, so adjudication is not independent of that leg in the way the Codex leg is; the 4 calls resolved toward the Claude rater are the direction in which this matters.

Table F1: Inter-Rater Agreement: Ballot Measure Policy Type Classification

Statistic	All	Statewide	Local
Measures classified	409	370	39
Rated by both raters	409	370	39
Raw agreement	389 (95.1%)	350 (94.6%)	39 (100.0%)
Cohen's κ	0.945	0.939	1.000
Disagreements adjudicated	20		
Resolved to Codex rater	16		
Resolved to Claude rater	4		

Distribution. Table F2 reports the final policy type distribution after adjudication.

Disagreement patterns. The 20 disagreements cluster on the taxonomy's seams rather than scattering. The most frequent unordered pairs are Other vs Tax & Fiscal (3 cases), Education vs Electoral & Governance (2), and Environment & Energy vs Tax & Fiscal (2). Each is a genuine boundary rather than noise. *Other* against *Tax & Fiscal* is the residual category's edge: when a measure raises revenue for a purpose no domain covers, the two raters must decide whether the naming of that purpose is enough to lift it out of the fiscal category. *Education* against *Electoral & Governance* arises where a measure changes how an education body is selected rather than what it does—a distinction my codebook draws but that a reader may reasonably contest. Both are decidable from the codebook, which is why adjudication cites the clause rather than re-scoring.

The purpose rule. Measures that raise or dedicate money while naming what the money is for can be classified two ways—by the financing mechanism or by the purpose it serves. I classify by purpose.

Table F2: Policy Type Distribution Across the Identified Statewide Measures

Policy Type	*N*	%
Industry Regulation	63	17.0
Electoral & Governance	56	15.1
Social & Civil Rights	55	14.9
Education	38	10.3
Tax & Fiscal Policy	38	10.3
Healthcare	35	9.5
Environment & Energy	34	9.2
Labor & Employment	24	6.5
Other	18	4.9
Housing & Land	9	2.4
Total	370	100.0

A measure that names a substantive policy purpose takes that purpose's category regardless of how it is financed: Medicaid expansion funded by a tobacco tax is Healthcare, not Tax & Fiscal Policy. Tax & Fiscal Policy is reserved for measures where the fiscal mechanism *is* the substance—a rate change with no named beneficiary, a spending cap, a budget or revenue rule such as a TABOR limit or a supermajority-to-tax requirement, or general bonding authority naming no project.

The rule follows from what the paper is measuring. The mechanism under test is whether a ballot outcome reaches employees' livelihoods, and that depends on what a measure does, not on how it is paid for. A worker at a hospital system has the same stake in Medicaid expansion whether it is financed by a tobacco tax or a sales tax. Classifying by financing mechanism would sort measures on a dimension orthogonal to the one the theory identifies, and would sever exactly the link being estimated.

Where no category fits the named purpose—transportation, public safety, veterans' services, libraries, arts and culture, none of which the ten categories cover—the measure takes an explicit residual category rather than being forced into the nearest neighbor. *The Other category* below explains why that residual is its own category and not a fallback to Tax & Fiscal Policy.

The Other category. The ten categories do not span the purposes measures actually serve. Transportation, public safety, veterans' services, libraries, broadband, general research, and arts funding all appear on ballots and none has a category. 18 statewide measures and 8 local episodes name purposes of this kind.

These take the explicit residual category, *Other*. It names the gap in the taxonomy, not the financing instrument.

The Other coefficient is not a domain effect and should not be read as one. It is a residual by construction: a category containing road bonds, broadband, and arts grants has no common stake mechanism for the theory to operate on, so its estimate averages across unrelated things. I report it for complete sample accounting, not as evidence about any policy domain.

Why Tax & Fiscal Policy is the reference. Tax & Fiscal Policy contains measures whose fiscal mechanism *is* the substance: rate changes with no named beneficiary, spending caps, and budget rules. It is the natural baseline against which a purpose-bearing measure should be read because it is the closest category to a ballot measure with no substantive policy domain.

F.2 Local Ballot Measure Policy Types

The 370 statewide measures above are keyed on an OpenSecrets entity identifier. Local ballot measures have no such entity, so they receive their policy types through a separate classification keyed on the measure name. All 39 estimated local episodes are classified.

Procedure and reliability. Claude Opus 5 (Anthropic) and an OpenAI Codex model (`gpt-5.6-sol`) independently classified every estimated local episode against the same codebook used for the statewide sample. They agreed on all 39 local episodes (100.0%, Cohen's $\kappa = 1.000$). The local sample spans 7 of the ten categories, with school funding accounting for 17 of 39 episodes and the remainder covering libraries, open space, public safety, a sports facility, and housing. It is nonetheless small, so I treat the statewide reliability estimate as the more informative one and report the local result for completeness.

Measures resting on a generic description. Three Washington episodes—Yes for Redmond Safety (2022), Yes for Vets & Seniors (2023), and Yes, Safe Spokane (2024)—are described in the source data only by a jurisdiction template that names neither a capital project nor a tax rate. Their policy-type assignment therefore rests on that template and on the sponsoring committee's name rather than on the measure's own text, and is correspondingly weaker than the other thirty-six. All three are assigned Tax & Fiscal, the category the local addendum specifies when no capital project is named.

F.3 Material Stake Classification

For each of the 3,883 firm–ballot measure pairs the case-identification pipeline recovered (3,333 statewide and 550 local), I score the *material stake*—the directness of the link between the ballot measure's outcome and the affected firm's employees' jobs, income, or working conditions—on a 1–10 scale using two independent LLM raters and a stronger-model adjudicator. This is an employee-livelihood stake coded at the firm–measure level. It is not a score of the firm's financial stake alone, nor does it measure individual employees' perceptions of what is at stake.

Categories. The 1–10 score maps deterministically to three categories:

- **High** (scores 8–10): the ballot outcome directly affects employees' livelihoods (e.g., a coal mining company opposing a ban on coal power plants; a taxi company opposing rideshare legalization)
- **Medium** (scores 4–7): the employee connection ranges from indirect or limited to meaningful but nonexistential (e.g., a bank opposing a financial transaction tax at the lower end; a trucking company opposing a fleet-replacement rule at the upper end)
- **Low** (scores 1–3): no meaningful connection to employees' work or job security (e.g., a software company weighing in on an animal welfare ballot measure)

Classification procedure. Each rater performs two tasks within one rendered prompt. In Pass 1, the model states the firm's primary business and what its employees work on. It receives the contributor name and a NAICS hint, together with a `firm_identity` field when available. That field supplies factual reference data on the resolved employer entity, sector, corporate parent, and workforce size; the prompt tells the model to use it instead of guessing from the contributor name alone. In Pass 2, the model uses those firm inputs, the ballot measure's description, and the firm's stance to assign a material stake score (1–10) and a written rationale.

Two raters score every pair independently: **Claude Sonnet 5** (Anthropic) and **GPT-5.6** (OpenAI, `gpt-5.6-terra`), each at high reasoning effort, each receiving a byte-identical prompt from a shared renderer. Where the two disagree on the three-category binning, **Claude Opus 5** adjudicates between the two written rationales rather than scoring from scratch, and is not shown the earlier classification's label—otherwise adjudication would collapse into a vote to retain the prior answer. Of the 3,883 pairs, 3,006 resolved by consensus and 873 required adjudication. A further 4 carry a documented manual patch.

What the raters see, and what they do not. The rater is shown the contributor name, a NAICS hint, the resolved employer identity (including sector, corporate parent, and workforce size when available), the measure's state, year, name and description, and the firm's stance. It is not shown any existing score, either rater's rationale, the contribution amount, or the identity of any other contributor to the measure. This is enforced in code: a denylist gate runs on the rater-facing frame at build time and again at prompt-render time, with a negative test confirming it fires on each withheld field.

The restriction is not incidental. A rater that can see the payment can infer stake from the fact or the size of that payment, and the moderator would then recover the contribution decision—which is the treatment—rather than the underlying stake it is meant to measure. For the same reason, the enriched measure descriptions used here are filtered at sentence level to remove campaign-finance content: a sentence naming a funder, a dollar amount, or a side assignment is dropped, while a sentence describing what the measure does to firms in an industry is retained. Naming a firm as the *target* of a measure is policy content; naming it as a *funder* is contribution data.

Exact production prompts. The two raters received the same rendered prompt. The following block reproduces its complete fixed instructions verbatim. After this text, the renderer appended the number of pairs in the batch and the batch data as CSV. I show that dynamic suffix's schema below rather than printing all 3,883 input rows.

```
You are classifying the MATERIAL STAKE that a company's employees have in the
outcome of a U.S. ballot measure. Do TWO passes for every row.

PASS 1 - Firm industry: from the company name (and the NAICS hint, if given),
state in a few words the company's primary business and what its employees work
on. If it is a trade association, PAC, advocacy group, union, or non-profit, say
so and name the sector/cause. If you cannot identify it, write "UNKNOWN" plus a
best guess.

PASS 2 - Material stake score (1-10): rate how directly the ballot OUTCOME
affects EMPLOYEES' livelihoods (jobs, income, working conditions) - not just the
firm's revenue.
  10 Employees lose their jobs if it passes/fails. Existential.
   9 Major workforce restructuring likely (layoffs, closures, pay cuts).
   8 Employees' core job functions or working conditions change substantially.
   7 Meaningful impact on job security or compensation, not existential.
   6 Some employees affected (one division), others not.
   5 Plausible but indirect - the firm cares, employees might notice.
   4 Firm has a financial interest but employees' daily work is unaffected.
   3 Marginal - industry-adjacent, employees keep jobs/conditions regardless.
   2 Firm takes a position on a policy tangential to its sector.
   1 No connection to the firm's business or employees' work. Purely
     political, reputational, or ideological.

Category from the score: HIGH (8-10), MEDIUM (4-7), LOW (1-3).

Anchors: coal miner co. vs coal-power ban = 10; taxi co. vs rideshare = 9;
hospital vs Medicaid repeal = 8; trucking vs emissions fleet rule = 7; pharma vs
drug-pricing = 6; tech vs data-privacy = 5; bank vs transaction tax = 4;
```

```
insurer vs tort reform = 3; oil co. on school vouchers = 2; software co. on
animal welfare = 1.

Key distinctions:
- Employees' LIVELIHOODS, not firm revenue. A measure can be 4-5 (firm cares)
  without being 7+ (employees keep their jobs either way).
- The same measure scores differently for different firms: a tobacco company vs
  a tobacco tax = 9; a random LLC donating to the same campaign = 1.
- Trade associations: score by the effect on member firms' EMPLOYEES.
- Contribution amount is NOT a signal of stake.

The firm_identity field, where present, is factual reference data about the
company: the employer entity its name resolves to, its sector, its corporate
parent, and its workforce size. Use it for Pass 1 instead of guessing from the
name alone, and note that a contributor may trade under a name different from
the operating company whose employees are actually at stake.

Reading the measure description:
- Where the description reveals that the measure TARGETS or PROTECTS a specific
  industry, reason about what the measure does to firms in that industry - not
  only about the measure's nominal subject. A measure whose title sounds
  procedural or fiscal may, in substance, decide the fate of a named sector; a
  measure whose title names an industry may in substance be routine for it.
  Score the substance the description describes, not the title.

OUTPUT - return ONLY a fenced CSV code block, nothing before or after it. The
block must start with a line containing exactly:

pair_id,pass1_industry,stake_score,stake_category,rationale_short

Then exactly one row per input pair, using the SAME pair_id values. Rules:
- pass1_industry: <= 12 words, no commas (use semicolons); the Pass-1 finding.
- stake_score: a single integer 1-10.
- stake_category: HIGH, MEDIUM, or LOW, consistent with the score.
- rationale_short: <= 30 words, no commas, why the outcome does or does not
  touch employees' livelihoods.
Quote no fields; keep every field comma-free so the CSV parses cleanly.
```

For a batch of N pairs, the renderer appended the following line and CSV header, followed by the N pair-specific rows. The header is wrapped here only for typesetting:

```
Classify all N pairs below (input CSV):

pair_id,contributor_name,state,measure_year,ballot_measure_name,
measure_description,stance,primary_naics,firm_identity
```

On category disagreements, the adjudicator received the same scoring rubric and industry-targeting instructions, the measure text seen by both raters, and the two scores and rationales. The following additional instructions are reproduced verbatim:

```
You are ADJUDICATING between two independent raters who scored the same
firm x ballot-measure pairs under the rubric above. For each pair below you see
the measure text both raters saw, plus each rater's score and rationale.

Decide which reading is right, or supply your own score if both misread the
measure. You are bound by the same rubric and the same cut points. You have no
information the raters lacked and you are not told any prior score for these
pairs; do not speculate about one.

OUTPUT - return ONLY a fenced CSV code block, header exactly:

pair_id,final_score,final_category,sided_with,adjudication_note

Rules:
- final_score: integer 1-10; final_category derived from it (HIGH 8-10 MEDIUM 4-7 LOW 1-3).
- sided_with: A or B or NEITHER.
- adjudication_note: <= 30 words, no commas, why that reading wins.
```

The adjudicator's dynamic CSV suffix used the following fields, again wrapped here only for typesetting:

```
pair_id,contributor_name,state,measure_year,ballot_measure_name,
measure_description,stance,primary_naics,score_A,cat_A,rationale_A,
score_B,cat_B,rationale_B
```

Input text. Each pair is rated against the fullest available description of its measure. Every measure in the universe carries a long-form description (median 524 characters); 243 pairs, covering 34 measures flagged as highest-risk by an exposure screen, additionally carry a primary-source research summary of what the measure did and what was contested. Local pairs keep their hand-researched descriptions on the committee-name key.

This matters because an earlier version of this classification rated the same pairs against a short subject line (median 41 characters), which for many measures named the fiscal instrument without naming what was at issue. Maine's 2023 Question 1 appeared to its raters as "Voter Approval of Borrowing"; the measure was the investor-owned utilities' counter-proposal to Question 3, the initiative that would have bought out Central Maine Power. Both raters agreed on a middling score, so the disagreement statistic recorded agreement—but the agreement was between two raters reading the same uninformative text. I report the reliability figures below with that caveat attached: they measure whether two raters concur, not whether either is right.

Inter-rater reliability. Table F3 reports agreement between the two raters across all 3,883 pairs, before adjudication. Because the 1–10 scale is fine-grained and the three-category binning is coarse, I report several metrics. The two raters place 77.5% of pairs in the same category ($\kappa = 0.625$) and their scores correlate at $r = 0.875$. Where they differ they differ by little: 92.1% of pairs fall within two points. The residual disagreement is largely calibration rather than ordering—GPT-5.6 scores higher than Claude Sonnet on average (4.43 against 3.70 on the 1–10 scale)—which is why the adjudication step compares rationales rather than averaging scores.

Table F3: Inter-Rater Agreement: Material Stake Classification

Statistic	Value
Firm–measure pairs scored by both raters	3,883
Category agreement (High/Medium/Low)	3,010 (77.5%)
Cohen's κ (three categories)	0.625
Pearson r (1–10 scores)	0.875
Within 1 point	2,825 (72.8%)
Within 2 points	3,575 (92.1%)
Disagreements adjudicated (Opus 5)	873

Distribution. Table F4 reports the final distribution after adjudication.

Relation to the earlier classification. An earlier pass over the same 3,333 statewide pairs, run against the short subject lines and with weaker raters, returned 512 High, 1,271 Medium and 1,550 Low. The re-rating moves the distribution toward the middle: High falls and Medium rises, with roughly two-thirds of the earlier High pairs retaining that label. Because the estimating equation compares High against Low, both the composition of the High arm and the size of the reference group change, so the

Table F4: Material Stake Distribution

Material Stake	Statewide	Local	Total	%
High (8–10)	394	7	401	10.3
Medium (4–7)	1,416	283	1,699	43.8
Low (1–3)	1,523	260	1,783	45.9
Total	3,333	550	3,883	100.0

earlier and current stake contrasts are not two estimates of the same quantity computed on the same labels. Section 6 reports the current one. Every pair's earlier score is retained in the classification file alongside its new one, so the reassignment is auditable pair by pair.

What a contribution-derived score cannot see. The universe of firm–measure pairs is generated by contributions, so the classification can only score a firm that paid. A firm with a large stake that declined to contribute has no row and receives no score, and neither does a firm that routed its money through an intermediary committee rather than to the ballot campaign directly. Both cases occur in this sample: Altria contested Montana's 2018 I-185 and stood down on Oregon's 2020 Measure 108, so it appears on the first and not the second. This is a property of the treatment definition rather than of the classification, and it bounds what the stake contrast can be estimated across—a point I return to in Section 6.

F.4 Proactive versus Reactive Mobilization: Ballot-Measure Origin

A natural question is whether firms mobilize differently when the ballot fight runs *through* their business than when it is aimed *at* it. I make no prediction either way—the account in Section 2 turns on whether the stake is shared, not on who initiated the contest—and I report the classification because the distinction is descriptively informative about how commercial ballot fights arise.

The classification composes two separately coded facts rather than asking a rater to label a firm directly. The first is the commercial origin of each statewide measure. Two independent raters from different vendors coded every measure's origin type from campaign-finance dossiers, under identical instructions and a hash-pinned codebook (cross-vendor agreement $\kappa = 0.819$ on the five-category coding and $\kappa = 0.913$ on the commercial-origin binary); the 31 disagreements were resolved by a third model, blinded to which rater produced which coding. Appendix F.4 reproduces every prompt issued. A measure is *industry-sponsored* when a commercial sector advanced and bankrolled it, *anti-industry* when it targets a sector that mobilizes against it, and *mixed dueling* when two industries duel on opposite sides. Measures with no commercial origin—governance, civil-rights, criminal-justice, and other ideological or non-commercial questions—have no industry to run through or at, and are excluded from this cut.

The second fact is each contributing firm's own stake in the measure, coded at the firm–episode level from the firm's dossier: whether the measure would advance the firm's own business, target it, target a rival, or leave it untouched. Crossing the two yields the classification estimated in Table F5. A firm is *proactive* where the industry that drove the measure onto the ballot is its own and passage would expand its operations, and *reactive* where the measure is aimed at its operations by others. On a mixed-dueling measure the firm's own stake decides which it is, so one measure can place firms on opposite sides of the classification. Firms with no business at stake divide by the side their contributions backed: an *ally* funded the support side, a *resister* the opposition. Combinations in which the two halves contradict each other—a measure its own industry sponsored that the rater also records as targeting the firm—are counted and set aside rather than forced into a category.

This replaces an earlier offense/defense coding that resolved the firm half by matching the firm's recorded industry code against the measure's sector vocabulary. That matcher could not confirm membership for over half the commercial sectors, and its *offense* label selected firms that joined a campaign already under way rather than firms that started one. The regression built on it is withdrawn. The origin coding itself, which supplies the measure half of the present classification, is unaffected: it is coded from who funded a measure, independently of any firm's industry code.

Table F5: Employee Mobilization and Voter Turnout by Whether the Measure's Industry Origin Runs Through the Firm or At It

	Firm's relation to the measure
Treated × Post (ally, 326 firm–episodes)	0.007
	(0.007)
… × Resister (170 firm–episodes)	0.014
	(0.013)
… × Proactive (486 firm–episodes)	0.007
	(0.010)
… × Reactive (565 firm–episodes)	0.027
	(0.019)
Num. Obs.	43,677,191
N treated workers	492,445
N control workers	4,708,442
N episodes	344
N active firms	1097
Voter × Episode FE	Yes
Episode × Period FE	Yes
Clustering	Firm + Episode

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Firm–episodes are classified by combining the measure-level industry origin with the firm's own stake and the side its contributions backed. A firm is *proactive* where the industry that drove the measure onto the ballot is the firm's own and passage would expand its operations, and *reactive* where the measure is aimed at its operations by others. Firms with no business at stake divide by the side they funded: an *ally* backed the measure's support side and a *resister* its opposition. Allies are the omitted category, so the Treated × Post coefficient is their estimated effect and each interaction is the difference from it. Control workers are shared across arms and carry no classification. Classification covers measures with a commercial-industry origin; treated firm–episodes on ideological or non-commercial measures are unclassified and excluded.

The classification covers 122 statewide measures with a commercial origin (66 industry-sponsored, 46 anti-industry, 10 mixed dueling). Treated firm–episodes on ideological or non-commercial measures carry no classification and are excluded; matched control workers are shared across arms and are retained in every fit.

No contrast in Table F5 separates from zero. Two features of the estimates bear statement anyway, neither as a finding. Reactive firm–episodes carry the largest point estimate—roughly triple the ally baseline—but the standard error on that difference is 1.9 percentage points and its 95% confidence interval runs from -1.6 to $+5.8$, so the design neither supports the gap nor rules it out at any interesting magnitude. And firms with no business at stake in the measure they funded mobilize at a rate that is a substantial share of the headline effect, which is not what an account resting on a shared material stake would predict. Two things bound how far either reading can be pushed. The counts are firm–episodes rather than firms, so a few heavily contributing firms can carry an arm. And the firm half of the classification carries measurement error whose direction is known. A blind audit of 100 firm–episodes, coded from the same dossiers without sight of the pipeline's assignment, agrees on the stake direction for 85% of them ($\kappa = 0.78$), and on the derived classification for 85% ($\kappa = 0.78$); a 20-case subset re-coded by a model from a different vendor agrees with the pipeline at $\kappa = 0.78$. The 15 disagreements are asymmetric: the auditor finds a business stake in 75 cases against the pipeline's 69, so the no-stake arm absorbs firms that belong in a stake arm. That direction shrinks the measured gap between the arms, which is a reason to read the null cautiously—and equally a reason not to lean on the no-stake baseline, since some of what it contains is not a firm without a stake but a stake the coding did not see.

What the audit does and does not establish. Most of the disagreements trace to rules the codebook leaves open rather than to thin evidence, and two show the same rule applied differently within a single measure: on California's Proposition 69 one heavy civil contractor is coded as having no stake in a transportation-revenue measure while two others on the same question are coded as advanced by it. Errors of that shape are not independent of the episode, so they do not average away across a stack. The audit's own limit is that the auditor and the pipeline are the same model family, and blinding removes the answer but not the shared prior: agreement of this size shows the codebook is applied *consistently*, not that it is *correct*. The cross-vendor subset is the only independent evidence here, and 20 cases establish that agreement is not visibly worse across vendors rather than certifying the codebook.

Two limits on this coding deserve statement. First, on coverage: an earlier vintage of the coding covered fewer measures than the certified sample, so a subset of statewide questions entered the firm-level file as *uncoded* and fell out of the cut for a coverage reason rather than a substantive one. That gap has since been closed—the outstanding measures were coded by the same two raters under the same archived codebook, with disagreements adjudicated blind—so every measure in the certified sample now carries an origin code and no firm leaves the classification for want of coding. The estimates reported here are computed on the completed coding. Second, the raters' evidence was asymmetric in quality. The contribution records placed in front of them—contributor names, entity types, amounts, and each side's totals and concentration—are administrative campaign-finance data from OpenSecrets. The accompanying description of what each measure would do was not: it is a research summary written for this project by a language model working from the measure's Ballotpedia page, carried into the dossier alongside a link to that page. The origin coding therefore rests on hard evidence for *who paid* and softer, model-generated evidence for *what the measure did*. That asymmetry is consequential where a measure's surface description hides its substance—Maine's 2023 Question 1 reads as a borrowing-approval question but was the investor-owned utilities' counter-measure to the Pine Tree Power buyout—and I audited the sample for exactly that pattern, flagging measures whose descriptions are short or procedural while the money behind them is concentrated in one sector.

What is coded. The classification rests on a prior coding step: the *origin* of each statewide ballot measure. I code origin as a set of facts about the measure and *derive* each firm's relation to it from those facts, rather than asking a model to label a firm directly. Two fields carry the origin. *Procedural origin* records how the measure reached the ballot—citizen initiative, legislatively referred, veto (people's) referendum, or other. *Origin type* records the political-economy driver, in one of five categories: *industry-sponsored* (a commercial sector advanced and funded the measure to further its own interest), *anti-industry* (labor, an advocacy or non-governmental organization, a rival industry, or a megadonor put it forward to tax, regulate, constrain, or dissolve an industry), *mixed dueling* (two commercial industries on opposite sides, each with corporate backing), *ideological/partisan* (an organized ideological or partisan campaign drives it, with no commercial industry advanced or targeted), and *non-commercial* (governance, rights, criminal-justice, or elections-mechanics questions with neither a commercial nor an organized ideological driver). Only the first three types carry a commercial origin; the last two mark measures with no commercial stake and are set aside. The companion fields *industry advanced* and *industry targeted* name the benefiting or targeted sector(s) from a controlled vocabulary, and supply the key that later links a measure to a firm's own industry.

Coding protocol. Each measure was coded independently by two large language models from different vendors—an Anthropic Claude model and OpenAI's Codex—working blind to each other and to the firm-level estimation data. Each rater saw the measure text and its campaign-finance dossier (contributor names, sides, and dollar totals) and returned the full field set above, a verbatim evidence quote, and a confidence rating. Both raters received the *same* instruction text, issued from a single source and pinned to an archived codebook by hash, so the agreement statistic measures disagreement between models rather than between instruction sets. The prompts are reproduced in Appendix F.4. Table F6 reports agreement. On the five-category origin type the two raters agreed on 86.6% of measures (Cohen's $\kappa = 0.819$); on the binary distinction that actually governs the cut—commercial origin (industry-sponsored, anti-industry, or mixed) versus not—agreement rises to 95.7% ($\kappa = 0.913$), because most disagreements sit inside the two non-commercial categories (non-commercial versus ideological/partisan) rather than across the commercial boundary.

Adjudication. The 31 origin-type disagreements were resolved by a third model, not by a human. The adjudicator saw each measure's dossier, the codebook version that measure had been rated under, and the two competing codings—presented as "Coder A" and "Coder B", with the assignment flipped from measure to measure on a hash of the measure identifier, so it could neither tell which vendor produced a coding nor follow one vendor across cases. It could also return a code neither rater proposed, which it did once. The adjudicated call is stored on each measure's final row and flagged there as adjudicated rather than agreed. I report this design because the obvious alternative is not neutral: an unblinded adjudicator drawn from one of the two rating vendors is being asked, in effect, to arbitrate its own coding. In an earlier unblinded pass the adjudicator sided with the same-vendor rater on 31 of 41 disputes (75.6%, $p = 0.002$ against an even split); under blinding it sides with that rater on 40.0% of disputes ($p = 0.36$). The two passes cover different disagreement sets and so do not constitute a controlled test, but the blinded design is the defensible one regardless of which way that comparison had fallen.

Table F6: Inter-Rater Agreement: Ballot-Measure Origin Classification

Statistic	Value
Statewide measures coded	231
Origin type (5-category) raw agreement	200 (86.6%)
Cohen's κ, origin type	0.819
Commercial origin (binary) raw agreement	95.7%
Cohen's κ, commercial origin	0.913
Disagreements adjudicated (blinded)	31

Decision rules. The codebook fixes the harder calls with fourteen rules; I summarize the decision logic rather than reproduce them. Origin is coded by who drove and who bears a measure, not by its surface type or title—a "millionaire tax" bankrolled by rideshare firms is industry-sponsored, not a tax question (R1). Initiation is read from a funding decision tree: the dominant funding side must be non-individual contributors whose sector matches the benefiting sector before a measure is called industry-sponsored; a lone megadonor or an advocacy organization on top instead points to anti-industry or ideological/partisan origin, with the industry a passive beneficiary rather than the originator (R2). A veto referendum is coded by what the underlying law does to the industry that referred it, so an industry defending itself through a referendum is scored anti-industry in substance (R3). Legal filers are frequently shell political-action committees, so funding composition, not the named sponsor, is the signal, with escalation to Ballotpedia to name an obscured backer (R4); co-beneficiary firms may jointly originate a measure (R5); and genuinely two-sided industry fights are coded mixed dueling with both sectors named (R6). Advancing and targeted sectors are drawn from a controlled vocabulary as semicolon-separated lists, never free text (R11). The non-commercial versus ideological/partisan boundary—which never affects the classification—turns on whether an organized ideological or partisan funder is visible in the dossier, with politically charged topic alone insufficient to code ideological/partisan (R13), and a final commercial-signal recheck guards against filing a commercially driven measure as non-commercial on a skimmed contributor list (R14). Evidence tiers cap confidence (documented sponsorship > funding dominance > industry inference), and thin or one-sided funding coverage triggers escalation before a row is finalized (R8, R12).

From origin to the firm's relation to the measure. The classification is derived at the firm–episode level by crossing a measure's origin type with the firm's own coded stake and the side its contributions backed. A firm whose own industry drove the measure and whose operations passage would expand is *proactive*; a firm whose operations the measure targets is *reactive*. A firm with no business at stake is an *ally* where it funded the support side and a *resister* where it funded the opposition. On a mixed-dueling measure the firm's own stake decides which of the two stake categories applies, so one measure can place firms in opposite categories. Stake is read from the firm's dossier rather than inferred from its industry code, which is what distinguishes this derivation from the offense/defense coding it replaces: that rule matched the firm's recorded NAICS against the measure's sector vocabulary and could not confirm membership wherever the recorded code was coarser than the sector, so it excluded firms on a notational property rather than a substantive one. Rows whose two halves contradict each other are counted and set aside rather than assigned.

Scope. Of the statewide measures coded, 122 carry a commercial origin and enter this analysis: 66 industry-sponsored, 46 anti-industry, and 10 mixed dueling. The remaining statewide measures are non-commercial or ideological/partisan—governance, civil-rights, criminal-justice, and other questions with no industry advanced or targeted—and are excluded by construction. Origin coding covers the statewide measures only. Local ballot measures are excluded by design, not by coding gap: local questions in the sample are referred to the ballot by an elected public body (e.g., a school board placing a bond or mill-levy under TABOR), so the funding firm is a campaign beneficiary rather than the measure's originator, and the origin classification—which requires an industry to have driven the measure—has no referent.

Prompt disclosure. The origin coding is model-produced, so the instructions that produced it are part of the method. This section reproduces the protocol issued at each stage and lists every prompt actually sent, with a SHA-256 digest of its exact bytes. The prompt files are in the replication repository at `analysis/expansion/origin/prompts/claude/`, and `prompt_manifest.csv` beside them records, for each prompt, the measures it covered, the codebook version pinned into it, and the artifact it produced. Reproduced below is the fixed instruction text; the per-batch payload (controlled vocabulary, codebook, and measure dossiers) runs to tens of thousands of characters per prompt and travels by digest rather than in print.

Rating protocol (both coders, both stages). Issued verbatim to both raters. `claude_rater.py` imports this text from `codex_rater.INSTRUCTIONS` rather than copying it, so the two vendors were scored under one instruction set; the phrase “independent second coder” is therefore addressed to both.

```
You are an independent second coder for a political-economy study of ballot measures.
Code each measure’s ORIGIN from the evidence dossier provided. Work only from the dossier
(do not browse, do not run commands, do not write files). Apply the codebook rules below.

Output ONLY a JSON array, wrapped EXACTLY between the lines <<<JSON>>> and <<<END>>>, with one
object per measure in the SAME order given, each:
{"entity_id": <int>,
  "procedural_origin": "legislatively_referred|citizen_initiated|veto_referendum|other",
  "origin_type_primary": "industry_sponsored|anti_industry|mixed_dueling|ideological_partisan|non_commercial",
  "industry_advanced": "<semicolon-separated sector_label list, or none>",
  "industry_targeted": "<semicolon-separated sector_label list, or none>",
  "confidence": "high|medium|low"}
Use ONLY sector_label values from the controlled vocabulary. No prose outside the sentinels.
```

Adjudication protocol (blinded). Issued to the adjudicator for each disputed measure. Candidate codings are labelled Coder A and Coder B, with the assignment flipped on a hash of the measure id, so vendor identity is not visible and cannot be tracked across cases.

```
You are a senior adjudicator for a political-economy study of ballot measures.
Two independent coders assigned DIFFERENT origin codes to each measure below. Decide the
correct code from the evidence dossier and the codebook rules. Work only from the dossier
(do not browse, do not run commands, do not write files).

The two candidate codings are labelled Coder A and Coder B. Which model produced which
coding is withheld deliberately, and the A/B order changes from measure to measure, so you
cannot follow one coder across the set. Judge the codings, not their source.

You are NOT restricted to the two candidates. If the codebook requires a code neither coder
proposed, return that code and set sided_with to "neither".

Output ONLY a JSON array, wrapped EXACTLY between the lines <<<JSON>>> and <<<END>>>, with one
object per measure in the SAME order given, each:
{"entity_id": <int>,
  "origin_type_primary": "industry_sponsored|anti_industry|mixed_dueling|ideological_partisan|non_commercial",
  "industry_advanced": "<semicolon-separated sector_label list, or none>",
  "industry_targeted": "<semicolon-separated sector_label list, or none>",
  "confidence": "high|medium|low",
  "sided_with": "coder_a|coder_b|neither",
  "rationale": "<one or two sentences naming the dossier evidence that decides it>"}
Use ONLY sector_label values from the controlled vocabulary. No prose outside the sentinels.
```

Table F7: Prompts Issued in the Origin Coding

Prompt	Stage	Measures	SHA-256 (first 16)
claude_primary_v3_batch_00	primary	20	9540c040fdf694db
claude_primary_v3_batch_01	primary	20	dc2571cc279d3388
claude_primary_v3_batch_02	primary	20	0098d5429facd24d
claude_primary_v3_batch_03	primary	20	aa4f2cd273531135
claude_primary_v3_batch_04	primary	20	9b030004e3c17975
claude_primary_v3_batch_05	primary	20	a98770796e5b7bf9
claude_primary_v3_batch_06	primary	20	a476fc724da54c9e
claude_primary_v3_batch_07	primary	20	e395d1f7f5070f90
claude_primary_v3_batch_08	primary	20	7ea3f7b45e95456e
claude_primary_v3_batch_09	primary	20	18e51504ca2f4623
claude_primary_v3_batch_10	primary	20	aee0d4a35cec0490
claude_primary_v3_batch_11	primary	11	fee2dbe339bfed65
claude_rerate_v3_batch_00	rerate	23	9cb1b96793bd111d
claude_rerate_v3_batch_01	rerate	23	d3710d3328484f2f
claude_rerate_v3_batch_02	rerate	23	74c4fc88eefcad6e
claude_rerate_v3_batch_03	rerate	23	8265e9aca9ca9f86
claude_rerate_v3_batch_04	rerate	23	abc2404fc0ff693d
claude_rerate_v3_batch_05	rerate	19	451c85f47e541581
claude_gap_v3_batch_00	gap	20	7dacd2cb5c0491d1
claude_gap_v3_batch_01	gap	20	2deca6a4aebaad16
adjudication_v3_batch_00	adjudication	6	492883763653db32
adjudication_v3_batch_01	adjudication	8	3b3df89e5a19e8c6
adjudication_v3_batch_02	adjudication	8	ffc9cd1f9a68c4fb
adjudication_v3_batch_03	adjudication	8	1affef82233e4493
adjudication_v3_batch_04	adjudication	1	35e037249c28eef6
adjudication_gap_batch_00	adjudication	7	fd7198c6cff5e92f

G Precinct Vote-Direction Construction

This appendix documents the precinct-level vote-direction analysis specified in Section 5 and reported in Section 6.2: where the returns come from, how firm stances are assigned, how workers are placed into precincts, which cells survive screening, and what each reported specification estimates. The ballot is secret, the unit is an aggregate, and nothing here licenses an individual-level reading.

G.1 Precinct-Level Returns: Sources and Coverage

Precinct-level returns on ballot measures are harder to obtain than precinct-level returns on candidate races, and the distinction governs the sample. The standardized academic collections—the MIT Election Data and Science Lab and the Voting and Election Science Team—publish precinct returns for *candidate* offices only. Neither publishes measure-level yes/no counts. Ballot-measure returns must therefore be obtained from each state's own election authority, one state at a time, in whatever format that authority publishes.

This is the binding constraint on the arm's coverage. The turnout analysis spans 39 states; the vote-direction analysis spans seven, because only a minority of states publish measure results at precinct granularity in a machine-readable form. Table G1 lists the source used for each.

Table G1: Precinct-Level Ballot-Measure Returns: Source by State

State	Authority	Granularity	Format
California	Statewide Database (SWDB)	Precinct (SRPREC)	CSV/DBF
Georgia	Secretary of State (historical)	Precinct	Precinct files
Massachusetts	Secretary of the Commonwealth	Precinct	Searchable database
North Carolina	State Board of Elections	Precinct	Flat file
North Dakota	Secretary of State	Precinct	CSV/Excel
Nevada	Secretary of State	Precinct	CSV/Excel
Oklahoma	State Election Board	Precinct	CSV/ASCII

Notes: Measure-level returns come from state authorities because the standardized academic collections carry candidate races only. Precinct *geometry* comes from a different source: the Voting and Election Science Team and the Redistricting Data Hub publish precinct shapefiles by state and vintage, and California is matched to the Statewide Database's own SRPREC shapefile. Returns and geometry are joined by precinct identifier, then workers are placed into the resulting polygons.

Precinct boundaries are redrawn between cycles, so geometry is vintage-specific: each measure is matched to the shapefile for its own state and year, and precinct identifiers are scoped to that layer. The same identifier in a different vintage is a different polygon and is never pooled across layers.

G.2 Why This Sample Is Smaller Than the Estimation Sample

The vote-direction analysis covers 31 ballot measures in seven states, against 39 states in the turnout analysis and a candidate universe of 337 measures. The gap is not a sampling choice. A measure enters the vote-direction analysis only if four conditions hold simultaneously, and each is a hard data requirement rather than a judgement about the measure.

First, and binding: the state must publish precinct-level returns for the measure itself. Measure-level yes/no counts at precinct granularity exist only where a state's own election authority publishes them, in machine-readable form, for the year in question. Many states report measures at county level only, or in scanned canvass documents, or not at all. Section G.1 lists the seven states that clear this bar. None of this constrains the turnout analysis, because turnout is observed in the voter file rather than in official returns.

Second, precinct geometry must exist for that state and year. Boundaries are redrawn between cycles, so a measure needs a shapefile of its own vintage before workers can be placed into precincts. Vintages are published unevenly, and odd-year elections are the sparsest.

Third, the measure must be one-sided, for the reason given in Section G.3: where contributing firms sit on both sides, "firm-preferred" is not a defined direction and the outcome cannot be oriented.

Fourth, its precincts must clear the coverage screen described below. A measure whose precincts are largely screened out contributes little even when it satisfies the first three conditions.

The surviving measures are distributed unevenly: California contributes 7, Massachusetts 10, Nevada 6, Oklahoma 3, Georgia 3, and North Carolina and North Dakota 1 each. That distribution reflects state reporting practice and geometry availability rather than anything about the measures, but it does mean these estimates are weighted toward states that publish well. This sample should be read as a convenience sample in a specific sense—its composition is set by administrative data practices—and not assumed representative of the 337-measure universe in stake, salience, or industry.

G.3 Firm Stances and the One-Sided Restriction

Each firm's position on each measure comes from the classification described in Appendix F, the same source used throughout the paper. No stance is coded specially for this analysis.

Within a measure, firms' contributions are signed by stance and netted at the firm level, so a firm supporting a measure and a firm opposing it partially offset in a precinct where both employ workers. A measure is *one-sided* when only one side places workers into precincts, and *dueling* when both do.

The classification is on *placed workers*, not on the contribution records, and the distinction has bite. A measure that drew corporate money on both sides, but whose minority side employs no one the panel can place in any precinct, is one-sided here: the cells can carry exposure from one direction only, so the outcome can be oriented. Classifying on contributions instead would put such a measure in the dueling group and hold it out of an analysis it can in fact support. A handful of measures place no workers on either side; their exposure is identically zero, so they contribute nothing but measure fixed effects and are excluded from both the one-sided and dueling samples.

The reported estimates cover one-sided measures only. On a dueling measure the "firm-preferred" direction is not defined—the two sets of employees are exposed to opposite signals—so the outcome cannot be oriented without arbitrarily privileging one side. Dueling measures are held out rather than dropped silently.

Because the outcome is oriented by firm stance, it is the affirmative share where the firm supports and one minus the affirmative share where it opposes. Stance is fixed within a measure, so this orientation is absorbed by the measure fixed effects in every reported specification. It does not absorb an interaction between a precinct's standing disposition toward ballot measures and the direction of firm stance. The adjoining-pair specification is immune to that by construction, since both members of a pair share a measure and therefore a stance.

G.4 Exposure, Outcome, and Worker Placement

The unit is the precinct × ballot-measure cell. Exposure is the net worker share: for precinct p and measure m, I count workers placed in p employed by firms mobilizing on m, sign each firm's contribution by its stance, net the signed counts, and divide by official ballots cast on the measure in that precinct.

Workers are placed into precinct polygons by spatial join on residential address rather than by matching administrative precinct labels, which are not consistently coded across sources. Placement succeeds for effectively the entire linked workforce.

The outcome is the firm-preferred share of ballots cast on the measure. Both the affirmative and the total count come from the official returns in Table G1; the denominator is ballots cast on that measure, not turnout and not registration, so undervotes are excluded from both. Table G4 reports the adjoining-pair estimate under two alternative registration-based denominators.

Because exposure and outcome are both shares, the estimated slope is in percentage points of firm-preferred vote share per percentage point of exposure. A one-unit change in exposure would be 100 percentage points, far outside the support of the data, so the coefficient must not be read as a per-unit effect. Exposure among exposed precincts is typically a fraction of one percentage point.

G.5 Joins and Screening

Linking official returns to the voter panel requires matching each authority's precinct code to the panel's county and precinct identifiers. The direct join succeeds for roughly three-quarters of cells. The residual is code formatting rather than genuine non-overlap—differences in zero-padding, punctuation, and ward suffixes—and is recovered per state either by fuzzy matching within county or by a crosswalk between precinct definitions.

Precincts enter the estimation sample only where panel coverage of the official registered electorate is adequate. Where an official registered total is published, I retain cells whose ratio of panel registrants to official registrants exceeds 0.8. Cells failing the screen are excluded, never zero-filled: treating an under-covered precinct as a zero-exposure precinct would manufacture the very contrast the design tests.

A further restriction is imposed by the inference procedure rather than by coverage. Conley standard errors are computed at the precinct centroid, so a cell whose precinct has no centroid geometry cannot enter a specification that uses them. Such cells are dropped before estimation rather than carried with a missing coordinate, which leaves 169,701 precinct × measure cells across 45,173 precincts and 31 measures. Of those precincts, 31,198 are observed on more than one measure.

G.6 Specifications

Table 6 reports three specifications. The first two are levels designs on precinct × measure cells and differ in what they hold fixed; the third compares adjoining precincts. They answer different questions and are not three estimates of one parameter.

Levels, measure fixed effects. Column (1) estimates equation (2) on 169,701 cells with measure effects λ_m alone. The slope is +4.13 (SE 0.47). Measure effects remove average support differences, so the comparison is across precincts voting on the same measure. The specification does not absorb stable precinct characteristics: residential sorting and other fixed differences in precinct composition can generate the association, which is why it is not read as a causal effect of workplace exposure on vote choice.

Levels, county × measure fixed effects. Column (2) replaces λ_m with a county × measure effect, so the comparison is between precincts in the same county voting on the same measure. The slope falls to +0.48 (SE 0.24, $p = 0.046$). Most of the column (1) association is therefore between counties rather than within them, and the within-county comparison is an order of magnitude smaller. The specification defines 4,746 county × measure groups, of which 1,833 contain the exposure variation that identifies the slope, covering 107,265 cells and 66.5% of ballot weight; 1,221 of the remaining 2,913 are singletons.

Adjoining pairs. Column (3) estimates equation (3) on 164,740 nonzero within-measure pair differences, drawn from the precinct adjacency graph of the relevant geometry layer and oriented so that i is the higher-exposure member. The slope is +0.20 (SE 0.10, $p = .054$). Both members are observed on the same measure, so anything constant within a measure—including the firm's stance and hence the outcome's orientation—differences out. Pairs whose members have identical exposure contribute no information about the slope and are excluded. Each pair is weighted by the smaller of its two ballot counts, so a pair is credited at the precision of its thinner side.

This design absorbs *less* of the exposure variation than column (1), not more. Its distinctive property is immunity to anything held fixed within a measure, not greater precision, and its estimates are correspondingly less sharp relative to their size.

G.7 Inference

Standard errors are Conley spatial heteroskedasticity- and autocorrelation-consistent, computed at the precinct centroid. I report no clustered standard errors. The dependence that matters in precinct data is spatial rather than administrative: adjoining precincts resemble one another whether or not they fall in the same county, and a county cluster captures that dependence neither within counties nor across county lines.

The main table reports the 5 km Bartlett bandwidth. Table G2 reports 10 and 25 km under both Bartlett and uniform kernels for every specification, so each estimate's sensitivity to the assumed correlation range is visible rather than a matter of the author's choice. Point estimates are identical across those columns; only the standard error varies.

As a second check I resample spatial blocks with replacement, using a grid at the Conley bandwidths rather than administrative units, and recompute the estimate on each draw. Grid blocks are used deliberately: blocking on counties would reintroduce the assumption that county boundaries track the spatial dependence, which is the assumption the move to Conley standard errors rejects. Table G3 reports the resulting intervals beside the Conley ones.

I do not report permutation inference. A within-county permutation of exposure destroys the spatial autocorrelation that exposure exhibits under the null as well as under the alternative, which makes the resulting null distribution too narrow and the associated p-values anti-conservative—in this sample by several orders of magnitude relative to both Conley and the block bootstrap.

Table G2: Conley Standard Errors at Alternative Bandwidths and Kernels

		Bartlett			Uniform		
Column	Specification	5 km	10 km	25 km	5 km	10 km	25 km
(1)	Precinct × measure, measure FE	0.472	0.627	0.826	0.653	0.815	1.031
(2)	Precinct × measure, county-by-measure FE	0.239	0.307	0.375	0.318	0.394	0.416
(3)	Adjacent pair, measure FE	0.104	0.106	0.108	0.104	0.111	0.111

Notes: Conley spatial heteroskedasticity- and autocorrelation-consistent standard errors for every column of Table 6, at three bandwidths under both kernels. Every reported specification includes measure fixed effects. County-by-measure fixed effects in column (2) restrict the levels comparison to precincts in the same county voting on the same measure. Reporting the full grid rather than one bandwidth makes each estimate's sensitivity to the assumed correlation range visible rather than a matter of choice. Point estimates are unchanged across columns; only the standard error varies.

Table G3: Spatial Block-Bootstrap Robustness

Specification	Estimate	10 km blocks 95% CI	10 km blocks p	25 km blocks 95% CI	25 km blocks p	Observations	Measures
Precinct cells, measure fixed effects	+4.126	[+3.025, +5.484]	$<$ .001	[+2.617, +5.875]	.001	169,701	31
Precinct cells, county-by-measure fixed effects	+0.477	[-0.147, +1.179]	.158	[-0.288, +1.289]	.225	169,701	31
Adjacent pairs, measure fixed effects	+0.201	[+0.036, +0.454]	.055	[+0.022, +0.456]	.063	164,740	31
Both precincts exposed, measure fixed effects	+0.227	[+0.036, +0.495]	.051	[-0.005, +0.505]	.081	90,871	26

Notes: Each row resamples spatial grid blocks with replacement for 999 replications and refits its reported fixed effects in every draw. The first three rows apply this inference check to columns (1)–(3), respectively, of Table 6. The final row restricts adjoining pairs to cases in which both precincts contain at least one mobilizing-firm worker, a pure dose comparison that removes the exposed-versus-unexposed margin; it uses the measure-fixed-effects specification corresponding to column (3).

Table G4: Exposure-denominator robustness for adjoining precincts

Sample	Exposure denominator	Estimate (SE)	N
Arm-specific sample	Official ballots cast	0.201 (0.104)	164,740
	Year-t linked registrants	0.656 (0.166)	163,468
	Closest-vintage L2 roster	0.146 (0.209)	163,534
Common sample	Official ballots cast	0.205 (0.106)	163,164
	Year-t linked registrants	0.656 (0.166)	163,164
	Closest-vintage L2 roster	0.146 (0.210)	163,164

Notes: Entries are exposure slopes; Conley spatial-HAC standard errors using a Bartlett kernel and 5 km bandwidth are in parentheses. The adjoining-pair estimate is a common slope with measure fixed effects and corresponds to baseline adjoining-pair column (2). Exposure is divided by the listed denominator, but regression weights remain official ballots cast in every row (the smaller count across pair members). The common sample retains only source-aware adjoining-pair rows observed in all three arms. The L2 denominators are roster-based proxies and are not election-day registration counts. A one-percentage-point exposure change represents different worker counts under different denominators, so coefficient magnitudes are not directly comparable across rows; the robustness check concerns sign, inference, and sample stability.

G.8 Measures in the Vote-Direction Sample

The vote-direction analysis rests on a smaller and differently selected set of ballot questions than the turnout analysis, so I enumerate it rather than characterize it. The binding requirement is that the firms mobilizing on a measure be on one side of it: where firms funded both the support and the opposition campaigns, there is no single corporate direction for a precinct to lean toward, and the measure is dropped rather than assigned one.

The precinct vote-direction exhibit (the pooled within-measure dose gradient, cast × contemporaneous-address arm) is estimated on **31 one-sided statewide ballot measures** across seven states (2016–2024). A measure enters the pooled sample when it meets all four of the following criteria:

1. **One-sided corporate mobilization.** A statewide measure with observed mobilizing-firm worker exposure on exactly *one* side. Measures with observed worker exposure on both sides (dueling) are listed below for transparency but excluded from the reported directional estimates.
2. **Official precinct returns.** Official precinct-level yes/no returns for the measure are obtainable from the state election authority.
3. **Geometry / returns-to-geometry join.** A precinct polygon layer for the measure's year exists and the returns can be joined to it, so worker residences place into precincts.
4. **Matched-worker placement coverage.** The state carries a linked worker panel with non-trivial placed mobilizing-worker coverage in the measure's precincts.

Table G5 lists the included measures, grouped by state. The committed `grad_IV_cast` manifest records two distinct steps. Sidedness indicates whether mobilizing-firm worker exposure is observed on one or both ballot sides. For a one-sided measure, *corporate side* is sign($\sum IVX$), summed over rows with nonmissing IVX, `yes_share`, and `log_cast`, and positive `cast`. The estimator uses the same sign. `yes_share` determines only whether a row is eligible; the vote outcome does not assign corporate side. A measure can therefore have contributing firms on both sides but observed worker exposure on only one side. For California, the *precinct cells* column reports the proposition-specific live manifest `source_cells` count. For other states it reports the number of official-return precinct cells for the measure (town–ward cells for Massachusetts).

Table G5: Ballot measures in the precinct vote-direction analysis (one-sided pooled sample)

State	Year	Measure	Corporate side	Precinct cells
CA	returns: CA SWDB (statewidedatabase.org)			
CA	2020	Proposition 14	Support (yes)	17,572
CA	2020	Proposition 17	Support (yes)	17,572
CA	2020	Proposition 18	Support (yes)	17,569
CA	2020	Proposition 19	Support (yes)	17,563
CA	2020	Proposition 20	Oppose (no)	17,556
CA	2020	Proposition 23	Oppose (no)	17,564
CA	2020	Proposition 25	Oppose (no)	17,556
GA	returns: GA SoS (sos.ga.gov historical)			
GA	2016	Amendment 2	Support (yes)	2,549
GA	2018	Amendment 1	Support (yes)	2,557
GA	2022	Referendum A	Support (yes)	2,704

continued on next page

Table G5 continued

State	Year	Measure	Corporate side	Precinct cells
MA	returns: MA SEC (electionstats.state.ma.us)			
MA	2016	Question 1	Oppose (no)	2,173
MA	2016	Question 2	Support (yes)	2,173
MA	2018	Question 1	Oppose (no)	2,172
MA	2018	Question 3	Support (yes)	2,172
MA	2022	Question 3	Support (yes)	2,320
MA	2022	Question 4	Support (yes)	2,320
MA	2024	Question 1	Support (yes)	2,321
MA	2024	Question 2	Oppose (no)	2,321
MA	2024	Question 4	Support (yes)	2,321
MA	2024	Question 5	Oppose (no)	2,321
NC	returns: NC SBE (dl.ncsbe.gov)			
NC	2016	HB 943 (primary)	Support (yes)	2,696
ND	returns: ND SoS (results.sos.nd.gov)			
ND	2024	Initiated Measure 4	Oppose (no)	383
NV	returns: NV SoS (nvsos.gov/electionresults)			
NV	2016	Question 2	Support (yes)	1,588
NV	2016	Question 3	Support (yes)	1,587
NV	2020	Question 1	Support (yes)	1,729
NV	2022	Question 3	Support (yes)	1,511
NV	2024	Question 6	Support (yes)	1,283
NV	2024	Question 7	Support (yes)	1,282
OK	returns: OK SEB (oklahoma.gov/elections)			
OK	2016	State Question 780	Support (yes)	1,782
OK	2016	State Question 781	Support (yes)	1,782
OK	2016	State Question 792	Support (yes)	1,782

Sample size. The 31 measures decompose as CA 7, OK 3, GA 3, NV 6, NC 1, ND 1, MA 10. Nevada's six arrive as two build stems (2016/2020 and 2022/2024) that pool to a single Nevada block. Pooled across all measures the one-sided sample carries 170,783 precinct–measure cells in the live cast-weighted, contemporaneous-address script-68 artifact.

Excluded measures. Dueling measures (12), excluded from the reported directional estimates. These carry observed mobilizing-firm worker exposure on both sides, so no single firm-preferred outcome can orient the pooled gradient: CA: Proposition 15 (2020), Proposition 16 (2020), Proposition 21 (2020), Proposition 22 (2020); MA: Question 4 (2016), Question 1 (2020), Question 1 (2022), Question 2 (2022); NV: Question 1 (2016), Question 3 (2024); OK: State Question 777 (2016), State Question 779 (2016).

No mobilizing workers (4). These Massachusetts questions have precinct geometry and returns but zero placed workers at a mobilizing firm, so they contribute no identifying variation and are dropped: MA: Question 3 (2016), Question 2 (2018), Question 2 (2020), Question 3 (2024).

Geometry- or returns-blocked (not built). A further set of candidate statewide measures could not be built because a precinct polygon layer for the measure's year is unavailable, the returns-to-geometry join is infeasible, or the measure sits on a primary or off-year ballot without an acquired geometry: ID: HJR 5 (2016), Proposition 2 (2018), SJR 102 (2022); MD: Question 2 (2020), Question 4 (2022); MT: I 185 (2018), LR 128 (2018); ND: Initiated Constitutional Measure 1 (2018), Initiated Constitutional Measure 3

(2018); NV: Question 3 (2018); OK: State Question 788 (primary) (2018), Question 802 (primary) (2020), Question 805 (2020); VA: Question 1 (2016). The substantively notable case is Idaho SJR 102 (2022, Micron; all-oppose), blocked at the 2022 geometry layer. (An earlier skip log listed 13 geometry-missing measures concentrated in OK/NC/ID; the frozen build subsequently recovered the Oklahoma 2016, North Carolina, and Nevada 2016/2020 measures, so the current not-built set is the inventory-minus-built residue above.)

H Ballot Measure Inventory

Table H1 lists the 264 statewide ballot measures identified by the case-identification pipeline—every measure that drew a contribution from a linkable corporate donor—spanning 39 states and election years 2016–2024. For each measure, the table reports total contributions for and against, the number of matched employer–voter firms contributing to the measure, and the electoral outcome. Measure descriptions were researched via Ballotpedia; contribution data are from OpenSecrets.

Table H1: Ballot Measures in the Estimation Sample. Table includes the 264 measures for which at least one firm–ballot-measure episode produced matched treated and control workers. "Firms" counts corporate contributors matched to Revelio Labs employer records. Dollar totals include all non-individual contributors, which includes contributors who do not appear in the empirical sample.

State	Measure	Year	Description	$ For	$ Against	Firms	Out
AK	MEASURE 001	2016	Allow voter registration via permanent fund dividend application.	$1.2M	—	6	P
AK	MEASURE 001	2018	Establish stricter salmon habitat protections and environmental permits.	$1.7M	$12.0M	29	F
AK	MEASURE 001	2020	Increase taxes on North Slope oil production significantly.	$1.5M	$18.8M	7	F
AK	MEASURE 002	2020	Replace primaries with open top-four system and ranked-choice voting.	$13.7M	$553K	4	P
AK	BALLOT MEASURE 001	2022	Call a convention to amend Alaska's constitution.	$62K	$4.6M	2	F
AL	AMENDMENT 002	2016	Protect state park funds from reallocation to general budget.	$62K	—	2	P
AL	AMENDMENT 001 (PRIMARY)	2020	Change state board of education from elected to appointed.	$476K	—	10	F
AR	ISSUE 003	2016	Remove 5% cap on state-issued economic development bonds.	$386K	$29K	28	P
AR	ISSUE 006	2016	Legalize marijuana for medical use with state regulation.	$1.7M	$426K	5	P
AR	ISSUE 005	2018	Raise minimum wage from $8.50 to $11.00 over three years.	$1.5M	$155K	3	P
AR	ISSUE 001	2020	Make permanent the temporary 0.5% sales tax for transportation.	$2.3M	$3K	33	P
AR	ISSUE 002	2022	Require 60% voter approval for constitutional amendments.	$1.3M	$2.0M	1	F
AR	ISSUE 004	2022	Legalize recreational marijuana for adults age 21+.	$14.1M	$2.8M	8	F
AZ	PROPOSITION 205	2016	Legalize marijuana for adults 21+; establish regulation system.	$6.5M	$8.4M	38	F
AZ	Proposition 127	2018	Increase renewable energy standards to 50% by 2030	$25.3M	$41.1M	6	F
AZ	Proposition 207	2020	Legalize recreational marijuana for adults 21+	$6.0M	$1.1M	28	P
AZ	Proposition 208	2020	3.5% income tax on earnings above $250,000 for education	$22.8M	$4.3M	13	P
AZ	Proposition 310	2022	0.1% sales tax for 20 years to fund fire districts	$563K	—	2	F
AZ	Proposition 134	2024	Require signature distribution across 30 legislative districts for initiatives	$584K	—	1	F
AZ	Proposition 138	2024	Allow restaurants to pay tipped workers 25% below minimum wage	$25K	$5.8M	1	F
AZ	Proposition 139	2024	Constitutional right to abortion before fetal viability	$35.3M	$1.3M	3	P
AZ	Proposition 140	2024	Open primary with all candidates competing; possible ranked-choice voting	$16.9M	$150K	4	F
AZ	Proposition 312	2024	Property tax refunds for non-enforcement of public nuisance laws	$1.1M	—	12	P
CA	Proposition 51	2016	$9 billion bond for K-12 and community college facilities	$11.3M	—	53	P

Continued on next page

Table H1 continued

State	Measure	Year	Description	$ For	$ Against	Firms	Out
CA	Proposition 52	2016	Extend hospital fees for Medi-Cal indefinitely without regular reauthorization	$15.3M	$11.1M	5	P
CA	Proposition 53	2016	Require voter approval for revenue bonds over $2 billion	$5.0M	$21.3M	40	F
CA	Proposition 55	2016	Extend income tax increase on earnings over $250K for 12 years	$61.1M	$2K	5	P
CA	Proposition 56	2016	Increase excise tax on cigarettes and tobacco products	$38.5M	$71.2M	20	P
CA	Proposition 57	2016	Parole consideration for nonviolent felons; judges decide juvenile prosecutions	$24.5M	$1.9M	14	P
CA	Proposition 58	2016	Allow multilingual education; repeal English-only immersion requirement	$5.0M	—	13	P
CA	Proposition 60	2016	Mandate condoms in adult films; perform testing and licensing	$4.7M	$555K	7	F
CA	Proposition 61	2016	Limit state prescription drug purchases to VA pricing standards	$19.2M	$109.3M	32	F
CA	Proposition 62	2016	Repeal death penalty; convert to life without parole	$15.6M	$5.0M	15	F
CA	Proposition 63	2016	Background checks for ammo; ban large-capacity magazines over 10 rounds	$4.1M	$743K	2	P
CA	Proposition 64	2016	Legalize recreational cannabis for adults 21+	$36.6M	$2.5M	7	P
CA	Proposition 66	2016	Accelerated death penalty appeals procedures.	$6.0M	$10.0M	8	P
CA	Proposition 67	2016	Upheld 2014 statewide single-use plastic bag ban.	$1.6M	$2.9M	16	P
CA	Proposition 1	2018	$4B housing and veterans' loans bond act.	$9.3M	—	13	P
CA	Proposition 10	2018	Repeal Costa-Hawkins; allow local rent control.	$25.8M	$65.1M	43	F
CA	Proposition 11	2018	Allow ambulance firms to require on-call paid breaks.	$30.0M	—	4	P
CA	Proposition 2	2018	$2B homelessness prevention bonds from Prop 63.	$6.8M	—	46	P
CA	Proposition 3	2018	$8.9B water conservation and habitat bonds.	$5.2M	—	29	F
CA	Proposition 4	2018	$1.5B children's hospital construction bonds.	$11.5M	—	7	P
CA	Proposition 6	2018	Repeal 2017 gas/vehicle tax increases; require voter approval for future.	$4.1M	$45.6M	51	F
CA	Proposition 68 (Primary)	2018	$4B parks, environment, water, climate bond.	$6.8M	—	46	P
CA	Proposition 69 (Primary)	2018	Lock SB 1 gas tax revenue to transportation only.	$42.2M	—	52	P
CA	Proposition 72 (Primary)	2018	Exclude rainwater systems from property tax.	$85K	—	11	P
CA	Proposition 8	2018	Cap dialysis clinic revenue; require refunds above 115% costs.	$38.8M	$111.5M	5	F
CA	Proposition 13 (Primary)	2020	$15B K-12 and college facilities bond.	$15.6M	—	27	F
CA	Proposition 15	2020	Tax commercial property at market value; fund schools/local govt.	$81.7M	$68.1M	44	F
CA	Proposition 16	2020	Repeal Prop 209; allow race/sex-based preferences.	$26.6M	$789K	22	F
CA	Proposition 17	2020	Restore voting rights for people on parole.	$2.5M	—	22	P
CA	Proposition 18	2020	Allow 17-year-olds to vote in primaries.	$1.3M	—	22	F
CA	Proposition 20	2020	Restrict parole; increase crime charges; mandate DNA collection.	—	$21.9M	8	F
CA	Proposition 21	2020	Allow local rent control on 15+ year occupied units.	$38.7M	$78.2M	43	F
CA	Proposition 22	2020	Classify app drivers as contractors, not employees.	$205.1M	$19.4M	9	P
CA	Proposition 23	2020	Mandate dialysis clinic physician, infection reporting, closure permits.	$11.7M	$105.2M	5	F
CA	Proposition 25	2020	Repeal 2018 bail reform; restore risk assessment system.	$2.1M	$24.8M	5	F
CA	Proposition 1 - Right to Reproductive Freedom	2022	Constitutional amendment establishing right to abortion and contraceptives.	$17.4M	$237K	14	P
CA	Proposition 26 - Legalize Sports Betting on Tribal Lands	2022	Authorization for in-person sports betting at tribal casinos and racetracks.	$120.7M	$43.8M	29	F

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Table H1 continued

State	Measure	Year	Description	$ For	$ Against	Firms	Out
CA	Proposition 27 - Legalize Online Sports Betting	2022	Legalization of online and mobile sports betting with homelessness funding.	$169.6M	$237.8M	24	F
CA	Proposition 28 - Arts and Music K-12 Education	2022	Annual 1% K-12 funding minimum for arts and music instruction.	$10.5M	—	6	P
CA	Proposition 29 - Dialysis Clinic Requirements	2022	Physician staffing and reporting requirements for dialysis clinics.	$16.2M	$74.6M	5	F
CA	Proposition 30 - Electric Vehicle and Wildfire Tax	2022	1.75% income tax on earnings above $2 million for green infrastructure.	$50.9M	$30.8M	6	F
CA	Proposition 31 - Flavored Tobacco Products Ban	2022	Referendum on 2020 law banning flavored tobacco, menthol, vape pods.	$48.1M	$2.2M	3	P
CA	Proposition 1 - Behavioral Health Bond (March 2024)	2024	$6.38 billion bond for mental health treatment and homeless housing.	$23.6M	$1.5M	35	P
CA	Proposition 2 - Public Education Facilities Bond	2024	$10 billion bond for K-12 and community college facility improvements.	$14.6M	—	39	P
CA	Proposition 3 - Right to Marry Amendment	2024	Constitutional amendment protecting right to marry regardless of gender.	$3.9M	—	5	P
CA	Proposition 32 - $18 Minimum Wage Initiative	2024	Phased increase of minimum wage to $18 per hour by 2026.	$1.0M	$855K	3	F
CA	Proposition 33 - Local Rent Control Authority	2024	Allow cities and counties to enact rent control without state restrictions.	$50.8M	$124.4M	8	F
CA	Proposition 35 - Managed Care Organization Tax Authorization	2024	Permanent tax on health insurance plans for Medi-Cal services.	$53.7M	—	17	P
CA	Proposition 36 - Drug and Theft Crime Penalties	2024	Increase penalties for drug possession and theft; add treatment-mandated felonies.	$14.8M	$7.9M	25	P
CA	Proposition 4 - Safe Drinking Water, Wildfire Prevention, and Climate Bond	2024	$10 billion bond for water, wildfire, and climate infrastructure.	$14.9M	—	6	P
CA	Proposition 5 - Lower Supermajority for Local Bonds to 55%	2024	Reduce local bond vote requirement from two-thirds to 55% for housing and infrastructure.	$29.3M	$28.7M	20	F
CO	Amendment 071	2016	Supermajority requirement and geographic signature distribution for constitutional amendments	$24.8M	$2.0M	40	P
CO	Amendment 072	2016	Tobacco tax increase by $1.75 per pack of cigarettes	$2.4M	$11.9M	24	F
CO	Amendment 69 - ColoradoCare State Health Care System	2016	Proposed universal single-payer health care system for Colorado.	$603K	$5.1M	47	F
CO	Amendment 70 - $12 Minimum Wage	2016	Phased increase of minimum wage to $12 per hour by 2020.	$5.3M	$2.3M	28	P
CO	Proposition 106	2016	Legalize medical aid-in-dying for terminally ill patients	$11.0M	$2.8M	4	P
CO	Proposition 107	2016	Establish presidential primary elections and allow unaffiliated voter participation	$6.0M	$71K	24	P
CO	Proposition 108	2016	Open primary elections to unaffiliated voters	$6.0M	$71K	24	P
CO	Amendment 073	2018	Progressive income tax and corporate tax increase to fund K-12 education	$1.5M	$4.2M	20	F
CO	Amendment 074	2018	Require compensation for property value reductions from state regulation	$11.2M	$8.2M	7	F
CO	Amendment Y	2018	Create independent commission for congressional redistricting	$6.6M	—	7	P
CO	Amendment Z	2018	Create independent commission for state legislative redistricting	$6.6M	—	7	P
CO	Proposition 109	2018	$3.5 billion transportation bond for roads and bridges	$779K	$7.2M	55	F
CO	Proposition 110	2018	Sales tax increase and bonds for multimodal transportation	$7.2M	$30K	54	F
CO	Proposition 112	2018	2,500-foot setback for new oil and gas development	$1.5M	$44.5M	55	F
CO	Proposition CC	2019	Allow state to retain revenue above TABOR limit	$4.6M	$1.1M	9	F
CO	Proposition DD	2019	Legalize and tax sports betting; fund water projects	$3.9M	—	8	P
CO	Amendment 077	2020	Allow local voter approval to expand casino games and increase bet limits	$4.6M	—	7	P

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Table H1 continued

State	Measure	Year	Description	$ For	$ Against	Firms	Out
CO	Amendment B	2020	Repeal Gallagher Amendment; freeze residential property tax rate	$7.8M	$722K	11	P
CO	Proposition 114	2020	Require reintroduction and management of gray wolves	$2.2M	$1.0M	1	P
CO	Proposition 118	2020	Paid family and medical leave insurance program funded by payroll tax.	$9.0M	$800K	23	P
CO	Proposition EE	2020	Tax increase on cigarettes, tobacco, and nicotine products.	$5.0M	$4.5M	9	P
CO	Proposition 119	2021	LEAP program for out-of-school enrichment funded by marijuana sales tax increase.	$3.0M	$84K	4	F
CO	Proposition 120	2021	Reduce property tax rates and retain $25 million TABOR surplus.	$1.6M	—	1	F
CO	Proposition 123	2022	Dedicate income tax revenue to affordable housing funding programs.	$6.6M	$10K	11	P
CO	Proposition 125	2022	Allow wine sales in grocery and convenience stores.	$14.6M	$903K	18	P
CO	Proposition 126	2022	Allow third-party alcohol delivery from retailers and restaurants.	$14.6M	$903K	18	F
CO	Proposition HH	2023	Property tax relief, backfill local governments, and new revenue cap.	$3.4M	$2.1M	7	F
CO	Proposition 131	2024	All-candidate primary and ranked-choice voting for general elections.	$19.1M	$650K	1	F
CT	Amendment 001	2018	Lockbox restricting Special Transportation Fund to transportation uses.	$85K	—	5	P
CT	Amendment 002	2018	Require two-thirds legislative vote to transfer state properties.	$180K	—	1	P
FL	Amendment 001	2016	Consumer protections for solar energy ownership and rooftop systems.	$26.4M	$514K	6	F
FL	Amendment 002	2016	Medical marijuana legalization with patient/caregiver regulations.	$5.7M	$3.5M	6	P
FL	Amendment 004	2016	Property tax exemption for solar and renewable energy devices.	$2.6M	—	7	P
FL	Amendment 002	2018	Cap nonhomestead property tax assessment increases at 10%.	$11.0M	$110K	1	P
FL	Amendment 003	2018	Voter approval required for casino gambling authorization.	$46.2M	$16.7M	12	P
FL	Amendment 002	2020	Raise minimum wage to $15 per hour over six years.	$6.2M	$686K	18	P
FL	Amendment 003	2020	Replace closed primaries with top-two open primaries for state offices.	$7.7M	—	1	F
FL	AMENDMENT 003	2024	Marijuana legalization for adults 21+	$133.2M	$33.2M	27	F
FL	AMENDMENT 004	2024	Right to abortion before fetal viability	$109.8M	$11.0M	13	F
FL	Amendment 002	2024	Constitutional right to hunt and fish using traditional methods.	$1.2M	$131K	17	P
GA	AMENDMENT 001	2016	State intervention in failing public schools	$3.4M	$5.2M	1	F
GA	AMENDMENT 002	2016	Safe Harbor for sexually exploited children fund	$531K	—	18	P
GA	AMENDMENT 001	2018	Outdoor recreation sales tax to land conservation	$609K	—	8	P
GA	REFERENDUM A	2022	Timber equipment exempt from property taxes	$461K	—	8	P
HI	SB 2922	2018	Surcharge on residential investment properties for education	$888K	$1.3M	9	P*
ID	HJR 005	2016	Veto-proof legislative oversight of administrative rules	$107K	—	8	P
ID	PROPOSITION 002	2018	Medicaid expansion to 133% federal poverty level	$1.7M	$60K	8	P
ID	SJR 102	2022	Legislature authority to call special session amendment	—	$794K	14	P
IL	HJRCA 036	2016	Transportation funds lockbox amendment	$3.8M	—	36	P
IL	SJR 001	2020	Allow graduated income tax rates	$62.3M	$61.3M	11	F
KY	AMENDMENT 002	2024	Allow state funding for non-public education	$5.2M	$10.3M	2	F

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Table H1 continued

State	Measure	Year	Description	$ For	$ Against	Firms	Out
LA	AMENDMENT 001	2017	No property tax on properties under construction	$533K	—	6	P
LA	AMENDMENT 002	2018	Require unanimous jury verdicts for felonies	$2.1M	—	3	P
LA	AMENDMENT 001	2021	State and local streamlined sales tax commission	$69K	—	6	F
LA	AMENDMENT 002	2021	Reduce maximum individual income tax rate	$69K	—	6	P
MA	QUESTION 002	2016	Charter school enrollment cap expansion proposal	$27.0M	$17.2M	5	F
MA	QUESTION 004	2016	Legalize and regulate recreational marijuana	$6.8M	$3.1M	17	P
MA	QUESTION 001	2018	Establish maximum patient assignments per nurse	$12.0M	$25.0M	3	F
MA	QUESTION 003	2018	Uphold law protecting transgender people in public accommodations	$4.5M	$442K	33	P
MA	QUESTION 001	2020	Vehicle data access for independent repair shops	$25.0M	$26.6M	30	P
MA	QUESTION 001	2022	4% tax on income over $1 million for education/transport	$31.9M	$14.0M	12	P
MA	QUESTION 002	2022	Require dental insurers spend 83% on patient care	$10.0M	$9.5M	6	P
MA	QUESTION 003	2022	Increase statewide alcohol retail license limits	$983K	—	12	F
MA	QUESTION 004	2022	Allow driver licenses without citizenship verification	$3.5M	$187K	10	P
MA	QUESTION 002	2024	Eliminate MCAS test as graduation requirement	$15.4M	$5.4M	3	P
MA	QUESTION 005	2024	Eliminate tipped minimum wage; phase in full minimum	$1.8M	$3.1M	42	F
MD	QUESTION 002	2020	Legalize sports betting for education funding	$5.5M	—	7	P
MD	QUESTION 004	2022	Legalize recreational marijuana for adults 21+	$450K	—	10	P
ME	QUESTION 002	2016	3% income tax surcharge on incomes over $200k	$3.9M	$505K	11	P
ME	QUESTION 004	2016	Raise minimum wage to $12 by 2020	$4.0M	$183K	14	P
ME	QUESTION 001	2017	Allow casino or slot machines in York County	$13.5M	$723K	4	F
ME	QUESTION 001	2018	Tax payroll/non-wage income for universal home care	$1.7M	$1.5M	27	F
ME	QUESTION 001 (JULY PRIMARY)	2020	High-speed internet infrastructure bond.	$134K	—	10	P
ME	QUESTION 001 (MARCH PRIMARY)	2020	Religious and philosophical vaccination exemptions referendum.	$461K	$943K	7	F
ME	QUESTION 001	2021	Stop the New England Clean Energy Connect transmission line.	$27.7M	$79.5M	5	P
ME	QUESTION 001	2023	Voter approval required for state borrowing above $1 billion.	$662K	—	1	P
ME	QUESTION 003	2023	Create consumer-owned Pine Tree Power Company utility.	$715K	$28.4M	4	F
ME	QUESTION 004	2023	Right to repair for vehicles; require diagnostic data access.	$4.9M	$117K	9	P
ME	QUESTION 002	2024	$25 million research and development bond for science/tech.	$109K	—	15	P
ME	QUESTION 004	2024	$30 million bond for trail design, development, maintenance.	$143K	—	5	P
MI	PROPOSAL 18-1	2018	Legalizes recreational marijuana for adults 21+.	$4.0M	$2.7M	11	P
MI	PROPOSAL 001	2020	Expand state and local park fund usage.	$905K	—	2	P
MI	PROPOSAL 001	2022	Limit legislature to 12 combined years; require financial disclosures.	$1.3M	$142K	11	P
MN	AMENDMENT 001	2024	Extend lottery revenue dedication for environment through 2050.	$1.4M	—	1	P
MO	CONSTITUTIONAL AMENDMENT 003	2016	Increase cigarette tax 60 cents over 4 years for early childhood.	$13.9M	—	10	F
MO	PROPOSITION A (PRIMARY)	2018	Right-to-work law veto referendum.	$6.6M	$29.3M	12	F
MO	Proposition D	2018	Gas tax increase and transportation infrastructure funding.	$4.9M	—	41	F

Continued on next page

Table H1 continued

State	Measure	Year	Description	$ For	$ Against	Firms	Out
MO	Amendment 002 (August Primary)	2020	Medicaid expansion to adults earning up to 138% of poverty.	$14.3M	$112K	20	P
MO	Amendment 003	2020	Redistricting, lobbying, and campaign finance reforms.	$158K	$7.4M	5	P
MO	Question 3	2022	Legalized recreational marijuana for adults age 21+.	$8.4M	—	20	P
MO	Amendment 002	2024	Legalized sports betting statewide with 10% wagering tax.	$45.6M	$14.2M	10	P
MO	Amendment 006	2024	Court fees to fund law enforcement salaries and benefits.	$247K	—	1	F
MS	Ballot Measure 001	2020	Medical marijuana amendment (Initiative 65/Alternative 65A).	$7.6M	$338K	5	P*
MT	I-181	2016	Biomedical research bonds for brain disease cures.	$420K	$732K	7	F
MT	I-185	2018	Tobacco tax increase funding Medicaid expansion.	$9.8M	$17.5M	10	F
MT	I-186	2018	Denied permits for hard rock mines requiring perpetual water treatment.	$1.8M	$5.6M	6	F
MT	LR-128	2018	6-mill property tax for Montana University System.	$1.9M	$4K	12	P
NC	HB 943 (Primary)	2016	$2 billion bond issuance for infrastructure and economic development.	$1.7M	$3K	35	P
ND	Initiated Constitutional Measure 001	2018	Ethics commission, transparency, conflicts of interest, foreign contribution ban.	$836K	$457K	5	P
ND	Initiated Constitutional Measure 003	2018	Legalized recreational marijuana for adults 21+.	$94K	$344K	2	F
ND	Initiated Measure 004	2024	Prohibited property taxes on assessed real property value.	$198K	$2.0M	9	F
NE	Initiative 429	2020	Constitutional amendment authorizing racetrack gambling	$7.3M	$3.5M	6	P
NE	Initiative 430	2020	Authorize gambling operations at licensed racetracks	$7.3M	$3.5M	6	P
NE	Initiative 431	2020	20% tax on racetrack gambling revenue	$7.3M	$3.5M	6	P
NE	Amendment 001	2022	Allow local governments to develop commercial air service	$284K	—	21	P
NE	Initiative 432	2022	Require photo ID for voting	$2.2M	$77K	3	P
NE	Initiative 433	2022	Increase state minimum wage to $15/hour	$3.1M	$51K	5	P
NE	Initiative 436	2024	Require employers to provide paid sick leave	$3.5M	—	5	P
NE	Referendum 435	2024	Repeal education scholarship/school choice program	$1.5M	$7.6M	5	P
NJ	Public Question 001	2020	Legalize recreational marijuana	$2.0M	$10K	5	P
NM	Bond Measure C	2016	$141 million in higher education bonds	$300K	—	3	P
NV	Question 001	2016	Require background checks for firearm purchases	$17.4M	$6.7M	7	P
NV	Question 002	2016	Legalize recreational marijuana	$3.6M	—	13	P
NV	Question 003	2016	Open electricity markets; eliminate utility monopoly	$3.4M	$910K	3	P
NV	Question 003	2018	Prohibit state-sanctioned electric utility monopolies	$34.2M	$64.0M	7	F
NV	Question 001	2020	Remove Board of Regents from state constitution	$1.3M	—	2	F
NV	Question 003	2022	Implement top-five ranked-choice voting	$23.0M	$2.4M	2	P
NV	QUESTION 003	2024	Top-Five Ranked-Choice Voting and Open Primaries	$29.0M	$3.3M	2	F
NV	QUESTION 006	2024	Right to Abortion until Fetal Viability	$13.1M	—	2	P
NV	QUESTION 007	2024	Voter Identification Requirement for Voting	$1.8M	$60K	2	P
NY	PROPOSAL 001	2017	Constitutional Convention Automatic Referendum	$1.0M	$4.1M	15	F
NY	PROPOSAL 001	2022	Environmental Bond Act for Climate Investment	$4.6M	—	6	P
NY	PROPOSAL 001	2024	Equal Protection Amendment Expanding Anti-Discrimination Protections	$7.3M	$8.6M	2	P

Continued on next page

Table H1 continued

State	Measure	Year	Description	$ For	$ Against	Firms	Out
OH	ISSUE 001	2018	Drug and Criminal Justice Sentencing Reductions	$17.6M	$1.7M	8	F
OH	ISSUE 001 (PRIMARY)	2018	Congressional Redistricting Procedures Amendment	$231K	—	4	P
OH	ISSUE 001	2022	Bail Determination Based on Public Safety Amendment	$466K	—	3	P
OH	ISSUE 001	2023	Right to Reproductive Freedom Including Abortion	$62.2M	$57.9M	4	P
OH	ISSUE 001 (SPECIAL)	2023	60% Vote Requirement for Constitutional Amendments	$36.8M	$22.0M	9	F
OH	ISSUE 002	2023	Legalization of Recreational Marijuana	$5.4M	$920K	18	P
OH	ISSUE 001	2024	Citizens Redistricting Commission for Fair Maps	$43.0M	$7.4M	8	F
OK	STATE QUESTION 777	2016	Right to Farm and Ranch Constitutional Amendment	$1.6M	$2.5M	8	F
OK	STATE QUESTION 779	2016	One Percent Sales Tax Increase for Education	$7.2M	$896K	21	F
OK	STATE QUESTION 780	2016	Drug and Property Crime Reclassification to Misdemeanors	$4.7M	$4K	4	P
OK	STATE QUESTION 781	2016	Rehabilitative Programs Fund from Prison Cost Savings	$4.7M	—	4	P
OK	STATE QUESTION 792	2016	Beer and Wine Sales in Retail Stores Amendment	$5.3M	$12K	5	P
OK	STATE QUESTION 788 (PRIMARY)	2018	Medical Marijuana Legalization and Regulation	$305K	$1.3M	22	P
OK	STATE QUESTION 793	2018	Optometrists and Opticians in Retail Establishments	$4.6M	$2.4M	4	F
OK	QUESTION 802 (PRIMARY)	2020	Medicaid Expansion to 133% Federal Poverty Level	$3.0M	$311K	4	P
OK	QUESTION 805	2020	Criminal History Sentencing Enhancement Prohibition	$2.5M	$303K	2	F
OR	MEASURE 097	2016	Corporate Minimum Tax Increase on Large Businesses	$18.6M	$22.3M	80	F
OR	MEASURE 098	2016	State Funding for Dropout Prevention and College Readiness	$10.5M	—	9	P
OR	MEASURE 099	2016	Outdoor School Education Fund	$2.3M	—	6	P
OR	MEASURE 100	2016	Ban on wildlife trafficking and endangered species sales	$3.2M	—	4	P
OR	MEASURE 101 (SPECIAL)	2018	Tax on hospital and insurer premiums for Medicaid expansion	$9.4M	$393K	14	P
OR	MEASURE 102	2018	Allow municipal bonds for private affordable housing development	$11.4M	—	18	P
OR	MEASURE 103	2018	Constitutional prohibition on future grocery taxes	$6.3M	$14.3M	14	F
OR	MEASURE 104	2018	Require three-fifths vote for revenue through tax changes	$2.8M	$11.7M	23	F
OR	MEASURE 105	2018	Repeal state law barring immigration enforcement aid	$235K	$14.2M	11	F
OR	MEASURE 106	2018	Constitutional ban on public funding for abortion	$608K	$14.8M	4	F
OR	MEASURE 108	2020	Increase tobacco/e-cigarette taxes for health treatment	$10.7M	$56K	17	P
OR	MEASURE 111	2022	First state constitutional right to healthcare	$2.4M	—	2	P
OR	MEASURE 112	2022	Remove slavery exception from criminal punishment	$2.6M	—	5	P
OR	MEASURE 114	2022	Firearm permit requirement and magazine capacity limits	$4.7M	$190K	2	P
OR	MEASURE 118	2024	Corporate sales tax with resident rebate distribution	$830K	$15.0M	65	F
RI	QUESTION 004	2024	$53 million environmental and recreational infrastructure	$216K	—	6	P
SD	AMENDMENT R	2016	Governing boards for higher education institutions	$486K	—	21	P

Continued on next page

Table H1 continued

State	Measure	Year	Description	$ For	$ Against	Firms	Out
SD	INITIATED MEASURE 022	2016	Creates ethics commission and publicly funded campaigns	$1.7M	$739K	6	P
SD	INITIATED MEASURE 023	2016	Right-to-work modification for non-union fees	$991K	$332K	10	F
SD	CONSTITUTIONAL AMENDMENT W	2018	Campaign finance and lobbying governance amendment	$1.0M	$231K	1	F
SD	INITIATED MEASURE 025	2018	Tobacco tax increase funds technical education	$1.0M	$6.0M	28	F
SD	CONSTITUTIONAL AMENDMENT A	2020	Marijuana legalization amendment (overturned)	$2.4M	$249K	2	P*
SD	CONSTITUTIONAL AMENDMENT C (PRIMARY)	2022	Supermajority vote requirement for tax measures	$906K	$2.8M	5	F
SD	CONSTITUTIONAL AMENDMENT D	2022	Medicaid expansion to 138% of poverty level	$5.0M	—	5	P
SD	CONSTITUTIONAL AMENDMENT H	2024	Top-two primary elections system	$1.6M	$136K	1	F
SD	INITIATED MEASURE 028	2024	Prohibit food and grocery sales taxes	$71K	$262K	3	F
SD	REFERRED LAW 021	2024	CO2 pipelines regulation referendum	$2.9M	$264K	2	F
TX	PROPOSITION 002	2017	Home equity loan regulations amendment	$1.6M	—	11	P
TX	PROPOSITION 005	2019	Sporting goods tax revenue for parks and wildlife	$753K	—	2	P
TX	PROPOSITION 002	2021	Infrastructure bonds for blighted areas	$520K	$5K	1	P
TX	PROPOSITION 001	2023	Right to farming and ranching agriculture	$1.1M	—	4	P
TX	PROPOSITION 005	2023	Rename and fund Texas University research	$1.5M	—	4	P
UT	PROPOSITION 002	2018	Medical marijuana legalization	$945K	$1.1M	7	P
UT	PROPOSITION 004	2018	Independent redistricting commission	$2.4M	—	5	P
UT	QUESTION 001	2018	10-cent gas tax for education and roads	$2.7M	—	16	F
VA	QUESTION 001	2016	Constitutionalize state right-to-work protections	$37K	—	2	F
WA	INITIATIVE 1464	2016	Public campaign financing via democracy credits	$4.2M	$28K	2	F
WA	INITIATIVE 1491	2016	Court orders preventing firearm access for high-risk persons	$3.3M	—	2	P
WA	INITIATIVE 732	2016	Nation's first carbon tax; reduce sales tax 1%	$2.9M	$1.5M	22	F
WA	INITIATIVE 1631	2018	Carbon emissions fee with environmental funding	$15.2M	$31.6M	19	F
WA	INITIATIVE 1634	2018	Ban local taxes on groceries; corporate-backed	$22.4M	$113K	6	P
WA	INITIATIVE 1639	2018	Raise minimum age and add restrictions for semiautomatic rifles	$5.4M	$656K	3	P
WA	INITIATIVE 940	2018	Require de-escalation training; reform deadly force standards	$3.2M	$289K	8	P
WA	INITIATIVE 976	2019	Cap vehicle licenses at $30; eliminate transport taxes	$66K	$4.8M	47	P*
WA	REFERENDUM 088	2019	Vote on legislature's affirmative action authorization	$408K	$649K	12	F
WA	REFERENDUM 090	2020	Approve comprehensive sexual health education mandate	$1.7M	$315K	4	P
WA	INITIATIVE 2066	2024	Prevent restrictions on natural gas; protect consumer choice	$9.5M	$6.0M	7	P
WA	INITIATIVE 2109	2024	Eliminate 7% capital gains excise tax on investments	$9.5M	$10.1M	7	F
WA	INITIATIVE 2117	2024	Repeal Climate Commitment Act; ban cap-and-trade	$9.5M	$22.0M	18	F
WA	INITIATIVE 2124	2024	Allow opt-out from WA Cares insurance program	$9.5M	$19.5M	6	F
WI	QUESTION 001 (PRIMARY)	2024	Prohibit legislature delegation of appropriation authority	$17K	$4.8M	2	F

Notes: "$ For" and "$ Against" report total contributions from all donors (not limited to firms in our sample) as recorded in OpenSecrets ballot measure contribution data. "Firms" is the number of companies in our matched employer–voter sample that contributed to this measure. Outcome codes: P = Passed, F = Failed, W = Withdrawn, P* = Passed but later overturned by court. Contribution data sourced from OpenSecrets (formerly FollowTheMoney). Measure descriptions researched via Ballotpedia.

Table H2: Statewide Ballot Measures by State and Election Year

State	2016	2017	2018	2019	2020	2021	2022	2023	2024	Total
CA	14	—	11	—	10	—	7	—	9	51
CO	7	—	7	2	5	2	3	1	1	28
OR	4	—	6	—	1	—	3	—	1	15
WA	3	—	4	2	1	—	—	—	4	14
ME	2	1	1	—	2	1	—	3	2	12
MA	2	—	2	—	1	—	4	—	2	11
SD	3	—	2	—	1	—	2	—	3	11
AZ	1	—	1	—	2	—	1	—	5	10
FL	3	—	2	—	2	—	—	—	3	10
NV	3	—	1	—	1	—	1	—	3	9
OK	5	—	2	—	2	—	—	—	—	9
MO	1	—	2	—	2	—	1	—	2	8
NE	—	—	—	—	3	—	3	—	2	8
OH	—	—	2	—	—	—	1	3	1	7
AR	2	—	1	—	1	—	2	—	—	6
AK	1	—	1	—	2	—	1	—	—	5
TX	—	1	—	1	—	1	—	2	—	5
GA	2	—	1	—	—	—	1	—	—	4
LA	—	1	1	—	—	2	—	—	—	4
MT	1	—	3	—	—	—	—	—	—	4
ID	1	—	1	—	—	—	1	—	—	3
MI	—	—	1	—	1	—	1	—	—	3
ND	—	—	2	—	—	—	—	—	1	3
NY	—	1	—	—	—	—	1	—	1	3
UT	—	—	3	—	—	—	—	—	—	3
AL	1	—	—	—	1	—	—	—	—	2
CT	—	—	2	—	—	—	—	—	—	2
IL	1	—	—	—	1	—	—	—	—	2
MD	—	—	—	—	1	—	1	—	—	2
HI	—	—	1	—	—	—	—	—	—	1
KY	—	—	—	—	—	—	—	—	1	1
MN	—	—	—	—	—	—	—	—	1	1
MS	—	—	—	—	1	—	—	—	—	1
NC	1	—	—	—	—	—	—	—	—	1
NJ	—	—	—	—	1	—	—	—	—	1
NM	1	—	—	—	—	—	—	—	—	1
RI	—	—	—	—	—	—	—	—	1	1
VA	1	—	—	—	—	—	—	—	—	1
WI	—	—	—	—	—	—	—	—	1	1
Total	60	4	60	5	42	6	34	9	44	264

Notes: Counts are unique statewide ballot measures in the estimation sample (measures producing at least one matched firm–ballot-measure episode). Odd years include off-cycle state elections.

H.1 Local Ballot Measures

Table H3 lists all 39 local ballot measure episodes included in the estimation sample, spanning Washington cities, counties and school districts, Colorado counties, El Paso TX, and Kansas City MO. Washington supplies 26 of them and most of the treated local workers. The identification procedure follows three stages: (1) scraping campaign finance records, (2) verifying committee–ballot-question linkages, and (3) filtering to estimable episodes.

Campaign finance scraping Local ballot measure committees and their corporate contributors were identified from state campaign finance databases: Colorado's TRACER system (Secretary of State bulk CSV downloads) and Washington's PDC (Public Disclosure Commission Socrata API). In these two programmatic crosswalks, contributors were filtered to corporate entities, matched to Revelio Labs employment records, and required to have at least 10 linked employees and a contribution of at least $1,000. Colorado applies the employee screen to the firm's linked Colorado workforce; Washington applies it to the firm's linked total across states. Industries classified as advocacy organizations (NAICS 813xxx) and campaign service vendors (advertising, PR, management consulting) were excluded. The El Paso and Kansas City cases use separately verified, hand-assembled company lists rather than the Colorado or Washington crosswalk rules.

Committee-to-ballot-question verification Unlike statewide ballot measures—where campaign finance records link directly to specific propositions—local campaign committees are often standing organizations that fundraise continuously across election cycles. A committee's presence in a given year's campaign finance records does not guarantee that a ballot question appeared on the ballot that year. To address this, each committee–year pair was independently verified against county election records, Ballotpedia, the Colorado Secretary of State's election results archive, and local news coverage. For each entry, I confirmed: (a) the specific ballot question number, (b) a description of the measure, and (c) the election outcome. Committee–year pairs that could not be linked to a specific ballot question were excluded. This verification identified one false positive in the initial crosswalk: a standing school district committee (Jefferson County, 2022) that received corporate contributions in a year with no school ballot measure on the ballot.

Episode construction After treated and control voters were restricted to the measure's jurisdiction, Colorado and Washington candidate episodes with fewer than 20 treated voters were not sent to estimation. This preliminary catalog screen did not replace the estimator's common pre-matching rule: local and statewide episodes alike required at least 50 treated voters with valid employment records before exact matching against employed controls whose employers do not mobilize on any measure in the state.

Table H3: Local Ballot Measure Episodes with Matched Corporate Contributors. Each row is one estimated episode (ballot measure campaign × stance). "Firms" counts corporate contributors matched to Revelio Labs employer records. "Treated" is the number of matched employees at contributing firms. Episodes require ≥50 treated voters and successful propensity score matching against employed controls at non-contributing firms.

Jurisdiction	Committee	Year	Stance	$ Contrib	Firms	Treated
Adams, CO	SUPPORT ANYTHINK LIBRARIES	2018	Support	$24K	7	126
Arapahoe, CO	SAFER ARAPAHOE COUNTY	2019	Support	$86K	17	67
Jefferson, CO	KEEP JEFFCO SAFE	2019	Support	$4K	2	273
Weld, CO	GROWING GREAT SCHOOLS	2019	Support	$59K	10	302
El Paso, CO	PRIORITIES COS	2020	Support	$34K	8	50
Arapahoe, CO	PRESERVE ARAPAHOE'S OPEN SPACES	2021	Support	$34K	7	787
Mesa, CO	CITIZENS FOR D51 SCHOOLS	2021	Support	$92K	17	86
Douglas, CO	INVEST IN DCSD	2023	Support	$159K	22	85
Arapahoe, CO	CITIZENS FOR CHERRY CREEK SCHOOLS FUTURE	2024	Support	$356K	19	102
Douglas, CO	INVEST IN DCSD	2024	Support	$208K	32	504
Mesa, CO	CITIZENS FOR D51 SCHOOLS	2024	Support	$50K	11	209
Kansas City, MO	Kansas City Sports Stadium Sales Tax (Question 1)	2024	Support	$6.0M	2	159
El Paso, TX	El Paso Proposition K	2023	Oppose	$589K	6	375
Spokane County, WA	YES FOR SPOKANE COUNTY LIBRARIES	2019	Support	$1K	1	161
King County, WA	Harborview Health for All	2020	Support	$188K	16	17,158
Seattle, WA	Yes for Transit	2020	Support	$128K	15	1,496
King County, WA	Best Starts for Kids	2021	Support	$592K	27	17,129
Seattle, WA	A Better Seattle	2021	Support	$6K	6	66
Seattle, WA	Compassion Seattle	2021	Support	$760K	41	211
Spokane, WA	Avista Employees FOR Proposition 1	2021	Support	$224K	1	123
Spokane SD 081, WA	Citizens for Spokane Schools (CFSS)	2021	Support	$19K	9	54
Bellevue, WA	Livability Bellevue	2022	Support	$37K	5	1,834
Highline SD 401, WA	Yes For Highline	2022	Support	$22K	9	162
King County, WA	Conserve Our Future (Yes on KC Prop 1)	2022	Support	$82K	8	16,783
Olympia, WA	COMMITTEE TO INSPIRE OLYMPIA!	2022	Support	$20K	3	145
Redmond, WA	Yes for Redmond Safety: Sponsored by Tanika Pad...	2022	Support	$22K	3	2,185
Seattle, WA	Seattle for Election Simplicity	2022	Oppose	$6K	4	69
Seattle SD 001, WA	Schools First Coalition	2022	Support	$50K	12	73
Tacoma SD 010, WA	Tacoma Citizens for Schools	2022	Support	$86K	23	588
Whatcom County, WA	Yes for Whatcom Kids!	2022	Support	$34K	4	672

Continued on next page

Table H3 continued

Jurisdiction	Committee	Year	Stance	$ Contrib	Firms	Treated
King County, WA	Building Behavioral Health	2023	Support	$132K	12	18,936
King County, WA	Yes for Vets & Seniors	2023	Support	$68K	9	18,186
Seattle, WA	Yes for Homes	2023	Support	$387K	43	5,943
Vancouver, WA	Bring Vancouver Home	2023	Support	$73K	8	557
Seattle, WA	Keep Seattle Moving 2024	2024	Support	$338K	44	5,941
Snohomish County, WA	Safe Snohomish County	2024	Support	$6K	3	255
Spokane, WA	Yes, Safe Spokane	2024	Support	$5K	1	159
Tacoma SD 010, WA	Tacoma Citizens for Schools 2024	2024	Support	$130K	32	628
Whatcom County, WA	Washingtonians for a Sound Economy	2024	Support	$50K	23	119

The stacked difference-in-differences estimate for the 39 local episodes is reported in the election-scope decomposition of Table A1 (Appendix A), whose local-only column refits the headline specification on the local sample alone.