When do voters respond to campaign finance disclosure? Evidence from multiple election types*

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Abstract

In recent American elections political candidates have actively emphasized features of their fundraising profiles when campaigning. Yet, surprisingly, we know comparatively little about how financial information affects vote choice specifically, whether effects differ across types of election, and how robust any effects are to other relevant political signals. Using a series of conjoint experiment designs, I compare the effects of campaigns' financial profiles on vote choice across direct democratic and representative elections, randomizing subjects' exposure to additional political cues. I find that while the financial profile of candidates can affect vote choice, these effects are drowned out by non-financial signals. In ballot initiative races, the explicit policy focus of the election appears to swamp any effect of financial information. This paper is the first to explore the comparative effects of financial disclosure across election type, contributing to our understanding of how different heuristics interact across electoral contexts.

*This version: October 5, 2021. This project was funded by an ESRC (Grand Union DTP) research training support grant and studentship. I would like to thank Andy Eggers, Raymond Duch, Jan Stuckatz, Nelson Ruiz, Sönke Ehret, and attendees of the 2019 LSE-Oxford Postgraduate conference for their insightful comments. A previous version of this paper was circulated under the title "Knowledge or Ignorance? Assessing the Causal Effects of Campaign Finance Disclosure on Vote Choice." The experiments in this paper were conducted in collaboration with the Centre for Experimental Social Sciences (CESS) at Nuffield College, University of Oxford. All experiments in this paper received ethical clearance from the Departmental Research Ethics Committee at the University of Oxford (SSH_DPIR_C1A_19_008) and the CESS Ethics Committee (OE_0027).

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In recent US elections, candidates have actively publicized aspects of their campaign finance profiles including the average size of donations, the proportion of small donations, and the local nature of their financial support. For example, in 2016, Bernie Sanders repeatedly campaigned on the fact his average donation was $27 (Bump, 2016). In the early stages of the 2020 presidential race, Elizabeth Warren tweeted "I won't take a dime of PAC money in this campaign",[1] and both President Trump and Kamala Harris highlighted that over 98 percent of their campaign funds came from small contributions (Rizzo, 2019).

That candidates emphasize their own campaign finance profiles suggests they believe this information matters to voters. Yet, surprisingly, we know little about how voters actually react to information about candidates' campaign finance profiles. While theoretical works argue financial transparency is a Pareto improving features of electoral systems that grant citizens greater information over their vote choice (Coate, 2004; Ashworth, 2006), in practice, findings on the impacts of disclosure are mixed. While voters' perceptions of candidates are not immune to the effects of disclosure (Wood, 2019; Spencer and Theodoridis, 2020), the marginal informational benefits may be limited (Primo, 2013).

Several fundamental aspects of the effect of financial cues on voters' behavior remain understudied. First, few studies have examined how disclosure affects vote choice, particularly in contexts that resemble the discrete choice voters face at the ballot box (Dowling and Wichowsky, 2013). Instead, studies typically focus on separate assessments of each candidate (Ridout, Franz and Fowler, 2015; Rhodes et al., 2019; Dowling and Wichowsky, 2015), perceptions of the substantive positions of candidates or interest groups (Sances, 2013; Primo, 2013), or perceptions of corruption (Spencer and Theodoridis, 2020). However, financial signals may shift voters' perceptions about candidates without inducing changes in vote choice, which has substantial implications for the practical utility of the regulation.

[1] https://twitter.com/ewarren/status/1094286090436075521

Second, it is unclear how the effects of financial information are themselves impacted by other relevant features of the electoral context. Do financial cues affect vote choice once other highly relevant information like partisanship and political experience are revealed? While some studies explicitly control for these cues (Dowling and Wichowsky, 2015; Dowling and Miller, 2016; Rhodes et al., 2019), we know little about *how* other signals mediate the effect of campaign finance information.

Third, previous experiments focus primarily on candidate elections, but the effects of financial information may differ in other democratic races that lack explicit partisan or valence signals. Given the policy implications of direct democratic outcomes, voters may infer useful information about a policy's likely beneficiaries from campaign finance information by inferring the interests of donors (Boudreau and MacKenzie, 2021). This issue is particularly important given the prominence of ballot initiative policymaking in the United States (Bowler and Donovan, 2000), and the vast sums of money now spent for and against propositions each electoral cycle (Stratmann, 2010). This paper, to the best of my knowledge, provides the first test of the comparative effects of financial information on vote choice across representative and direct democratic elections.

In this paper, I develop a simple theoretical account of how financial cues affect vote choice given the broader informational context. In the absence of other signals, cues taken from financial disclosure can lead to meaningful shifts in the estimation of a voters' utility, and thus substantive shifts in vote choice. However, in the presence of other political signals, these financial cues may be "swamped" yielding negligible changes in vote choice. This mechanism also appears to translate to direct democratic elections: the explicit policy focus of referendums and ballot initiatives may overwhelm informative cues gleaned from a campaign's financial profile.

The primary empirical contributions of this paper are to establish whether financial signals can affect voters' choices, and how robust these potential effects are across different

contexts. Using a series of conjoint designs, I test the efficacy of financial signals across two types of election – gubernatorial and ballot initiative races. I also vary the presence of other politically-relevant signals by randomising the number of conjoint attributes displayed to respondents (Sen, 2017). Half of all subjects receive additional information about candidates' ideology, partisanship and political experience. Random assignment across conditions allows for an unconfounded analysis of whether the marginal effects of financial cues are robust to the presence of other political signals prevalent in contemporary electoral contexts.

I find that campaign finance information can have an effect on vote choice, but that these effects are swamped by other political signals. When subjects are only presented with financial cues they are less likely to choose candidates with high average donations, a majority of donations from out of the state, and relatively concentrated groups of donors. However, when candidates' ideology, partisanship, and experience are known, these effects are indistinguishable from zero – with the exception of the geographic origin of donations. Disclosure also does not appear to affect vote choice in initiative elections either. Subjects appear to have relatively fixed political views on policy issues, rendering disclosure ineffective. The results therefore provide little evidence that campaign finance information has a distinct impact on vote choice conditional on other highly-salient cues. In Section 4 I discuss the policy implications of these results.

1 The informational benefit of disclosure

In *First National Bank of Boston v. Bellotti* (1978), which struck down expenditure limits in ballot initiative races, the Supreme Court argued that disclosure allows voters to evaluate the arguments presented for and against proposed legislation, and thus bolster their ability to make informed decisions in elections – what is known as the "informational benefit" of disclosure (Jiang, 2019). More recently, in *Citizens United v. FEC* (2010), the Court opined that, since contributions are effectively a form of speech, voters should have the

right to know who is speaking because this knowledge informs voting behavior.[2]

Research on political advertisements demonstrates that disclosure can affect voter decisionmaking, specifically by inferring different intentions by different donor sources (Dowling and Wichowsky, 2013; Sances, 2013; Dowling and Wichowsky, 2015). At the aggregate campaign-level, voters appear to value transparent profiles (Wood, 2019), with disclosure influencing voters' perceptions of candidate corruption (Spencer and Theodoridis, 2020). Other work, however, finds that the marginal benefit of disclosure to voters' knowledge of interest group positions is negligible (Primo, 2013).

These studies on voter perceptions are important but leave open the question as to whether changes in perceptions translate to changes in voting behavior. Only one paper (to the best of my knowledge) directly assesses the effect of aggregate disclosure on the likelihood of voting for a candidate, finding moderate support that this additional information alters vote choice even in the presence of partisan information (Dowling and Miller, 2016).

In this section, I present a simplified theoretical motivation for how disclosure might affect vote choice by refining individuals' perceptions of the utility gain from choosing one candidate over another. I discuss several financial features of campaigns that may influence vote choice. I then show how the presence of other politically relevant signals, and the type of electoral race, may "swamp" the effects of these cues.

1.1 Financial disclosure as a heuristic

Assume, in the most abstract terms, that a voter must make a choice between two candidates with the goal of maximising their own utility. Assume further that the problem the voter faces is about estimation – given a set of signals can voters adequately estimate which candidate will make them better off?

Researching candidates and campaigns is taxing (Primo, 2013). Moreover, some factors

[2]Section B of the Supplementary Materials discusses the implications of this decision in more detail.

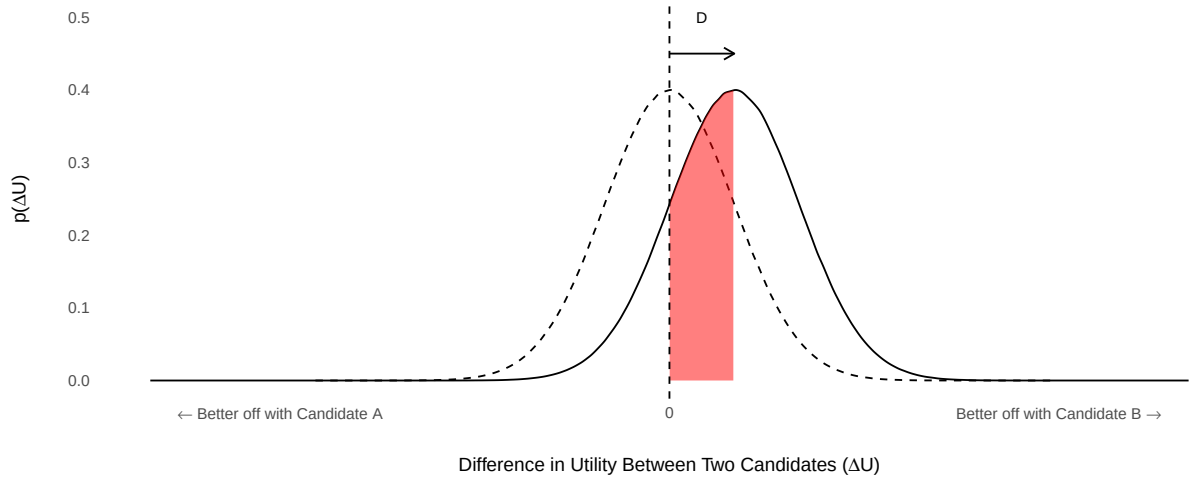
such as a candidate or campaign's competence, trustworthiness, and viability are harder to observe directly. Candidates and campaigns may even suppress information or qualities that are deemed harmful to their electoral prospects. To overcome these overly-taxing cognitive demands, individuals use information signals that enable less costly estimates of the position or valence of a campaign. These heuristics, "efficient cognitive processes ...that ignore part of the information" (Gigerenzer and Gaissmaier, 2011, p.451), often provide useful shortcuts for evaluating campaigns and political choices (Lupia, 1994).

Campaign finance information, particularly when simplified, may play the role of a heuristic device. Voters may prefer campaigns with higher total donations, for example, because it signals something about hard to observe characteristics like political viability (Wood, 2019). The "informational benefit" of disclosure, therefore, is the extent to which this information enables voters to refine (i.e. reduce uncertainty about) their estimates of candidates and campaigns along the relevant dimensions.

Suppose that, in the absence of any information, the voter is completely uncertain over which candidate will make them better off. Figure 1 captures this intuition graphically, by plotting a probability distribution of a voter's utility when choosing one candidate over another. Without informative signals, the probability of the difference in utility from choosing Candidate B over Candidate A is centred around zero (represented by the dashed probability mass in Figure 1).

Now suppose that the voter receives effective additional cues from campaign finance disclosure (labelled D in Figure 1). The effect of this financial information is to refine the individuals' evaluations of the two candidates, shifting the probability mass towards Candidate B. The red-shaded area indicates the increase in probability mass in favour of Candidate B as a result of the disclosure signal. In other words, the financial information makes it easier for the voter to discern which candidate is the optimal choice. In this hypothetical example, the change is substantial, making it much more likely that the individual will

Figure 1: The effect of financial cues (D) on a voter's estimate of the difference in utility choosing between two candidates.



vote for Candidate B.

1.2 Relevant facets of disclosure

There are various facets of financial disclosure that may act to shift a voter's utility distribution in favour of one candidate (Prat, Puglisi and Snyder Jr., 2010). Here, I focus on five aggregate aspects of a campaign's financial profile – the total dollar-amount of donations, the average donation size, the proportion of funds from the largest donor, the type of largest donor, and the origin of donations. These facets may activate different heuristic mechanisms, and affect voters' behavior in different ways.

Total donations. The total size of donations is an indication of a campaign's scale. A relatively under-funded campaign, for example, is more restricted in its ability to carry out the political functions often seen as necessary for electoral success – for example, opinion research, advertising, and get-out-the-vote operations. The total amount of campaign funding (holding constant its composition) may indicate to voters' its *viability*. Voters may use the size of the campaign as a signal of how donors, who may be more politically informed, have "pre-screened" campaigns to choose those they think are most likely to

succeed.

Alternatively, voters may be distrustful of campaigns with very large donation totals (again, holding constant the composition of the campaign). Voters may (rightly or wrongly) perceive that large amounts of money mean a campaign has as an unfair electoral advantage, and react by tempering their support for it. Theoretically, therefore, it is not clear which of these dynamics (if any) will affect voters' decisionmaking.

Average donation. Where the total size of donations gives voters an indication of a campaign's viability and/or electoral capacity, the average donation seems likely to tell voters more about the breadth of support for a campaign. Those with a low average donation can tout this as an indicator of broader political support (holding constant the total donations), or at least that the typical donor comes from comparatively limited means. Conversely, a very high average donation might indicate that narrow but well-funded interests are the predominant supporters of a campaign. It seems unlikely that the opposite effect would be true, namely that voters infer some positive quality from candidates whose average donation is very high.

Type of largest donor. The type of donor may separately signal information about what sectors of society a campaign is aligned with, and potentially valence information about the campaign. Donations may predominantly come from individuals, corporations, labor unions, or other political advocacy groups. Corporations and labor union labels likely signal pro-business and pro-employee alignment, and thus the effect of these donor types are likely to depend on a voter's pre-existing political leanings. Without these ideological cues, however, knowing a campaign is funded by a political advocacy group may indicate a high level of political organisation and support from policy elites and leaders, suggesting an effective or experienced campaign.

Separately, a growing concern in the US system is the ability of certain donors to obscure their contributions through nonprofit "501(c)(4)" entities that, as charitable organisa-

tions, are not obliged to reveal their donors (Wood, 2018; Rhodes et al., 2019; Oklobdzija, 2019). Dark money vehicles are useful primarily to exceptionally wealthy individuals and groups who wish to obscure their involvement in the political process. This obscurity makes it very difficult to highlight these donors to voters through disclosure. While a 501(c)(4) organisation will be named, it will typically be uninformative – for instance, "Americans for Prosperity". If voters are unable to infer the source of donations – because the name is withheld or nondescript – they may shift their support away from that campaign. Or, as perhaps these groups hope, the name's obscurity cancels out any potential cue to voters based on name recognition.

Proportion of funds by largest donor. Alongside the largest donor's identity, the proportion of a campaign's funds that are donated by a single donor may reflect the degree of "capture" by a particular donor or interest. Separate from the average donation, this feature explicitly encapsulates the concentration of financial support, rather than providing a signal about the base of that support. Intuitively, if voters use this cue, they may be averse to campaigns where the largest donor is responsible for a high proportion of funding, since this likely signals a campaign is beholden to a narrow set of donor interests.

Geographic origin of donations. Finally, voters may care about *where* financial support comes from. Given the federal structure of the United States, voters may be concerned about whether campaigns are funded locally or not (especially for state level races). When a majority of donations come from outside the state in question this may signal "capture" by external interests. In other words, large numbers of donations from out-of-state actors could suggest interference in a state's affairs and thus may diminish voters' willingness to support a campaign or candidate. It may also be a signal of whether candidates care about the concerns of their constituents.

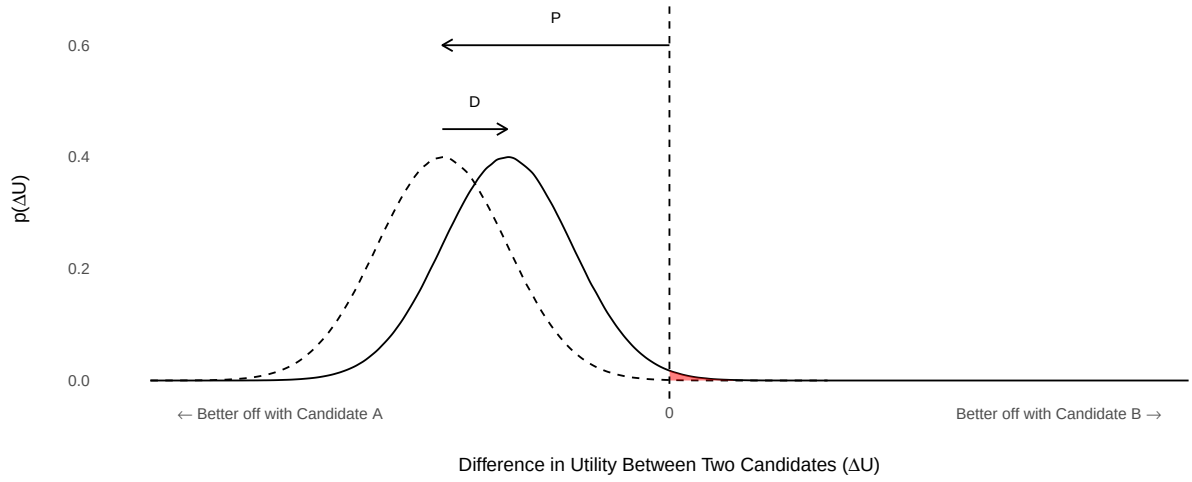
1.3 Effects of financial cues in the presence of other signals

Even if the hypothesised heuristics hold, it is not guaranteed that this information will, in real electoral contexts, affect vote choice. The scenario in Section 1.1 assumes, without financial information, the voter is equally torn between the two candidates. Therefore even relatively small shifts in the distribution yield large changes in the probability one candidate makes the voter better off. During political campaigns, however, individuals may use other heuristics to inform their vote choice. In turn, these additional signals may alter the utility distribution and thus impact the *marginal* effect of campaign finance cues.

Figure 2 illustrates that, if other political signals are particularly strong, the marginal effect of financial information on vote choice may approach zero. In this scenario, the cumulative effect of other political signals (labelled P) is much stronger than those from campaign finance information. Consequently, the same signal gleaned from the campaigns' financial profiles now results in a much smaller change in the probability that some voter would prefer candidate B over A – the increase in probability mass in favour of Candidate B (the red shaded area) is minuscule. Importantly, this occurs even though the rightward shift of the distribution as a result of disclosure is of the same size as in Figure 1. The effect of financial information on *vote choice* is much smaller because the relative importance of these signals on voters' utility is less than that of other politically-relevant cues. In other words, they *"swamp"* the effects of a campaigns' financial profile on vote choice.

The potential for campaign finance effects to be swamped is important given the wider context in which this information is revealed. In candidate campaigns, voters will receive political signals both about the experience of candidates (and other valence characteristics) as well as the ideology and/or partisanship of that candidate. If, as we would expect, these signals weigh heavily on the decisions made by voters, then the informational benefit gleaned from disclosure may not translate into changes in voter behavior. It is not that financial disclosures are uninformative, but that in the presence of other electorally-

Figure 2: Effect of financial cues (D) and other political signals (P) on a voters' estimate of the difference in utility choosing between two candidates



relevant information, inferences from campaign finance are simply less efficacious on vote choice itself.

1.4 Effects of financial signals across types of electoral campaign

The type of electoral contest may also impact the efficacy of financial disclosure. Ballot initiative elections are an important form of policymaking, in which citizens can draft and submit legislation directly to the ballot. If other political signals like partisanship swamp the effects of disclosure, then perhaps in contexts typically devoid of these cues the effects of financial information will be greater (Garrett and Smith, 2005; Briffault, 2010; Primo, 2013).[3] Boudreau and MacKenzie (2021), for example, find that campaign finance information has substantial effects on initiative *support*, although this effect is confined to high-knowledge voters. To the best of my knowledge, the comparative aspect of disclosure (between election types) has not been tested empirically before.

Voters may use financial disclosure to assess the "valence" of initiative campaigns. Since these elections ask voters to endorse or reject an item of legislation, voters may want to

[3]Parties can support/oppose initiatives, but initiatives are not labelled as 'Democrat' or 'Republican' and many issues are not explicitly supported by either party.

estimate the likelihood that the bill will achieve its stated aim. They might also be concerned that the legislation is anticompetitive, or favours out-of-state interests, even if they are supportive of the policy in general. Voters may also want to estimate the compatibility of the proposal with their existing political beliefs. Absent explicit ideological, partisan, and valence signals typically present in candidate elections, divulging information about the supporters of these policymaking efforts may be particularly informative to voters.

Conversely, while initiative campaigns typically lack overt partisan signals, the specific policy focus of the election – for example, whether to increase the minimum wage, lower prescription drug prices, or curtail state governments' taxation powers – could, in fact, override other cues in a similar way to how political signals may swamp the effects of disclosure in candidate elections. If voters' utility functions prioritise the policy content, disclosure is unlikely to make a difference.

2 Conjoint experiment to assess impact of disclosure on vote choice

To assess the causal effects of these various financial heuristics, across electoral venues and varying the presence of other information signals, I expose subjects to a series of conjoint experiments examining vote choice. Across all experiments, subjects are presented with a forced choice between two campaigns – either two candidates or the support and opposition groups for an initiative proposal.

Conjoint survey experiments are an efficient way to test the extent to which different attributes affect subjects' choices (Hainmueller, Hopkins and Yamamoto, 2014). Since these designs typically ask respondents to choose between two profiles, this discrete choice task is a natural analogue for the sorts of decisions voters make in American elections. This is useful even for ballot initiative elections where each voter makes a binary decision over

whether to endorse a policy proposal. Initiative elections typically have separate "Yes" and "No" campaigns. Opposition groups raise their own funding and play a prominent role in advocating for the status quo (Gerber, 1999), and given this involvement, there is good reason to inspect how their financial profiles affect vote choice too.

2.1 Randomizing the *number* of attributes

Conjoint designs randomly vary the content of a fixed number of attributes. This allows researchers to estimate the marginal effects of different features on respondents' choices within the experiment. The causal interpretation of these effects is defined with respect to the experimental context. That is, claims about the causal effect of any attribute only hold in situations where subjects are exposed to the same signals (and only those signals).

Claims about generalisability of any causal effect beyond the experimental context, however, require more stringent assumptions. Among other things, researchers must assume that the given set of attributes fully describe the pertinent features over which respondents make a choice. The observed causal effects may not hold up in contexts where some feature not included in the conjoint experiment also acts on individuals' behavior.

To test for differences in the effects of campaign finance information across different informational environments, I randomly assign subjects to one of two candidate conjoint experiments (Sen, 2017). Half of subjects are exposed to disclosure attributes only. The other half also see other relevant political cues: partisanship, ideology, and previous experience. Randomization across these two conditions at the subject-level ensures unconfoundedness between those presented the full set of conjoint attributes, and those presented only the disclosure cues. Comparing the estimated marginal effects for the common set of attributes shared across both groups therefore helps illustrate how robust any effects of campaign finance information are to the inclusion of other relevant signals.

2.2 Experimental protocol

All participants completed two separate conjoint experiments – one choosing candidates in a hypothetical state gubernatorial election (either with or without additional attributes), and one asking subjects to consider four initiative policy proposals. In both experiments, subjects were presented with randomized information (levels) for each facet of disclosure (attributes).

Table 1 provides details of the conjoint attribute-levels across the two experiments. The large differences in dollar-amounts are intended to clearly distinguish campaign finance profiles and provoke stronger differences in subjects' behavior. These donation totals are nevertheless broadly plausible: Beto O'Rourke's 2018 senate campaign had receipts in excess of $70 million, and Proposition 61 (2016) in California saw opposition donations exceed $100 million. It is worth noting, however, that legal contribution limits to gubernatorial candidates vary across states (no such limits exist for initiative campaigns).[4]. This variance limits the external validity of these findings - a large average donation will be implausible given some states' contribution limits, but this should not affect the internal validity of the experiment.

The experiments also used abstract descriptors for the largest donor type since, without increasing the number of attribute levels, subjects would regularly be presented with rounds in which the two campaigns have the same, named largest donor. While this is not statistically problematic, nor theoretically impossible, it might be cognitively dissonant within the experiment – particularly if it happens multiple times.[5] With abstract descriptors, displaying the same attribute-level for both profiles can conceivably be rationalised as different entities of the same type. Moreover, abstract labels are a more direct test of the *type* of donor. While a 'Political Advocacy Group' could be either left- or right-leaning

[4]Section C in the Supplementary Materials lists these limits

[5]With random assignment, 20 percent of vignettes would include the same donor label, occurring approximately 1.2 times per subject.

Table 1: Conjoint attributes and levels

Disclosure signals	
Attribute	**Level**
Total Donations	$100,000 to $200,000 $1 million to $10 million $70 million to $90 million
Average Donation	$75 $10,000 $1 million
Largest Donor	Private individual Political Advocacy Group Labor Union Corporation/Trade Association Identity not disclosed
Proportion of funding from largest donor	10% 50% 90%
Origin of donations	Majority from donors within the state Majority from donors out of state
Politically relevant signals	
Party	Democrat Republican Independent
Ideology	Very liberal Moderate liberal Centrist Moderate conservative Very conservative
Elected to previous office?	No previous elected positions Elected to state office Elected to federal office

(or neither), it nevertheless signals an organised political entity focused on advancing the electoral prospects of campaigns.[6] Since I also include ideologically-aligned groups (labor unions and corporations), this label tests whether voters infer anything from the *type* of donor separable from its alignment with a sector of society.

[6] I opted for "political advocacy" rather than "special interest" because it plausibly describes a broader set of groups.

Candidate conjoint. Respondents ($n = 390$) were presented with the funding profiles of two candidates running for gubernatorial office, with half seeing three politically relevant signals in addition to the financial cues. The exact same funding attributes and attribute-levels were used across the two candidate scenarios. Figure 3 displays an example vignette from the candidate conjoint experiment.[7]

The experiment's focus on hypothetical candidates and its simple design likely increases subjects' attention on the financial cues. In neither condition are voters aware of the candidates' gender, their policy priorities, or other contextual features that might distract from the financial signals. What matters for testing the theory of this paper is whether disclosure *can* have an effect on vote choice (which a simple design is likely going to be more sensitive to) and then how robust those effects are to a small number of politically relevant, non-financial signals. The candidate scenario without additional information is designed to assess the possible effects of disclosure, rather than the actual effect of these cues in real elections. The introduction of additional cues is a controlled way of assessing the resilience of these effects in a noisier information context.

Initiative conjoint. All participants also voted on four initiative topics, which were chosen to reflect the sorts of salient issues considered on the ballot in recent electoral cycles. The text for each initiative is shown in Table 2. These topics are, with the exception of the bond issuance initiative, reasonably ideologically clear-cut. While this is in part a consequence of choosing salient topics, this clarity also aids comparisons with the candidate conjoint where ideological cues are included. We might expect to observe larger effects for those issues that are less salient, less ideologically-aligned, or with more confusing ballot language (Lupia, 1994). The inclusion of the bond initiative provides some leverage over this dimension, but I also return to this point in the discussion.

[7]Figures D1 and D2 in the Supplementary Materials, respectively, show screenshots of the candidate conjoint with additional attributes and the initiative conjoint.

Figure 3: Example screenshot of the candidate conjoint condition without ideological, partisan, and valence attributes

In this section, you are going to be presented with the descriptions of two hypothetical candidates running for **state governor**. Again, you can imagine this sort of information as what you would see in the run up to voting in an election.

We would like you to first evaluate the two candidates, and then to indicate **which you would vote for if you had to choose**. You will also be asked to rate how strongly you approve or disapprove of each candidate, on a scale from 1-7.

The table summarises the candidate's campaign funding - including total donations, average donation, type of largest donor, the size of their contribution, and the origin of donations.

You will be asked to choose between 6 pairs of candidates.

Q56.

	Candidate A	**Candidate B**
Average Donation	$1 million	$1 million
Largest Donor	Political Advocacy Group	Political Advocacy Group
Proportion of Campaign Funds from Largest Donor	90%	90%
Origin of Donations	Majority from donors within the state	Majority from donors out of state
Total Donations	$70 million to $90 million	$70 million to $90 million

Alongside the initiative title and a brief description of the proposed policy, participants were presented with the same funding table as in the candidate conjoint, showing both the support *and* opposition campaigns. Participants were asked to consider this information and choose whether they would vote 'for' or 'against' the proposed policy. Each participant made a total of four choices within the initiative experiment – one per issue.

Randomization procedure. The order in which the candidate and initiative conjoint experiments were presented to each subject was randomized, as was assignment to the two candidate conditions. For the initiative component, the order of the issues was randomized for each subject. Finally, across all experiments, attributes were randomized with minimal restrictions to prevent implausible attribute-level combinations.[8]

Sample. The conjoint experiments were conducted using an online subject pool of adults resident in the United States operated by the Centre for Experimental Social Science, Nuffield

[8]For instance, a campaign raising $100,000-200,000 could not have an average donation of $1 million.

Table 2: Hypothetical initiative policies

Title and description
Marijuana legalisation If passed, this initiative would legalize the sale of marijuana within the state for recreational use for those aged 21 and over, subject to taxation and regulation by state authorities.
State minimum wage increase If passed, this initiative would raise the state minimum wage for adult workers to $14 per hour within two months of enactment.
Bond issuance for sewage redevelopment If passed, this initiative would authorise the state government to issue a bond worth $300 million in order to fund a sewage system redevelopment scheme, updating the sewage network within the state.
Carbon emissions tax If passed, this initiative would impose a 5% emission-based CO2 tax on the sale of all non-electric and non-hybrid vehicles, as well as an additional point-of-sale surcharge of 2 cents per litre on all fuel purchases.

College, University of Oxford. Further information on this subject pool is presented in Section A of the Supplementary Materials. Individuals were invited to participate if they were resident in a state which used the initiative process and therefore were likely to be familiar with the process. The first round of invitations was sent to those resident in California, Washington, Oregon, Arizona, Ohio, Florida, Colorado, and Massachusetts - all states with relatively high usage of the initiative process. Further invitations were then sent to those resident in the other 17 states where some form of initiative policymaking is used. In total, 390 eligible participants completed the experiment.

Table A1 in the Supplementary Materials describes the demographic composition of this sample, which is reasonably balanced in terms of gender and age. 46 percent of respondents identified (post-experiment) as Democrats, 30 percent as independents, and 15 percent as Republicans. The imbalance in party identification is to be expected given West Coast states (where initiative elections are most common) were over-sampled in the first round of invitations and these states are broadly Democratic-leaning. To further assess

the plausibility of the partisan distribution in the sample I take the average of the difference in party affiliations at the state level,[9] weighted by the proportion of respondents per state in the experimental sample. While the lean in the experimental sample is larger, the expected lean towards the Democrats is nevertheless substantial (7.3 percentage points) suggesting that Democratic bias is to be expected. Given this study tests for the causal effects of disclosure, rather than its generalisability, this Democratic lean does not affect the validity of the inference.

Since every subject completes multiple rounds of the experiment, the effective sample size is larger than the number of subjects. *Post hoc* tests of the experimental power demonstrate that, given the size of coefficients observed in the candidate conjoint, the experiments are sufficiently powered for us to attribute declines in effects across conditions to the presence of additional cues. Section H of the Supplementary Materials provides a detailed discussion of this simulation and the results. The sample size does, however, limit the conclusions that can be drawn from subgroup analyses where the number of observations in each group is smaller. In particular, with relatively few Republican respondents, comparisons of behavior between partisan identities should be treated as indicative, rather than conclusive, evidence. I discuss this further in Section 3.4.

Causal assumptions. For conjoint models to have a causal interpretation, several design assumptions must be met (Hainmueller, Hopkins and Yamamoto, 2014). Section E of the Supplementary Materials provides a detailed discussion of these criteria, and presents analyses that verify the stability of effects, no profile-order effects, effective randomization, and balanced profiles.

[9] Data taken from Gallup's 2017 summary of state party affiliation, available at https://news.gallup.com/poll/226643/2017-party-affiliation-state.aspx.

3 Results

3.1 The effects of financial cues

The first candidate conjoint presented only financial attributes to voters. To recover the marginal effect of each attribute-level, I estimate a linear probability model (LPM) that contains indicator variables for each attribute-level (excluding reference categories). The resultant coefficients reflect the average marginal component effect (AMCE) of an attribute-level on the probability of selecting a candidate, relative to the corresponding reference category. All standard errors are clustered at the individual-level since subjects (from which we take multiple observations) are sampled from a much wider population of interest.[10]

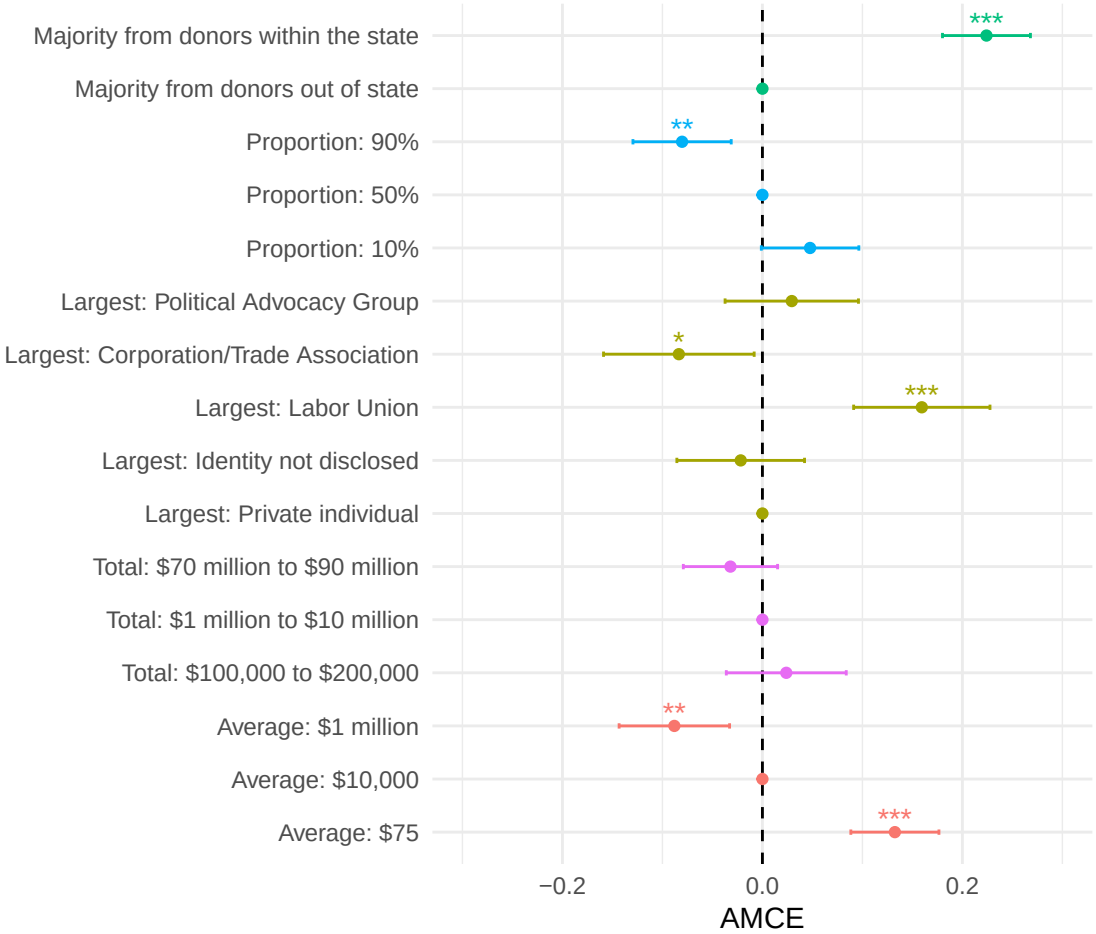
In this low-information environment, financial attributes do have clear effects on vote choice. Figure 4 plots the AMCEs for each attribute-level. Subjects are less likely to vote for candidates who receive a large proportion of their funds from a single donor, or where the average donation amount is high. Donations mainly from within the state, labor union donors, and lower average donation amounts all have a positive effect on the likelihood of a candidate being chosen.

These results show that aggregate disclosure *can* influence voter decisionmaking. In the absence of other cues, subjects are averse to instances where candidates appear to be captured by a particular interest or group. In particular, voters appear to care about political capture rather than the scale of a campaign itself. This feature is reflected in the relative importance of the geographic, average donation, and proportion attributes.

The substantively large and positive effect of the largest donor being a labor union (and the negative effect of corporations) is perhaps telling of the Democratic bias in the sample. It is nevertheless noteworthy that voters adjust their vote choice when presented with this

[10]These results are robust to using logistic rather than linear regression models. All replication code is available on Dataverse.

Figure 4: Candidate conjoint results *without* additional cues ($N = 2068$).



Coefficient estimates for each attribute-level are shown with 95 percent confidence intervals, clustered by participant. Reference categories are included as coefficients with values of zero. Stars above coefficients indicate significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ respectively.

information. The neutral "political advocacy group" label did not affect vote choice, which perhaps suggests that voters do not infer valence information about campaigns funded by organised political entities separately from those groups' ideological alignment.

Moreover, the effect of the "identity not disclosed" level is statistically insignificant (relative to individual donors). This null result suggests that voters are not immediately deterred from voting for a campaign if identifying information is withheld. This may be because financial disclosures typically preserve the anonymity of individual donors if their contributions are below a certain limit. Respondents may therefore not consider the sim-

ple lack of disclosure problematic. I return to this point in the discussion.

It is also possible that voters make inferences on the basis of multiple attributes in combination, which the present model specification would not detect.[11] The absence of modelled interaction effects would be problematic if we observed only null effects, since null results could mask significant interaction effects. However, the results demonstrate that individuals can and do parse the financial attributes separately. For four of the five attributes, there are substantial and statistically significant independent effects of the financial information. In the following section, therefore, I test whether these *independent* effects drop out in the presence of other political signals.

3.2 Priming subjects' perceptions of candidates' ideology and valence

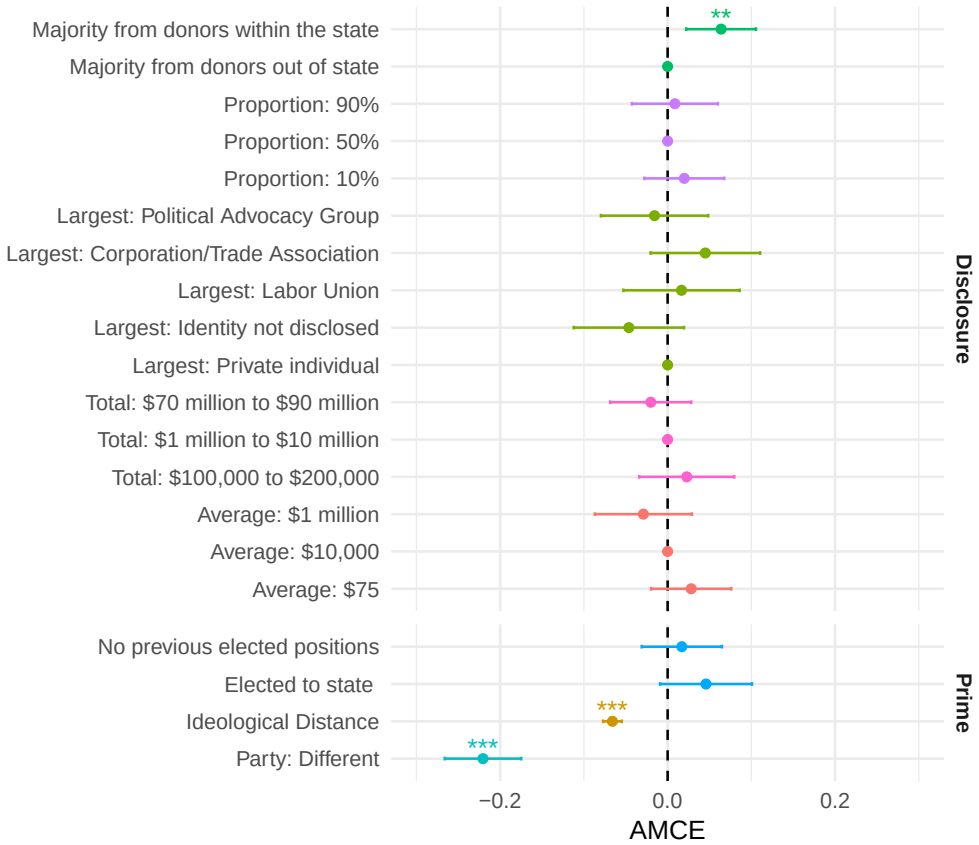
Elections are rarely fought on campaigns' financial profiles alone. Indeed, voters typically receive signals about candidates' ideology, partisanship and valence characteristics. The first set of results demonstrate the ability of disclosure mechanisms to affect vote choice. But do financial cues have an independent effect once we control for other relevant political signals?

For those subjects randomly exposed to additional political signals, I include additional parameters for partisanship, ideology, and previous office-holding in the LPM. We should expect differential effects dependent on subjects' own partisan affiliation for these attributes. I therefore compare the revealed partisanship of the candidate to that of the subject, coding whether the partisanship is the "same" or "different". Similarly, for ideology, I project the ideological factor levels evenly between (0-10) and measure the absolute difference in ideology between candidate and subject.

Figure 5 displays the results of this model. The independent effects of all but one feature of disclosure are indistinguishable from zero once subjects' political priors are primed:

[11] Adding interaction terms would require a substantially larger sample given the number of attribute-levels.

Figure 5: Candidate conjoint results *with* partisan, ideological and previous experience attributes included ($N = 2089$)



Coefficient estimates for each attribute-level are shown with 95 percent confidence intervals, clustered by participant. Stars above coefficients indicate significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ respectively.

only the positive effect of a majority of within-state donors has a significant effect on vote choice. Moreover, all previously significant coefficients in Figure 4 are statistically significantly different from those in Figure 5 ($p < 0.05$), except for the $1m average donation coefficient.[12]

The political controls in this model, however, have substantial effects. Voters unsurprisingly are averse to voting for candidates of a different party to themselves. Similarly, as the ideological distance between candidate and subject increases, this decreases the likelihood of voting for that candidate. The previous experience attribute does not exhibit significant

[12]See Section H.1 in the Supplementary Materials for more details on this test.

differences between the attribute levels.[13]

These null results are noteworthy because, without additional political signals, financial attributes had statistically significant and substantial effects on vote choice. For example, the AMCE of the labor union attribute-level is approximately one-tenth its original size. Even the strongest financial signal – having a majority of donors within the state – is reduced to a third of its size once political signals are included.

Since respondents were randomly assigned across conditions, the reduction in efficacy of the financial attributes can be attributed to the presence of other political signals. Taken together, the results suggest that while financial cues *can* affect vote choice, they are relatively inert once other political signals are present. I explore one tentative reason for this drop in efficacy in Section 3.4.

3.3 Financial cues in initiative elections

Figure 6 displays the estimated AMCEs from a LPM on the pooled observations across the four initiative topics, with issue fixed effects to control for the underlying support for each policy. The only statistically significant disclosure attribute is, again, the geographic origin of donations.[14] This attribute's effect is notably strong across all three conjoint experiments in this study. Subjects again favour those campaigns that are funded predominantly by donors within their own state. For all other attributes, however, the financial cues did not have a marginal impact on subjects' vote choice.[15]

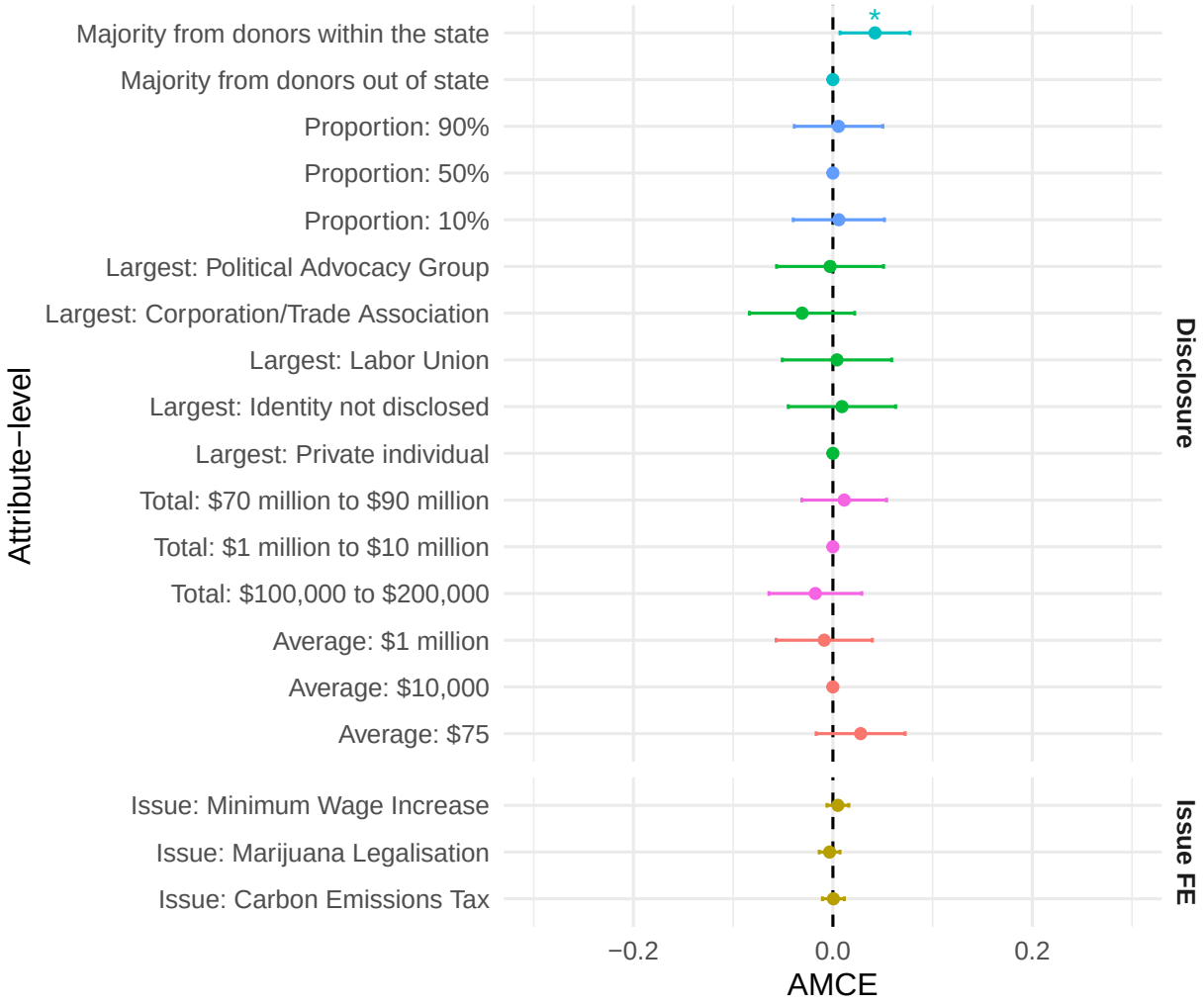
It is plausible, however, that voters use financial disclosures in different ways when considering different policy issues. To check this possibility, I estimate separate models for each initiative question. Figure 7 plots the estimated coefficients. Large campaign finance

[13]Figures F3-F5 in the Supplementary Materials present further results estimating separate models for each partisan/ideological identity.

[14]Figure F6 in the Supplementary Materials demonstrates that these results are unaffected when controlling for respondents' partisanship.

[15]See Section G in the Supplementary Materials for further analysis of subjects' support for these issues.

Figure 6: Pooled initiative conjoint results ($N = 3003$).

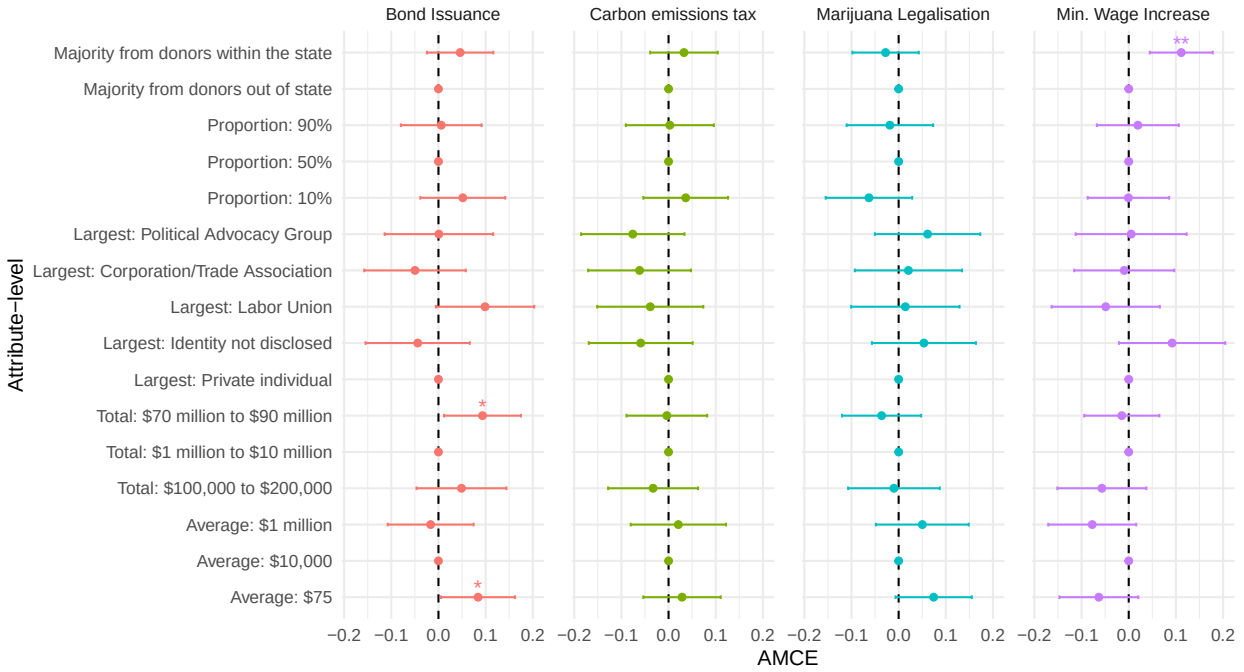


All responses across the four issues are pooled with issue fixed effects. Coefficients are shown with 95 percent confidence intervals, clustered by participant. Stars above coefficients indicate significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ respectively.

totals and a low average donation are statistically significant, positive predictors of support for the sewage bond issue. No attribute level is significant for either the marijuana legalisation or environmental taxation issues, and only the majority of within-state donations attribute is statistically significant for the minimum wage issue. These results suggest (but by no means confirm) that, for less salient issues where we might expect voters' issue preferences to be less strongly held, financial information can impact vote choice.

Figure F7 in the Supplementary Materials shows the differences in marginal means for the

Figure 7: Initiative conjoint results by issue area.



$N = 752$ (bond issuance), 750 (carbon emissions tax), 755 (marijuana legalisation), and 746 (minimum wage increase). All coefficients are shown with 95 percent confidence intervals, clustered by participant. Stars above coefficients indicate significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ respectively.

attributes between issues. On the whole, differences across the four issues are insignificant. The marginal mean of an anonymous largest donor is significantly larger for the comparison between minimum wage and bond initiatives, and labor unions significantly lower for the comparisons between minimum wage and bond initiatives as well as between marijuana and bond initiatives. All other attributes are statistically indistinguishable from zero at conventional levels of significance. These comparisons provide further evidence that, at least with the power available in this study, voters do not exhibit clear differences in how they act on disclosure information across issues.

3.4 Partisan differences

Financial cues may act differently dependent on individuals' own stance. For example, Republicans' trust in business leaders is much higher than for Democrats (Rainie, Keeter and Perrin, 2019), with corporate donors tending to be Republicans themselves (Francia

et al., 2005). Having a corporate entity as a largest donor may therefore be a positive signal for Republican voters but a negative signal for Democrats. Absent explicit cues, as is the case in the first candidate conjoint experiment, the financial cues may act as "informational equivalences" that voters use as proxies for other relevant information (Dafoe, Zhang and Caughey, 2018).

To assess this possible mechanism, I estimate LPMs on separate subsets of the data for Democrats, Republicans, and independents respectively. Figure 8 displays the coefficients for each of these three models. The typical caveats regarding subgroup analyses apply here – smaller sample sizes (particularly for Republican subjects) increase the uncertainty around the coefficient estimates.

The results do not suggest substantial differences in the size or direction of effects across partisan identities.[16] All three groups have positive, significant and substantially large effects of within-state donations, positively signed coefficients for the 10% largest donor proportion, and negatively signed or essentially zero AMCEs for the 90% donor proportion. Interestingly, for the labor union attribute-level, arguably the most overtly partisan signal, all three groups have positive AMCEs (strongly statistically significant for both Democrats and independents). There is some suggestion that the effect of labor union donations on Republicans is smaller, although with a large variance it is hard to draw concrete conclusions.

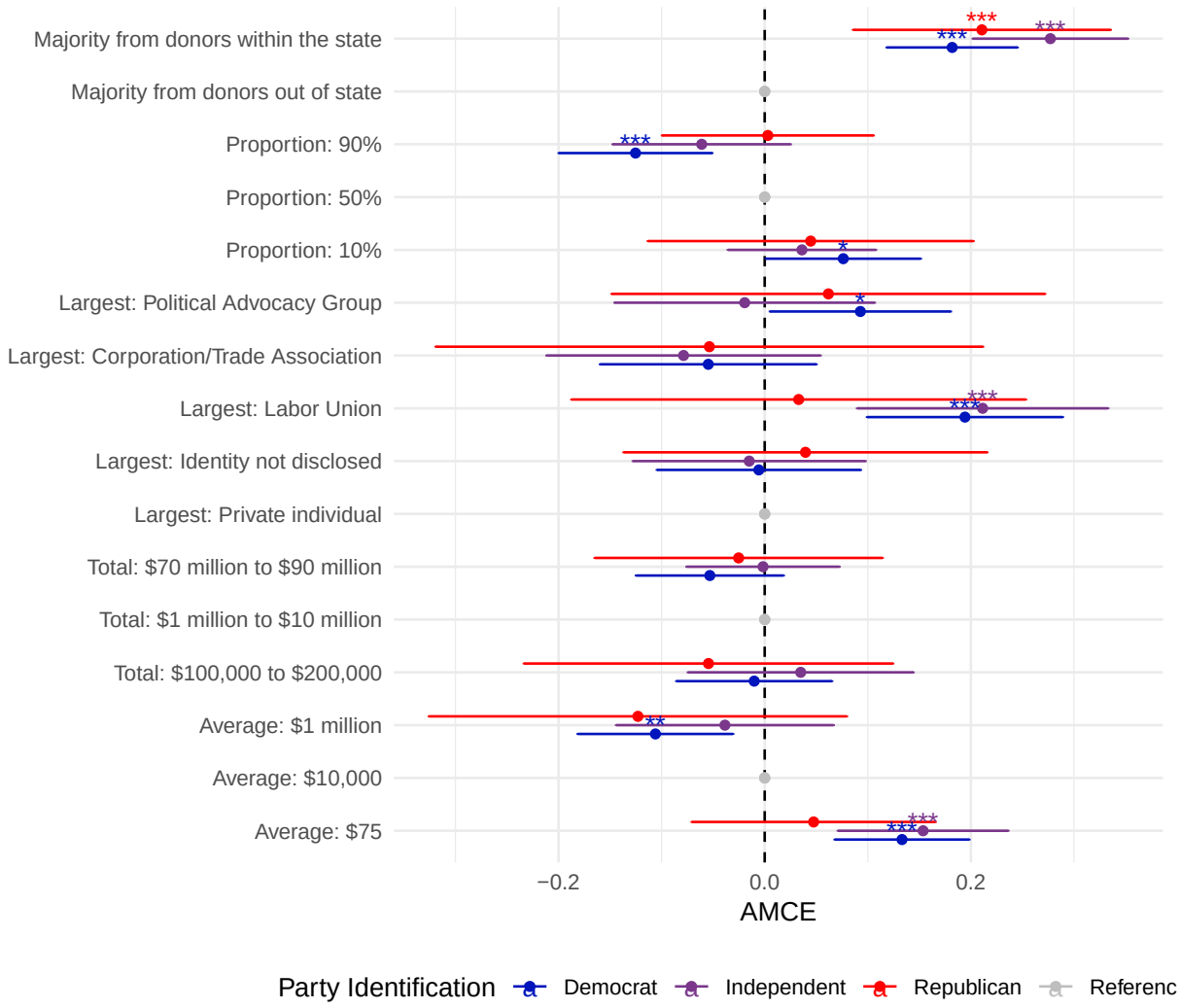
Z-scores for the differences between coefficients across models confirm the similarity suggested in Figure 8.[17] The only group-level difference that meets conventional levels of significance is that between Republicans and Democrats on the "Proportion donated by the largest donor: 90%" attribute-level ($p < 0.05$).

Figure F8 and Table F2 in the Supplementary Materials present similar results for the ini-

[16] In Supplementary Materials Figure F2 I find very similar results subsetting by ideology rather than partisanship.

[17] See Table F1 in the Supplementary Materials.

Figure 8: Comparison of estimated AMCEs across respondents' partisan identities for the candidate conjoint experiment *without* other political signals.



$N = 1070$ (Democrats), 349 (Republicans), and 718 (independents). All coefficients are shown with 95 percent confidence intervals, clustered by participant. Stars above coefficients indicate significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ respectively.

tiative conjoint. While there are some differences in the sign of coefficients across groups, the generally small effect sizes and lack of statistical significance for all but one coefficient preclude any definitive conclusions.

4 Discussion

In the absence of other relevant information, campaigns' financial profiles influence vote choice. Voters appear concerned about campaigns captured by narrow interests or out-of-state actors. This finding accords with appeals made by political candidates to their low average donation amounts in elections. Crucially, however, once subjects are (randomly) primed with other ubiquitous political cues these effects all but disappear. Disclosure appears to be equally ineffective in ballot initiative campaigns too.

One exception is the positive effect of having a majority of within-state donations, which is robust across experiments and across partisan identities. Voters appear averse to the influence of actors outside their state, even in the presence of partisan and policy signals. The resilience of this effect is notable and more research is needed on why this effect is so robust when other disclosure cues are not.

Subjects in the experiments did not appear to balk at campaigns for whom they cannot attribute a donors' identity. The "identity not disclosed" label used in this study is a comparatively weak cue, and future work should explore whether framing non-disclosure as deliberate is more impactful. More generally, this experiment cannot distinguish the effect of donating from taking a public stance on an issue or candidate. It remains to be explored whether voters have differential reactions to financial cues based on whether these donors also take visible positions on the issues or candidates in an election. Moreover, these dynamics may also depend on the *number* of groups or individuals taking public stances.

Financial cues may also be more influential for lower salience elections, races where the political differences between candidates are more limited, or where the consequences of initiative legislation are less clear. Consistent with the theory of this paper, general elections may be a "hard" environment for financial cues to impact vote choice precisely because campaigns may differ starkly on other important political dimensions. Future work may, therefore, want to consider whether the impact of disclosure differs in other contexts

like school board races or nonpartisan judicial elections, or where the complexity of the initiative wording is such that ideological implications of voters' decisions are less clear-cut. Of particular relevance to American elections, might disclosure cues have a stronger impact in primary elections where candidates share partisan labels?

From a policy perspective, ensuring transparency has become the *modus operandi* of campaign finance regulation, upheld by courts and encouraged by good government organizations. Evidence that voters react strongly against out-of-state donors suggests regulators may wish to pursue policies that emphasise this feature in elections. More generally, however, these findings question the merit of focusing on transparency as a mechanism for limiting the influence of money in elections. While this evidence does not preclude *ex ante* benefits of transparency – for example, by perturbing unethical behavior – it does challenge the assumption that the influence of donors in elections can be counterbalanced by informing voters of that influence. If policymakers cannot rely on voters using financial transparency information to alter their vote choice, prioritising transparency in reform efforts may not be worth the effort. Those in favour of reform may instead wish to pursue proactive measures that limit the extent to which donors *can* be involved, for example through public or matched funding tied to restricted fundraising activity or much tighter limits on non-profit electioneering groups' donations.

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Supplementary Materials for "When do voters respond to campaign finance disclosure? Evidence from multiple election types"

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A Subject Description

All subjects were recruited via the CESS Online US subject pool, according to the exclusion restrictions noted in the main body of the text. The CESS Online US subject pool is an online pool of participants maintained by the Centre for Experimental Social Science at Nuffield College, University of Oxford. Subjects are initially recruited into the pool via Facebook advertisements, targeting different geographic and demographic groups to ensure a more diverse set of potential subjects. Once recruited into the pool, subjects are invited via email to take part in specific experiments run by the Centre. One benefit of this pool is that subjects have pre-registered for experiments with the centre, and provided basic demographic information including their location. Recruitment emails were targeted to those who resided in states with direct democratic elections so that subjects were likely familiar, or at least aware of, the ballot initiative process. Further information about the online pools, CESS's experimental procedures and the centre more generally can be found at https://cess-nuffield.nuff.ox.ac.uk/.

Responses were collected between 18th February and 8th March 2019. Table A1 outlines key demographic information about the subjects, and Figure A1 plots the frequency of participants by their state of residence. Post data-collection, 13 respondents who answered that they lived in Alabama were excluded from all analyses since Alabama does not have the initiative process.

To ensure a similar baseline level of understanding across the sample, participants were asked to first read a passage of text describing basic features of candidate and ballot initiative elections, as well as campaign finance. Participants were told they would have to answer three factual questions related to the text, and would be remunerated for each correct answer given. On average, subjects answered 2.4 questions correctly indicating a good level of understanding about these elections having read the information.

Figure A1: Frequency of subject participation by states

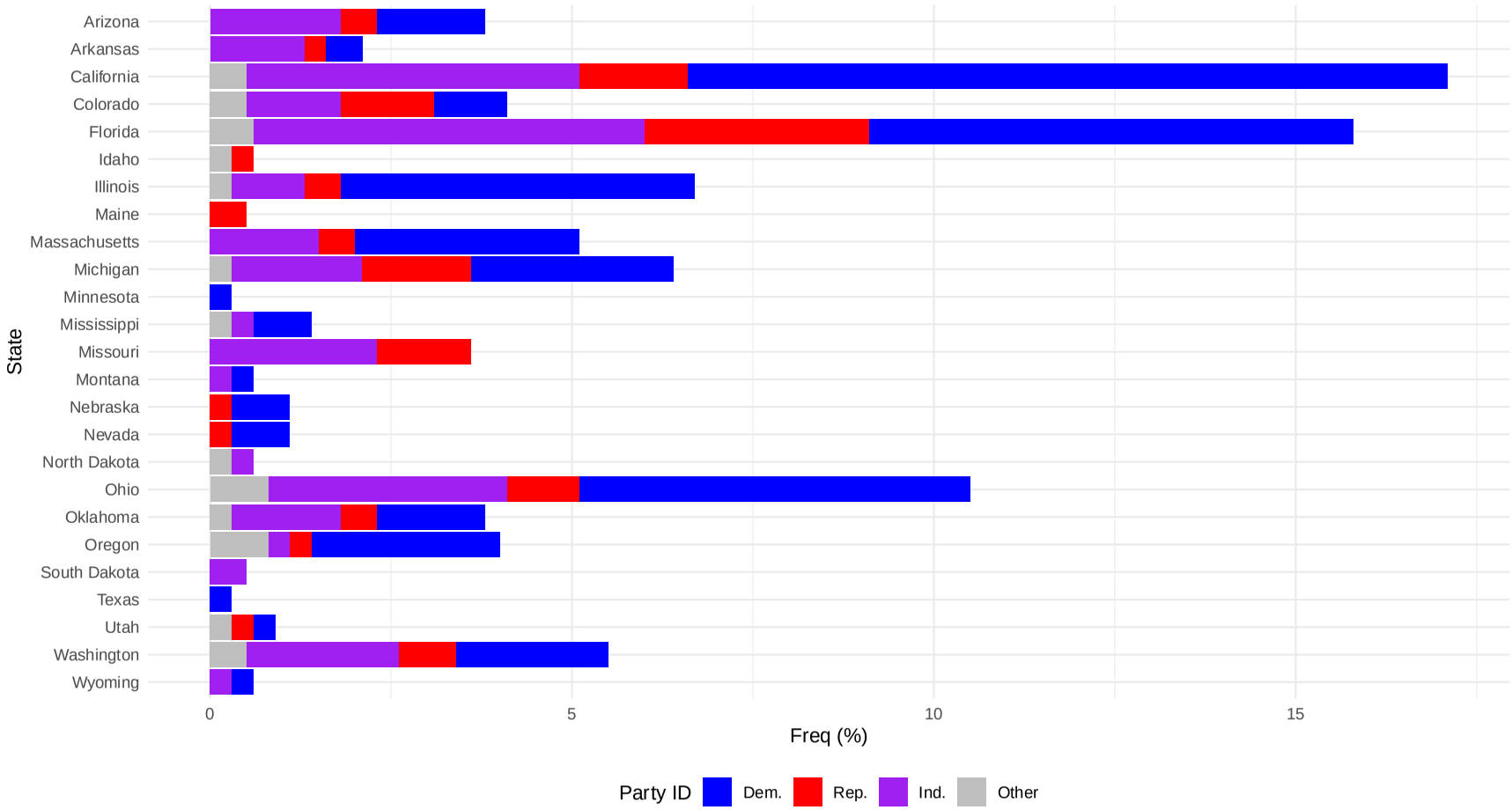


Table A1: Descriptive summary of key demographics for conjoint experiment subjects.

Variable	Value	Freq (%)
Age	*Mean*	38.00
	Standard Deviation	14.14
Gender	Female	53.80
	Male	44.90
	Other:	0.30
	Prefer not to say	0.30
	Transgender	0.80
Ethnic	American Indian or Alaska Native	1.50
	Asian	6.70
	Black or African American	8.70
	Hispanic or Latino	3.30
	Native Hawaiian or Pacific Islander	0.30
	Other	5.40
	Prefer not to say	2.10
	White	71.80
	(Missing)	0.30
Party ID	A Democrat	46.20
	A Republican	14.60
	An independent	29.70
	Other	4.10
	Prefer not to say	1.30
	(Missing)	4.10
Ideology	*Mean*	4.26
	Standard Deviation	2.39

B Citizens United and its Implications For Disclosure

The *Citizens United v. FEC* (2010) decision struck down independent expenditure limits on corporations and unions. It did so on the basis of the First Amendment, ruling that freedom of speech extends to incorporated interests. Preventing these organisations from spending on political activity was, the Court ruled, an effective but unconstitutional impediment to their political expression.

As a result of this ruling businesses, unions, and other organisations like non-profit groups can now spend unlimited amounts of money in political races, so long as it is independently coordinated from candidates. This latter clause is crucial – corporations cannot donate unlimited amounts to candidates, there are strict limits at both the federal and state level (which I outline in further detail in the following sectioin). Instead, corporations can *spend* unlimited amounts in favour of a given candidate – for example producing advertisements and signage, holding events, and distributing pamphlets.

Citizens United represents a general weakening of regulation aimed at curbing expenditure in political campaigns. Those institutions typically most able to spend comparatively large amounts of money, compared to regular voters, face fewer obstacles when trying to advocate for candidates or parties. However, businesses themselves have been wary of direct spending in campaigns, and therefore the most visible impact immediately following the ruling has been with respect to non-profit spending (Briffault, 2011).

In and of itself the ruling is not about disclosure. But the changing regulatory landscape after *Citizens United* has at least two notable implications for the disclosure regulation in the United States. First, as limits on political spending are generally weakened, the regulatory 'workload' of disclosure has increased (Jiang, 2019). Simply put, without spending limits there are fewer pre-donation regulations that limit the acitivities of various classes of actors. Thus, the relative input of disclosure on regulating activity increases.

Second, the increase in political spending by non-profit organisations specifically has implications for the transparency of campaign finance activity. As mentioned in the main text, non-profit entities registered under 501(c)(4) terms do not face the same disclosure requirements as other organisations. While these non-profits must disclose what *they* spend, they are not obliged to reveal their donation sources in the same way as candidates or candidate-affiliated PACs. As Mayer (2016) demonstrates, this can lead to complicated and convoluted networks of donations routed via 501(c)(4) organisations such that the original source of campaign spending is not revealed.

This latter feature is extremely consequential for disclosure construed in normative terms. Corporations can use hidden funding routes to spend in campaigns without being discovered, potentially misleading voters about the sources of electoral resources and who, ultimately, the benefactors are. If a donor has a negative reputation, or a corporation has a clear vested interest, strategically altering how electoral spending is presented to voters has the potential to change voter behaviour and potentially change electoral results (Wood and Spencer, 2016; Wood, 2019; Oklobdzija, 2019).

C Summary of State Campaign Finance Laws

As noted in the main text, campaign finance laws for gubernatorial candidates vary in terms of the contribution limits by state. Table C1 details the total amount individuals, PACs, corporations and unions can donate to a single candidate per year (or electoral cycle where relevant). To reiterate, contribution limits in ballot initiative races are proscribed by federal court rulings.

[18] "Mega" PAC's can contribute $10,100

[19] $5675 for small donor committees.

[20] $5675 for small donor committees.

[21] Gubernatorial candidates with more than $250,000 independent expenditures are exempt, or if opposition candidate is self-funded (spending over $250,000.)

[22] Independent PACs can contribute up to $68,000.

Table C1: Contribution limits per year for individuals, PACs, corporations, and unions. Amounts quoted are for gubernatorial candidates. Data from National Conference of State Legislatures.

State	Individual	PAC	Corporate	Union
Arizona	$5100	$5100[18]	Prohibited	Prohibited
Arkansas	$2700	$2700	Prohibited	Prohibited
California	$29200	$29,200	$29,200	$29,200
Colorado	$575	$575[19]	Prohibited	$575[20]
Florida	$3000	$3000	$3000	$3000
Idaho	$5000	$5000	$5000	$5000
Illinois	$5600[21]	$55,400	$11,100	$11,100
Maine	$1600	$1600	$1600	$1600
Massachusetts	$1000	$500	Prohibited	$500
Michigan	$6800	$6800[22]	Prohibited	Prohibited
Minnesota	$4000	$4000	Prohibited	$4000
Mississippi	Unlimited	Unlimited	$1000	Unlimited
Missouri	$2600	$2600	Prohibited	Prohibited
Montana	$1990	$10610	Prohibited	Prohibited
Nebraska	Unlimited	Unlimited	Unlimited	Unlimited
Nevada	$5000	$5000	$5000	$5000
North Dakota	Unlimited	Unlimited	Prohibited	Prohibited
Ohio	$12707.79	$12707.79	Prohibited	Prohibited
Oklahoma	$2700	$5000	Prohibited	Prohibited
Oregon	Unlimited	Unlimited	Unlimited	Unlimited
South Dakota	$4000	Unlimited	$4000	$4000
Texas	Unlimited	Unlimited	Prohibited	Prohibited
Utah	Unlimited	Unlimited	Unlimited	Unlimited
Washington	$2000	$2000	$2000	$2000
Wyoming	$2500	Unlimited	Prohibited	Prohibited

D Additional conjoint screenshots

Figure D1: Screenshot of candidate conjoint with additional information signals

	Candidate A	Candidate B
Total Donations	$1 million to $10 million	$100,000 to $200,000
Average Donation	$1 million	$75
Origin of Donations	Majority from donors out of state	Majority from donors within the state
Previously held elected office?	Elected to federal office	No previous elected positions
Party	Independent	Democrat
Ideology	Very liberal	Moderate conservative
Largest Donor	Political Advocacy Group	Labor Union
Proportion of Campaign Funds from Largest Donor	50%	10%

If you had to choose, which candidate would you vote for?

Candidate A

Candidate B

On a scale from 1 to 7, where 1 indicates that you strongly disapprove of the candidate and 7 indicates that you strongly approve of the candidate, how would you rate the two candidates?

1 = you strongly **disapprove** of the candidate
7 = strongly **approve** of the candidate

	Strongly Disapprove 1	2	3	4	5	6	Strongly Approve 7
Candidate A	○	○	○	○	○	○	○
Candidate B	○	○	○	○	○	○	○

Figure D2: Screenshot of initiative conjoint

Initiative Title: Marijuana legalisation

If passed, this initiative would legalize the sale of marijuana within the state for recreational use for those aged 21 and over, subject to taxation and regulation by state authorities.

	Support	**Opposition**
Origin of Donations	Majority from donors out of state	Majority from donors within the state
Average Donation	$75	$75
Total Donations	$100,000 to $200,000	$70 million to $90 million
Largest Donor	Private individual	Corporation/Trade Association
Proportion of Campaign Funds from Largest Donor	90%	50%

init_marij_choice. If you had to choose, would you vote for or against this initiative?

- (●) For
- ( ) Against

init_marij_rate.
On a scale from 1 to 7, where 1 indicates that you strongly disapprove of the campaign and 7 indicates that you strongly approve of the campaign, how would you rate the two sides of the campaign?

1 = you strongly **disapprove** of the campaign
7 = strongly **approve** of the campaign

	Strongly Disapprove 1	2	3	4	5	6	Strongly Approve 7
Support	( )	( )	( )	( )	(●)	( )	( )
Opposition	( )	( )	( )	(●)	( )	( )	( )

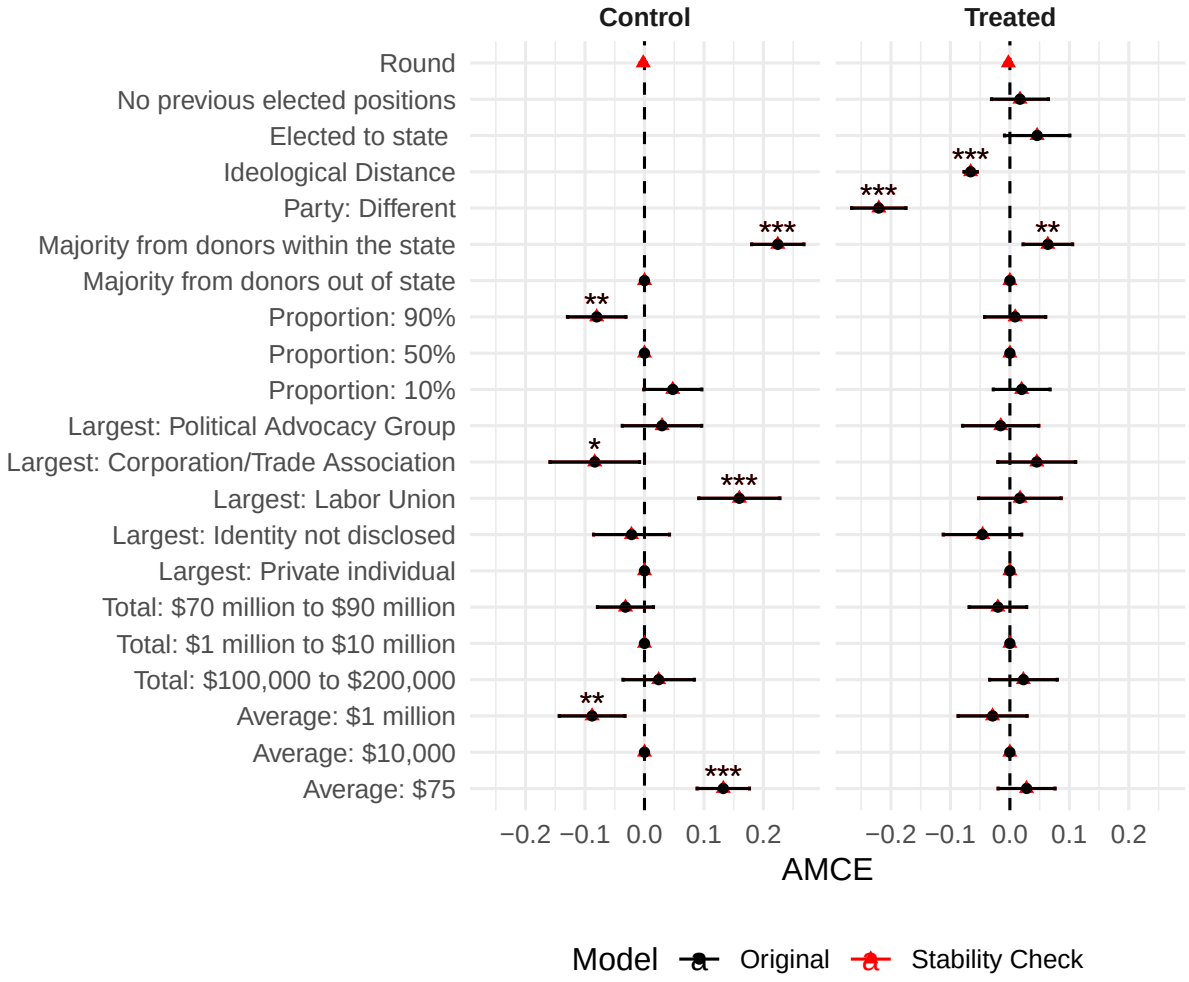
E Causal Assumptions

Stability and no carryover. In line with similar conjoint experiments about political candidates, I do not expect there to be carryover effects between rounds of the same conjoint experiment. The marginal effect of disclosing a majority of out-of-state donations, for instance, should remain stable whether it is presented in the first or last round of the experiment. To ensure this assumption holds, I reran the regressions including a numeric control variable for the round the choice-profile was presented in (1-6). When this variable is included, the coefficients of the candidate conjoint attribute-levels are substantively unchanged, nor is the round variable statistically significant – suggesting that the stability assumption holds. Figure E1 demonstrates these results compared to the original models.[23]

No profile-order effects. The profile-order assumption states that there is no distinct effect of the order of profiles within each task i.e. that any effect of a large total donations is constant whether it appears under Candidate A or B. This concern is mitigated, in part, by randomizing the order of attributes across profiles. To the extent I recover the average marginal effect by pooling across subjects and conjoint rounds, any profile-order effect (if present) should be netted-out. As a further robustness check, I regress a new model on the control-condition candidate data, interacting the disclosure variables with the profile indicator ("A" and "B"). None of the interactive terms approach conventional levels of statistical significance ($0.19 < p < 0.95$; see replication code for the full models), suggesting there is no difference whether an attribute was displayed in the first or second profile.

[23]This check assumes that the direction of any carryover-effect is uniform across attributes. Of course, the cumulative carryover effect could be statistically indistinguishable from zero whereas the marginal carryover effect for each attribute is non-zero. As a further check, one could rerun models on the subset of data for respondents' 'uncontaminated' first choice task alone. The diminished number of observations in this case, however, limits the extent to which this is a useful check.

Figure E1: Comparison of coefficients between models reported in the main text and models including a continuous variable of the conjoint round, to check for stability and any carryover effects. Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.



Randomized and atypical profiles. For the estimated marginal effects to be causally robust, the conjoint design should in theory assign non-zero probabilities to every possible vector of treatments. Across the three conjoint experiments, however, I impose a very limited set of restrictions to ensure that the conjoint profiles are plausible. Across all three experiments, I prevented profiles where the average donation exceeded the total value of donations. Furthermore, for the informational equivalence candidate conjoint, I prevented profiles where the candidate was both an "extreme" liberal (conservative) and a

Republican (Democrat). Given the limited set of restrictions imposed, the advantages of external validity and subject engagement outweighed the smaller benefits of including atypical profiles.[24]

Balanced profiles. Finally, as with any randomisation procedure, it is crucial to show that the mechanics of said randomisation in fact lead to both balanced attribute profiles and subject characteristics. Tables E1 and E2 show the proportion of times each attribute-level was displayed within the three conjoint experiments. No attribute-level was displayed a significantly higher or lower amount of times, relative to the other levels for the same attribute except for those attributes subject to restrictions. Here, it is the case that the unrestricted levels are nonetheless relatively equal in the proportion of times they were presented.

[24]Very early on in the implementation, a coding error led to a limited number of profiles displaying implausible attribute combinations (these observations were subsequently excluded from the analysis). This prompted a respondent to email the experimental administrator to point out the incomprehensibility of the profiles, suggesting omitting these profiles is indeed the correct design decision.

Table E1: Balance test: proportion of times each attribute-level was displayed to participants in the candidate conjoints

Attribute	Level	Control	Treat
Average	$1 million	0.24	0.24
Average	$10,000	0.38	0.40
Average	$75	0.38	0.36
Cand. Ideology	Centrist		0.22
Cand. Ideology	Moderate conservative		0.23
Cand. Ideology	Moderate liberal		0.24
Cand. Ideology	Very conservative		0.16
Cand. Ideology	Very liberal		0.16
Cand. Ideology	–	1.00	
Largest	Corporation/Trade Association	0.19	0.21
Largest	Identity not disclosed	0.20	0.21
Largest	Labor Union	0.21	0.19
Largest	Political Advocacy Group	0.21	0.20
Largest	Private individual	0.20	0.19
Office	Elected to federal office		0.34
Office	Elected to state office		0.34
Office	No previous elected positions		0.33
Office	–	1.00	
Origin	Majority from donors out of state	0.49	0.49
Origin	Majority from donors within the state	0.51	0.51
Party	Democrat		0.30
Party	Independent		0.37
Party	Republican		0.33
Party	–	1.00	
Prop	10%	0.35	0.33
Prop	50%	0.33	0.35
Prop	90%	0.33	0.31
Total	$1 million to $10 million	0.38	0.39
Total	$100,000 to $200,000	0.25	0.26
Total	$70 million to $90 million	0.37	0.35

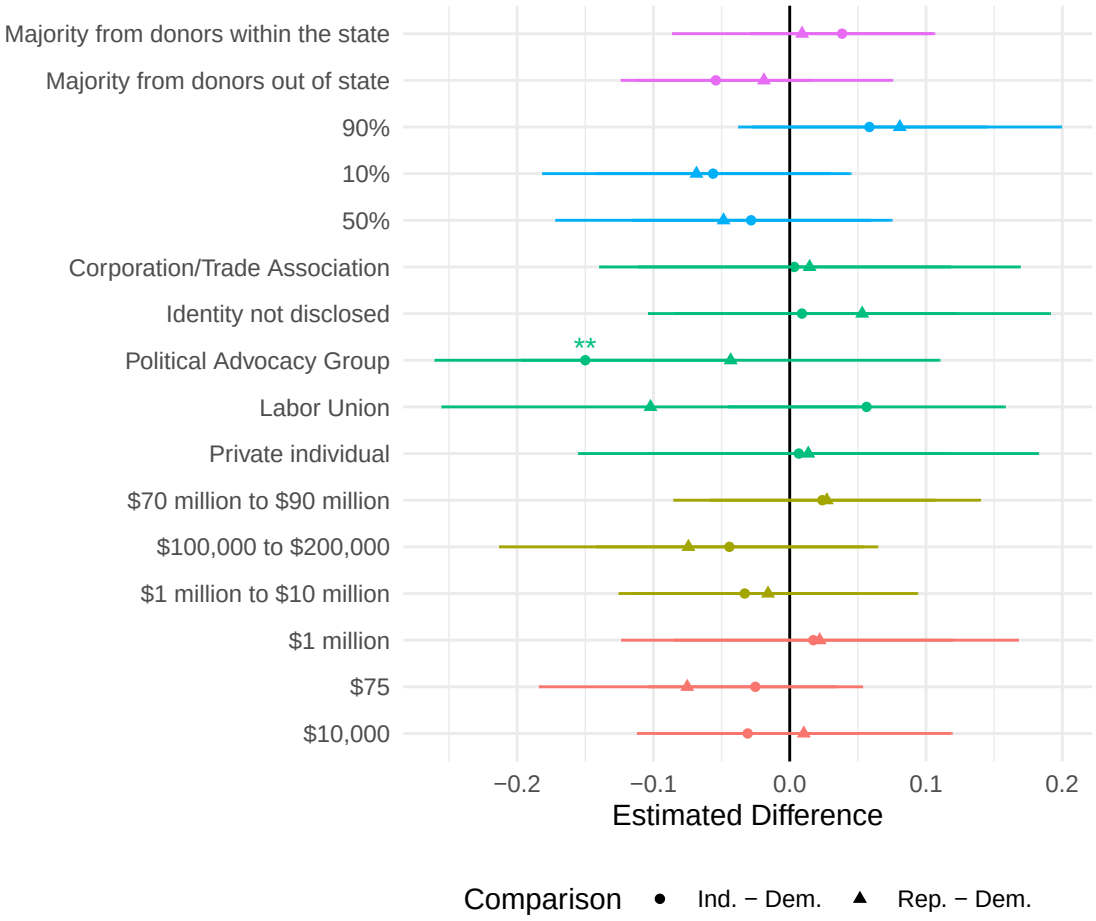
Table E2: Balance test: proportion of times each attribute-level was displayed to participants in the initiative conjoint

Attribute	Level	Bond	Enviro.	Marij.	Wage
Average	$1 million	0.27	0.23	0.26	0.27
Average	$10,000	0.38	0.37	0.37	0.35
Average	$75	0.35	0.40	0.37	0.38
Largest	Corporation/Trade Association	0.22	0.20	0.18	0.23
Largest	Identity not disclosed	0.19	0.22	0.21	0.18
Largest	Labor Union	0.21	0.20	0.18	0.21
Largest	Political Advocacy Group	0.17	0.19	0.22	0.19
Largest	Private individual	0.21	0.19	0.20	0.19
Origin	Majority from donors out of state	0.47	0.49	0.50	0.50
Origin	Majority from donors within the state	0.53	0.51	0.50	0.50
Prop	10%	0.31	0.36	0.35	0.32
Prop	50%	0.31	0.33	0.28	0.33
Prop	90%	0.38	0.31	0.37	0.35
Total	$1 million to $10 million	0.38	0.35	0.35	0.39
Total	$100,000 to $200,000	0.25	0.27	0.27	0.23
Total	$70 million to $90 million	0.37	0.37	0.38	0.38

F Additional Results

Conjoint models

Figure F1: Difference in marginal means for each attribute level by respondents' party identification, for subjects **not** exposed to additional party, ideology, and valence attributes.



Estimated differences are shown with 95 percent confidence intervals. Stars indicate statistically significant differences at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Table F1: Z-score differences between model coefficients for the candidate conjoint experiment (coefficients presented in Figure 8).

	$\beta_{\text{Dem.}}$	$\beta_{\text{Rep.}}$	$\beta_{\text{Ind.}}$	$\Delta_{\text{D-R}}$	$\lvert z\rvert_{\text{D-R}}$	$\Delta_{\text{D-I}}$	$\lvert z\rvert_{\text{D-I}}$	$\Delta_{\text{R-I}}$	$\lvert z\rvert_{\text{R-I}}$
Average: $75	0.133	0.047	0.154	0.086	1.244	-0.020	0.381	-0.106	1.443
Average: $1 million	-0.106	-0.123	-0.039	0.017	0.154	-0.067	1.019	-0.084	0.724
Total: $100,000 to $200,000	-0.010	-0.055	0.035	0.044	0.447	-0.045	0.665	-0.089	0.835
Total: $70 million to $90 million	-0.053	-0.025	-0.002	-0.028	0.349	-0.052	0.981	-0.024	0.294
Largest: Labor Union	0.194	0.033	0.211	0.161	1.317	-0.017	0.218	-0.178	1.389
Largest: Political Advocacy Group	0.093	0.062	-0.019	0.031	0.267	0.112	1.431	0.081	0.649
Largest: Identity not disclosed	-0.006	0.040	-0.015	-0.045	0.438	0.009	0.122	0.055	0.510
Largest: Corporation/Trade Association	-0.055	-0.054	-0.079	-0.001	0.007	0.024	0.278	0.025	0.165
Proportion: 10%	0.076	0.045	0.036	0.032	0.354	0.040	0.756	0.008	0.096
Proportion: 90%	-0.125	0.003	-0.061	-0.128	1.984	-0.064	1.104	0.064	0.938
Majority from donors within the state	0.182	0.211	0.277	-0.029	0.403	-0.095	1.897	-0.066	0.893

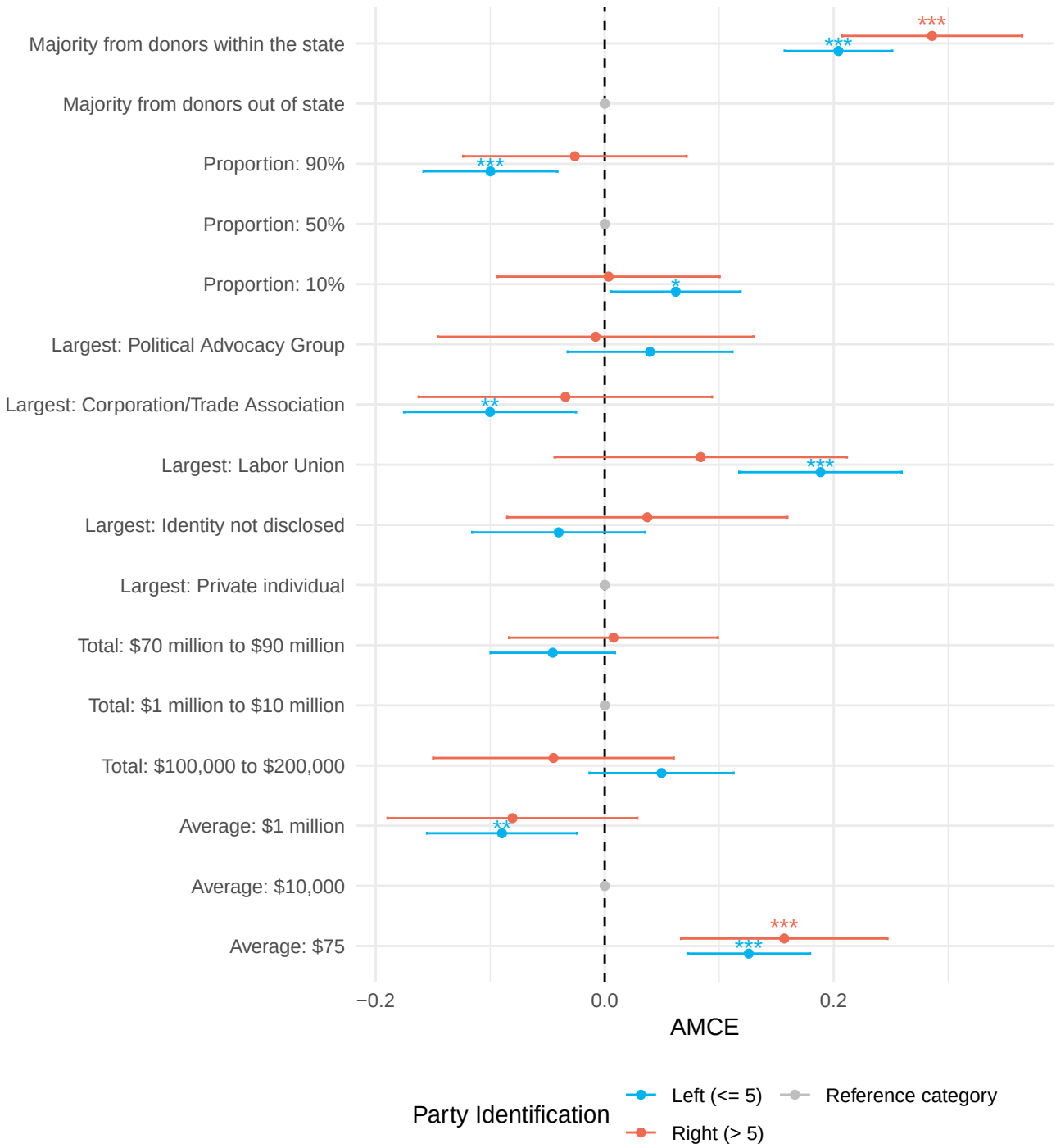
The three models are estimated on those respondents who were *not* exposed to additional political cues. $\Delta_{\text{D-R}}$ denotes the difference in coefficients between Democrat and Republican-identifying subjects (I indicates Independents). $\lvert z\rvert_{\text{D-R}}$ denotes the Z-score of $\Delta_{\text{D-R}}$, and is calculated as $\left|\Delta_{\text{D-R}}/\sqrt{\sigma_{\text{D}}^2+\sigma_{\text{R}}^2}\right|$.

Table F2: Z-score differences between model coefficients for the initiative conjoint experiment.

	$\beta_{\text{Dem.}}$	$\beta_{\text{Rep.}}$	$\beta_{\text{Ind.}}$	$\Delta_{\text{D-R}}$	$\lvert z\rvert_{\text{D-R}}$	$\Delta_{\text{D-I}}$	$\lvert z\rvert_{\text{D-I}}$	$\Delta_{\text{R-I}}$	$\lvert z\rvert_{\text{R-I}}$
Average: $75	0.039	0.007	0.023	0.032	0.509	0.015	0.279	-0.017	0.245
Average: $1 million	-0.019	0.027	0.008	-0.046	0.601	-0.028	0.477	0.019	0.231
Total: $100,000 to $200,000	-0.064	-0.013	-0.017	-0.050	0.806	-0.046	0.795	0.004	0.059
Total: $70 million to $90 million	0.009	-0.050	0.041	0.060	0.846	-0.031	0.657	-0.091	1.255
Largest: Political Advocacy Group	0.020	-0.022	-0.048	0.042	0.524	0.068	1.061	0.026	0.303
Largest: Labor Union	0.019	-0.082	0.039	0.100	1.249	-0.021	0.313	-0.121	1.379
Largest: Corporation/Trade Association	-0.042	0.025	-0.020	-0.068	0.789	-0.023	0.362	0.045	0.498
Largest: Identity not disclosed	0.053	-0.007	0.028	0.061	0.747	0.025	0.393	-0.036	0.432
Proportion: 10%	0.055	-0.041	-0.030	0.096	1.337	0.084	1.514	-0.012	0.155
Proportion: 90%	0.045	-0.041	-0.001	0.086	1.222	0.047	0.899	-0.039	0.552
Majority from donors within the state	0.013	0.045	0.067	-0.032	0.539	-0.054	1.297	-0.022	0.364
Issue: Environment	-0.008	0.009	0.014	-0.018	0.853	-0.022	1.439	-0.004	0.200
Issue: Marijuana	-0.011	-0.007	0.012	-0.005	0.253	-0.023	1.656	-0.019	0.932
Issue: Wage	0.007	-0.008	0.006	0.015	0.754	0.001	0.041	-0.014	0.650

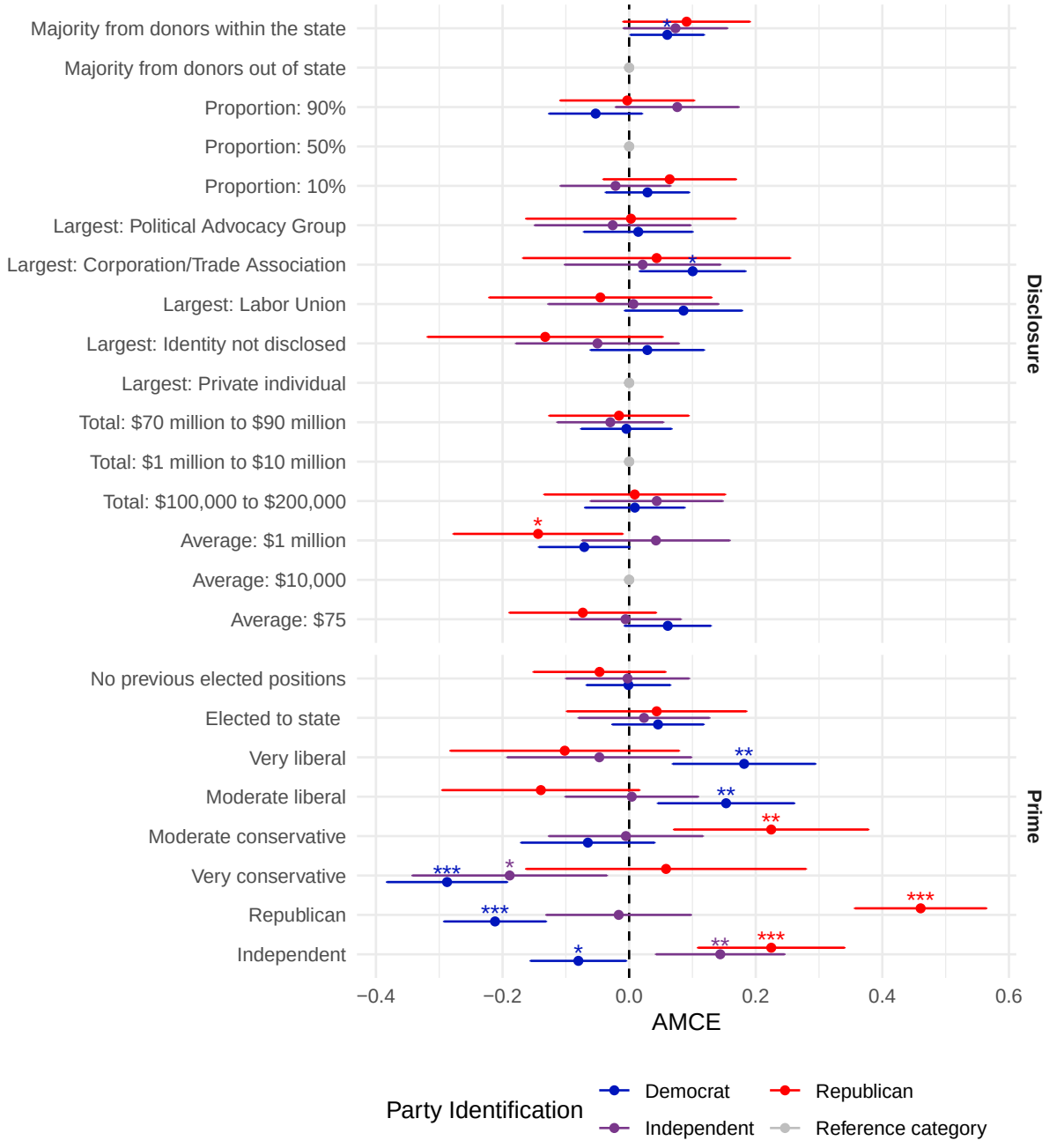
The three models pool observations across each of the four proposed policies, and include issue fixed effects. $\Delta_{\text{D-R}}$ denotes the difference in coefficients between Democrat and Republican-identifying subjects (I indicates Independents). $\lvert z\rvert_{\text{D-R}}$ denotes the Z-score of $\Delta_{\text{D-R}}$, and is calculated as $\left|\Delta_{\text{D-R}}/\sqrt{\sigma_{\text{D}}^2+\sigma_{\text{R}}^2}\right|$.

Figure F2: Comparison of causal effects between left- and right-identifying respondents in the sample, for those *not* exposed to additional political cues.



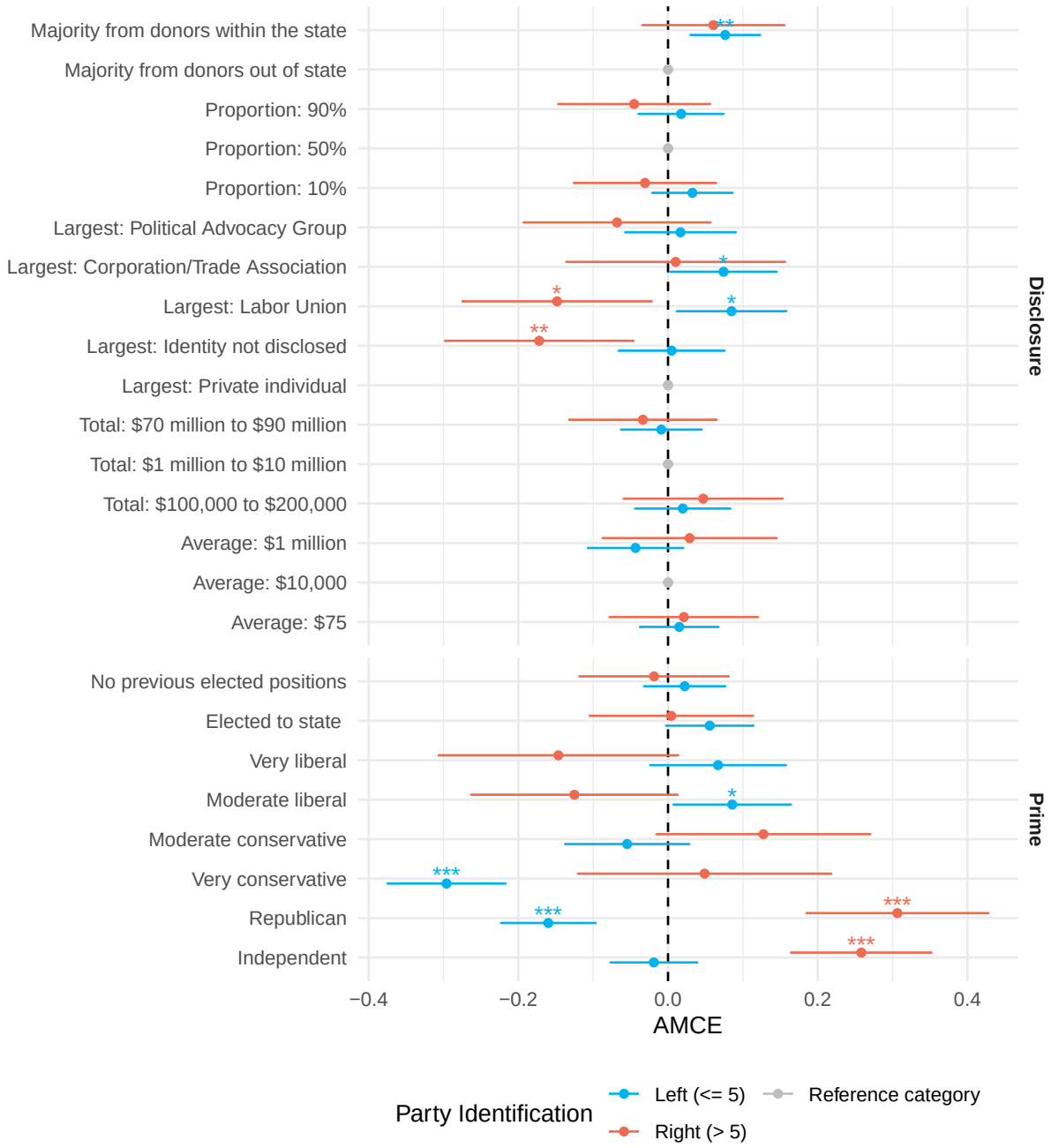
Self-reported ideology was measured on a 0-10 scale. 'Left' is coded as having a score less than or equal to 5 and 'Right' as greater than 5. Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Figure F3: Comparison of causal effects between Democratic, Independent, and Republican respondents in the sample, for those exposed to additional political cues.



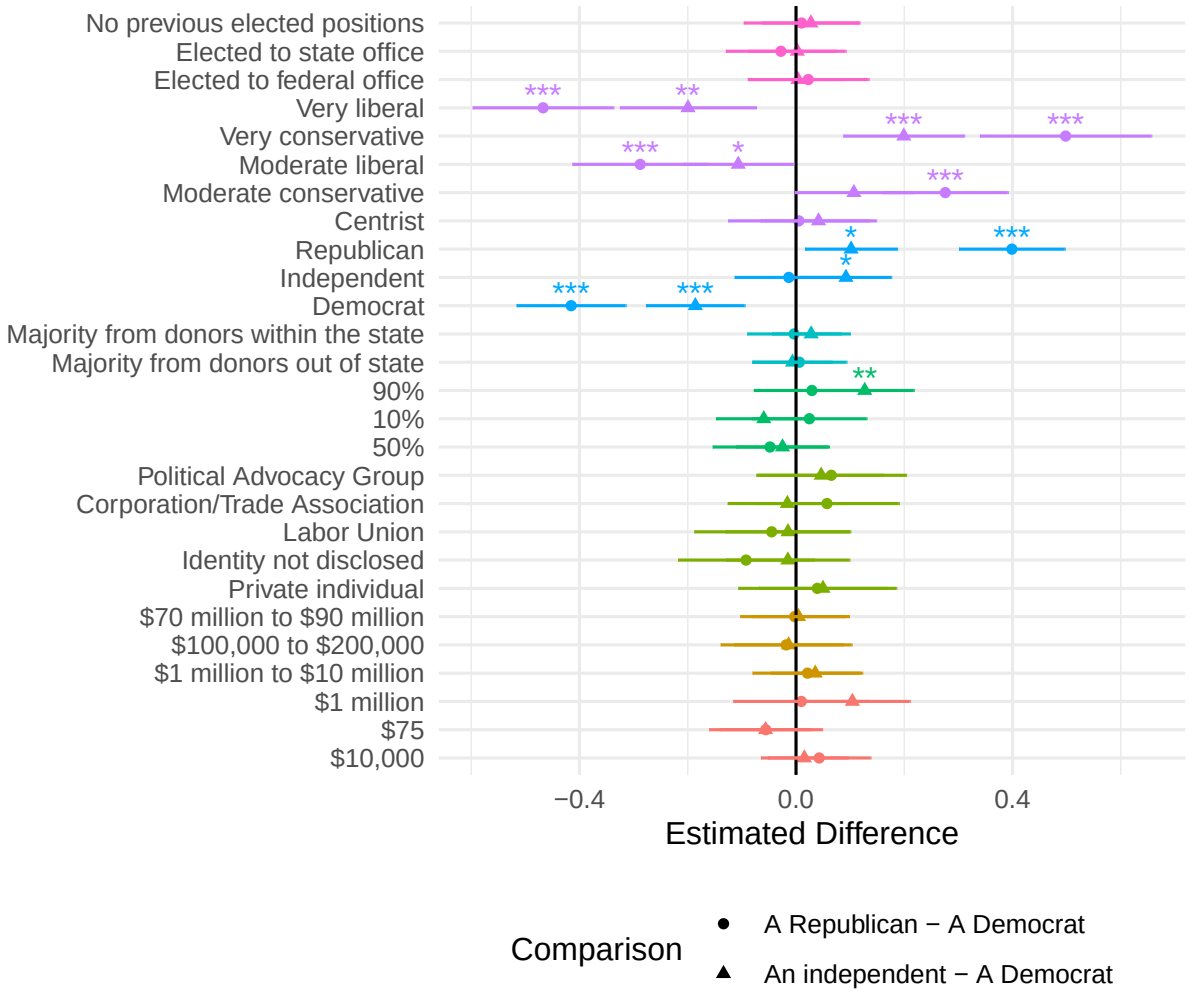
Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Figure F4: Comparison of causal effects between between left- and right-identifying respondents in the sample, for those exposed to additional political cues.



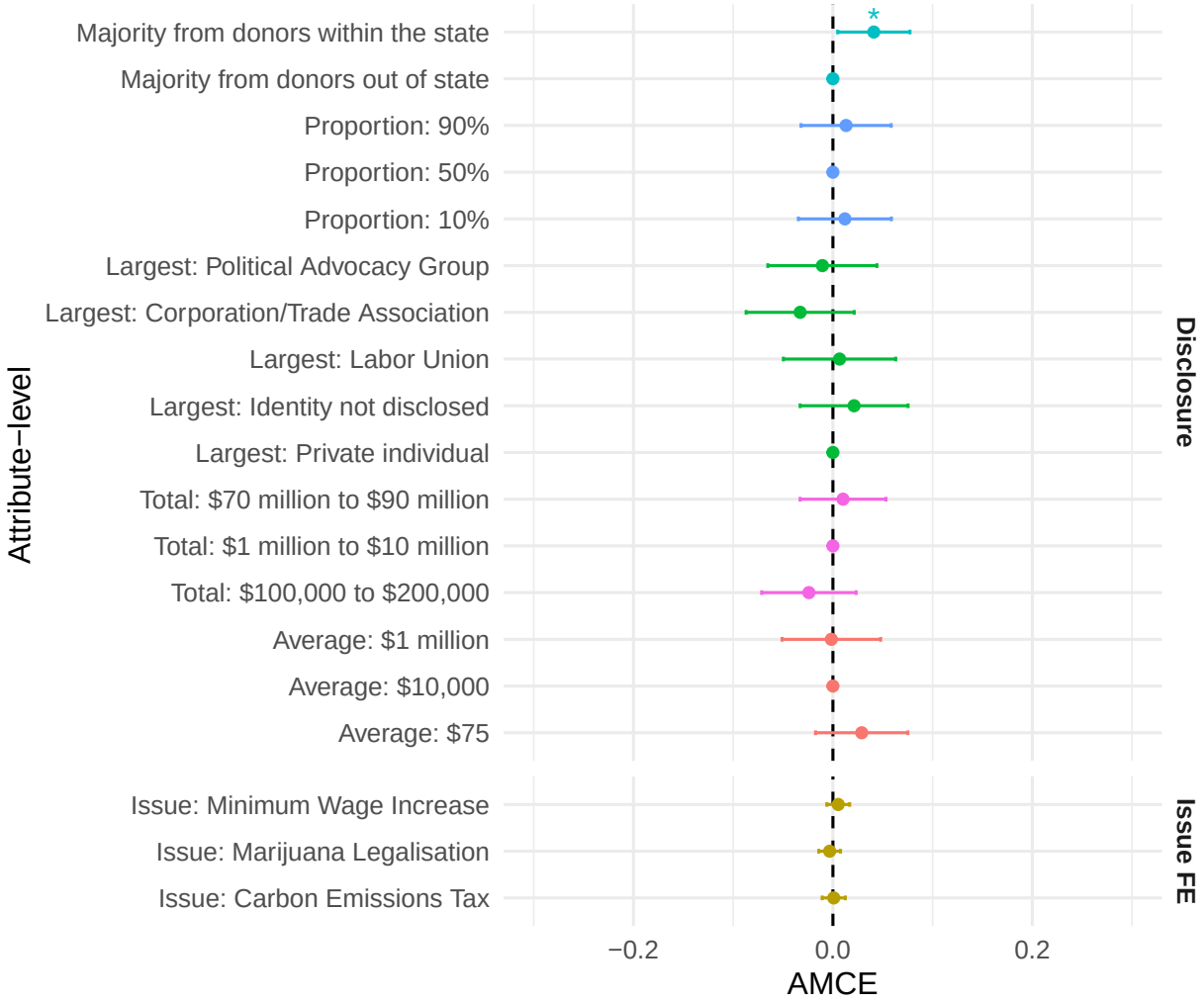
Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Figure F5: Difference in marginal means for each attribute level by respondents' party identification, for subjects exposed to additional party, ideology, and valence attributes.



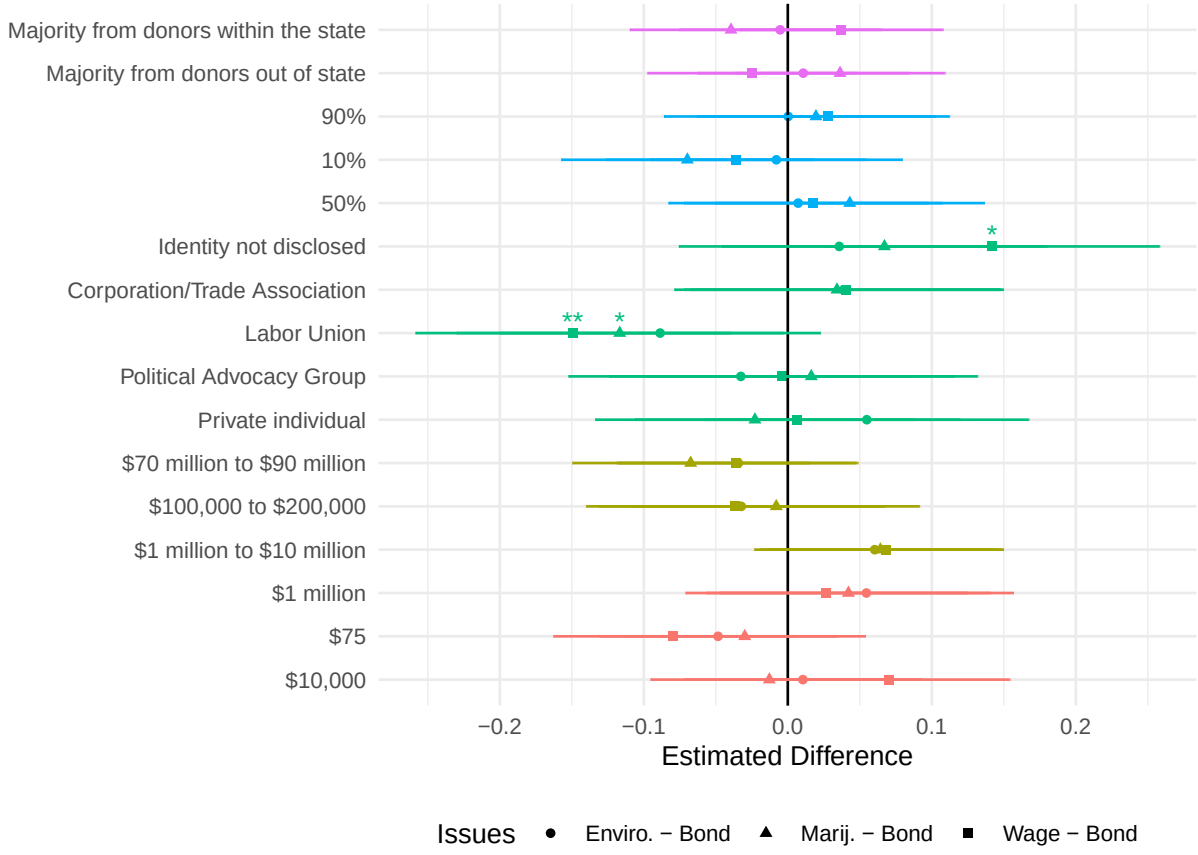
Estimated differences are shown with 95 percent confidence intervals. Stars indicate statistically significant differences at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Figure F6: Initiative conjoint model pooled across all four policy issues, and *including controls for respondents' self-reported partisanship*



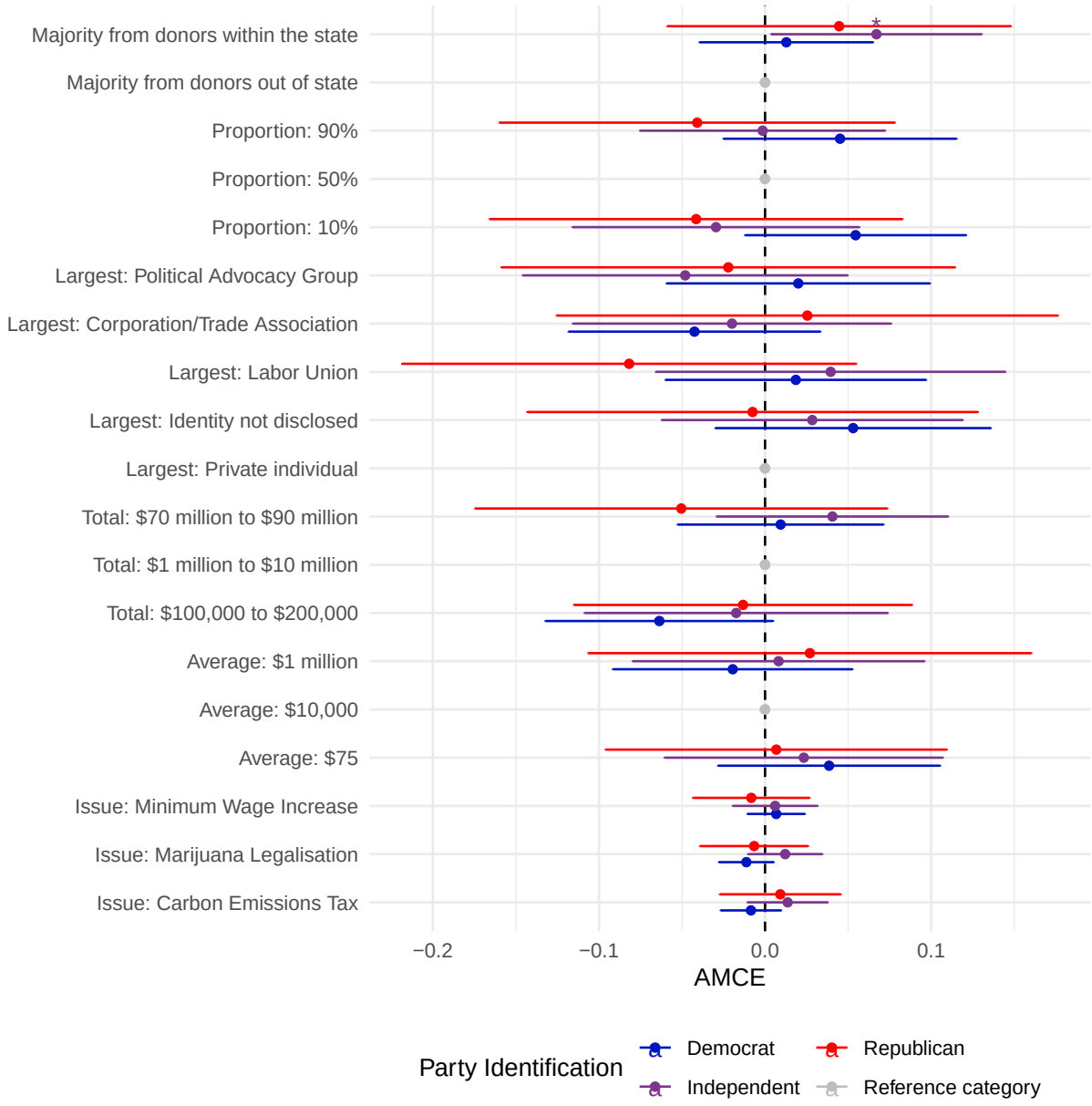
Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Figure F7: Difference in marginal means by issue for the initiative conjoint.



Estimated differences are shown with 95 percent confidence intervals. Stars indicate statistically significant differences at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Figure F8: Comparison of estimated AMCEs across respondents' partisan identities for the initiative conjoint experiment



Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

G Subjects' Support For Hypothetical Initiative Campaigns

Table G1 shows the proportion of participants that voted for each proposal. The sewage bond, marijuana legalization and minimum wage increase initiatives all received greater than 70 percent support. Only the environmental protection initiative was a marginal race. Even without further analysis, therefore, it is clear that voters' opinions on these issues are relatively fixed (given that disclosure attribute levels are completely randomised).

Table G1: Subject support and rating of initiative campaigns

Issue	Vote	Oppose	Support
Bond issuance	0.74	3.51	4.80
Environment tax	0.49	3.96	4.07
Marijuana legalisation	0.78	3.24	5.17
Wage increase	0.72	3.27	5.11

These levels of support accord with evidence at the national level that citizens overwhelmingly favour some policy changes. The Cooperative Congressional Elections Survey (CCES) fielded a set of hypothetical policy questions similar to those in this paper during the 2018 midterm election period (Ansolabehere, Schaffner and Luks, 2019). 69.9 percent of those interviewed nationwide favoured an increase in the state minimum wage to $12 an hour (question CC18_414A); a millionaire's tax to fund school and road spending was supported by 70.6 percent of budgets (question CC18_414B); policies granting the Environmental Protection Agency the power to regulate Carbon Dioxide (question CC18_415a) or requiring states to use a minimum amount of renewable fuels (question CC18_415c) both received 61.2 percent support. These questions are not perfectly comparable with those fielded in this conjoint experiment, but they do at least suggest that the levels of support are feasible and not incongruent with other surveys on citizens' policy positions.

Table G1 also reports the mean approval rating of each side of the campaign (on a scale of 1-7), for each issue. Interestingly, there is some variation between subjects' approval of campaigns and their respective vote choice. While a majority voted against the envi-

ronmental initiative, subjects were still marginally more favourable of the proponent side of the debate (albeit by a statistically insignificant amount, $p = 0.43$). And for the other three campaigns, despite the high proportions voting in favour of change, the difference in ratings are substantively closer (albeit statistically significant) than the vote proportions would suggest.

H Statistical tests of model differences across experimental conditions

H.1 Z-score differences

In the main paper, I demonstrate that in the presence of additional political cues, the effect sizes of campaign finance information on vote choice diminish considerably. For this analysis to be valid, the reduction in effect size should itself be statistically significant, for those attribute-levels that were statistically significant in the model without additional cues. To assess these differences statistically, I calculate the Z-score differences between corresponding coefficients from the two candidate conditions. These scores are calculated for each financial attribute-level k as:

$$Z_k = \left(\hat{\beta}_{k,C} - \hat{\beta}_{k,T}\right) / \left(SE^2_{\beta_k,C} + SE^2_{\beta_k,T}\right),$$

where C denotes the experimental condition *without* additional political cues, and T denotes the experimental condition *with* additional cues. SE denotes the standard error for the respective coefficient β_k.

Table H1 reports the standardised differences and p-values across the two information conditions of the experiment for every coefficient. For all except one coefficient that are statistically significant in Figure 4, the corresponding coefficients in the model *with* addi-

tional cues are statistically distinguishable at the 95 percent confidence level. In each case, the absolute coefficient sizes are substantially reduced once additional cues are included.

Table H1: Z-score differences between the conjoint model without additional cues (as shown in Figure 4) and the model with additional cues (as shown in Figure 5)

Attribute-Level	$\beta_{\text{No additional cues}}$	$\beta_{\text{Additional cues}}$	Z	p
Average: $75	0.13	0.03	3.14	0.00
Average: $1 million	-0.09	-0.03	-1.45	0.15
Total: $100,000 to $200,000	0.02	0.02	0.02	0.98
Total: $70 million to $90 million	-0.03	-0.02	-0.34	0.73
Largest: Identity not disclosed	-0.02	-0.05	0.52	0.60
Largest: Labor Union	0.16	0.02	2.88	0.00
Largest: Corporation/Trade Association	-0.08	0.05	-2.53	0.01
Largest: Political Advocacy Group	0.03	-0.02	0.95	0.34
Proportion: 10%	0.05	0.02	0.80	0.42
Proportion: 90%	-0.08	0.01	-2.46	0.01
Majority from donors within the state	0.22	0.06	5.18	0.00

Coefficients listed in bold were significant ($p < 0.05$) in the model without additional political cues.

H.2 Post hoc power simulations

Another feature that would threaten the validity of the analysis is if the statistically significant results in the experimental condition without additional cues were the result of an under-powered conjoint experiment. If it is unlikely that a model would distinguish effects of that size then the decrease in effect sizes across experimental conditions may reflect a lack of statistical power rather than the impact of other political cues.

I therefore estimate the *post hoc* power of the experimental condition where subjects do not see additional cues. For the analysis to be valid, those statistically significant results should be well-powered. If this is the case, then we can be more confident that the decline in statistical significance (and size of coefficient) is a result of the additional cues.

To estimate the experimental power using the empirical model results, I simulate a conjoint-like experiment in which every observation is randomly assigned attribute-levels for each

of the five campaign finance attributes in the experiment without additional cues. I set the number of observations equal to 195 subjects $\times 5$ rounds $\times 2$ profiles $= 1950$, which reflects the size and structure of the data used to estimate the model in Figure 4. I model the outcome for each observation as:

$$s \sim \mathcal{N}(X\hat{\beta},\ \sigma_{\text{Res.}}),$$

where s is the simulated outcome, X is a matrix of attribute-level assignments, $\hat{\beta}$ is the vector of coefficients from the empirical model, and $\sigma_{\text{Res.}}$ is the standard deviation of the residuals from the empirical model.[25] The standard deviation, in other words, represents the (square root of) unexplained variance in the outcome.

Since X is a random variable with equal probabilities of assignment within each attribute, by repeatedly assigning the conjoint levels in X we can estimate the probability of detecting a statistically significant coefficient (at the 95 percent confidence level), using Monte Carlo simulations:

$$\text{Power}_{\beta_k} = \frac{1}{B}\sum_{b=1}^{B} \mathbb{I}[\text{p-value}_{\beta_k} < 0.05],$$

where k indexes model coefficients and B is the number of simulations.

Table H2 reports the estimated *post hoc* power for each of the campaign finance coefficients *given the observed effect sizes in the actual empirical model*. For the statistically significant coefficients observed in Figure 4 of the main paper, the model is well-powered. For example, the power of the within-state donations level is 1.00, the $75 average donation level is 0.98, and the labor union largest donor coefficient is 0.95. The only significant attribute-level with a substantially low power value is the 'Corporation/Trade Association' largest donor label, which somewhat limits the strength of inference over this label specifically. In general, however, the results of this analysis are supportive of the main paper's claim that the

[25] To better resemble the binary outcome of a conjoint experiment, I transform s to equal either 0 or 1 using 0.5 as the cut-off value.

decline in effect sizes is explained by the presence of other political cues.

Table H2: *Post hoc* power simulations using observed coefficient values reported in Figure 4 of the main paper.

Coefficient	Significant without political cues?	*Post hoc* power
Average: $75	**Yes**	0.983
Average: $1 million	**Yes**	0.750
Total: $100,000 to $200,000	No	0.106
Total: $70 million to $90 million	No	0.167
Largest: Labor Union	**Yes**	0.947
Largest: Political Advocacy Group	No	0.098
Largest: Identity not disclosed	No	0.099
Largest: Corporation/Trade Association	**Yes**	0.492
Proportion: 10%	No	0.312
Proportion: 90%	**Yes**	0.682
Majority from donors within the state	**Yes**	1.000

Coefficients listed in bold were significant ($p < 0.05$) in the model without additional political cues.

More broadly, with a larger sample the conjoint experiment without additional political cues may have been able to distinguish smaller effects of other attributes on vote choice. With greater statistical precision, it would be possible to test further whether additional political signals crowd out the effects of financial labels that, absent these other information cues, appear less efficacious.

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