

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

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Abstract

Campaign finance information is abundant in the United States, but we have only scratched the surface of how this information affects voter behaviour. 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 experiments, 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 in electoral contexts.

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Information about campaign finance is abundant in the United States. At both the federal and state level, political campaigns are required to disclose publicly the money they have raised and its sources. Since *Buckley v. Valeo* (1976) the Supreme Court has continuously reaffirmed its position that mandatory disclosure has an “informational benefit” (Briffault, 2010) by enabling voters to make more informed electoral decisions. Theoretical works supports this position: financial transparency is construed as Pareto improving features of electoral systems that grant citizens greater information over their vote choice (Coate, 2004; Ashworth, 2006).

However, in practice, findings on the impacts of disclosure are mixed (Primo, 2013; Sances, 2013; Dowling and Wichowsky, 2013; Ridout, Franz and Fowler, 2015; Dowling and Wichowsky, 2015; Dowling and Miller, 2016; Wood, 2019, 2018; Rhodes et al., 2019; Spencer and Theodoridis, 2020).¹ While voters’ perceptions of candidates are not immune to the effects of disclosure (Wood, 2019; Spencer and Theodoridis, 2020), the marginal informational benefits of doing so may be limited (Primo, 2013).

Several fundamental aspects of the effect of financial cues on voters’ behaviour remain understudied. First, very few studies have examined how disclosure affects vote choice, particularly in contexts that resemble the decision 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.

¹Estimating the effect of campaigns’ financial disclosures on observed vote choice is difficult given the endogeneity of electoral viability and popularity to the actions of donors (Arceneaux, 2010). Hence, the vast majority of research has focused on experimental manipulations.

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 on candidate elections, but the effects of financial information may differ in other democratic races that lack explicit partisan or valence signals. Given the explicit policy implications of direct democratic outcomes, voters may infer useful information about a policy's likely beneficiaries from campaign finance information. 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 random utility model of vote choice that explicitly incorporates how financial cues affects 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 translates to direct democratic elections: the explicit policy focus of referendum 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 experiments, 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 randomly varying not only the content but also the number of conjoint attributes displayed to respondents (Sen, 2017). Half of all subjects receive additional randomized information about candidates’ ideology, partisanship and political experience. Random assignment across informational contexts allows for an unconfounded analysis of whether the 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 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 in contexts that resemble typical elections. Finally, I present suggestive evidence that the effects of campaign finance disclosure (absent other signals) do not differ by individuals’ partisanship.

The findings of this paper make a direct contributions not only to how we study disclosure, but also how and why we justify financial transparency in campaigns more generally. I demonstrate that, in terms of the efficacy of this information on voters, transparency initiatives must take into account the wider informational context in which disclosures are received. This finding is not limited to candidate elections: given the ubiquity of ballot initiative policymaking across the United States (and the use of referendums around the world more generally), the inefficacy of financial information cues in direct democratic

ances also has important implications. From a policy perspective, these findings challenge the conventional wisdom that democratic values of political equality can be safeguarded by transparency alone. That is not to say there are no benefits to disclosure. Campaigns may act differently, soliciting unsavory donations, if their actions were not visible to the public. In that sense, disclosure (irrespective of who then accesses it) may dissuade unethical behaviour. But voters do not appear to revise or “correct” their vote choice on the basis of disclosure. This has important implications for how policymakers design campaign finance regulation if their intention is not only to curb corrupt behaviour by campaigns but also to better inform voters.

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 bolsters a voter’s ability to make informed decisions on both issue and candidate elections – 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. The Court’s position is that voters can productively use this information to inform their voting behaviour.²

Research on political advertisements demonstrates that disclosure can affect voter decisionmaking, specifically by inferring (rightly or wrongly) 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

²In Appendix Section B I discuss the implications of the *Citizens United* decision for disclosure-based regulation in more detail.

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 behaviour. 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 (Dowling and Miller, 2016) even in the presence of partisan information.

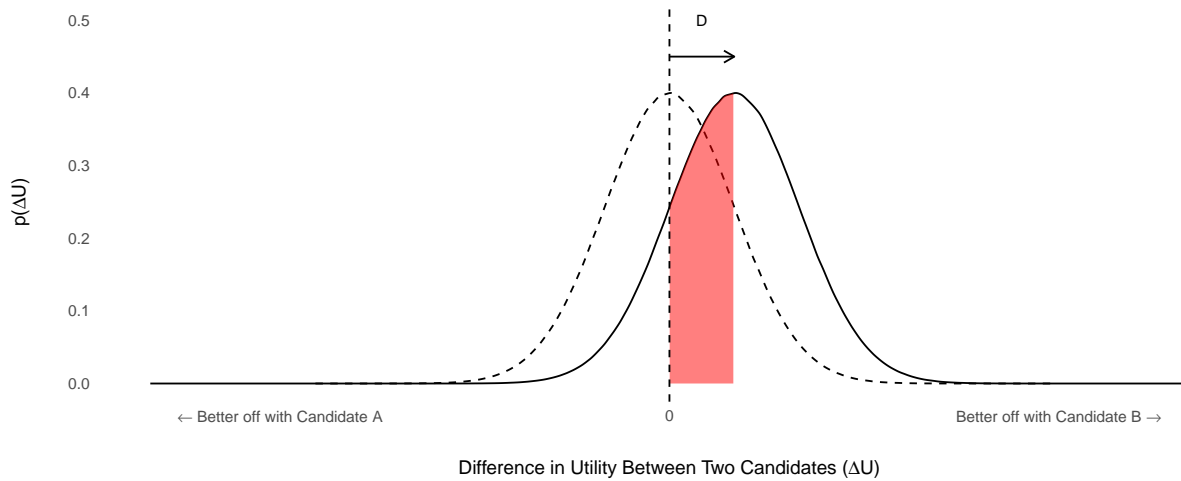
In this section, I present a random utility model of how disclosure can affect vote choice by refining individuals' perceptions of the utility gain from choosing one candidate over another. I then discuss several financial features of campaigns that may influence vote choice. Finally, I show using the same random utility model how the presence of other politically relevant signals, and the type of electoral race, may "swamp" the effects of disclosure on vote choice.

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 the relevant quantities needed to make the most appropriate choice, that is, the choice they would make under perfect information (Primo, 2013)?

Tasks like researching candidates' biographies, listening to speeches, or checking federal campaign submissions are taxing (Primo, 2013). Moreover, some plausibly relevant factors 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 pro-

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



cesses... 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 but relevant characteristics of campaigns like 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.

Figure 1 represents this intuition using a random utility model. Suppose that a voter is trying to discern between two candidates and, in the absence of other information, is uncertain over which candidate will make them better off. As a result, 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 V receives effective additional cues from campaign finance disclosure (labelled D in Figure 1).³ The effect of this financial information is to refine the individuals’

³Different aspects of a campaign’s financial profile may inform voters about valence and/or ideology. I

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 likely that the individual will now choose Candidate B over Candidate A.

1.2 Relevant facets of disclosure

There are various specific facets of financial disclosure that may act to shift a voter's utility distribution in favour of one candidate (Prat, Puglisi and Jr, 2010; Dowling and Miller, 2016; Wood, 2019; Spencer and Theodoridis, 2020). 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. The information these facets signal may activate different heuristic mechanisms and so I briefly consider each of these features in turn.

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) per-

discuss these specific expectations in Section 1.2.

ceive 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 (though not impossible) 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.⁴ Donations may predominantly come from individuals, corporations, labor unions, or specific political advocacy groups. Voters may react differently to campaigns depending on which sectors of society fund them. This signal is therefore likely to depend on a voter's pre-existing political leanings.

Moreover, a growing concern in the US system is the ability of certain donors to obscure their contribution activity through nonprofit "501(c)(4)" entities that, as charitable organisations, are not obliged to reveal their donors (Wood, 2018; Rhodes et al., 2019; Oklobdzija, 2019). Dark money vehicles are useful primarily to those exceptionally wealthy individuals and groups who wish to obscure their involvement in the political process (Mayer, 2016). This obscurity makes it very difficult to report such entities to voters through disclosure. While a 501(c)(4) organisation will be named, it will typically be uninformative

⁴The Supreme Court itself has ruled on similar issues related to the type of donor. In *McIntyre v. Ohio Elections Commission* (1995) the Court argued there was limited informational benefit to disclosing information about private individuals.

– for instance, “Americans for Prosperity”.

If voters are unable to infer the source of donations – either because the name is withheld or nondescript – they may shift their support away from that campaign. Or, as perhaps these groups hope, the reported name’s obscurity may cancel out any potential cue to voters based on group or 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 also matter to voters. This proportion reflects the degree of “capture” by any one particular donor or interest. Separate from the average donation, this feature explicitly captures the concentration of financial support, rather than providing a signal about the base of that support. Intuitively, if voters use this cue, they would be averse to campaigns funded by very few donors (i.e. where the largest donor donates a high proportion of funds). Campaigns with a high concentration of interested parties are likely to be those that most represent narrow 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). Whether a majority of donations come from within or outside of the state in question may provide some cue about “capture” by external interests. Large numbers of donations from out-of-state actors could represent apparent 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 constituency. This is particularly relevant for the sorts of elections considered in this article; gubernatorial and ballot initiative campaigns deal with state-wide political issues.

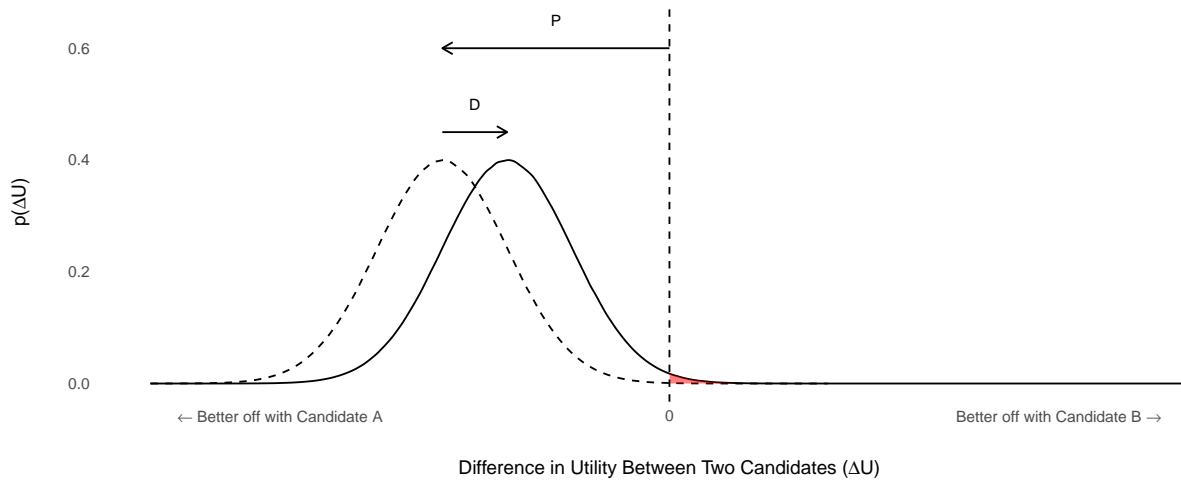
1.3 Effects of financial cues in the presence of other signals

Even if the hypothesised heuristics hold, it is not guaranteed that this information will, in real electoral contexts, affect vote choice. The random utility model in Figure 1 assumes that, 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 demonstrates that, if other political signals are particularly strong, the marginal effect of financial information on vote choice may approach zero. In this model, the cumulative effect of other political signals (labelled P) is much stronger than those from campaign finance information. Consequently, the same signal gleaned from campaigns' financial details 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 now 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 ben-

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



enefit gleaned from disclosure may not translate into changes in voter behaviour. It is not that financial disclosures are uninformative, but that in the presence of other electorally-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 (Magleby, 1994). If we think that other political signals like partisanship may 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).⁵ To the best of my knowledge, this comparative aspect of disclosure (between election types) has not been tested empirically before.

One particular heuristic mechanism through which financial disclosure may inform voters is in signalling the “valence” of initiative campaigns. Since initiative elections ask voters

⁵Parties and their members can come out in favour of initiatives, but these initiatives are not labelled as ‘Democrat’ or ‘Republican’ and many issues are not explicitly supported by either party.

to endorse or reject an item of legislation, voters may want to estimate what the likelihood is that the bill in question will achieve its stated aim. They might also be concerned that the given legislation is anticompetitive, or favours a narrow set of out-of-state interests, even if they are broadly supportive of the policy in general. And voters may want to estimate the compatibility of novel policy proposals with their existing political beliefs. Absent clear 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 are weighted heavily towards the policy dimension (irrespective of other concerns), 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 conduct 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 in a robust, inferential manner ([Hainmueller, Hopkins and Yamamoto, 2014](#)). Since conjoint survey experiments typically ask respondents to choose

between two profiles, this design is a natural analogue for the sorts of decisions voters make in American elections. This is useful even in the context of 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 key role in advocating for the status quo (Gerber, 1999). Given the prominent role opposition groups play in initiative races, there is good reason to inspect how their own financial profiles affect vote choice.

2.1 Randomizing the *number* of attributes

Typical 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’ behaviour.

To assess for potential differences in the effects of campaign finance information given 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 see these same disclosure attributes as well as other relevant political cues – partisanship, ideology, and previous experience. Randomization across these two designs 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 dollar-amounts in the conjoint levels are intended to clearly distinguish campaign finance profiles within each relevant attribute. It is worth noting, however, that legal contribution limits to gubernatorial candidates vary across states (no such limits exist for initiative campaigns). Section C in the Supplementary Material summarises contribution limits within each state in the sample. This variance limits the external validity of these findings - a large average donation will be implausible given some state's contribution limits. However, the scenarios were presented as hypothetical and these levels did not seem to hinder subjects' completion or comprehension of the survey. Moreover, these levels are broadly plausible even if there is some mismatch with actual state laws. 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.

The experiments also used abstract descriptors for the largest donor type since, without increasing the number of levels within this attribute, it was likely subjects would regularly be presented with rounds in which the two campaigns have the same, named largest

Table 1: Conjoint attributes and levels

<i>Disclosure signals</i>	
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
<i>Politically relevant signals</i>	
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

donor. While this is not statistically problematic, nor theoretically impossible in the real world, it might be cognitively dissonant within the experiment – particularly if it were to happen multiple times to the same subject.⁶ With abstract descriptors, even when the same attribute-level appears for both profiles, these can conceivably be considered as different entities of the same type. Moreover, using abstract labels is a more direct test of the

⁶With total random assignment over this variable, 20 percent of possible combinations across two profiles would include the same level for the donor type. This would equate to seeing the same donor name for each profile 1.2 times per subject.

type of donor. While a ‘Political Advocacy Group’ could be either left- or right-leaning (or neither), it nevertheless clearly signals an organised and focused political entity. In other words, abstract levels avoid conflating the effect of the type of donor with a specific group or issue cause implied by a concrete name. Finally, the inclusion of corporate and labor union donor types does retain some indirect ideological content that can be examined (see Section 3.4).

Candidate conjoint. Respondents ($n = 390$) were presented with the funding profiles of two candidates running for gubernatorial office. As noted, half of all participants were randomly assigned to see three additional politically relevant signals: the candidates’ party affiliations, their ideological position, and whether or not they have been elected to either state or federal office before. Inclusion of these three variables fix subjects’ priors on these dimensions and thus enable us to test for the independent causal effect of financial cues on vote choice. The exact same funding attributes and possible-levels were used across the two versions of the candidate conjoint. The value of each attribute for each candidate were randomly assigned. Figure 3 displays an example of how the information was presented to subjects in each round of the candidate conjoint experiment.⁷

Initiative conjoint. All participants were also presented with four different initiative topics in separate conjoint rounds. The text for each initiative is shown in Table 2. These topics represent the sorts of issues likely to be considered on the ballot given proposals that have occurred in recent electoral cycles. The policy proposals are hypothetical but constructed to appear sufficiently realistic that a subject could imagine such proposals being placed on the ballot.

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, and presented for both the support *and* opposition campaigns. Participants were asked to consider this

⁷A screenshot of the initiative conjoint can be found in the Supplementary Materials (Figure C1).

Figure 3: Screenshot of candidate conjoint (without ideological, partisan, and valence control 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.

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	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

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 conjoints were presented to each subject was randomized, as was the assignment to the two different candidate conjoints. Within the initiative conjoint component, subjects were shown each initiative issue once and the order of these issues was randomized to limit any order-effect. Finally, across all experiments, attributes were randomized with minimal restrictions to prevent implausible attribute-level combinations.⁸ Tables D1 and D2 of the Supplementary Materials demonstrate that these randomization procedures were successful across all three conjoint experiments.

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

Table 2: Hypothetical initiative policies

Initiative title and description
<p style="text-align: center;"><i>Marijuana legalisation</i></p> <p>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.</p>
<p style="text-align: center;"><i>State minimum wage increase</i></p> <p>If passed, this initiative would raise the state minimum wage for adult workers to \$14 per hour within two months of enactment.</p>
<p style="text-align: center;"><i>Bond issuance for sewage redevelopment</i></p> <p>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.</p>
<p style="text-align: center;"><i>Carbon emissions tax</i></p> <p>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.</p>

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 College, University of Oxford.⁹ Members of the subject pool 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 provision is in operation. In total, 390 eligible participants completed the experiment.

Table A1 in the Supplementary Materials describes the demographic composition of this sample. In summary, the sample 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 ex-

⁹<https://cess-nuffield.nuff.ox.ac.uk/>

pected given states on the West Coast (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,¹⁰ 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.

Causal assumptions. For the models in conjoint analyses to have a causal interpretation, several assumptions about the design of the conjoint experiment must be met (Hainmueller, Hopkins and Yamamoto, 2014). Section D of the Supplementary Materials provides a detailed discussion of these criteria, and presents a series of tests that verify each assumption – stability, no profile-order effects, randomization, and balance.

3 Results

3.1 The effects of financial cues

The first candidate conjoint (fielded to half of all subjects) presented only financial attributes to voters (without partisan, ideological, or valence cues). To recover the marginal effect of each attribute, I estimate a linear probability model 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 mul-

¹⁰Data taken from Gallup's 2017 summary of state party affiliation, available at <https://news.gallup.com/poll/226643/2017-party-affiliation-state.aspx>.

multiple observations) are sampled from a much wider population of interest.

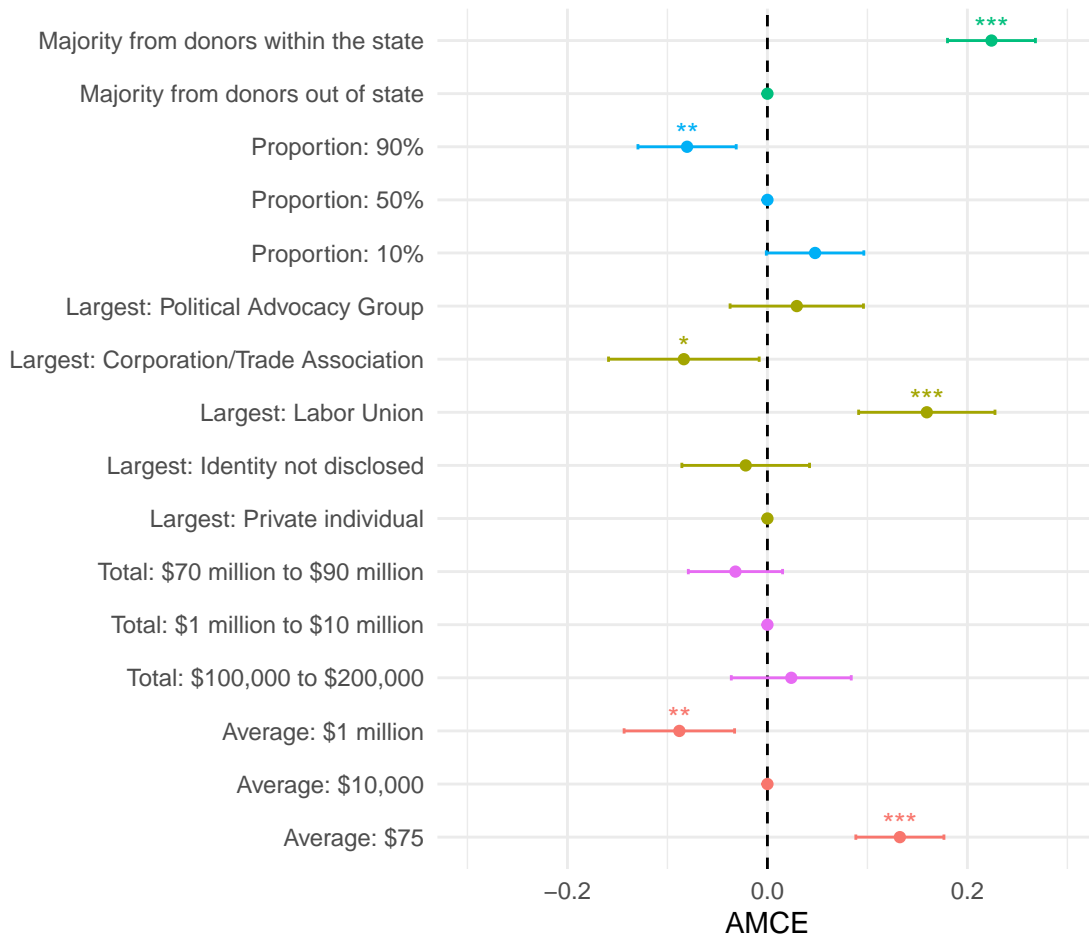
Indeed, in this low-information environment, financial attributes do have clear effects on vote choice. Figure 4 plots the AMCEs for each attribute-level. Voters are less likely to vote for candidates who receive a large proportion of their funds from a single donor, or where the average donation amount is high. Donations mainly from within the state, labor union funds (relative to private individuals), and lower average donation amounts (relative to the baseline \$10,000) 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 likewise the negative effect of corporations) is perhaps telling of the Democratic bias in the sample. I explore the causal effects by party identification in Section 3.4. It is nevertheless noteworthy that voters adjust their vote choice when presented with this information. PACs did not affect vote choice which perhaps suggests that their ubiquity in the American electoral landscape has dulled their informative quality to voters.

Moreover, the obscure attribute level for the largest donor - "identity not disclosed" - is insignificant (relative to individual donors). The absence of an effect for this opaque attribute level 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 simple lack of disclosure problematic. A stronger statement of *deliberately* withholding identifying information may have been

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



AMCEs are shown for each attribute-level in the model, with reference categories included as coefficients with values of zero. All standard errors are clustered by participant, and the figure displays the 95 percent confidence interval around each estimate. Stars above coefficients indicate significance at $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$ respectively.

more efficacious, but in reality deliberate avoidance may not be immediately discernible from financial records (at least, not without considerable research).

It is also possible that voters make heuristic decisions on the basis of multiple attributes in combination, which the present model specification would not detect.¹¹¹² The absence of

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

¹²For example, the results in Figure 4 show that total size of a campaign had no significant effect on voters' choices – perhaps because, holding constant other features of the financial profile, this cue is uninformative. It could nevertheless be the case that voters would be deterred from voting for a very large campaign if, at the same time, a large proportion of donations came from a single donor.

modelled interaction effects would be problematic if we observed only null effects in this specific experiment, 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

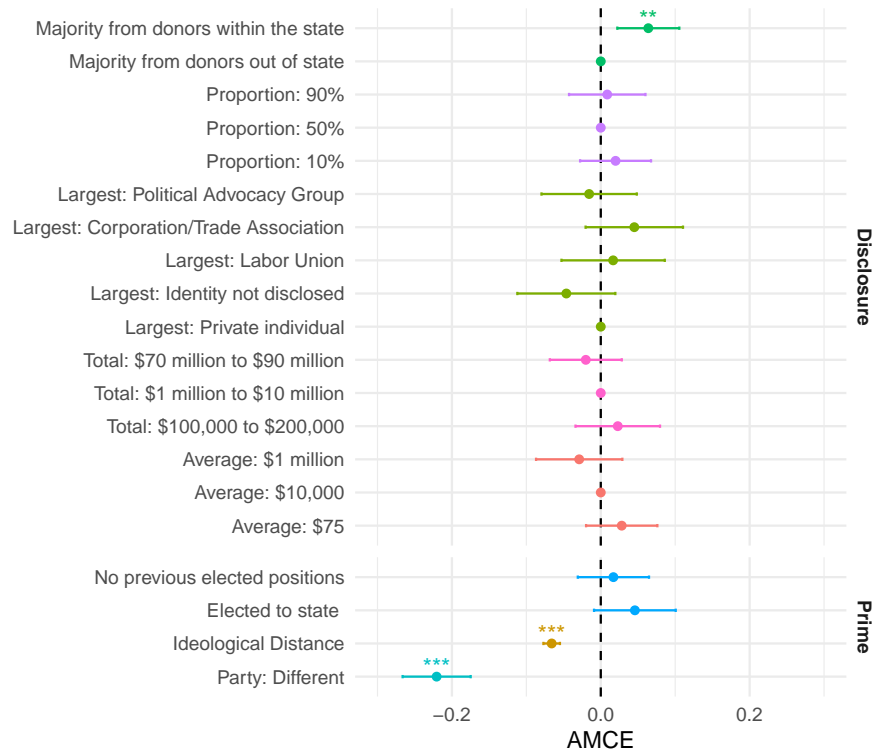
Elections are rarely fought on campaigns' financial profiles alone. Indeed, voters typically receive signals about candidate's ideology, partisanship and valence characteristics. The first set of results demonstrate the ability of disclosure mechanisms to affect vote choice. But do financial cues have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

For those subjects randomly exposed to additional political signals, I include the additional parameters for partisanship, ideology, and previous office-holding in the linear probability model. Given the explicit party and ideological primes, 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: only the positive effect of a majority of within-state donors has a significant effect on vote choice.

On the other hand, the political controls themselves have substantial effects. Voters unsur-

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, and standard errors are clustered by participant. Stars above coefficients indicate significance at $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$ respectively.

prisingly 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 differences between the attribute levels.

Figure E2 in the Supplementary Materials shows separate model coefficients for Republican and Democrats in this sample. Across all disclosure attributes, there are few substantive differences between Democrats and Republicans. Wide confidence intervals for the smaller Republican sample, however, suggest this model on its own is under-powered. Figure E3, similarly, reports the difference in marginal means between Democrat and Republican identifying subjects. Again, while there are large and highly significant differ-

ences for partisan and ideological primes, all differences in marginal means for the disclosure attributes are statistically insignificant.

These null results are noteworthy precisely 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, reducing from 0.159 ($p < 0.001$) to 0.016 ($p > 0.05$). Even the strongest financial signal – having a majority of donors within the state – is less than a third of the size once political signals are included.

Since respondents were randomly assigned across conditions, we can be confident that the reduction in efficacy of the financial attributes is as a result of the presence of other political signals. Taken together, the results suggest that while financial cues *can* affect vote choice, they are relatively inert in more realistic contexts in which other relevant cues are present. I explore some tentative reasons for this drop in efficacy in Section 3.4.

3.3 Financial cues in initiative elections

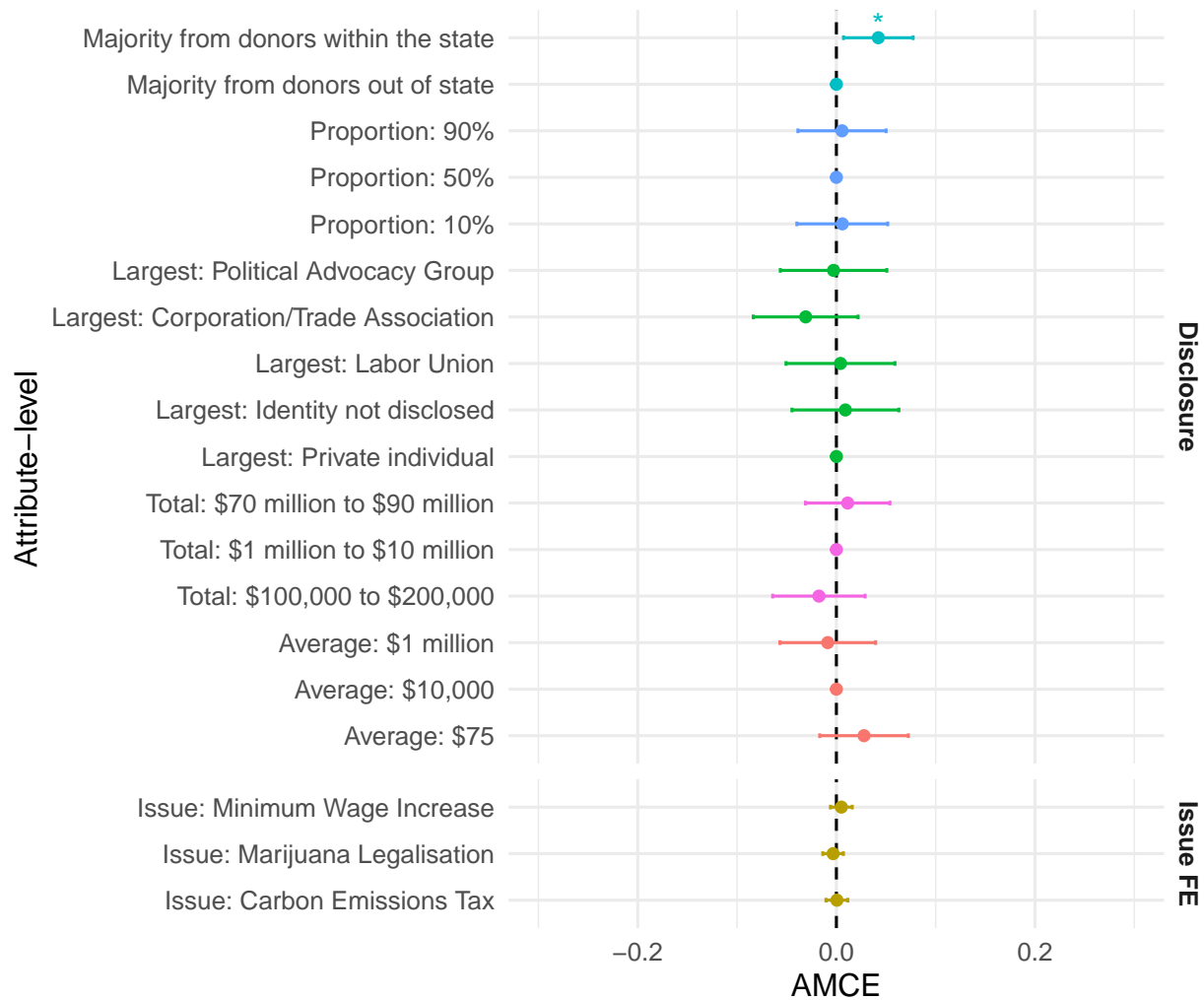
Unlike the candidate conjoints where participants chose between two candidates, in the initiative conjoint participants chose whether to endorse or reject a given proposal – in other words, to pick between supporting the ‘Yes’ or ‘No’ campaign.

Figure 6 displays the estimated AMCES from a linear probability model on the pooled observations across the four initiative topics, with issue fixed effects to control for the underlying support of each policy. The only statistically significant disclosure attribute is, again, the geographic origin of donations.¹³ This attribute’s effect is notably strong across all three conjoint experiments in this study. Subjects do seem to favour those campaigns that are funded predominantly by donors within their own state. This feature notwithstanding, it appears that voters’ attitudes towards initiative policies are reasonably fixed

¹³Figure E4 in the Supplementary Materials demonstrates that these results are unaffected by including control variables for respondents’ partisanship.

and that financial cues in general do not impact subjects' vote choice.¹⁴

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



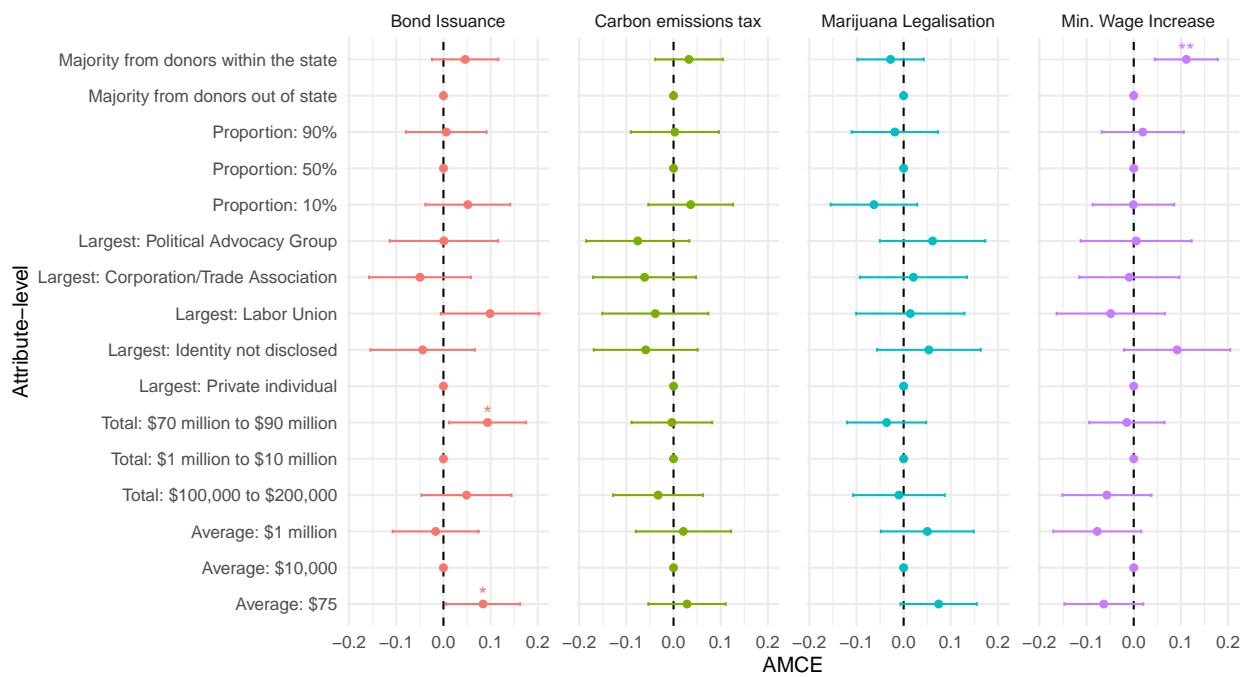
All responses across the four issues are pooled, and issue fixed effects are included to control for varying levels of support not contingent on disclosure mechanisms. Coefficients are shown with 95 percent confidence intervals, and standard errors are clustered by participant. Stars above coefficients indicate significance at $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$ respectively.

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 totals and a low average donation are statistically significant, positive predictors of support for

¹⁴Further analysis of subjects' support for these four issues is reported in Section F of the Supplementary Materials.

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 that, for less salient issues where we might expect voters' issue preferences to be less strongly held, financial information can alter vote choice. However, these results are purely indicative and further research is needed to test this hypothesis further.

Figure 7: Initiative conjoint results by issue area.



Separate OLS models are run for each of the four initiative policies posed to subjects. The number of observations per model are: 752 (bond issuance), 750 (carbon emissions tax), 755 (marijuana legalisation), and 746 (minimum wage increase). All coefficients are shown with 95 percent confidence intervals, and standard errors are clustered by participant. Stars above coefficients indicate significance at $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$ respectively.

Figure E5 in the Supplementary Materials shows the differences in marginal means for the attributes by issue. 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.

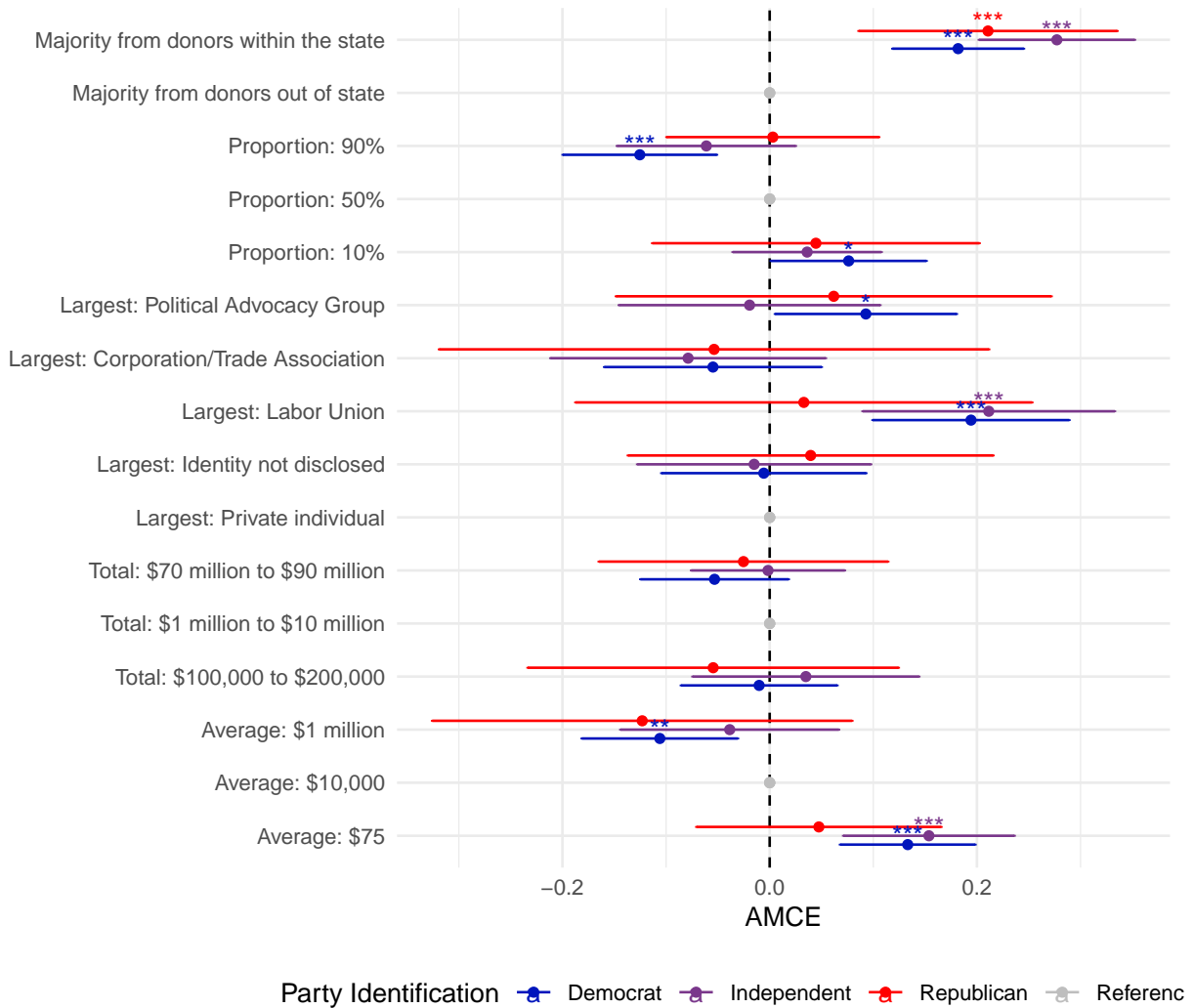
3.4 Partisan differences

In this final section, I explore whether the observed effects of financial cues (in the absence of other political signals) differ by respondents' self-reported partisan identity. It may be that the financial cues act differently dependent on individuals' own partisan stances. 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 (Burris, 1987; 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 linear probability models with the same specification as in Section 3.1 but on separate subsets of the data for individuals who identify as 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 indicative evidence, however, does not suggest substantial differences in the size or direction of effects across partisan identities.

All three groups have positive and substantially large effects of within-state donations. For the proportion of funds given by the largest donor attribute, all three groups have positively signed coefficients for the 10% attribute-level, and negatively signed or essentially zero AMCEs for the 90% level (only the Democrat coefficients are statistically significant).

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



The number of observations per model are 1070 (Democrats), 349 (Republicans), and 718 (independents). All coefficients are shown with 95 percent confidence intervals, and standard errors are clustered by participant. Stars above coefficients indicate significance at $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$ respectively.

Interestingly, for the labor union attribute-level, which arguably is the most overt political signal available in this financial information, all three groups have positive AMCEs. The labor union coefficients for independents and Democrats are both strongly statistically significant. There is some suggestions that the effect of labor union donations on Republicans is smaller, although with such large variance it is hard to draw concrete conclusion. The corresponding coefficient for corporate entities was weakly negative across all three

groups (albeit statistically insignificant).

Z-scores for the differences between coefficients across models confirm the visual differences in Figure 8. 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 ($p < 0.05$).¹⁵ Figure E1 in the Supplementary Materials shows, similarly, a lack of statistical difference between groups.

Figure E6 in the Supplementary Materials presents similar results for the initiative 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 coefficient¹⁶ preclude any definitive conclusion. As reported in Table E2, no difference in the value of coefficients across the three partisan models is statistically significant.

4 Discussion

In the absence of other relevant information, campaigns’ financial profiles can influence vote choice. Subjects in the experiments presented in this paper appear particularly concerned about features of campaign funding profiles that indicate the extent to which campaigns are captured by narrow interests, or by out-of-state actors. Crucially, however, once subjects are primed with other political cues, features that are ubiquitous to real candidate campaigns, the effects of financial cues all but disappear.

One exception is the positive effect of having a majority of within-state donations, which is robust across both candidate and initiative conjoints. 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 future work should consider precisely why this effect is so robust when other disclosure mechanisms are not. For instance, how might disclo-

¹⁵Full results from the Z-score calculations are available in Supplementary Materials Table E1.

¹⁶Independent-identifying respondents have a positive AMCE for campaigns with a majority of donations from within the state ($p < 0.05$).

sure affect vote choice when salient local or state-level figures are known contributors to campaigns?

Disclosure appears to be equally ineffective in ballot initiative campaigns. This null result was true even though no additional signals beyond the policy proposal and funding information were disclosed. Strong policy cues may drown out any effect that disclosure has on voters. This mechanism is harder to identify experimentally, since excluding policy signals renders subjects' decisions meaningless. Future work, therefore, may seek to develop protocols that tease out whether financial cues are more influential for some policy areas compared to others. Further work could also explore whether issue salience mediates cues about valence and ideological features of initiative campaigns. More generally, it remains to be tested whether any interactive effects between financial attributes are robust to the presence of other political signals.

From a policy perspective, these findings suggest that the informational benefit of disclosure is highly dependent on the wider informational context in which such information is given. In the presence of other political cues, across both representative and direct democratic races, financial disclosures do not appear to substantively impact vote choice. That is not to say that the disclosure is uninformative in a broader sense, and more targeted forms of disclosure focusing on specific donations, may be more effective. Voters' lack of sensitivity to the funding profile of candidates at the point of voting, challenges the informational benefit logic of disclosure-based regimes. This finding is particularly concerning at a time when rates of contributions and political spending are rising.

Consequently, the trade-off that some have suggested exists between greater exposure and voters' aversion to interest group involvement ([Ashworth, 2006](#)), does not seem to be supported by the results of this study. The relatively limited effects of financial cues on voter behaviour observed in this study suggest policymakers should think carefully about what the goals of disclosure are. While transparency initiatives may perturb uneth-

ical behaviour by campaigns *ex ante*, disclosure does not appear to curb the influence of unrepresentative interests via voters' behaviour. If policymakers wish to ensure political equality in the electoral process, and reduce the influence of donors on the viability and ultimately success of campaigns, disclosure alone appears insufficient.

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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. 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

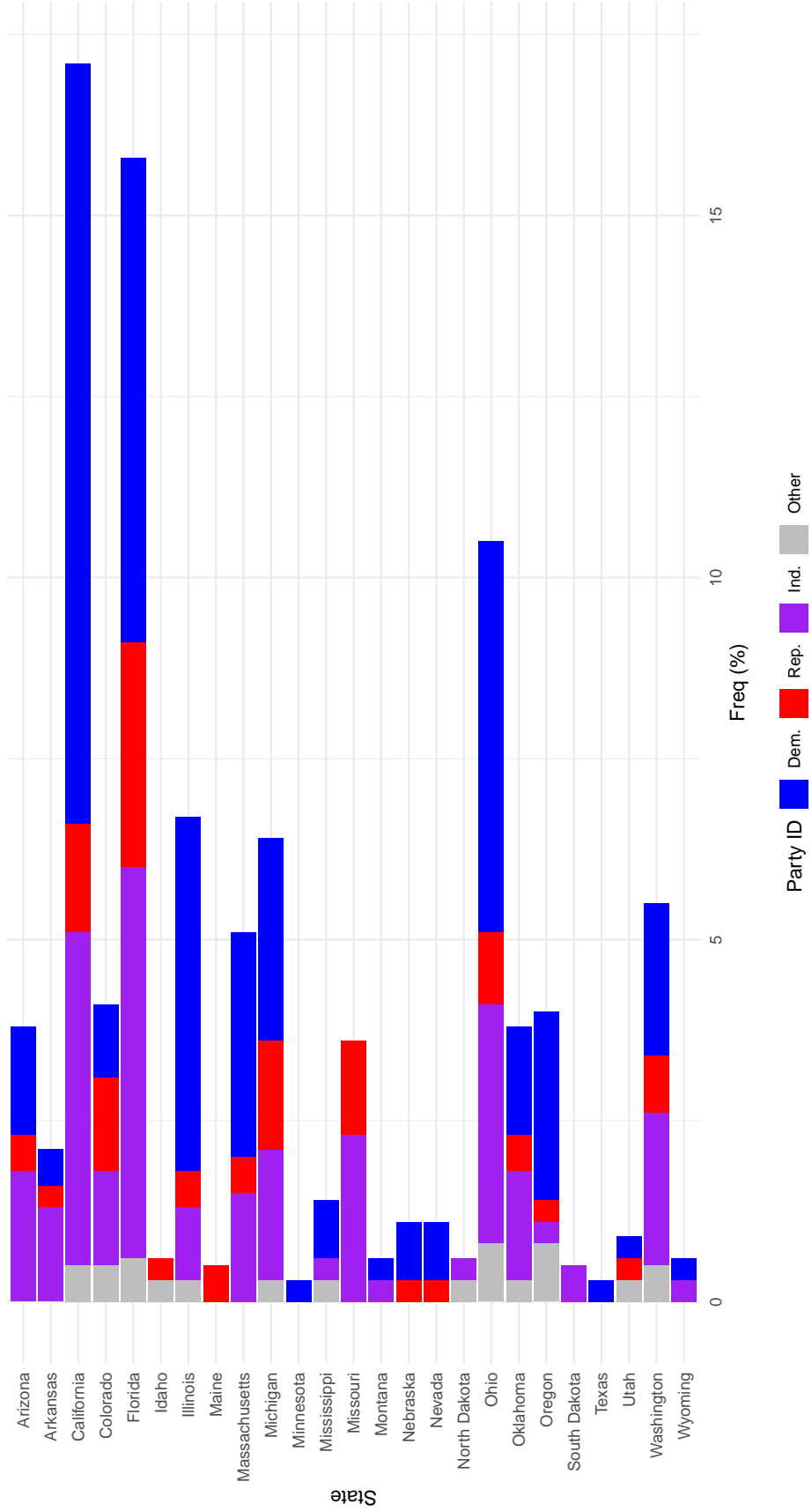


Table A1: Descriptive summary of key demographics for conjoint experiment subjects.

Variable	Value	Freq (%)
Age	<i>Mean</i>	38.00
	<i>Standard Deviation</i>	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	<i>Mean</i>	4.26
	<i>Standard Deviation</i>	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 section). 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 activities 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.

¹⁷“Mega” PAC’s can contribute \$10,100

¹⁸\$5675 for small donor committees.

¹⁹\$5675 for small donor committees.

²⁰Gubernatorial candidates with more than \$250,000 independent expenditures are exempt, or if opposition candidate is self-funded (spending over \$250,000.)

²¹Independent 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 ¹⁷	Prohibited	Prohibited
Arkansas	\$2700	\$2700	Prohibited	Prohibited
California	\$29200	\$29,200	\$29,200	\$29,200
Colorado	\$575	\$575 ¹⁸	Prohibited	\$575 ¹⁹
Florida	\$3000	\$3000	\$3000	\$3000
Idaho	\$5000	\$5000	\$5000	\$5000
Illinois	\$5600 ²⁰	\$55,400	\$11,100	\$11,100
Maine	\$1600	\$1600	\$1600	\$1600
Massachusetts	\$1000	\$500	Prohibited	\$500
Michigan	\$6800	\$6800 ²¹	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

Example of initiative conjoint round

Figure C1: 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_marj_choice. If you had to choose, would you vote for or against this initiative?

- For
- Against

init_marj_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	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input checked="" type="radio"/>	<input type="radio"/>	<input type="radio"/>
Opposition	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input checked="" type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

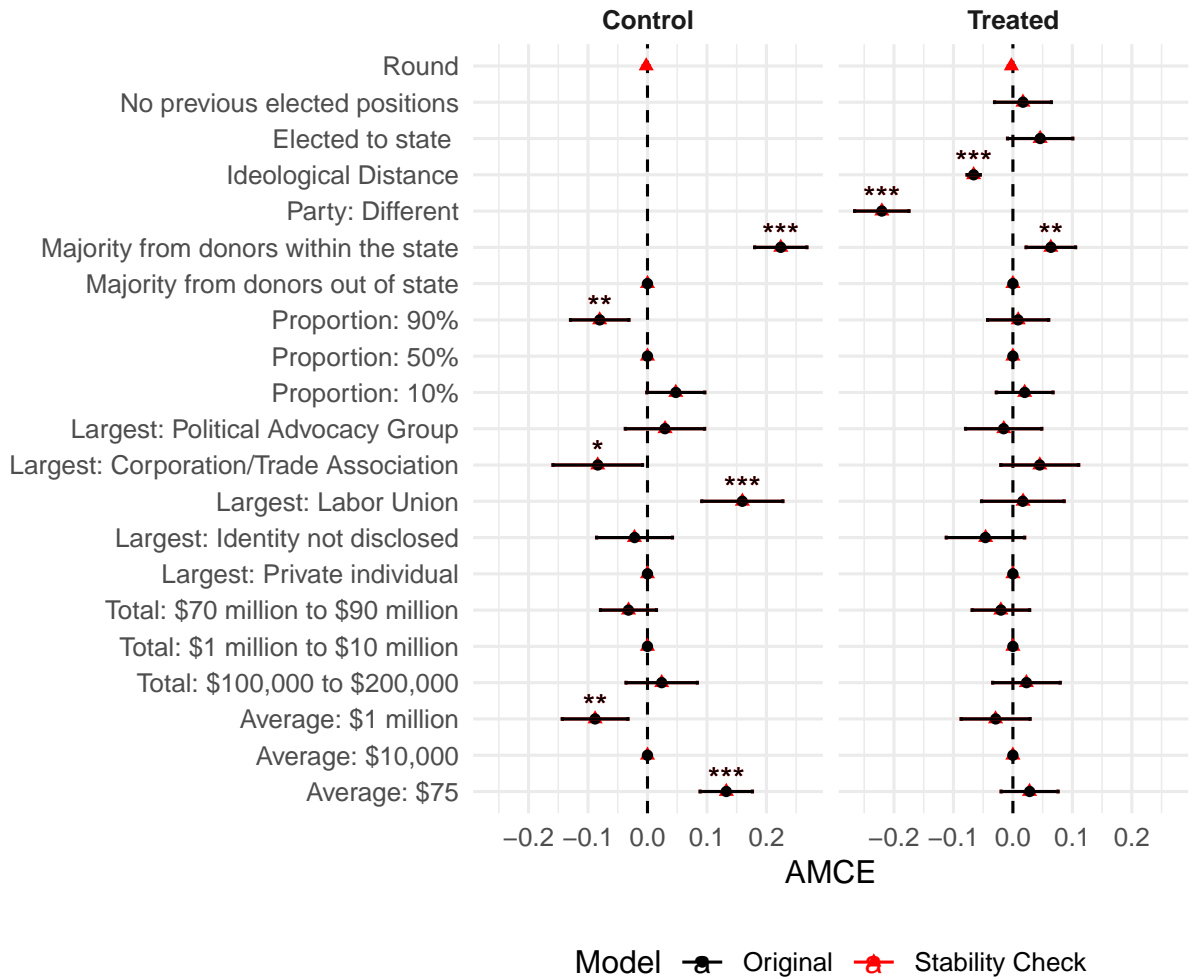
D Causal Assumptions

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 logistic 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 D1 demonstrates these results compared to the original models.²²

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.

²²This 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 D1: 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.²³

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 [D1](#) and [D2](#) 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.

²³Indeed, 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 D1: 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

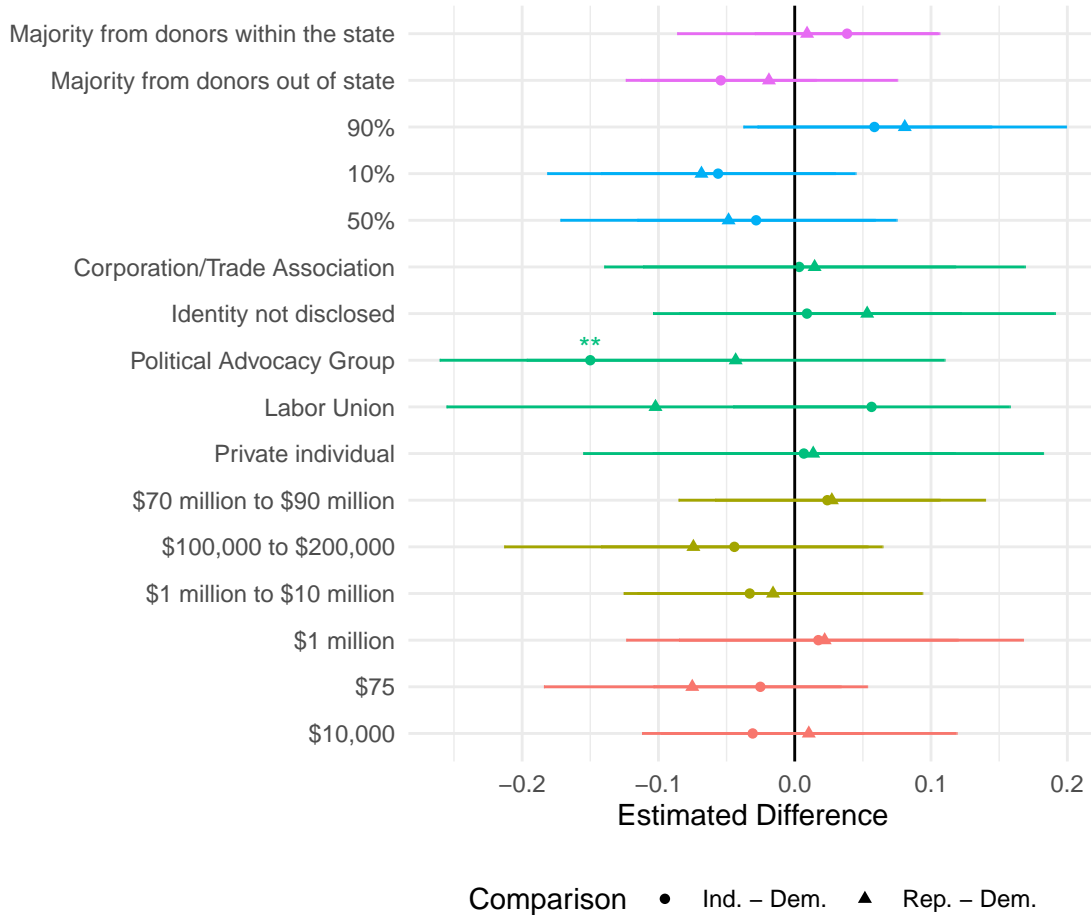
Table D2: Balance test: proportion of times each attribute-level was displayed to participants in the initiative conjoint

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

E Additional Results

Conjoint models

Figure E1: 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 E1: Z-score differences between model coefficients for the candidate conjoint experiment (coefficients presented in Figure 8).

	$\beta_{\text{Dem.}}$	$\beta_{\text{Rep.}}$	$\beta_{\text{Ind.}}$	$\Delta_{\text{D-R}}$	$ z _{\text{D-R}}$	$\Delta_{\text{D-I}}$	$ z _{\text{D-I}}$	$\Delta_{\text{R-I}}$	$ z _{\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

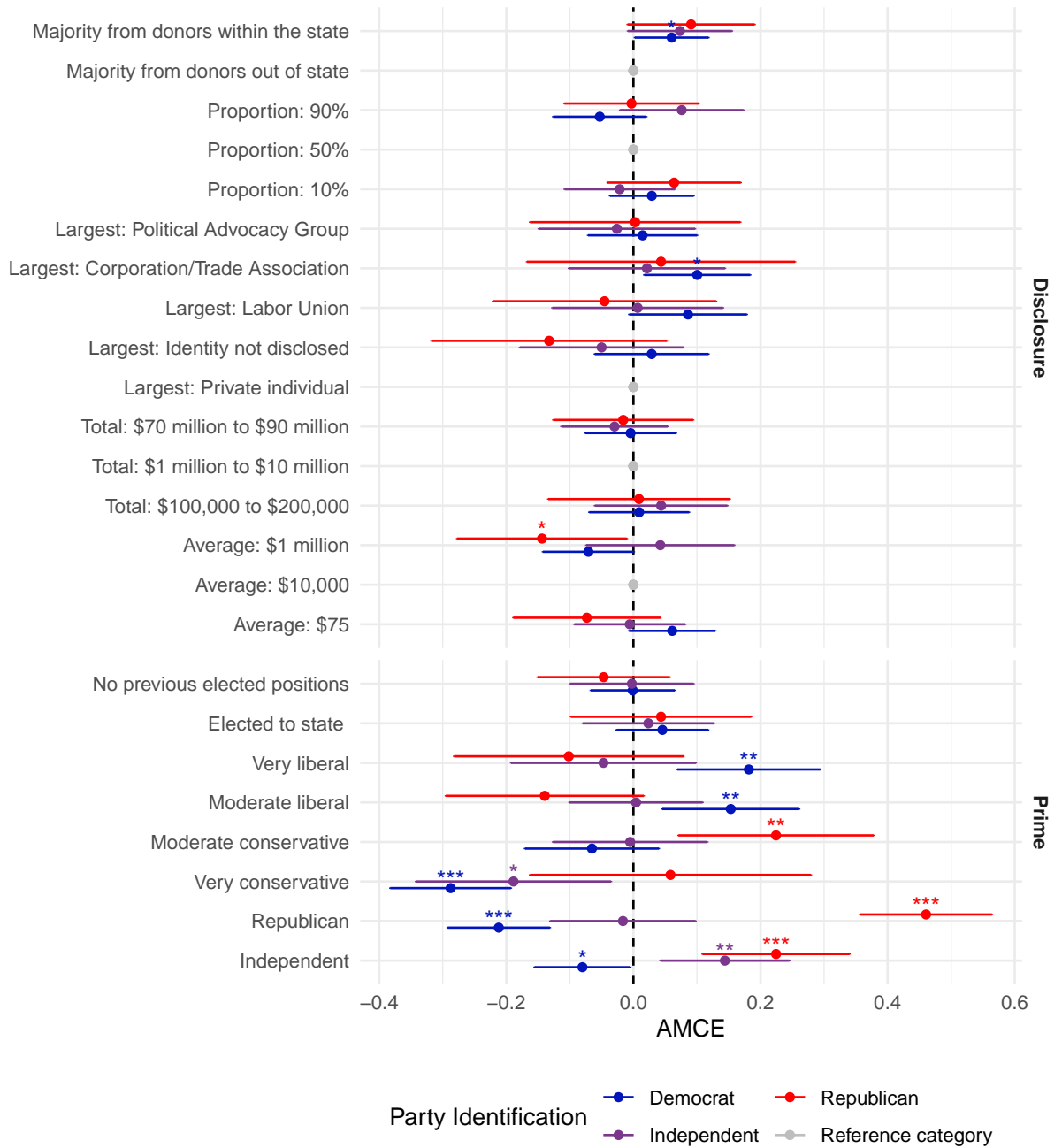
The three models are estimated on those respondents who were *not* exposed to additional political cues. $\Delta_{\text{D-R}}$ denotes the difference in coefficients between Democrat and Republican-identifying subjects (I indicates Independents). $|z|_{\text{D-R}}$ denotes the Z-score of $\Delta_{\text{D-R}}$,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

Table E2: Z-score differences between model coefficients for the initiative conjoint experiment.

	$\beta_{\text{Dem.}}$	$\beta_{\text{Rep.}}$	$\beta_{\text{Ind.}}$	$\Delta_{\text{D-R}}$	$ z _{\text{D-R}}$	$\Delta_{\text{D-I}}$	$ z _{\text{D-I}}$	$\Delta_{\text{R-I}}$	$ z _{\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

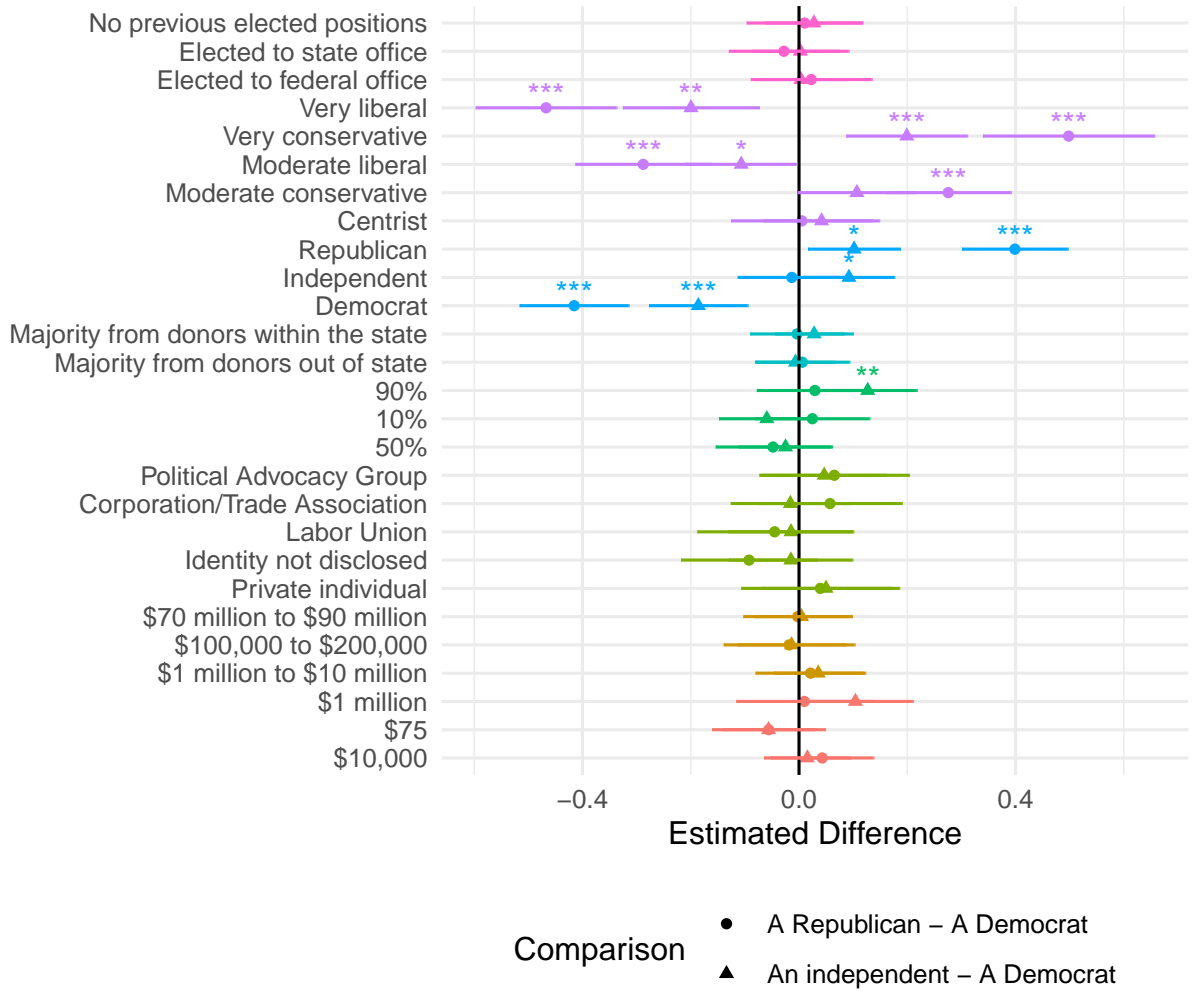
The three models pool observations across each of the four proposed policies, and include issue fixed effects. $\Delta_{\text{D-R}}$ denotes the difference in coefficients between Democrat and Republican-identifying subjects (I indicates Independents). $|z|_{\text{D-R}}$ denotes the Z-score of $\Delta_{\text{D-R}}$,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

Figure E2: Comparison of causal effects between Democratic, Independent, and Republican respondents in the sample, for those exposed to additional political cues.



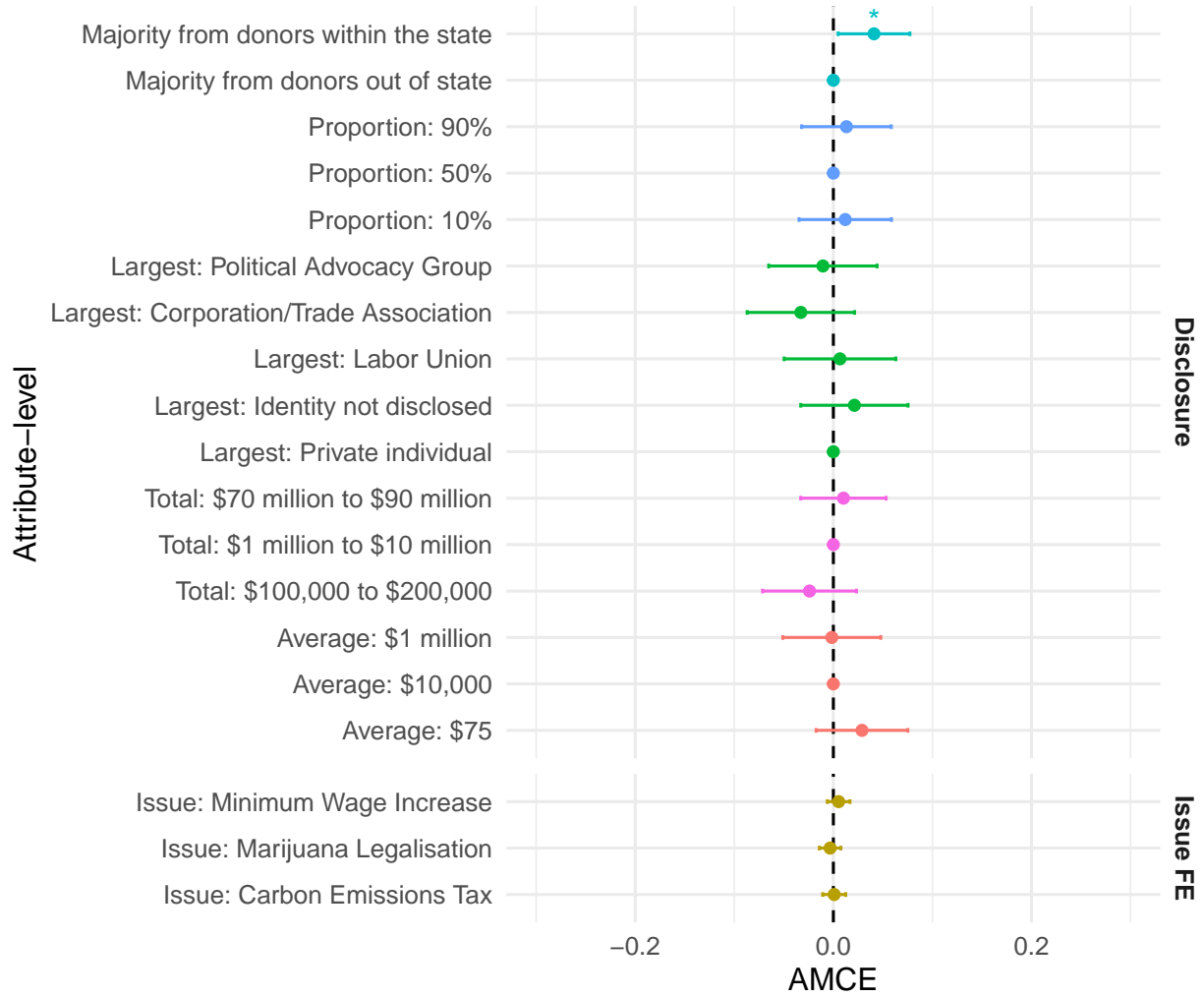
Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$.

Figure E3: Difference in marginal means for each attribute level by respondents' party identification, for subjects exposed to additional party, ideology, and valence attributes.



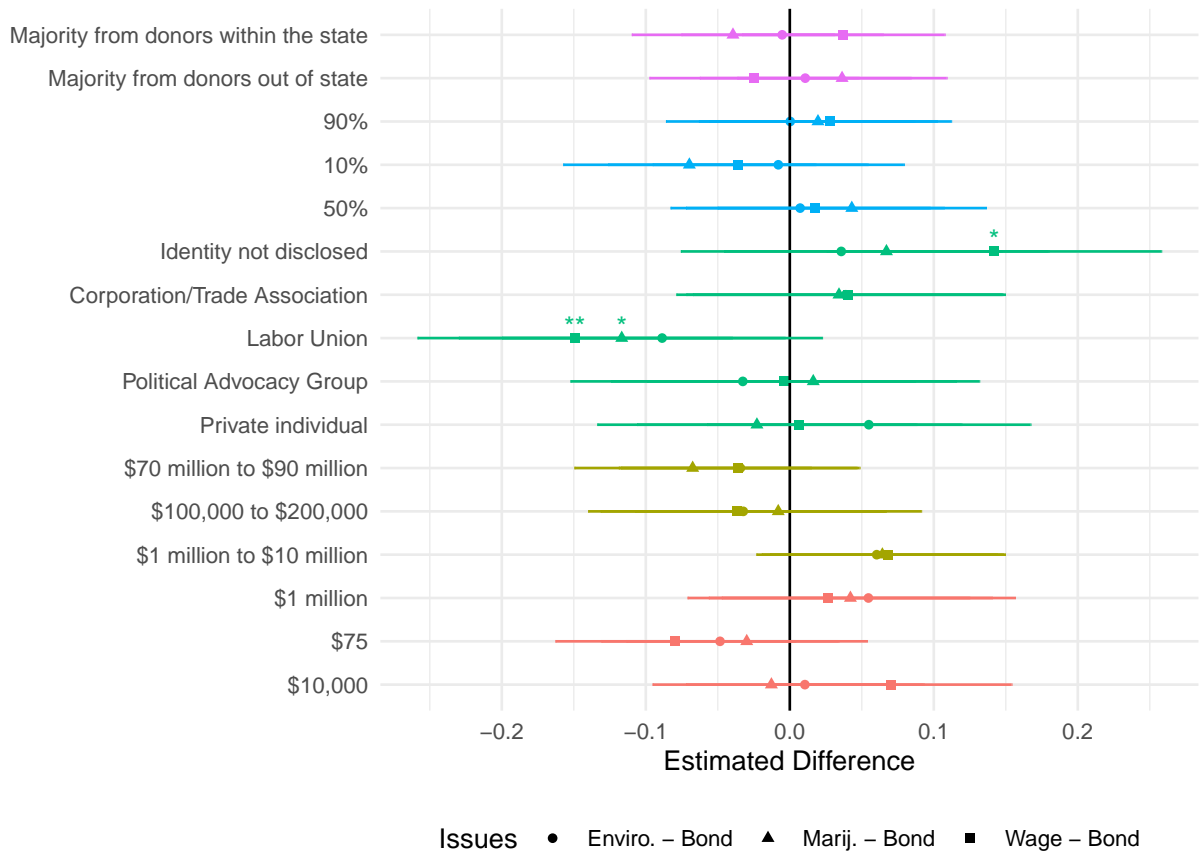
Estimated differences are shown with 95 percent confidence intervals. Stars indicate statistically significant differences at $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$.

Figure E4: Initiative conjoint model pooled across all four policy issues, and including controls for respondents' self-reported partisanship



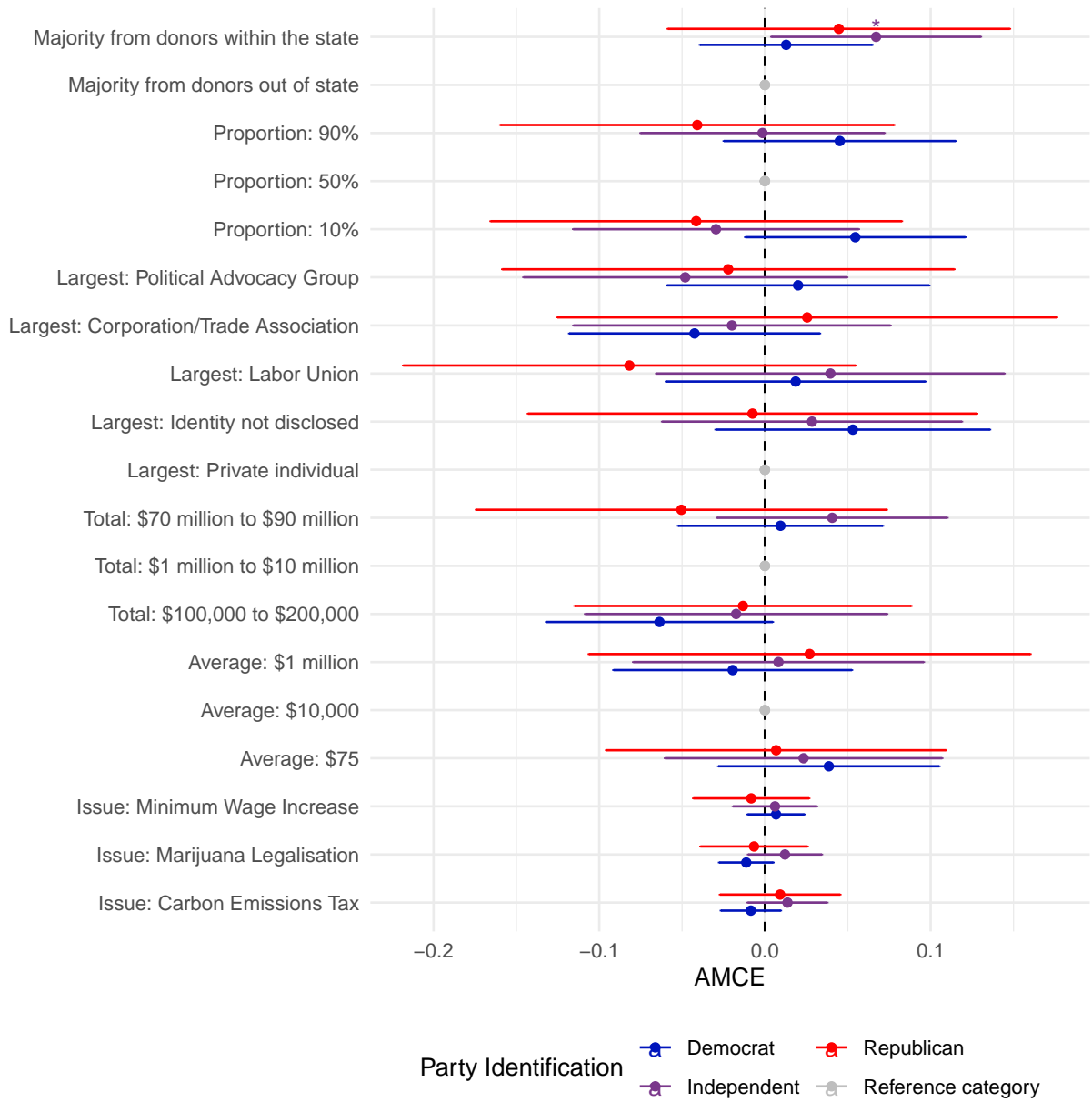
Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at $*p < 0.05$, $**p < 0.01$, and $***p < 0.001$.

Figure E5: 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 E6: 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 $***p < 0.001$.

F Subjects' Support For Hypothetical Initiative Campaigns

Table F1 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 F1: 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 F1 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.

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