

# Knowledge or ignorance? Assessing the causal effects of campaign finance disclosure on vote choice\*

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## Abstract

Fine-grained information about campaign finance is abundant in the American political system. But do these disclosures affect vote choice? Existing studies on the effects of disclosure tend to focus on voter perceptions, few explicitly test the robustness of disclosure to partisan signals, and none have compared the effects of disclosure across representative and direct democratic elections. This paper addresses these outstanding issues by implementing a series of conjoint experiments that jointly assess the effects of disclosing campaign finance information across types of election and varying levels of political information. I find that when partisan cues are not primed, participants do use various features of a candidate's financial profile to influence their vote choice. When these partisan signals are explicit, however, the effects of describing a campaign's financial profile on vote choice drop away. In ballot initiative contexts, I find similar results: in the presence of explicit policy choices, vote choice is not affected by disclosure.

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Information about campaign finance is abundant in the United States. At both the federal and state level, most donors are required to disclose publicly both the amount and to whom they give money. Since *Buckley v. Valeo* (1975),<sup>1</sup> the Supreme Court has continuously reaffirmed its position that mandatory disclosure provides voters with an “informational benefit” (Briffault, 2010). Voters, the Court argues, can infer relevant information about political campaigns. In the formal theory literature, too, disclosure mechanisms are argued to be Pareto improving features of electoral systems (Coate, 2004). Campaign spending gives voters greater information about candidates, but comes at the expense of favours granted to those interest groups who fund that spending. Voters therefore face a trade-off between knowledge about candidates and the compromises made in favour of narrow but well-funded interests (Ashworth, 2006).

Theoretical work has discussed potential mechanisms through which voters might use information from campaign finance disclosures (Prat, Puglisi and Jr, 2010), but what do voters do in practice?<sup>2</sup> Recent empirical research on the impacts of disclosure is mixed (Primo, 2013; Sances, 2013; Dowling and Wichowsky, 2013; Ridout, Franz and Fowler, 2015; Dowling and Wichowsky, 2015; Dowling and Miller, 2016; Wood, 2017, 2018; Rhodes et al., 2019; Spencer and Theodoridis, 2020). Estimating the independent effect of disclosure on observed vote choice is difficult given the endogeneity of factors like electoral viability and popularity to the actions of donors (Arceneaux, 2010; Brown, 2013). Hence the vast majority of research in this area has focussed on experimental manipulations in survey and lab environments. This research shows that while voters’ attitudes towards candidates are not immune to the effects of disclosing donor information (Wood, 2017; Spencer and Theodoridis,

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<sup>1</sup>*Buckley v. Valeo* (Argued November 10, 1975)

<sup>2</sup>This paper does not address the related issue of whether voters, of their own volition, access disclosure information. Some evidence suggests that voters exhibit limited proclivity to do so (Primo, 2013). Indeed, the sheer volume of disclosure now made available as a result of online reporting may make it harder for voters to access and infer relevant snapshots of campaigns (Briffault, 2010). Organisations like OpenSecrets.org and the National Institute of Money in State Politics do seek to simplify and summarise these disclosures, and in this paper I focus primarily on aggregated statements about campaigns’ funding.

2020), the marginal informational benefits of doing so may be limited (Primo, 2013).

Several issues within this experimental field remain understudied. First, few studies have examined how disclosure affects vote choice in a forced choice context (Dowling and Wichowsky, 2013). Studies typically focus on separate assessments of each candidate (Ridout, Franz and Fowler, 2015; Rhodes et al., 2019; Dowling and Wichowsky, 2015), perceptions of corruption (Spencer and Theodoridis, 2020), or perceptions of a candidate’s positions (Sances, 2013) or those of interest group’s (Primo, 2013). While disclosure may shift voters’ perceptions about candidates, are these shifts sufficient to induce a change in vote choice? Second, it is unclear how robust disclosure’s effects are to other relevant features of the electoral context. Do the observed effects of disclosure persist when partisan signals voters are likely to receive during an election are revealed? While some studies explicitly control for partisanship (Dowling and Wichowsky, 2015; Dowling and Miller, 2016; Rhodes et al., 2019), the extent to which partisanship intervenes on any effect of disclosure remains understudied. Third, previous experiments focus on candidate elections, but the effects of disclosure may be different in initiative campaigns. The lack of explicit partisan signals, and the single-issue focus, may mean voters attach different weights to various dimensions of an initiative campaign’s funding compared to candidate campaigns. Initiative campaigns, moreover, are prominent policymaking venues in the United States, with vast sums of money spent for and against propositions (Stratmann, 2010). Despite this, we know little about the comparative differences on the effects of disclosure across types of election.

In this paper, I address these outstanding concerns using a series of experimental conjoint surveys on a subject pool of US citizens. I test the effects of disclosure on vote choice across two types of election – gubernatorial elections and ballot initiative races. I focus on five different facets of disclosure that may affect vote choice: the financial size of the political campaign, the average size of a donation to the campaign, the type of largest donor, proportion of funds provided by that donor, and the geographic origin of donations. Isolating effects of each of these facets refines our understanding of *how* voters use disclosure

information to inform their vote choice, if at all. Conjoint experiments efficiently isolate the causal impact of disclosure when delivered in an aggregated and simplified manner, at the point at which individuals cast their “vote”. Since respondents are posed with a forced choice between two candidates, or the choice to endorse or reject a policy proposal, these experiments approximate voting scenarios more closely than some previous work.

I also implement a novel modification to typical conjoint designs by randomly varying not only the content of various campaigns attributes, but also the number of attributes displayed to respondents. This design builds on recent efforts to control for informational equivalences – explicit cues that affect beliefs about unobserved factors upon which a respondent acts – within experimental research (Dafoe, Zhang and Caughey, 2018). Varying the number of attributes allows researchers to test whether observed causal effects are resilient to richer information environments that more closely resemble the electoral context voters face when choosing between candidates.

I use this approach to help disentangle the “possible” effect of disclosure – whether voters are ever receptive to campaign finance cues – from the “actual” effect of disclosure – what voters are likely to do given other relevant cues that are common in more realistic electoral settings. I use the additional layer of randomisation to test whether the effects of disclosure are resilient to respondents’ partisanship (and that of the candidate, campaign or donor in question) (Dowling and Miller, 2016; Rhodes et al., 2019; Spencer and Theodoridis, 2020). Half of all subjects receive additional randomised information about candidates’ ideology, partisanship and political experience. This strategy more robustly tests whether any observed effects of disclosure operate only by priming individuals’ perceptions of relevant background characteristics.

I then compare these results to the same subjects’ behaviour in hypothetical ballot initiative elections. By comparing across types of election, I can assess whether the impact of disclosure differs when voters consider specific policy issues as opposed to candidates for public office. This paper provides the first test of whether the extent of informational benefit

differs between direct and representative forms of democracy (Briffault, 2010).

I argue that while disclosure can impact vote choice its total effect is negligible once overt but common partisan signals are included. When subjects are presented with aggregate disclosure information alone, voters do show some propensity to move away from candidates with high average donations and relatively concentrated groups of donors. But when the candidates' previous experience, ideology and partisanship are revealed, these effects are substantively reduced and statistically indistinguishable from zero. This paper finds little support, therefore, that in real world contexts disclosure has a distinct impact on vote choice.

With respect to initiative elections, moreover, I do not find any significant effect of disclosure on vote choice. The similarity between these results and the candidate condition with partisan signals suggests specific policy information plays a similar role to overt partisan cues in the case of candidate elections. Subjects in the experiment appear to have relatively fixed political views on issues, which disclosure does not seem to alter.

Taken together, these results refine our understanding of what information voters find relevant when voting in different electoral contexts. Given the null findings, these results help explain why voters exhibit limited proclivity to access disclosure information of their own volition (Primo, 2013). More broadly, contributing to a longstanding field of work surrounding how relatively uninformed voters use new information to make political decisions (Lupia, 1994; Carpini and Keeter, 1996), these results suggest campaign finance disclosures do not provide substantial heuristic cues.

From a policy perspective, my results suggest that consolidating campaign finance data into a clear and easy-to-digest format has little effect on voters' evaluations of candidates and propositions. The Supreme Court's justification for disclosure, which increasingly rests on the informational benefit assumption, appears unfounded – at least in the aggregate form it is presented in this experimental context. That is not to say there are no benefits to disclosure at all. Campaigns may act differently, soliciting unsavory donations, if their actions are not visible. In that sense, disclosure (irrespective of who then accesses it) may

dissuade unethical behaviour. And other forms of aggregated disclosure – news bulletins, investigative reporting, criticisms made by competing candidates – may still affect vote choice. More narrowly, democratic values do not appear to be safeguarded by voters actively refining or altering their preferences based on campaigns’ financial profiles.

## 1 The informational benefit of disclosure

In *First National Bank of Boston v. Bellotti* (1978),<sup>3</sup> 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 (Jiang, 2018). More recently, in *Citizens United v. FEC* (2010)<sup>4</sup>, 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.<sup>5</sup>

These justifications, however, leave it unclear *how* voters use disclosure to evaluate campaigns, and whether in reality this has a substantial effect on vote choice (Jiang, 2018). Moreover, despite the vast amount of money spent in initiative campaigns in the US, it is unclear whether we should expect the informational benefit to be similar across types of election. Does disclosure make up for informational deficiencies when other sources of information are not forthcoming? Or does disclosure have a separable, independent effect on voter evaluations irrespective of the presence of other information?

Assume, in the most abstract terms, that voters make a choice (for a single candidate, or an ordering of preferences etc.) with the goal of maximising their own utility. This decision is multi-dimensional: voters may care about a candidate’s or campaign’s ideology, specific

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<sup>3</sup>*First Nat. Bank of Boston v. Bellotti* (Argued November 9, 1977)

<sup>4</sup>*Citizens United v. Federal Election Com’n* (Argued March 24, 2009)

<sup>5</sup>In Appendix Section A.2 I discuss the implications of the *Citizens United* decision for disclosure-based regulation in more detail.

issue positions, valence characteristics, and so on. Assume further that the problem the voter faces is about estimation. Given a set of signals (across these multiple dimensions) 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 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 typically hard to observe. Candidates and campaigns may well suppress information or qualities that are deemed harmful to their electoral prospects. Individuals overcome these overly-taxing cognitive demands by using 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”, are not necessarily more error-prone than other forms of knowledge acquisition (Gigerenzer and Gaissmaier, 2011, p.451). With respect to political decisionmaking, heuristics can be very effective for evaluating the content and viability of various policies (Lupia, 1994).

In theory, following the logic of the US Supreme Court, campaign finance disclosure may play the role of a heuristic device.<sup>6</sup> It seems unlikely that voters would favour candidates with higher donation totals, in and of itself. Rather voters may prefer campaigns with higher total donations because it signals something about hard to observe but relevant characteristics of candidates (Wood, 2017). 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.

To make this logic clearer, suppose voters’ estimations of each candidate depend on only

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<sup>6</sup>Disclosure is, more accurately, two separate processes. First, electoral actors must disclose to government agencies their financial interests (contributions and expenditures) within electoral campaigns. Dependent on the size of the donation, disclosure is required both of the donor and the recipient. A separate stream of disclosure research focusses on the potential “chilling effect” of requiring individuals to declare their donations in the public sphere. The extent of this effect is contested, with some evidence suggesting disclosure has a negative effect on the likelihood of contribution (La Raja, 2014), while others argue the effects are, in reality, negligible (Wood and Spencer, 2016). Second, these financial submissions are disseminated to the public either through public records or via endorsement statements in political advertising.

two factors: the candidate's ideology and their valence.<sup>7</sup> Voters have some utility function to discriminate between candidates across these two dimensions. A candidate may trade-off some level of competence for closer ideological proximity, and *vice versa*.

Panel (i) of Figure 1 represents a configuration of a voter's ideal preference, indifference curves, and uncertainty surrounding the positions of two candidates (A and B). V knows that A and B lie somewhere within the two grey circles respectively, with the grey crosses indicating their midpoint. Since the midpoint of B is just closer to V than the midpoint of A, V will choose B.

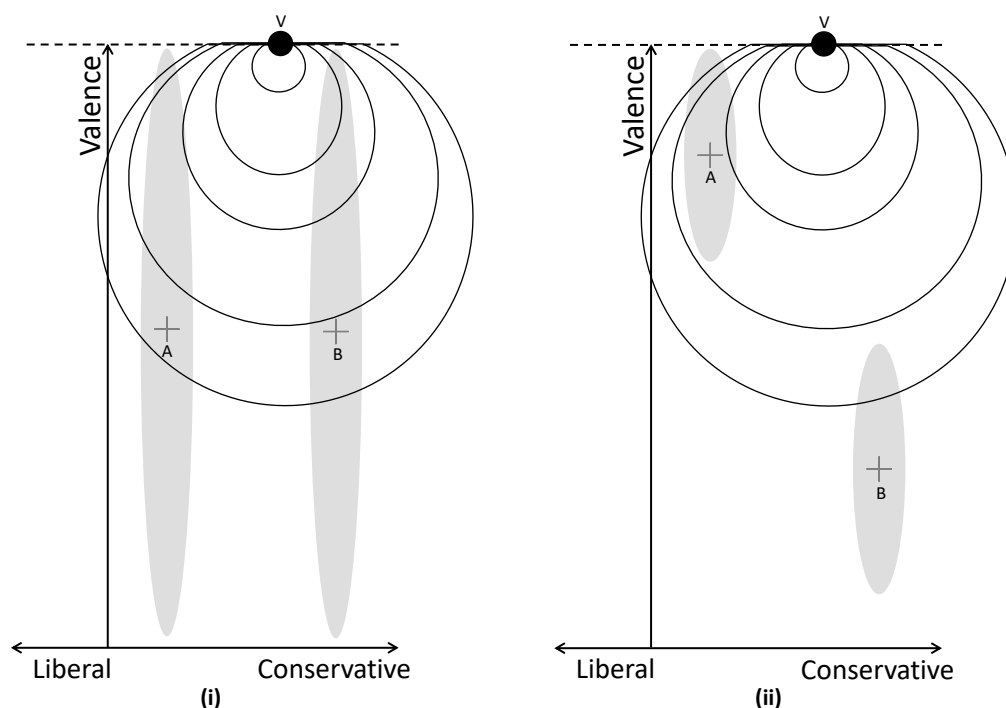


Figure 1: Hypothetical placement of a single voter (V) and two candidates (A and B) along ideological and valence dimensions. V is the voters ideal point. The capped oval lines are V's indifference curves. Grey circles represent the uncertainty of V about the two candidate's respective positions, with midpoints marked by crosses. Panel (i) indicates a situation of high uncertainty; panel (ii) indicates a situation of lower uncertainty after V receives additional cues.

Now suppose that V receives (effective) additional cues from campaign finance disclosure.<sup>8</sup>

<sup>7</sup>For the sake of abstraction, valence here is a bundled dimension that includes concerns about trustworthiness, effectiveness in office, and previous experience in office. In practice, these are likely to be separable dimensions with varying levels of uncertainty in electoral contexts.

<sup>8</sup>Different aspects of a campaign's financial profile may inform voters about valence and/or ideology. A



These cues alter the shape and size of the uncertainty surrounding the positions of A and B. Panel (ii) demonstrates this logic. The reduced uncertainty, as a consequence of signals gleaned from financial disclosure, mean it is much easier for V to discern which candidate is the optimal choice. A is now closer to V than the midpoint of B, so V will change their choice to A.

In summary, this model shows how disclosure can impact vote choice by refining uncertainty over candidate characteristics, making it easier to discriminate between multiple candidates. Campaign finance cues improve the precision of voters' estimates regarding the position of a candidate in multi-dimensional space. But what about when relevant information is made explicit to voters? And does this logic also apply in non-candidate elections? I next discuss each of these issues in turn, before setting out my expectations regarding how specific facets of disclosure may affect voter evaluations.

## 1.1 Effects of disclosure with known information

The theory above suggests that irrespective of other visible characteristics of the candidate or campaign, information about funding reduces the uncertainty surrounding voters' estimations of valence. Disclosure, in other words, provides *additional* information for voters to use when assessing candidates. But not all characteristics are hard to observe. Candidates' partisanship is typically very visible and not subject to uncertainty. While a candidate's ideological position may be more uncertain, we would expect voters to typically know if candidates are "liberal" or "conservative" (or infer it from their partisanship). And voters are likely to know whether a candidate is an incumbent or not, even if they are uncertain over their trustworthiness or effectiveness.

Whether disclosure provides an additional informational benefit will depend on how sensitive voters' decisionmaking calculus is to the different dimensions at play. More concretely, campaign funded by business interests is likely to act as a cue about the conservative-leaning nature of a candidate. Large total donations, however, may signal that the candidate has high valence characteristics (Prat, Puglisi and Jr, 2010). I discuss these specific expectations at the end of this section.

if voters place more emphasis on the ideological dimension (compared to valence) and a candidate's ideology is not subject to much uncertainty, then disclosure may not in fact have an impact on voters' choices at the ballot. In which case, there will be no additional informational benefit of disclosure.

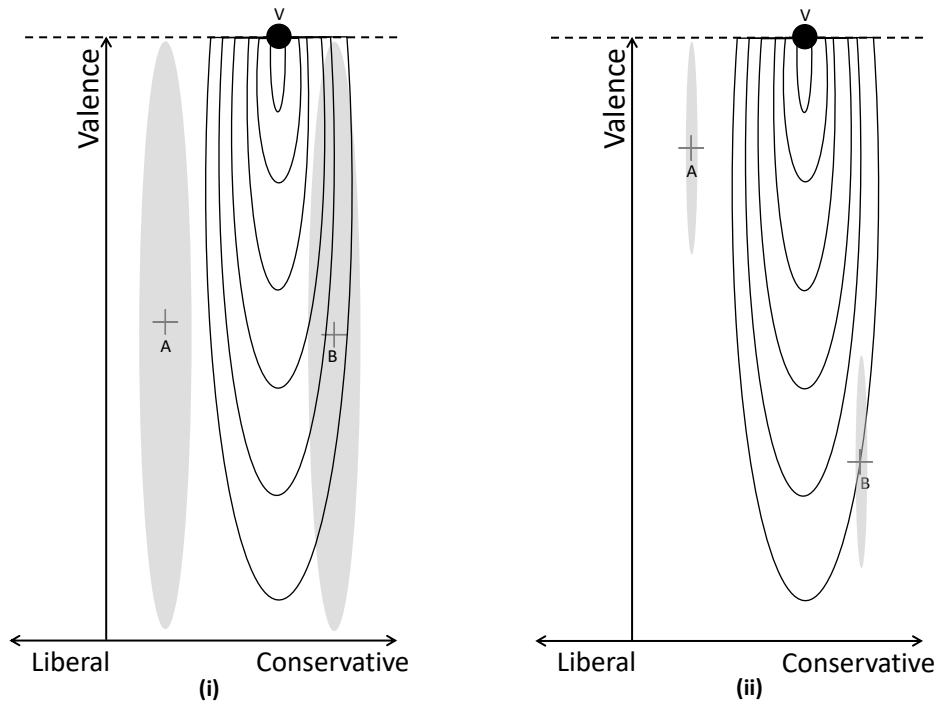


Figure 2: Hypothetical placement of a single voter (V) and two candidates (A and B) along ideological and valence dimensions. V's preferences are now weighted more heavily towards ideological concerns.

Consider Figure 2, which alters the weighting a voter places on the ideological and valence dimensions compared to Figure 1. V is now more sensitive to the ideological position of candidates than their valence, as indicated by the compressed indifference curves that are much more sensitive to shifts along the ideological dimension. The arrangement of positions is identical to Figure 1, but V now prefers B in panel (i). As before, panel (ii) depicts the refined estimates of V once disclosure and other relevant cues are taken into account. V's indifference curves remain constant. Since some background features about ideology are now explicit, V is almost certain of the ideological position of the two candidates. While disclosure has also reduced uncertainty on the valence dimension, V still continues to prefer

B. Disclosure here, then, would not affect vote choice.

Put simply, if voters' preferences are dominated by ideological (or partisan) concerns, explicit primes may simply swamp any effect of disclosure. This result matters because such information is typically observed and highly salient in candidate elections. Existing evidence on the effect of partisanship on disclosure information is mixed. While some argue the effects of disclosure are mediated by copartisanship (Dowling and Miller, 2016), others have recently argued that the effects of disclosing group support can in fact be robust to the inclusion of partisan controls (Rhodes et al., 2019).

For campaign finance to provide an informational benefit, it must refine voters' choices even in the presence of other explicit signals. Put another way, the effects of disclosure (if there are any) should be robust to the inclusion of other information, like party affiliation, that voters would expect to directly observe in an election and which increase voters' certainty over some, but not all, electoral dimensions.

## 1.2 Effects of disclosure across types of electoral campaign

The type of electoral contest may also mediate the effect of disclosure. Since ballot initiative elections are devoid of explicit party labels and other cues that typically guide vote choice, the benefit of disclosure could be *greater* (Garrett and Smith, 2005; Briffault, 2010; Primo, 2013). The US federal Courts itself is inconsistent on this issue. The opinion in *Sampson v. Buescher* (2010) for instance, argues the informational benefit of campaign finance is limited in direct democratic contexts. But other rulings have upheld the notion that the informational benefit is at least as important in initiative campaigns (Jiang, 2018).

Theoretically, there do seem to be “valence” concerns that are hard to observe directly, but which might influence vote choice. Voters may be concerned about the likelihood that the drafted legislation will in fact achieve its stated aims. They might also be concerned that the given legislation is anticompetitive, favouring a narrow set of out-of-state interests, even if they are broadly supportive of the policy in general. And voters may still want to

estimate the ideological position of the policy to compare its compatibility with their other political beliefs.

On the other hand, while initiative campaigns lack overt partisan signals, they do focus on specific policy proposals – whether to increase the minimum wage, lower prescription drug prices or curtail state governments’ taxation powers, for instance.<sup>9</sup> The specificity of initiative campaigns could, in fact, override other cues in a similar way to how partisanship 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.

What remains unanswered is whether voters’ behaviour is influenced differently across representative and direct democratic elections. To the best of my knowledge, this comparative aspect of disclosure (between election types) has not been tested before.

### **1.3 Relevant facets of disclosure**

Measuring the effects of financial profiles on vote choice using observational data is difficult (Arceneaux, 2010; Brown, 2013). A campaign’s financial profile is endogenous to the political context itself. Whether a candidate receives funding is a function of that candidate’s prior likelihood of victory and assessments of electoral viability. Differences between safe and marginal seats will also alter the financial profile of candidate’s campaigns. As a result, most works on the effects of disclosure have used experimental designs - predominantly with respect to political advertising. This research suggests that, absent other partisan signals, declaring the sponsor of an advertisement increases voters’ abilities to place candidates ideologically (Sances, 2013). And while “attack ads” are less effective when disclosure reveals the identity of the sponsoring group (Dowling and Wichowsky, 2013; Ridout, Franz and Fowler, 2015), candidates can avoid a voter backlash if the attacking ad is sponsored by someone external

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<sup>9</sup>It is worth noting that parties or their members can come out in favour of initiatives. These races are not necessarily totally devoid of partisan cues. The point is simply that initiatives are not labelled as ‘Democrat’ or ‘Republican’, and many issues are not explicitly supported by a party.

to the campaign (Dowling and Wichowsky, 2015).

The focus on advertisements provides specific and compelling evidence that disclosure can affect voter decisionmaking, specifically by bolstering the inferences individuals make about politically relevant characteristics of candidates and campaigns. Voters do seem to infer (rightly or wrongly) different intentions by different donor sources (Dowling and Wichowsky, 2013). Yet advertisements are just one form of political expenditure, and revelations about specific ads tell voters little about the overall funding of campaigns. Attack ad sponsors may not be representative of a campaign’s full financial profile (even if they are often visceral examples), and certain donors may select into or out of this form of highly visible political activity.

To understand the broader effects of disclosure, therefore, I consider aggregate level disclosure – summaries of a campaign’s full financial profile. Existing studies on aggregate disclosure demonstrate that voters value transparent profiles (Wood, 2017), and that aggregate disclosures can influence voters’ perceptions of candidates’ corrupt tendencies (Spencer and Theodoridis, 2020). On the other hand, Primo (2013) finds that the marginal benefit to voters of disclosure contained in news reports is ‘trivial’ – measured with respect to correctly identifying the positions of interest groups on ballot measures.<sup>10</sup>

These studies on voter perceptions are important, but leave open the question as to whether changes in perceptions based on disclosure affect voting itself. One paper (to the best of my knowledge) directly assesses the effect of aggregate disclosure on the likelihood of voting for a candidate, finding some support that this additional information alters vote choice (Dowling and Miller, 2016). Partisan information about the candidate is given to all respondents, rather than randomised between control and partisan-treated subsets.

To refine our understanding of these mechanisms, across electoral contexts, this paper focuses on five fundamental factors that describe different aspects of a campaign’s donor

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<sup>10</sup>While access to information is randomised in Primo’s (2013) experiment, the option to actually view that information is left up to the subject. Primo finds both a low proclivity to access those disclosure-related articles, and insignificant differences in the number of correctly identified interest groups once other information accessed by respondents is taken into account.

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 factors are drawn from a combination of previous studies (Prat, Puglisi and Jr, 2010; Dowling and Miller, 2016; Wood, 2017; Spencer and Theodoridis, 2020) and further considerations about the possible facets of disclosure that might influence vote choice.

In comparison to previous studies, these facets seek to characterise financial profiles more comprehensively by capturing different types of inferences that voters might make. Below I discuss each of these factors in turn. Given the findings outlined above, there are competing intuitions as to both the size and direction of these effects. The purpose of the remainder of this section therefore is to motivate why these factors *might* matter. Where applicable, I highlight pre-experimental uncertainty about whether a given feature will have a positive or negative effect.

**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. A larger campaign is relatively unrestricted in the number of research, advertising, and get-out-the-vote operations it conducts. On the one hand, therefore, the total amount of campaign funding (holding constant its composition) is a potential indicator of *viability*. Smaller campaigns, unable to attract significant financial capital (relative to other campaigns), may be viewed as less viable by voters. Voters may use the size of the campaign as a signal of how donors, who may be more politically informed, have “pre-screened” the viability of the campaign in question.

On the other hand, voters may be distrustful of campaigns with very large donation totals (again, holding constant the composition of the campaign). If a campaign raises very large amounts of money, then voters may question the motivations of that campaign, since it might appear as if the campaign is trying to “buy” the election. 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 the campaign.

**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 what *sort* of donor typically contributes to the campaign. This heuristic is already utilised by candidates in political campaigns. Both Bernie Sanders (Bump, 2016*a*) and Donald Trump (Bump, 2016*b*), for instance, touted their low average donation size (despite the relatively large total donations) as an indicator of their popularity in the 2016 electoral cycle. This trend continued into the midterms, particularly among candidates claiming strong ties to grassroots communities (Finkelstein, 2018).

Low average donations would suggest 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 (the very wealthy or corporate groups) are the predominant base of support for that campaign. In all likelihood, if voters respond to this cue, we would expect them to be averse to candidates that are seen to serve narrow interests. It seems unlikely that the opposite effect would be true, that voters infer some positive quality from candidates whose average donation is very high.

**Type of largest donor.** The *type* of donor may give voters additional information about whether a candidate or proposal is broadly aligned with the sections of society they view favourably.<sup>11</sup> Broadly, donations can come from individuals or organisations. These organisations, for simplicity, can be divided into corporate, labor union and political advocacy types. Voters may associate any particular group’s involvement in the campaign as a signal of the section of society that candidate or campaign is likely to serve. These signals may have differential effects dependent on one’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” entities that, as charitable organisations,

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<sup>11</sup>The Supreme Court itself has ruled on similar issues related to the type of donor. In *McIntyre v. Ohio Elections Commission* (Argued October 12, 1994) the Court argued there was limited informational benefit to disclosing information about private individuals.

are not obliged to reveal their donors (Wood, 2018; Mayer, 2016; Rhodes et al., 2019). These “dark money” entities, while existing in a legal grey zone as 501(c)(4) entities, are especially problematic since their use is not uniformly distributed across the donor-spectrum. Dark money vehicles are useful primarily to those exceptionally wealthy individuals and groups who wish to obscure their involvement in the political process. This obscurity makes it very difficult to report such entities to voters through disclosure. While a 501(c)(4) organisation will be named, that name is typically mundane and uninformative to voters – 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/interest. A campaign solely funded by one corporate Political Action Committee (PAC) may cue voters differently to one where the same PAC is the largest donor, but contributes less than half of all funding. Different to average donations, this feature explicitly captures the concentration of financial support, rather than providing a signal about the base of support.

Intuitively, it seems likely that if voters use this cue, they would be adverse 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.

**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.

This feature would have greater impact, hypothetically, if voters perceive some integrity to the *state* polity. 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 (weak) signal of whether candidates care about the concerns of their constituency. This is particularly relevant for the sorts of elections considered in this article: state level races – for governor or ballot initiatives – that deal with state-specific political questions.

The above five factors are not an exhaustive list of features at the aggregate level. Taken together, however, they are salient and aggregate-level indicators that characterise the overarching structure and composition of campaigns’ financial profiles. These attributes can also be compared across different types of electoral races, and accord with similar features explored in previous research (Prat, Puglisi and Jr, 2010; Dowling and Miller, 2016; Spencer and Theodoridis, 2020).

## 2 Conjoint experiment to assess impact of disclosure on vote choice

To assess the causal effects of disclosure mechanisms across election types and controlling explicitly for potential informational equivalence, I conduct a series of conjoint experiments examining vote choice in the presence of disclosure. Subjects are presented with a forced choice between two campaigns – either two candidates or the support and opposition groups for an initiative proposal.<sup>12</sup>

Conjoint survey experiments are an efficient way to test the extent to which different attributes affect voters’ choices in a robust, inferential manner (Hainmueller, Hopkins and

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<sup>12</sup>All replication materials – including R code and experimental data – is available at [https://github.com/tsrobinson/cf\\_conjoint](https://github.com/tsrobinson/cf_conjoint).

Yamamoto, 2014). By randomising multiple attributes simultaneously, conjoint analyses enable researchers to distinguish the effects of each facet of disclosure separately. Concurrent randomisation of each attribute gives reasonable statistical power even for relatively small numbers of participants.

Moreover, since conjoint survey experiments typically ask respondents to choose between two profiles – in this case, to support or oppose an initiative campaign, or to vote for candidate A or B – this design is a natural analogue for the sorts of decisions voters make in American elections. Thus, we can assess subject behaviour within an experimental environment that more closely replicates the actual environment we wish to generalise our claims to. While this point is reasonably obvious with regards to two-party candidate elections, it also has benefits for the experimental study of ballot initiatives too. The decision a voter makes with respect to initiative policy is binary – whether or not to endorse a proposal – but there are typically separate “Yes” and “No” campaigns. Opposition groups raise their own funding sources and motivations, and play a key role in advocating for the status quo. Given the important role opposition groups play in initiative races (Gerber, 1999), there good reason to include them in the study of disclosure mechanisms.

## 2.1 Randomising the *number* of attributes

Typical conjoint designs randomly vary the content of each attribute (the levels). 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, if an attribute-level has a significant positive effect on the likelihood of choosing a candidate, for example, the causal validity holds only given the context in which the decision was made. Such claims are very useful when we want to test whether a given feature or level *can* have an effect on a voter.

Claims about generalisability of the observed 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.<sup>13</sup> The observed causal effects may not hold up in other contexts where some feature not included in the conjoint experiment acts on individuals’ behaviour. For instance, voters may react, in abstract, to financial disclosures within the confines of an online survey but ignore them in reality when they make actual, electoral decisions.

Implicitly in this discussion we are referring to two different causal concepts. On the one hand, there is the purely experimental causal effect of shifting voter behaviour given the experimental conditions alone. This is the “possible” effect, in the sense that researchers probe whether it is possible to shift the behaviour of respondents via certain treatments – in this case, attributes and attribute-levels. Regardless of whether all relevant features of the choice are included, the internal validity of any individual marginal effect is broadly ensured by randomisation procedures. On the other hand, the “actual” effect of a feature – namely that effect we expect to observe in real-life contexts – is the generalised effect from a given experiment. All controlled experiments will, to varying extents, only approximate the “actual” effect. The mere fact of controlled randomisation means that, even in field experiments, there is some manipulation that makes the study unrealistic.<sup>14</sup> In the case of disclosure-related behaviour, both of these effects are relevant. We want to know both whether respondents are capable of reacting to financial disclosures, and separately whether this effect is robust to more realistic contexts in which respondents know more information about the candidates themselves.

To distinguish these two separate effects, I randomly vary the number of attributes, as well as the levels within each attribute. Randomisation at the attribute level ensures unconfoundedness between those presented the full set of conjoint attributes, and those presented just the subset of financial disclosure cues. Comparing the estimated marginal effects for the common set of attributes shared across both groups helps illustrate whether

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<sup>13</sup>Note this is a separate issue to the generalisability criticisms raised by Abramson, Koçak and Magazinnik (2019).

<sup>14</sup>The exception are natural experiments, where randomisation is a natural feature of the context of study and is not controlled by the researcher (Titunik, 2020).

there is both any potential effect of disclosure, and how robust this is to the inclusion of other relevant signals.

This approach is particularly useful if we suspect that certain mechanisms may be swamped by stronger information signals. A conjoint containing all pertinent features may show that a given feature has no significant effect on voter behaviour, but it tells us very little about why that is the case. Researchers cannot discern from this design alone whether the potential effect is swamped by other features or whether the feature itself is simply inert on decisionmaking entirely. The reduced conjoint without additional attributes, moreover, gives a more sensitive analysis of which, if any, mechanisms disclosure operates through.

In short, the adaptation to the conjoint design I propose in this paper allows me to test whether disclosure has an effect on vote choice, and if so whether this effect is robust to the inclusion of other relevant cues not related to campaign finance disclosure. More broadly, this method can be adapted to other studies where researchers want to tease out the difference between the “possible” causal effect of an attribute, and the effect of the same attribute in more realistic, information-rich contexts.

## 2.2 Design

Participants were asked to complete two separate conjoint experiments – one choosing candidates in a hypothetical state gubernatorial election, and one focussed on a set of four initiative policy proposals. Table 1 provides details of all the conjoint levels across the two experiments. The specific attribute-levels are designed to give clear comparisons between different sorts of campaign funding. Figure 3 shows a demonstration of the funding information as presented to respondents in the candidate conjoint, and a screenshot of the initiative conjoint can be found in the Appendix.

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). While individuals in Oregon can donate unlimited amounts to gubernatorial candidates, Colorado caps private individuals' donations at \$575 per candidate (the lowest limit in the sample of states).<sup>15</sup> 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, given the salience and scale of the type of elections under consideration, these levels are broadly plausible even if there is some mismatch with actual state laws. For instance, while \$70 million in funding is large, Beto O'Rourke's 2018 senate campaign had receipts in excess of this level, and Proposition 61 (2016) in California saw opposition donations exceed \$100 million suggesting they are not implausibly high values in abstract.

**Candidate conjoint.** Respondents were presented with the funding profiles of two candidates running for gubernatorial office. Half of all participants ( $n = 204$ ) were randomly assigned to see three additional informational equivalence treatments: the candidates' party affiliations, their ideological position, and whether or not they have been elected to either state or federal office before. These three variables fix subjects' priors and thus enable us to test for the independent causal effect of disclosure on vote choice. The exact same funding attributes and levels were used across these two versions of the candidate conjoint. The value for each of attribute and candidate were randomly assigned. Figure 3 displays an example of what respondents were presented for each round of the candidate conjoint experiment.

**Initiative conjoint.** Participants were 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, and have been selected to capture a variety of different contemporary issues. The policy proposals are hypothetical but constructed to appear

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<sup>15</sup>See the appendix for a summary of contribution limits within each state in the sample.

Table 1: Conjoint attributes and levels

<i>All conjoints</i>	
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>Information Equivalence Treatment</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

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. Funding information was listed for 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 makes a total of four choices within the initiative experiment – one per issue. The order in which the issues appear was randomised.

Note that, unlike most typical conjoint designs, in the case of the initiative conjoint there is only one campaign over which respondents make a choice – whether to accept or reject

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.

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

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

the campaign. That said, as in actual initiative elections, groups mobilise around support and opposition campaigns to persuade voters to vote for or against the initiative. I treat these opposing campaigns as if they are separate ‘candidates’ in a typical conjoint design. Once an initiative has been qualified to the ballot, both groups are able to separately raise and spend money. It is important to capture the fact that opposition campaigns in initiative races are their own separate entities that make strategic decisions and attempt to influence the race’s outcome independently of the proposition’s campaign (Gerber, 1999).

**Randomisation procedure.** The order in which the candidate and initiative conjoint were presented to each subject was randomised, as was the assignment to the two different

Table 2: Hypothetical initiative policies

Initiative title and description
<p><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><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><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><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>

candidate conjoints. Within the initiative conjoint component, subjects were shown each initiative issue once and the order of these issues was randomised to limit any order-effect. Finally, across all conjoints, attributes were randomised with minimal restrictions to prevent implausible attribute-level combinations.<sup>16</sup> The appendix presents a series of tests verifying that these randomisation procedures were successful.

**Sample.** To ensure greater external validity of the results of this study, and to ensure that the types of election and information presented are meaningful to respondents, the conjoint experiments were conducted using a U.S. specific subject pool maintained by the Centre for Experimental Social Science, at Nuffield College, University of Oxford.

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

<sup>16</sup>For instance, a campaign raising \$100,000-200,000 could not have an average donation of \$1 million.



17 states where some form of initiative provision is in operation. In total, 390 eligible participants completed the experiment.

To ensure a similar baseline level of understanding across the sample, participants were asked to 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.

It is worth noting that there is a reasonably substantial bias in the sample with respect to partisan identification. 46 percent of respondents identified (post-experiment) as Democrats, 30 percent as independents, and 15 percent as Republicans. Initiative states may simply have more Democratic supporters (particularly those states on the West Coast oversampled in the first round of invitations). To assess the plausibility of the Democratic lean observed in the sample I take the average of the difference in party affiliations at the state level,<sup>17</sup> weighted by the proportion of respondents per state in the experimental sample. While the lean in the experimental sample is much larger, the expected lean towards the Democrats is nevertheless substantial (7.3 percentage points) suggesting that some Democratic bias is to be expected.<sup>18</sup> Further demographic information is available in the appendix.

**Causal assumptions.** For the models in conjoint analyses to have a causal interpretation, further assumptions about the design and implementation must be met (Hainmueller, Hopkins and Yamamoto, 2014). In short, these assumptions do hold. In the appendix I provide a detailed discussion of these criteria, and present a series of tests that verify each assumption – stability, no profile-order effects, randomisation, and balance – for this experiment.

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<sup>17</sup>Data taken from Gallup’s 2017 summary of state party affiliation, available at <https://news.gallup.com/poll/226643/2017-party-affiliation-state.aspx>.

<sup>18</sup>Moreover, given the aim of the study is to test for the presence of disclosure mechanisms, rather than estimate their effect in the wider population, the representativeness of the sample is not of paramount importance.

## 3 Results

### 3.1 The effect of aggregate disclosure

The first candidate conjoint (fielded to half of all subjects) presented only financial information to voters (and without partisan, ideological, or valence cues). To recover the marginal effects of each attribute, I estimate the following model<sup>19</sup>:

$$\text{logit}(\text{Vote for candidate?}) = \beta_1 \times \text{Average Donation} + \beta_2 \times \text{Total Donations} + \beta_3 \times \text{Largest Donor} + \beta_4 \times \text{Proportion from Largest Donor} + \beta_5 \times \text{Origin of Donations}. \quad (1)$$

Equation 1 recovers an estimate of the marginal effect for each attribute-level, relative to a baseline. The model is wholly additive – I estimate the independent marginal effects for each attribute, and not the interaction between any two attributes. This strategy best matches the theory in Section 1.3, which isolates non-interactive mechanisms through which different facets of vote choice might affect voter decisionmaking. The goal is to isolate what features of disclosure, if any, condition vote choice.<sup>20</sup> All standard errors are clustered at the individual-level to reflect the fact that individuals (from which we take multiple observations) are sampled from a much wider population of individuals (see Chapter ??).

Figure 4 plots the model coefficients. In this low-information environment, atypical of real electoral contexts, various disclosure mechanisms do have clear effects on vote choice. Voters are less likely to vote for candidates who receive a large proportion of their funds from a single donor, nor where the average donation amount is high. Donations mainly from within the state, labor union funds (relative to private individuals), and lower average

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<sup>19</sup>The results reported in this paper are not sensitive to the estimation strategy. Similar results can be obtained replacing the logistic estimator with OLS regressions (see replication code).

<sup>20</sup>An interactive theory would suggest that voters combine multiple aspects of disclosure to form an assessment of candidates. For instance, they compare both the average donation size (say it is small) and the total raised (assume it is high) and infer that this candidate is widely supported by regular voters. These sorts of interactions are plausible, but in theory assume more complex heuristic processes. Given the limited research in this area exploring the fundamental effects of disclosure, I defer these theoretical issues to future research. More practically, to estimate the marginal interactive effects of attributes within this conjoint design would require a larger sample size. Without strong theoretical reasons to exclude certain interactions, a fully-interactive model would inflate the number of estimated parameters and therefore require more observations to maintain sufficient statistical power. Inferences on the basis of an underpowered interactive model would be misleading.

donation amounts (relative to the baseline \$10,000) all have a positive effect on the likelihood of a candidate being chosen.

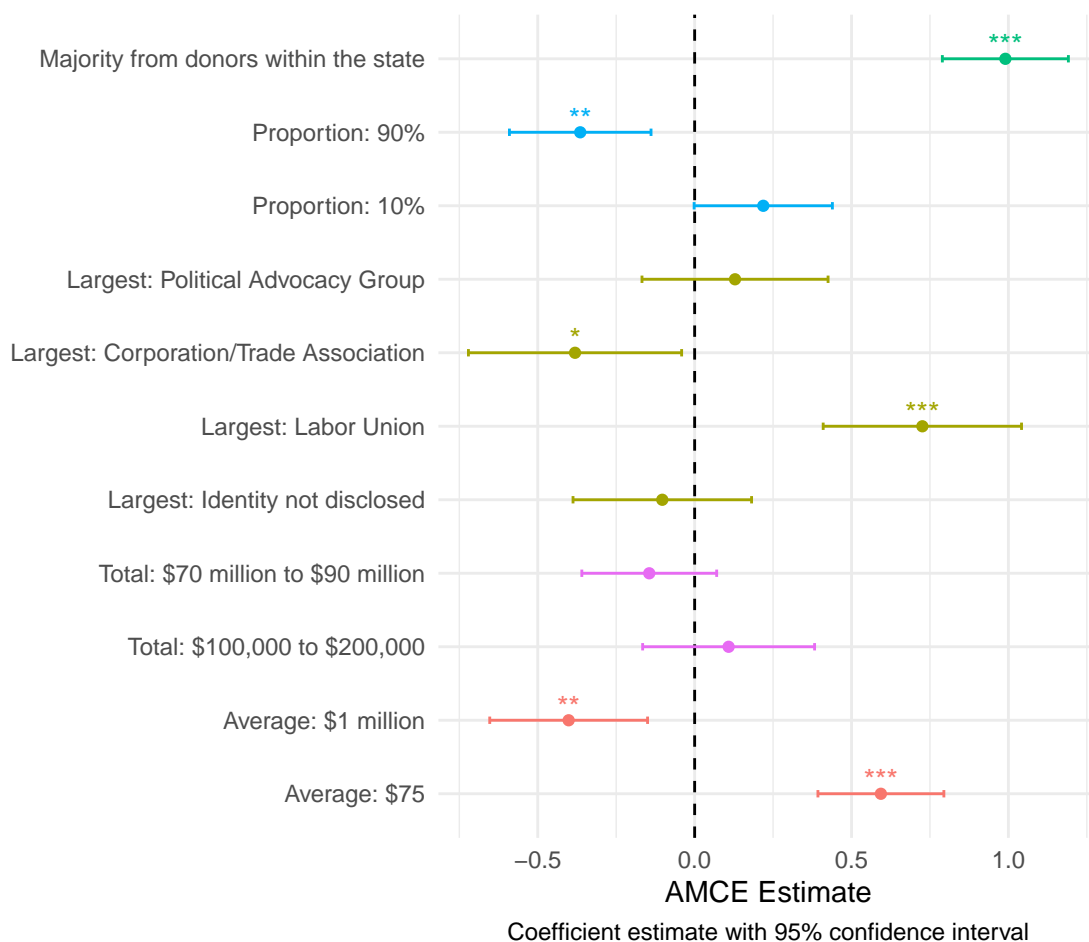


Figure 4: Candidate conjoint results (no additional information). Model coefficients are shown for the “control” experimental condition in which subjects only saw the funding information about the two candidates. All standard errors are clustered by participant, and the figure displays the 95 percent confidence interval around each estimate. Stars above coefficients indicate significance at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$  respectively.

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. Notably, the total size of a campaign had no substantial effect on voters’ choices.

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. It is nevertheless noteworthy that voters adjust their vote choice when presented with this information (absent other cues). 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 “dark money” 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, which aims to mirror the obscurity granted to those who use non-profit corporations to hide their identity, suggests that donor anonymity does not deter individuals from voting for that campaign. In other words, there is little evidence in this experiment of electoral punishment for obscuring the source of one’s donations.

Descriptively, the impact of these disclosure mechanisms are not dependent on subjects’ own political identification. I calculate the difference in marginal means for the attributes given subjects’ partisan affiliation (full results are reported in the appendix). There are no substantive divergences between the two groups in terms of effect direction. Only the difference between the marginal means of the PAC largest donor attribute-level is statistically significant at the 5 percent level, and then only between Democrat and Independent voters. Given the reduced sample size when considering these groups separately, however, caution should be exercised when extrapolating from these comparisons.

### **3.2 Priming subjects’ perceptions of candidates’ ideology and valence**

Elections are rarely fought on the topic of disclosure. Indeed, the real empirical question is whether disclosure augments voter behaviour in *typical* electoral scenarios - ones in which candidate ideology, partisanship and valence characteristics are known. The first set of results demonstrate the ability of disclosure mechanisms to affect vote choice. But does disclosure

have an independent effect once we control for the most relevant electoral variables?

For those subjects exposed to additional attributes, I run a similar logistic model to Equation 1 while also including additional parameters for the priming attributes of partisanship, ideology, and past experience. Given the inclusion of these variables, we should expect differential effects of the partisan and ideological primes dependent on subjects’ own partisan affiliation. 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:

$$\text{logit}(\text{Vote for candidate?}) = \sum_{i=1}^5 \beta_i \times \text{Disclosure Mechanism}_i + \beta_6 \times \text{Party} + \beta_7 \times \text{Ideological Distance} + \beta_8 \times \text{Valence}. \quad (2)$$

Figure 5 displays the results of this model. The independent effects of all but one feature of disclosure are negligible 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 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 valence attribute, somewhat surprisingly, does not exhibit significant differences between the attribute levels.

In the Appendix I report two further analyses to test the conditional causal effects of disclosure based on subjects’ partisanship. Broadly, I do not find that the effects of disclosure affect Democratic and Republican identifiers differently. Figure A5 estimates separate models for Republican and Democrats in the sample.<sup>21</sup> These results are substantively similar to

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<sup>21</sup>Running separate models for different subjects can be misleading (Leeper, Hobolt and Tilley, 2019). Comparing differences in the size of the causal effect tell us very little about preferences across subgroups. Suppose, for instance, we consider the effect of party label on Democrats and Republicans in separate models. If Republicans are already more supportive in general, the baseline level of support will also be high. Therefore, we would expect a smaller effect for Republicans compared to Democrats (given a bounded scale). The smaller effect, however, does not reflect less favourable attitudes by Republican respondents. Put

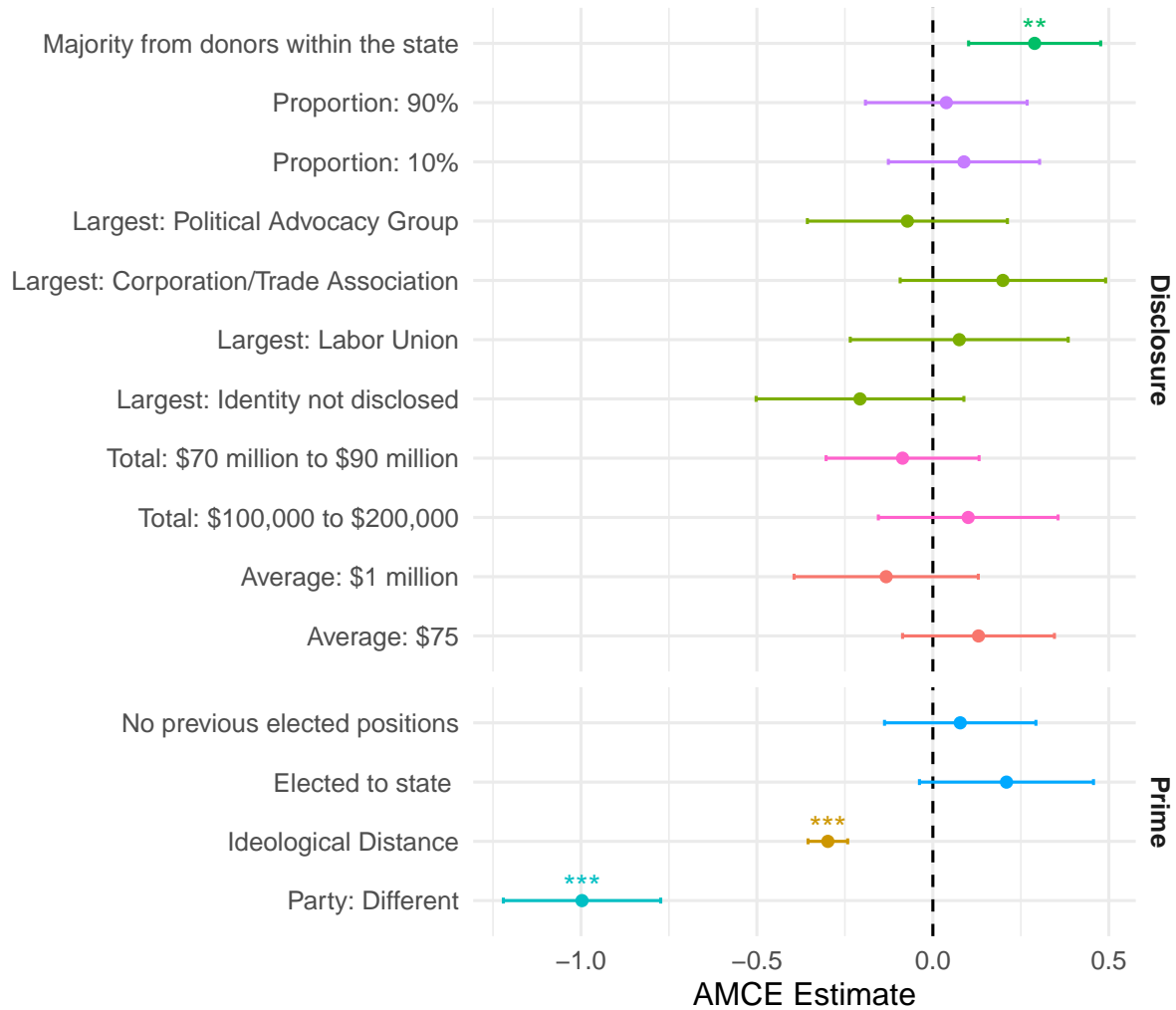


Figure 5: Candidate conjoint results - treatment condition in which partisan, ideological and previous experience attributes are included. Coefficient estimates for each attribute-level are shown with 95 percent confidence intervals. Stars above coefficients indicate significance at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$  respectively.

the analysis presented above. Partisan and ideological primes on the whole have large and opposing effects dependent on respondent's own partisan identification. Across all disclosure attributes, there are few substantive differences between Democrats and Republicans in terms of effect direction. Wide confidence intervals for the smaller Republican sample, however, suggest this model on its own is underpowered. Figure A6 reports the difference in

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simply, “similarity of conditional AMCEs therefore only convey similarity of the causal effect of the feature, but do not convey similarity of preferences unless preferences toward profiles with the reference category are equivalent across groups” (Leeper, Hobolt and Tilley, 2019, p.12).

marginal means between Democrat and Republican identifying subjects. These descriptive results again show that while there are large and highly significant differences for partisan and ideological primes, all differences in marginal means for the disclosure attributes are indistinguishable from zero.

Taken together, the results of the two candidate conjoint conditions suggest that while disclosure *can* affect vote choice, it is relatively inert in more realistic contexts in which overt political cues are present. Disclosure mechanisms that were shown to affect vote choice when subjects consider funding information alone, fail to augment vote choice once respondents are primed with candidates' ideological positions and partisan affiliation.

### 3.3 Disclosure's effect on initiative outcomes

Partisan and ideological cues appear to diminish the effect of aggregate disclosure considerably for candidate elections. But what about direct democratic races like initiative elections that typically lack these overt partisan and ideological cues?

Unlike the candidate conjoints where participants chose between two candidates, in the initiative conjoint participants chose whether to endorse or reject a given proposal. Table 3 shows the proportion of participants who 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).<sup>22</sup>

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<sup>22</sup>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 3 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 environmental 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 still statistically significant) than the vote proportions would suggest.

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

Table 3: Subject support and rating of initiative campaigns

In the remainder of this analysis, I report the results using the binary vote choice variable, although the substantive results of the model do not differ if I instead use subjects’ ratings of the two campaigns. Figure 6 displays the results of a logistic regression very similar to Equation 1, except I also include issue fixed effects.

The only statistically significant disclosure attribute is the origin of donations. This attribute’s effect is robust across all three conjoint experiments in this paper. Voters do seem to favour those campaigns that are funded mostly by donors within their own state. I return to the implications of this finding in the discussion.

For all other disclosure mechanisms tested in this model, there are no significant effects on vote choice (or the ratings of the two campaigns). Even in the absence of partisan cues, voters do not appear to change their opinions on both the campaigns or the issues when given information about the funding profiles of direct democratic campaigns (geographic disclosure notwithstanding). It appears that voters’ attitudes towards policies are reasonably fixed and that these issue preferences swamp any impact of disclosure on vote choice or individuals’



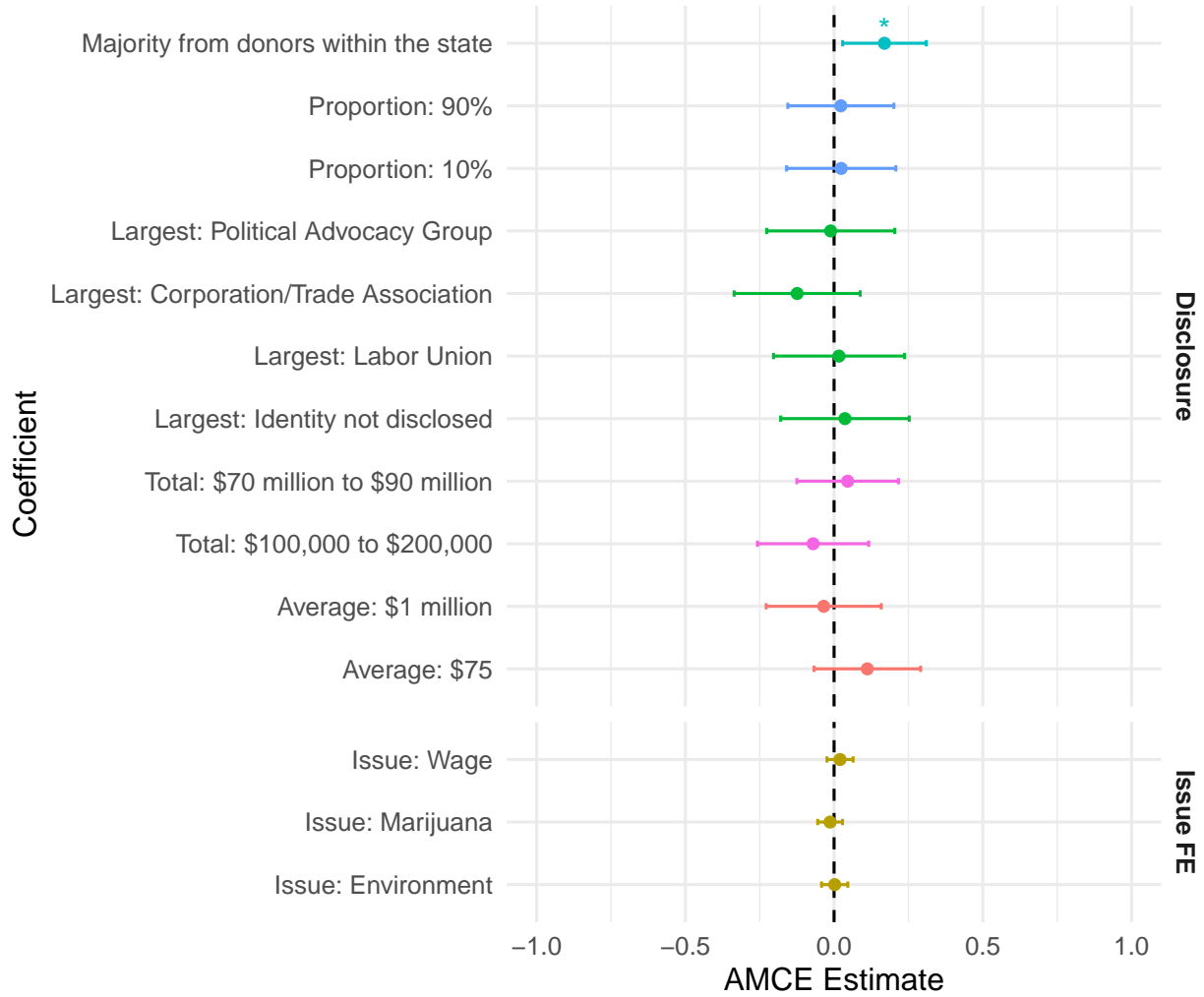


Figure 6: Initiative conjoint results. All responses across the four issues are pooled, and issue fixed effects are included to control for varying levels of support not contingent on disclosure mechanisms. Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate significance at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$  respectively.

assessments of campaigns.

The model reported in Figure 6 controls for the latent support of each issue area but assumes that disclosure affects voters the same way for each type of issue. It is plausible, however, that voters may use disclosure information to different extents when considering different issues. To check this argument I estimate separate logit models for each initiative issue separately.

Figure 7 plots the estimated coefficients. Large campaign finance totals are a significant

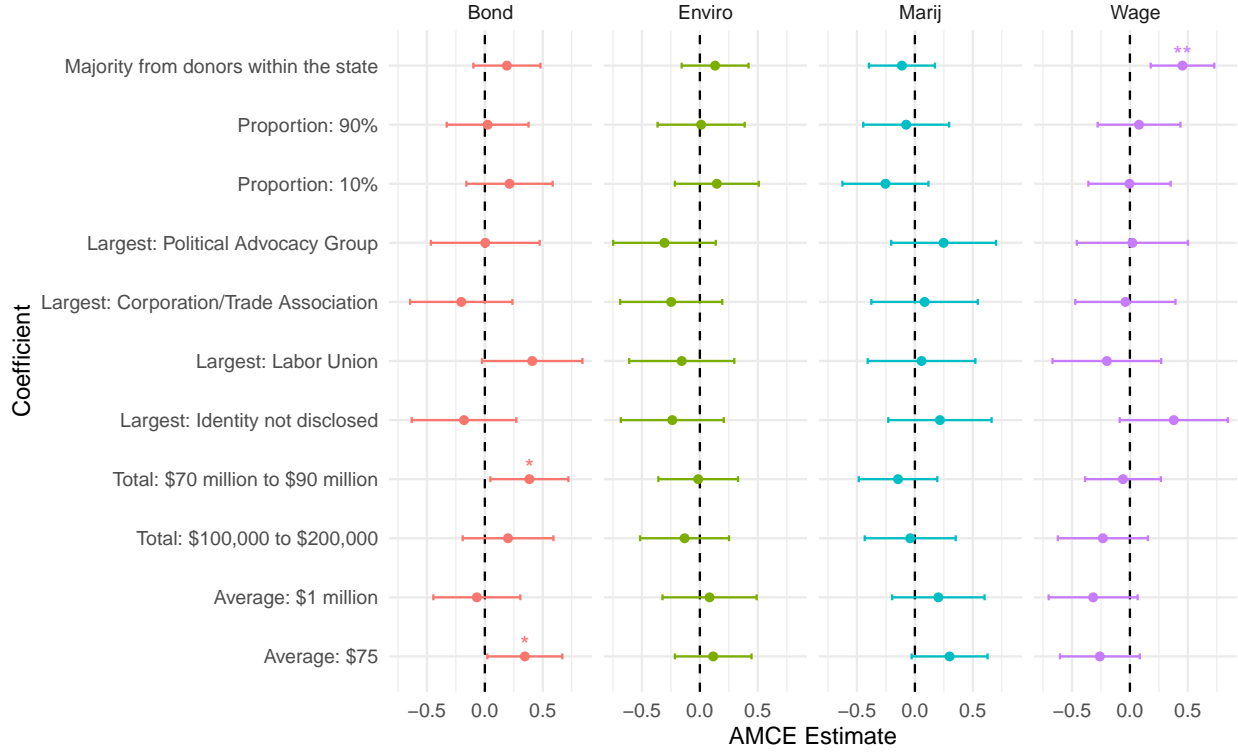


Figure 7: Initiative conjoint results with separate logit models run for each of the four issue areas posed to voters. All coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate significance at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$  respectively.

positive predictor of support for the sewage bond issue, and so too for the labor union donor attribute. No attribute level is significant for either the marijuana legalization or environmental taxation issues, and only the majority of within-state donations attribute is impactful for the minimum wage issue. These results are suggestive that for issues that are less salient (where we might expect voters' issue preferences to be less strongly held, or which covary less with voters' ideological position) that disclosure can alter vote choice. These results are purely indicative, however, and further experimental evidence is needed to test this hypothesis further.

To ensure that the lack of differences is not driven by different effects of the baseline category across issues (Leeper, Hobolt and Tilley, 2019) I also compare the differences in marginal means for the attributes by issue. Figure A7 in the Appendix plots these results.

On the whole, differences in the marginal means of attribute levels 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.

## 4 Discussion

In the absence of other relevant information, disclosure can influence vote choice. Voters seem 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. Voters are less responsive to the overall size of campaigns.

Crucially, however, once voters are primed with overt partisan and ideological cues, features that are ubiquitous to candidate campaigns, the effects of disclosure all but disappear. In initiative campaigns, where voters must decide whether to endorse particular policies, disclosure is similarly ineffective at influencing vote choice. It appears that the issue prime that is necessarily a part of each initiative swamps any effect of disclosure. Moreover, while recognising the lower power of the models subsetting this data, the results do not suggest that the (null) effects of disclosure vary meaningfully across issue areas.

One exception is the positive effect of having a majority of in-state donations, which is robust across all the main models in this paper. Voters appear to be very averse to the influence of actors outside their state, even as they overlook other forms of capture by narrow interests once a candidates' partisanship and ideology are revealed. The strength of this effect, and its resilience to both partisan information and issue primes, is notable. Future

work should consider precisely why this effect is so robust when other disclosure mechanisms are not.

Future work should further probe how partisan and ideological features mediate the effects of disclosure. Does partisan affiliation and ideology swamp disclosure, to the extent disclosure mechanisms are separable but merely substantively very small? Or do the effects of disclosure cue voters’ beliefs about partisanship and ideology, such that priming voters renders the effects of disclosure redundant?

It is also worth noting that the conjoint attribute levels were deliberately abstract. One limitation of the conjoint method, and particularly those presenting initiative races, is that it becomes less believable if the exact same entity is both the major donor for and against an initiative, for example. Increasing the number of unique names to decrease the likelihood of same-name pairs decreases the power of one’s survey instrument (holding the number of participants fixed). Researchers may wish to develop strategies that compare the effects of disclosure when (un)recognisable names are used. Some recent evidence suggests that there are few differences between attitudes when voters are exposed to charged statements about ‘secret donors’ compared to a more neutral framing (Rhodes et al., 2019). That said, it would be particularly interesting to consider names with varying local salience, to see if the results presented in this paper replicate when “stronger” labels are used. This would also potentially allow researchers to understand why voters react so negatively to the effect of out-of-state donors.

From a policy perspective, the implication of these findings suggest the informational benefit of disclosure appears to be highly dependent on the wider informational context in which such information is given. In the presence of overtly political cues, across both representative and direct democratic races, disclosure does not appear to substantively impact vote choice. That is not to say that the disclosure is uninformative in a broader sense, but it does not appear to drive vote choice (as measured in this study). Voters’ lack of sensitivity

to the funding profile of candidates at the point of voting,<sup>23</sup> 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.

A consequence of these findings is that campaigns (and donors) should be unperturbed by the visibility of their donations. The trade-off that Ashworth (2006) poses, between greater exposure and voters' aversion to interest group involvement, does not seem to exist in this study. Spending has consequences not only for the success of campaigns (Stratmann, 2005, 2010; Figueiredo et al., 2011; Rogers and Middleton, 2015) but also the representation of sectional interests (Gilens, 2015; Przeworski, 2015). The relatively limited effects of disclosure on voter behaviour observed in this study suggest we should think carefully about how best to curb the influence of unrepresentative interests. Disclosure alone appears insufficient.

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<sup>23</sup>The effects of less immediate disclosure methods like information pamphlets may well be further attenuated (which can be empirically tested).

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# A Appendix

## A.1 Subject description

All subjects were recruited via the CESS Online USA subject pool, according to the exclusion restrictions noted in the main body of the text. 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 state included within the sample. 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.

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
Party ID	(Missing)	0.30
	A Democrat	46.20
	A Republican	14.60
	An independent	29.70
	Other	4.10
	Prefer not to say	1.30
Ideology	(Missing)	4.10
	<i>Mean</i>	4.26
	<i>Standard Deviation</i>	2.39

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

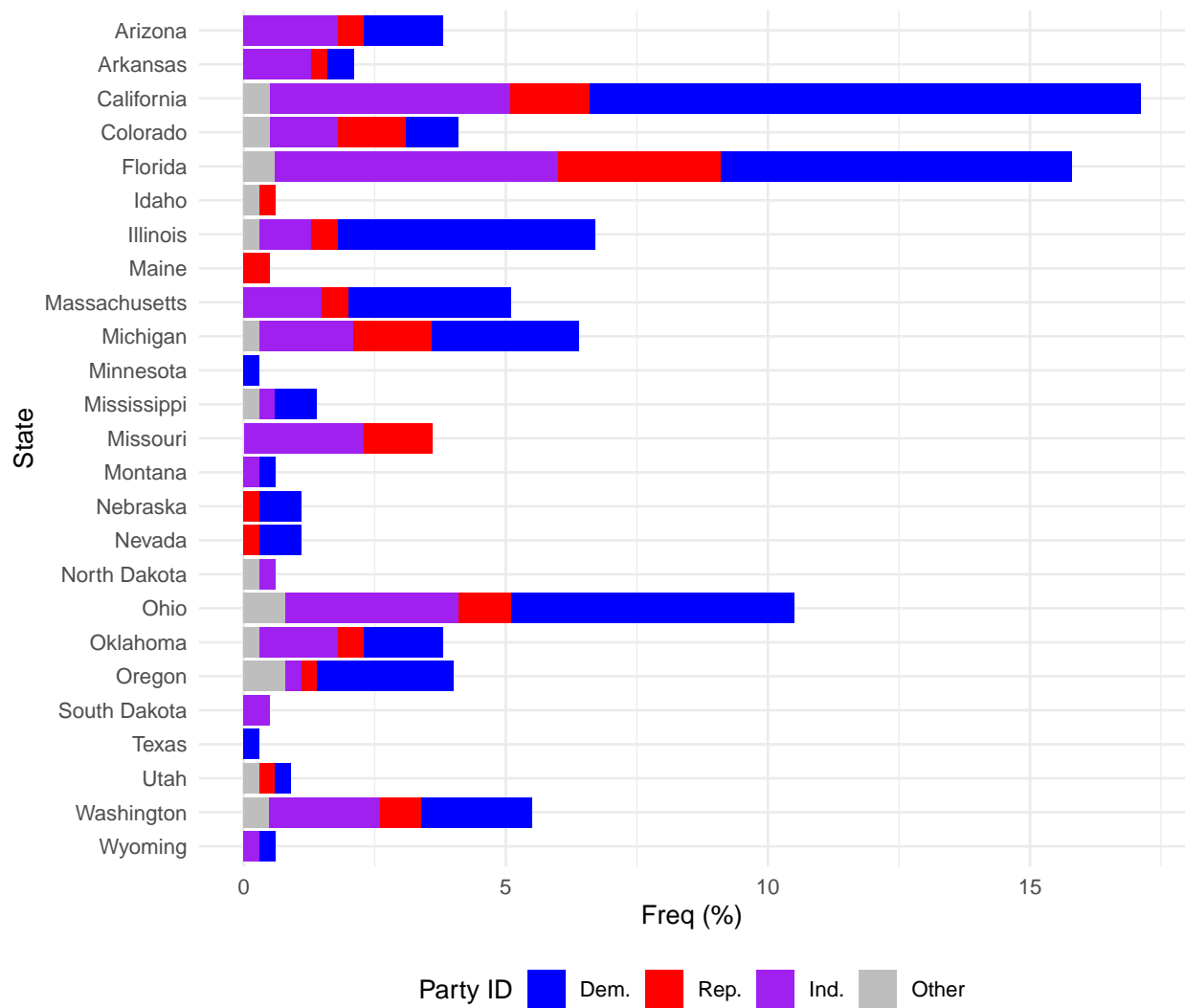


Figure A1: Frequency of subject participation by states

## A.2 Citizens United and its implications for disclosure

The *Citizens United v. FEC* (Argued March 24, 2009) 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, 2018). 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, 2017; Oklobdzija, 2019).

### A.3 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 A2 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.

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<sup>1</sup>”Mega” PAC’s can contribute \$10,100

<sup>2</sup>\$5675 for small donor committees.

<sup>3</sup>\$5675 for small donor committees.

<sup>4</sup>Gubernatorial candidates with more than \$250,000 independent expenditures are exempt, or if opposition candidate is self-funded (spending over \$250,000.)

<sup>5</sup>Independent PACs can contribute up to \$68,000.

State	Individual	PAC	Corporate	Union
Arizona	\$5100	\$5100 <sup>1</sup>	Prohibited	Prohibited
Arkansas	\$2700	\$2700	Prohibited	Prohibited
California	\$29200	\$29,200	\$29,200	\$29,200
Colorado	\$575	\$575 <sup>2</sup>	Prohibited	\$575 <sup>3</sup>
Florida	\$3000	\$3000	\$3000	\$3000
Idaho	\$5000	\$5000	\$5000	\$5000
Illinois	\$5600 <sup>4</sup>	\$55,400	\$11,100	\$11,100
Maine	\$1600	\$1600	\$1600	\$1600
Massachusetts	\$1000	\$500	Prohibited	\$500
Michigan	\$6800	\$6800 <sup>5</sup>	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

Table A2: Contribution limits per year for individuals, PACs, corporations, and unions. Amounts quoted are for gubernatorial candidates. Data from National Conference of State Legislatures.

## A.4 Example of initiative conjoint round

### 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	<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"/>

Figure A2: Screenshot of initiative conjoint

## A.5 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 A3 demonstrates these results compared to the original models.<sup>6</sup>

**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.

**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

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<sup>6</sup>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.



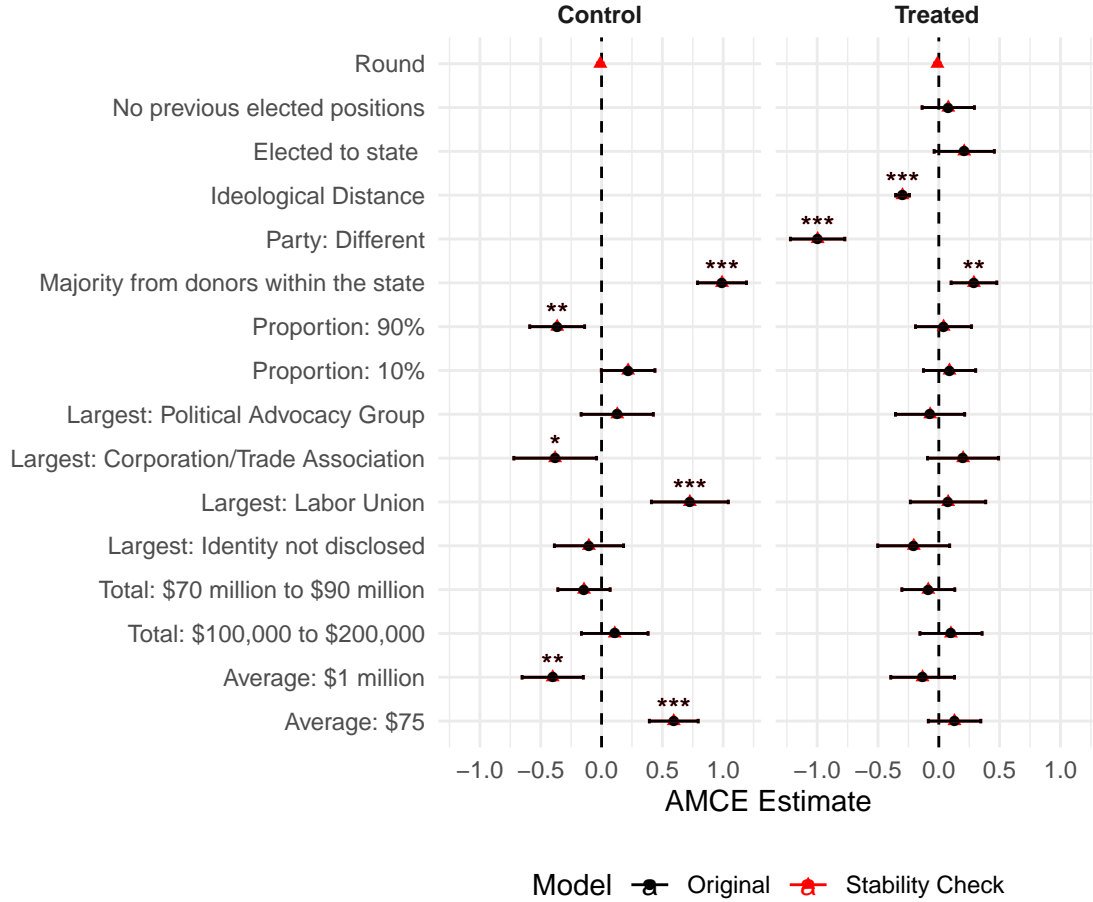


Figure A3: Comparison of coefficients between models reported in the main text and models including a continuous variable of the conjoint round, to check for stability and any carryover effects. Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$ .

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.<sup>7</sup>

<sup>7</sup>Indeed, 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.

**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. As a first check, Tables A3 and A4 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.

Moreover, I regress subjects' characteristics (age, gender, ideology) on profile attribute-levels using multinomial logit models. These tests check for imbalances in the assignment of levels across demographic categories. Overall, the attribute levels are well-balanced based on subjects' individual characteristics with *very* few attribute levels having a statistically higher or lower likelihood of being shown.<sup>8</sup> Overall, the evidence suggests the randomisation procedures across the experiment were successful.

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<sup>8</sup>Given the number of models, by chance we would expect to observe some statistically significant coefficient irrespective of balance.

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

Table A3: Balance test: proportion of times each attribute-level was displayed to participants in the initiative conjoint

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 A4: Balance test: proportion of times each attribute-level was displayed to participants in the candidate conjoints

Table A5: Multinomial balance test for candidate (control) attribute: Average

	<i>Level:</i>	
	\$10,000	\$75
Age	−0.004 (0.004)	−0.001 (0.004)
Gender: Male	−0.160 (0.112)	−0.078 (0.129)
Ideology	−0.019 (0.023)	−0.053* (0.027)
Constant	0.324 (0.182)	−0.169 (0.208)
Akaike Inf. Crit.	3,802.436	3,802.436
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001	

Table A6: Multinomial balance test for candidate (control) attribute: Origin

	<i>Level:</i>
	Majority from donors out of state
Age	0.001 (0.003)
Gender: Male	−0.042 (0.098)
Ideology	−0.044* (0.020)
Constant	0.272 (0.159)
Akaike Inf. Crit.	2,444.169
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001

Table A7: Multinomial balance test for candidate (control) attribute: Prop

	<i>Level:</i>	
	50%	10%
Age	0.010* (0.004)	0.001 (0.004)
Gender: Male	0.068 (0.120)	0.053 (0.122)
Ideology	−0.007 (0.025)	0.005 (0.025)
Constant	−0.317 (0.193)	−0.088 (0.197)
Akaike Inf. Crit.	3,881.700	3,881.700
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001	

Table A8: Multinomial balance test for candidate (control) attribute: Total

	<i>Level:</i>	
	\$1 million to \$10 million	\$100,000 to \$200,000
Age	0.002 (0.004)	0.003 (0.004)
Gender: Male	−0.043 (0.126)	0.013 (0.113)
Ideology	−0.002 (0.026)	−0.021 (0.023)
Constant	−0.480* (0.204)	−0.102 (0.182)
Akaike Inf. Crit.	3,826.984	3,826.984
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001	

Table A9: Multinomial balance test for candidate (control) attribute: Largest

	<i>Level:</i>			
	Private individual	Labor Union	Political Advocacy Group	Identity not disclosed
Age	-0.003 (0.005)	0.003 (0.005)	-0.004 (0.005)	-0.005 (0.005)
Gender: Male	0.106 (0.154)	0.300 (0.155)	0.121 (0.154)	0.257 (0.157)
Ideology	0.053 (0.032)	0.007 (0.032)	0.039 (0.032)	0.019 (0.033)
Constant	-0.174 (0.248)	-0.280 (0.248)	-0.055 (0.248)	-0.099 (0.252)
Akaike Inf. Crit.	5,697.256	5,697.256	5,697.256	5,697.256

*Note:* \*p<0.05; \*\*p<0.01; \*\*\*p<0.001

Table A10: Multinomial balance test for candidate (control) attribute: Average

	<i>Level:</i>	
	\$10,000	\$75
Age	0.004 (0.004)	0.004 (0.005)
Gender: Male	-0.111 (0.114)	0.024 (0.128)
Ideology	-0.021 (0.024)	-0.008 (0.027)
Constant	-0.109 (0.191)	-0.658** (0.216)
Akaike Inf. Crit.	3,672.010	3,672.010
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001	



Table A11: Multinomial balance test for candidate (control) attribute: Largest

	<i>Level:</i>			
	Private individual	Labor Union	Political Advocacy Group	Identity not disclosed
Age	0.0004 (0.006)	0.004 (0.006)	0.009 (0.006)	-0.002 (0.006)
Gender: Male	-0.125 (0.157)	-0.117 (0.160)	-0.023 (0.156)	0.029 (0.158)
Ideology	0.019 (0.033)	0.048 (0.034)	0.036 (0.033)	0.028 (0.034)
Constant	0.031 (0.264)	-0.312 (0.269)	-0.397 (0.264)	-0.019 (0.266)
Akaike Inf. Crit.	5,489.995	5,489.995	5,489.995	5,489.995

*Note:* \*p<0.05; \*\*p<0.01; \*\*\*p<0.001

Table A12: Multinomial balance test for candidate (control) attribute: Origin

	<i>Level:</i>
	Majority from donors out of state
Age	0.004 (0.004)
Gender: Male	0.106 (0.099)
Ideology	0.035 (0.021)
Constant	−0.335* (0.167)
Akaike Inf. Crit.	2,356.619
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001

Table A13: Multinomial balance test for candidate (control) attribute: Prop

	<i>Level:</i>	
	50%	10%
Age	0.005 (0.004)	0.002 (0.004)
Gender: Male	0.009 (0.120)	−0.047 (0.122)
Ideology	0.058* (0.026)	0.056* (0.026)
Constant	−0.435* (0.203)	−0.361 (0.205)
Akaike Inf. Crit.	3,739.065	3,739.065
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001	

Table A14: Multinomial balance test for candidate (control) attribute: Total

	<i>Level:</i>	
	\$1 million to \$10 million	\$100,000 to \$200,000
Age	−0.002 (0.005)	−0.004 (0.004)
Gender: Male	0.014 (0.126)	0.188 (0.115)
Ideology	−0.012 (0.027)	0.053* (0.024)
Constant	−0.292 (0.210)	−0.268 (0.195)
Akaike Inf. Crit.	3,687.805	3,687.805
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001	

Table A15: Multinomial balance test for initiative conjoint attribute: Average

	<i>Level:</i>	
	\$10,000	\$75
Age	−0.003 (0.003)	−0.0001 (0.004)
Gender: Male	−0.011 (0.094)	0.128 (0.103)
Ideology	0.010 (0.020)	−0.014 (0.022)
Constant	0.106 (0.157)	−0.315 (0.172)
Akaike Inf. Crit.	5,451.445	5,451.445
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001	

Table A16: Multinomial balance test for initiative conjoint attribute: Largest

	<i>Level:</i>			
	Private individual	Political Advocacy Group	Labor Union	Corporation/Trade Association
Age	0.014** (0.005)	0.006 (0.005)	0.011* (0.005)	0.011* (0.005)
Gender: Male	-0.010 (0.130)	-0.015 (0.127)	-0.086 (0.128)	-0.054 (0.128)
Ideology	-0.027 (0.027)	-0.048 (0.027)	-0.015 (0.027)	-0.024 (0.027)
Constant	-0.482* (0.218)	0.041 (0.212)	-0.290 (0.213)	-0.275 (0.214)
Akaike Inf. Crit.	8,069.141	8,069.141	8,069.141	8,069.141

*Note:* \*p<0.05; \*\*p<0.01; \*\*\*p<0.001

Table A17: Multinomial balance test for initiative conjoint attribute: Origin

	<i>Level:</i>
	Majority from donors out of state
Age	0.002 (0.003)
Gender: Male	0.058 (0.081)
Ideology	-0.034* (0.017)
Constant	0.092 (0.135)
Akaike Inf. Crit.	3,472.717
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001

Table A18: Multinomial balance test for initiative conjoint attribute: Prop

	<i>Level:</i>	
	50%	10%
Age	0.002 (0.004)	0.004 (0.003)
Gender: Male	-0.016 (0.101)	0.165 (0.099)
Ideology	0.015 (0.021)	-0.009 (0.021)
Constant	-0.074 (0.168)	-0.075 (0.165)
Akaike Inf. Crit.	5,501.713	5,501.713
<i>Note:</i>	*p<0.05; **p<0.01; ***p<0.001	

Table A19: Multinomial balance test for initiative conjoint attribute: Total

	<i>Level:</i>	
	\$1 million to \$10 million	\$100,000 to \$200,000
Age	−0.003 (0.004)	−0.001 (0.003)
Gender: Male	−0.080 (0.105)	0.090 (0.093)
Ideology	0.005 (0.022)	0.015 (0.020)
Constant	−0.230 (0.174)	−0.044 (0.156)
Akaike Inf. Crit.	5,437.388	5,437.388
<i>Note:</i> *p<0.05; **p<0.01; ***p<0.001		

## A.6 Additional models

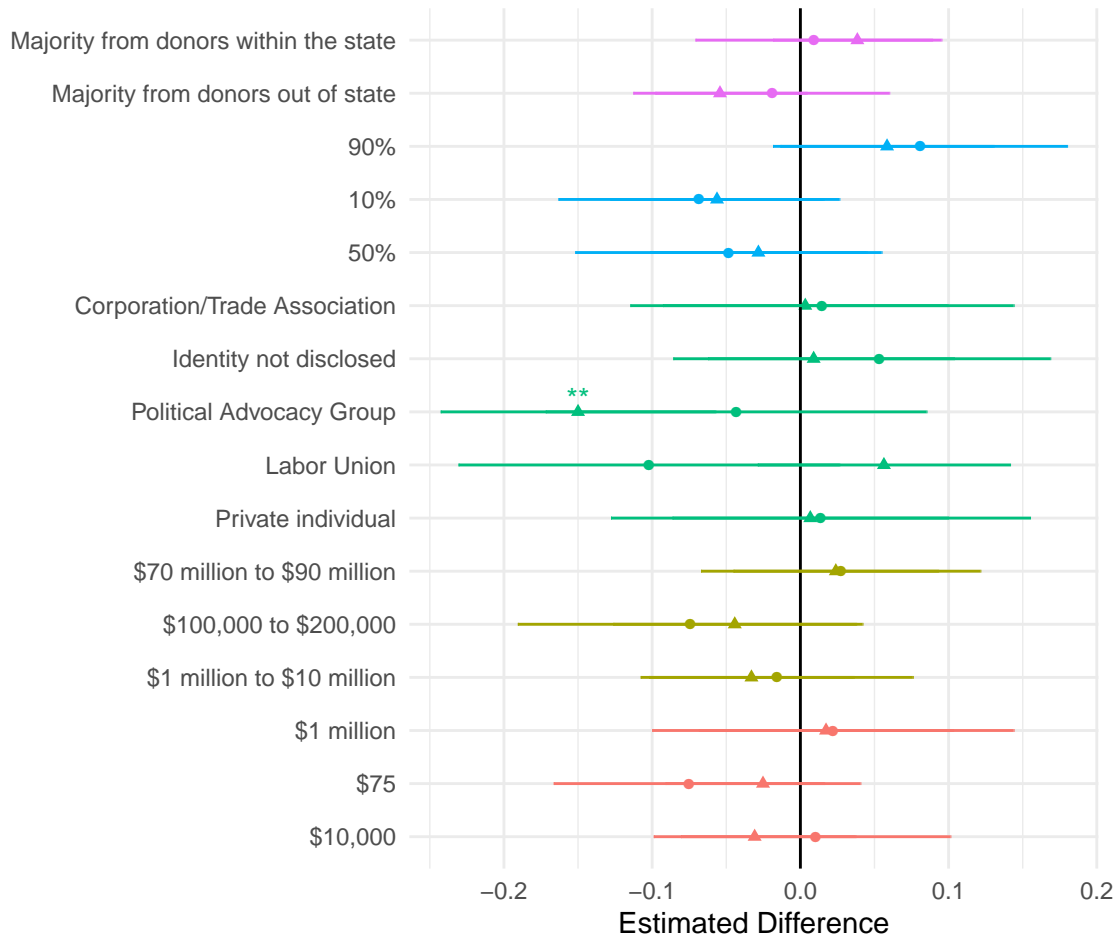


Figure A4: Difference in marginal means for each attribute level by respondents' party identification, for subjects **not** exposed to additional party, ideology, and valence attributes. Estimated differences are shown with 95 percent confidence intervals. Stars indicate statistically significant differences at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$ .

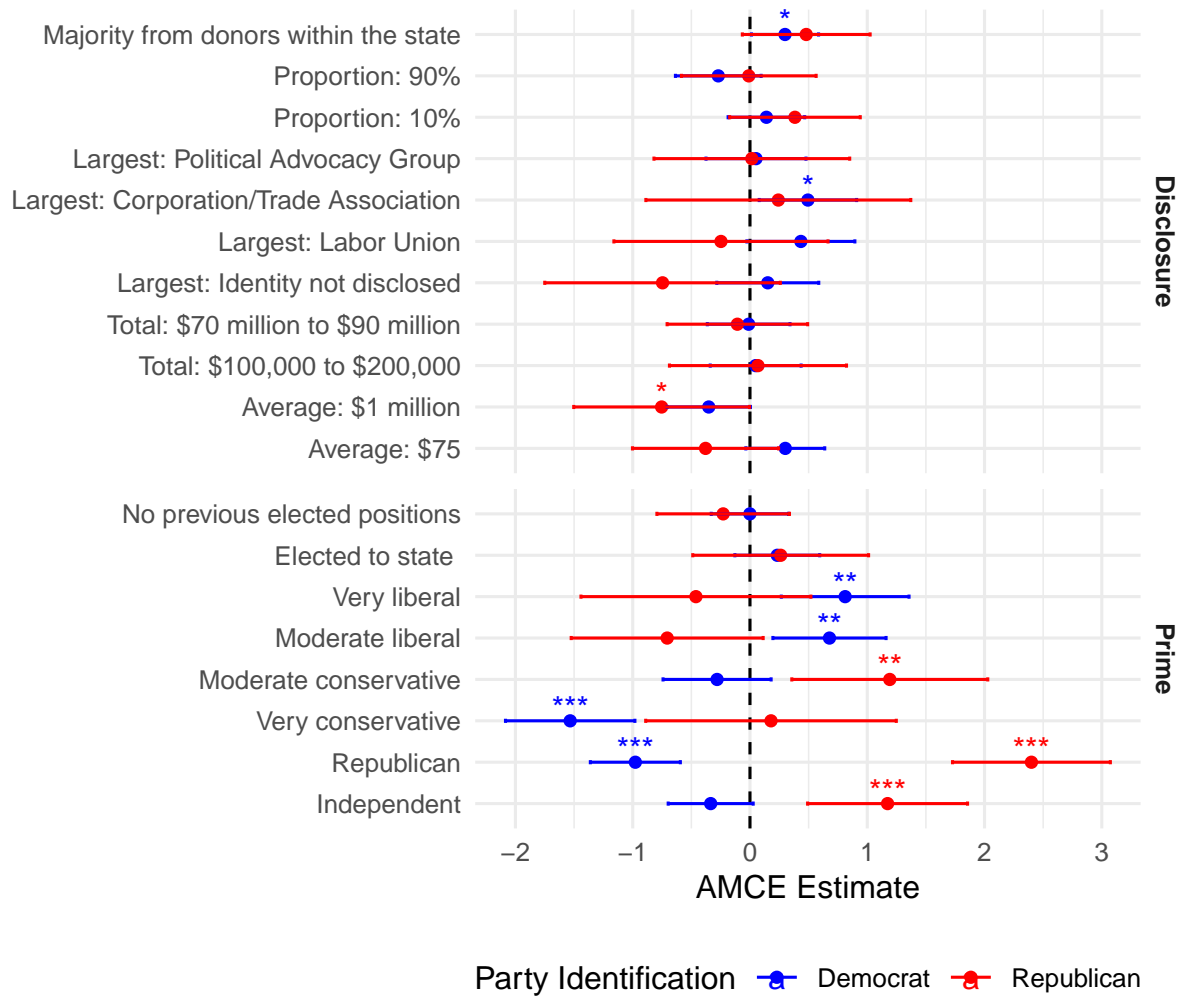


Figure A5: Comparison of causal effects between Democratic and Republican respondents in the sample. Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$ .



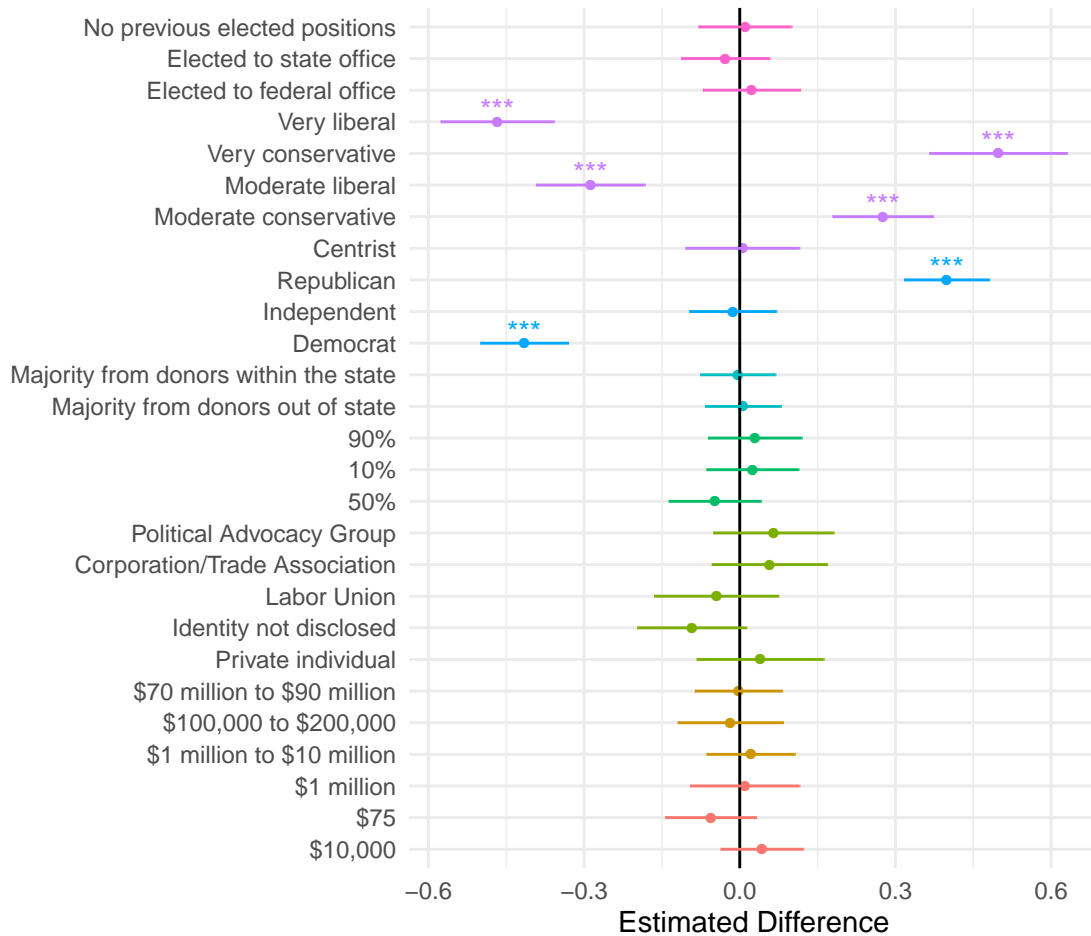


Figure A6: Difference in marginal means for each attribute level by respondents' party identification, for subjects exposed to additional party, ideology, and valence attributes. Results for comparison between Republican and Democratic respondents only. Estimated differences are shown with 95 percent confidence intervals. Stars indicate statistically significant differences at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$ .

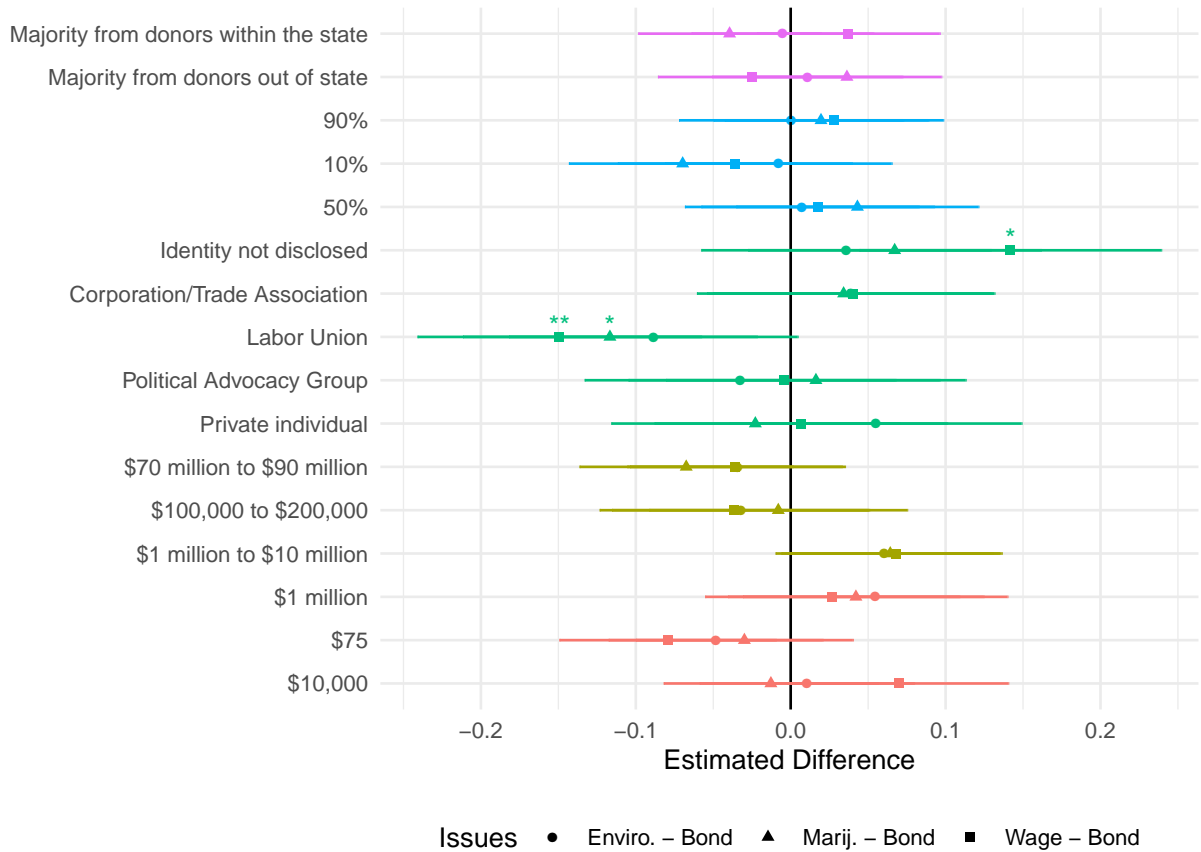


Figure A7: Difference in marginal means by issue for the initiative conjoint. Estimated differences are shown with 95 percent confidence intervals. Stars indicate statistically significant differences at  $*p < 0.05$ ,  $**p < 0.01$ , and  $***p < 0.001$ .