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

Thomas Robinson†

Abstract

Information about how campaigns are financed is abundant in the United States, but we have only scratched the surface of how this information affects voter behavior. How does disclosure affect vote choice and how is this effect mediated by partisan signals? Do the effects of disclosure differ across election types? Using a series of conjoint experiments, I compare the effects of campaigns' financial profiles on vote choice across direct democratic and representative election types, randomizing subjects' exposure to additional political cues. I find that while disclosure can affect vote choice, these effects are drowned out by partisan signals. In ballot initiative races, explicit policy information also appears to negate any effect of disclosure. This paper is the first to explore the comparative effects of disclosure across election type, contributing to our understanding of how separate heuristics interact in electoral contexts, with important implications for the design of campaign finance regulation.

*This version: June 2, 2020. This project was funded by an ESRC (Grand Union DTP) research training support grant and studentship. I would like to thank Andy Eggers, Raymond Duch, Jan Stuckatz, Nelson Ruiz, Sönke Ehret, and attendees of the 2019 LSE-Oxford Postgraduate conference for their insightful comments. A previous version of this paper was circulated under the title "Knowledge or Ignorance? Assessing the Causal Effects of Campaign Finance Disclosure on Vote Choice." The experiments in this paper were conducted in collaboration with the Centre for Experimental Social Sciences (CESS) at Nuffield College, University of Oxford. All experiments in this paper received ethical clearance from the Departmental Research Ethics Committee at the University of Oxford (SSH_DPIR_C1A_19_012) and the CESS Ethics Committee (OE_0027).

†Wadham College and Department of Politics and International Relations, University of Oxford, UK. thomas.robinson@politics.ox.ac.uk

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

However, in practice, 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, 2019, 2018; Rhodes et al., 2019; Spencer and Theodoridis, 2020).[1] While voters' perceptions of candidates are not immune to the effects of disclosure (Wood, 2019; Spencer and Theodoridis, 2020), the marginal informational benefits of doing so may be limited (Primo, 2013).

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

Second, it is unclear how the effects of disclosure are mediated by other relevant features

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

of the electoral context. Do the observed effects of disclosure persist when other highly relevant information like partisanship or ideology are revealed? While some studies explicitly control for these cues (Dowling and Wichowsky, 2015; Dowling and Miller, 2016; Rhodes et al., 2019), we know little about *how* partisanship signals mediate the effect of disclosure. Is disclosure used to infer candidate partisanship, or does it have an independent effect on vote choice?

Third, previous experiments focus on candidate elections, but the effects of disclosure may differ in other democratic races that lack explicit partisan signals. Ballot initiatives are prominent policymaking tools in the United States, with vast sums of money spent for and against propositions each electoral cycle (Stratmann, 2010). This paper, to the best of my knowledge, provides the first test of the comparative effects of disclosure across representative and direct democratic elections.

In this paper, I develop a random utility model of vote choice that explicitly incorporates how the effects of disclosure are mediated by the broader informational context. In the absence of other signals, various attributes of disclosure are likely to lead to meaningful shifts in the estimation of a voters' utility, and thus substantive shifts in vote choice. However, the presence of partisan or ideological cues are likely to reduce the efficacy of disclosure. I suggest there are two competing heuristic mechanisms that would explain this reduction: an informational equivalence mechanism, where disclosure primes voters' beliefs about candidate's partisanship such that explicit partisan cues render the effects of disclosure inert; and a swamping mechanism, where disclosure acts independently of partisanship but is drowned out by more dominant political signals. The swamping mechanism also translates to direct democratic contexts where explicit policy cues may overwhelm heuristic information gleaned from a campaign's financial profile.

I test this theory using a series of experimental conjoint surveys. These experiments jointly and explicitly test the efficacy of disclosure across both the type of election – gubernatorial

and ballot initiative races – and different informational contexts. 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 better approximate the types of voting scenarios that citizens face.

To examine how the efficacy of disclosure changes across informational contexts, I modify the typical conjoint design by randomly varying not only the content but also the number of conjoint attributes displayed to respondents (Sen, 2017). Half of all subjects receive additional randomized information about candidates' ideology, partisanship and political experience. 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). Unconfounded estimates of the effects of disclosure with and without partisan signals allow for precise analysis of how disclosure is mediated by political signals that are prevalent in contemporary electoral contexts.

The results show that while disclosure can impact vote choice its effects are negligible once overt but common partisan signals are included. When subjects are only presented with disclosure information, subjects move away from candidates with high average donations and relatively concentrated groups of donors. But when the candidates' ideology and partisanship are revealed, these effects are indistinguishable from zero – with the exception of the geographic origin of donations. Disclosure does not appear to affect vote choice in initiative elections either. Subjects in the experiment appear to have relatively fixed political views on policy issues, rendering disclosure ineffective. This paper, therefore, finds little support that disclosure has a distinct impact on vote choice in realistic electoral contexts.

Overall the fragility of disclosure's effects are consistent with the hypothesis that disclo-

sure heuristics are being "swamped" by partisan signals. In other words, the independent effects of disclosure appear to be drowned out by other politically relevant cues (once these are made available to subjects). Disclosure does not appear to act to prime subjects' beliefs about candidates' partisanship – Democrat and Republican subjects appear to respond similarly to disclosure cues in the absence of partisan cues. 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 refine our understanding of the relative efficacy of different heuristic cues on vote choice.

This paper makes two further contributions to our study of disclosure. First, this paper demonstrates how disclosure affects vote choice, and the importance of incorporating the wider informational context in which disclosure signals operate. Moreover, given the ubiquity of ballot initiative policymaking across the US, and the vast expenditures made by both proponents and opponents of these efforts, this paper helps disentangle the effects of disclosure in a context where there are direct and immediate policy consequences to voters' decisions, but where partisan information is less prominent. Second, from a policy perspective, these findings challenge the conventional wisdom that democratic values of political equality can be safeguarded by transparency alone. That is not to say there are no benefits to disclosure 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. But voters do not appear to revise or "correct" their vote choice on the basis of disclosure. This has important implications for how policymakers design campaign finance regulation.

1 The informational benefit of disclosure

In *First National Bank of Boston v. Bellotti* (1978), which struck down expenditure limits in ballot initiative races, the Supreme Court argued that disclosure allows voters to evaluate the arguments presented for and against proposed legislation, and thus bolsters a

voter's ability to make informed decisions on both issue and candidate elections – the "informational benefit" of disclosure (Jiang, 2019). More recently, in *Citizens United v. FEC* (2010), the Court opined that, since contributions are effectively a form of speech, voters should have the right to know who is speaking. The Court's position is that voters can productively use this information to inform their voting behaviour.[2]

Research on political advertisements demonstrates that disclosure can affect voter decisionmaking, specifically by inferring (rightly or wrongly) different intentions by different donor sources (Dowling and Wichowsky, 2013; Sances, 2013; Dowling and Wichowsky, 2015). At the aggregate campaign-level, voters appear to value transparent profiles (Wood, 2019), with disclosure influencing voters' perceptions of candidate corruption (Spencer and Theodoridis, 2020). Other work, however, finds that the marginal benefit of disclosure to voters' knowledge of interest group positions is negligible (Primo, 2013). These studies on voter perceptions are important but leave open the question as to whether changes in perceptions translate to changes in voting behavior. Only one paper (to the best of my knowledge) directly assesses the effect of aggregate disclosure on the likelihood of voting for a candidate, finding moderate support that this additional information alters vote choice (Dowling and Miller, 2016) even in the presence of partisan information.

In this section, I present a random utility model of how disclosure can affect vote choice by refining individuals' perceptions of the utility gain from choosing one candidate over another. I then consider how the presence of other politically relevant signals, and the type of electoral race, mediate the effect of disclosure on vote choice. Finally, I present specific expectation as to how different facets of a campaign's funding profile affect voters' choices.

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

1.1 Disclosure as a heuristic

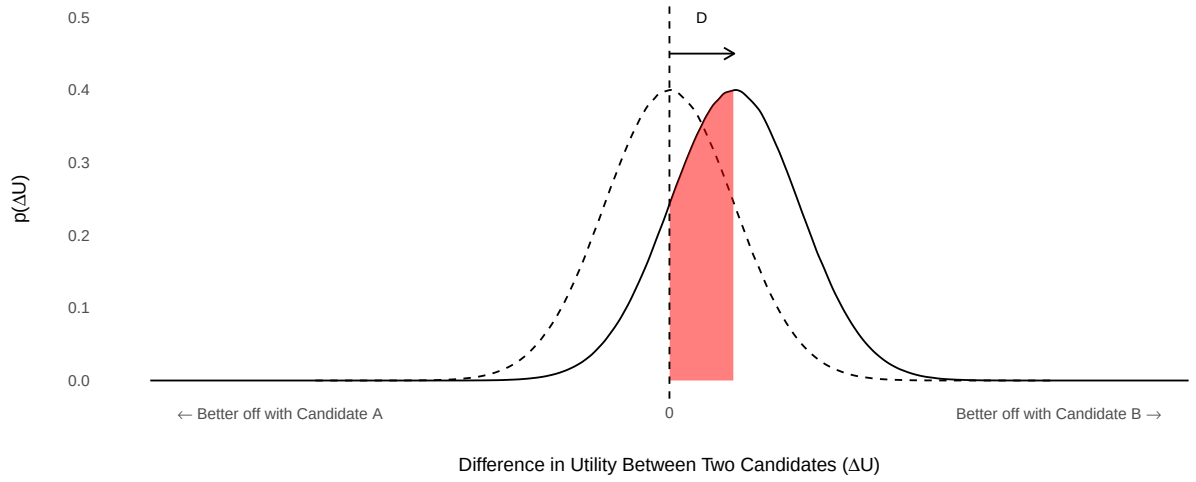
Assume, in the most abstract terms, that a voter must make a choice between two candidates with the goal of maximising their own utility. Assume further that the problem the voter faces is about estimation. Given a set of signals can voters adequately estimate the relevant quantities needed to make the most appropriate choice, that is, the choice they would make under perfect information (Primo, 2013)?

Tasks like researching candidates' biographies, listening to speeches, or checking federal campaign submissions are taxing (Primo, 2013). Moreover, some plausibly relevant factors such as a candidate or campaign's competence, trustworthiness, and viability are harder to observe directly. Candidates and campaigns may even suppress information or qualities that are deemed harmful to their electoral prospects. To overcome these overly-taxing cognitive demands, individuals use information signals that enable less costly estimates of the position or valence of a campaign. These heuristics, "efficient cognitive processes… that ignore part of the information" (Gigerenzer and Gaissmaier, 2011, p.451), often provide useful shortcuts for evaluating campaigns and political choices (Lupia, 1994).

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

Figure 1 represents this intuition using a random utility model. Suppose that an individual is trying to discern between two candidates and, in the absence of other information, is uncertain over which candidate will make them better off. As a result, the probability of the difference in utility from choosing Candidate B over Candidate A is centred around zero (represented by the dashed probability mass in Figure 1).

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



Now suppose that V receives (effective) additional cues from campaign finance disclosure.[3] The effect of D is to refine the individuals' evaluations of the two candidates, shifting the probability mass towards Candidate B. The red-shaded area in Panel (ii) indicates the increase in probability mass in favour of Candidate B as a result of disclosure signal D. In other words, the disclosure heuristic makes it easier for the voter to discern which candidate is the optimal choice. This change is substantial, making it likely that the individual will now choose Candidate B over Candidate A.

What about when other relevant signals are made explicit to voters? And does this logic also apply in non-candidate elections? I discuss each of these issues in turn, before setting out specific expectations about the effects of different facets of disclosure.

1.2 Effects of disclosure in the presence of other signals

The model above formalises how information about campaigns' funding affects vote choice in otherwise information-less contexts. When other relevant signals are revealed, however, this may alter voters' decisionmaking and thus impact the effect of disclosure on

[3] Different aspects of a campaign's financial profile may inform voters about valence and/or ideology. I discuss these specific expectations in Section 1.4.

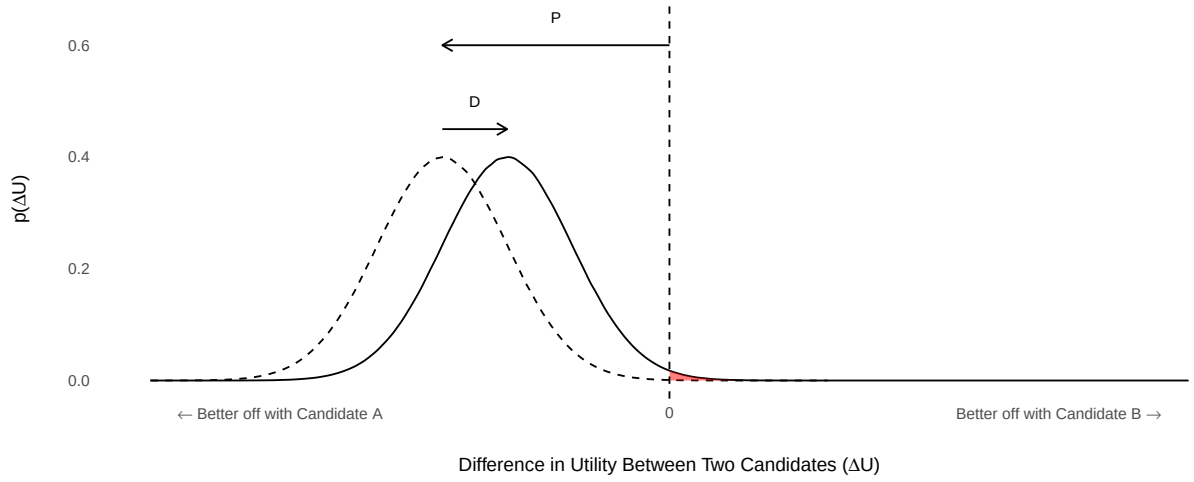
vote choice. How it does so, will depend on the interaction and relative importance of the disclosure heuristics and other political signals. Here, I outline two alternative explanations that would suggest disclosure is not robust to the inclusion of these signals. In Section 4 I provide further evidence to discriminate between them.

On the one hand, disclosure may act to signal the partisanship of candidates. In the absence of explicit partisan signals, disclosure may allow voters to estimate candidates' partisanship, and in turn these perceptions drive vote choice. In other words, voters use a two-stage heuristic mechanism. This mechanism operates via an *informational equivalence* (Dafoe, Zhang and Caughey, 2018) – disclosure only acts to inform subjects about some latent characteristic of the campaigns, which in turn affects their vote choice. For example, knowing that the largest donor to a political campaign is a business corporation might suggest that the candidate is likely to be a Republican. Consequently, when partisanship signals are made explicit, disclosure should cease to affect vote choice. By removing any uncertainty over the partisanship of the campaign, voters do not need to use disclosure to estimate this quality of the candidates.

On the other hand, individuals may use multiple, independent heuristics to inform their vote choice – disclosure and partisanship may both affect voter decisionmaking but for different reasons. For instance, a voter may perceive a campaign with many donors as being more viable, while a moderate Democrat may seem more ideologically consistent. Note that, unlike in the informational equivalence mechanism, the presence of partisan signals does not necessarily rule out an independent effect of disclosure.

However, if other political signals are particularly strong or dominant, the effect of disclosure on vote choice may still reduce to zero. Figure 2 demonstrates this logic. In this model, partisanship (P) has a stronger impact on voter utility compared to disclosure information (D). Consequently, the information gleaned from D now results in a much smaller change in the probability that some voter would prefer candidate B over A – the increase in proba-

Figure 2: Effect of disclosure (D) and partisanship (P) on a voters' estimate of the difference in utility choosing between two candidates



bility mass in favour of Candidate B (the red shaded area) is minuscule. Importantly, this occurs even though the rightward shift in the probability mass as a result of disclosure is of the same size as in the original model in Figure 1. The effect of disclosure on vote choice is much smaller because the relative importance of disclosure on voters' utility is less than that of partisanship. In other words, partisanship may *"swamp"* the effects of disclosure on vote choice.

Under both the *informational equivalence* and *swamping* mechanisms, therefore, it is possible that the effects of disclosure will drop out in the presence of partisan information. Under the informational equivalence theory, this is because disclosure no longer usefully informs voters about a candidate's partisanship. In the case of the swamping mechanism, however, this is only true if partisan signals are more important to voters than disclosure signals. Therefore, two separate empirical tests are needed. First, are the effects of disclosure robust to the inclusion of other relevant political signals? Second, if the effects of disclosure are not robust to these additional cues, is this because disclosure only acts to prime perceptions of partisanship or because the effects are swamped by stronger signals?

1.3 Effects of disclosure across types of electoral campaign

The type of electoral contest may also mediate the effect of disclosure. Ballot initiative elections are an important from of state policymaking, in which citizens can draft and submit legislation directly to the ballot (Magleby, 1994). Since ballot initiative elections are typically devoid of explicit party labels and other cues, the benefit of disclosure could be *greater* (Garrett and Smith, 2005; Briffault, 2010; Primo, 2013).[4] To the best of my knowledge, this comparative aspect of disclosure (between election types) has not been tested empirically before.

Intuitively, disclosure may inform voters about the "valence" of initiative campaigns. For instance, voters may want to estimate what the likelihood is that the drafted legislation will achieve its stated aim. They might also be concerned that the given legislation is anticompetitive, or favours a narrow set of out-of-state interests, even if they are broadly supportive of the policy in general. And voters may want to estimate the compatibility of novel policy proposals with their existing political beliefs. Absent clear ideological or partisan signals that are present in candidate elections, divulging information about the supporters of these policymaking efforts may be particularly informative to voters.

However, while initiative campaigns lack overt partisan signals, they do focus on specific policy proposals – for example, whether to increase the minimum wage, lower prescription drug prices, or curtail state governments' taxation powers. 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.

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

1.4 Relevant facets of disclosure

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 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 designed to capture the variety of ways disclosure may influence vote choice, building on previous works (Prat, Puglisi and Jr, 2010; Dowling and Miller, 2016; Wood, 2019; Spencer and Theodoridis, 2020). Several facets have competing intuitions, which I highlight explicitly below.

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

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

Average donation. Where the total size of donations gives voters an indication of a campaign's viability and/or electoral capacity, the average donation seems likely to tell voters more about the breadth of support for a campaign. Those with a low average donation can tout this as an indicator of broader political support (holding constant the total dona-

tions), or at least that the typical donor comes from comparatively limited means. Conversely, a very high average donation might indicate that narrow but well-funded interests are the predominant supporters of a campaign. It seems unlikely (though not impossible) that the opposite effect would be true, namely that voters infer some positive quality from candidates whose average donation is very high.

Type of largest donor. The type of donor may separately signal information about what sectors of society a campaign is aligned with.[5] Donations may predominantly come from individuals, corporations, labor unions, or specific political advocacy groups. Voters may react differently to campaigns depending on which sectors of society fund them. This signal is therefore likely to depend on a voter's pre-existing political leanings.

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

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

This proportion reflects the degree of "capture" by any one particular donor or interest. Separate from the average donation, this feature explicitly captures the concentration of financial support, rather than providing a signal about the base of that support. Intuitively, if voters use this cue, they would be averse to campaigns funded by very few donors (i.e. where the largest donor donates a high proportion of funds). Campaigns with a high concentration of interested parties are likely to be those that most represent narrow interests.

Geographic origin of donations. Finally, voters may care about *where* financial support comes from. Given the federal structure of the United States, voters may be concerned about whether campaigns are funded locally or not (especially for state level races). Whether a majority of donations come from within or outside of the state in question may provide some cue about "capture" by external interests. Large numbers of donations from out-of-state actors could represent apparent interference in a state's affairs and thus may diminish voters' willingness to support a campaign or candidate. It may also be a signal of whether candidates care about the concerns of their constituency. This is particularly relevant for the sorts of elections considered in this article; gubernatorial and ballot initiative campaigns deal with state-wide political issues.

2 Conjoint experiment to assess impact of disclosure on vote choice

To assess the causal effects of these various disclosure mechanisms, 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. Conjoint survey experiments are an efficient way to test the extent to which different attributes affect subjects' choices in a robust, inferential manner (Hainmueller, Hopkins and Yamamoto, 2014). Since conjoint survey experiments typically ask respondents to choose between two profiles, this

design is a natural analogue for the sorts of decisions voters make in American elections. This is useful even in the context of ballot initiative elections where each voter makes a binary decision over whether to endorse a policy proposal. Initiative elections typically have separate "Yes" and "No" campaigns. Opposition groups raise their own funding and play a key role in advocating for the status quo (Gerber, 1999). Given the important role opposition groups play in initiative races, there is good reason to include them in the study of disclosure mechanisms. They therefore represent an alternative choice for voters.

2.1 Randomizing 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, claims about the causal effect of any attribute only hold in situations where subjects are exposed to the same signals (and only those signals). Conjoints are particularly useful when we want to test whether a given feature *can* influence subject decisionmaking.

Claims about generalisability of any causal effect beyond the experimental context, however, require more stringent assumptions. Among other things, researchers must assume that the given set of attributes fully describe the pertinent features over which respondents make a choice. The observed causal effects may not hold up in contexts where some feature not included in the conjoint experiment also acts on individuals' behaviour. Researchers may, in fact, want to distinguish between the size of an effect for the same attribute in different informational or experimental contexts.

To assess for potential differences in the effects of disclosure given different informational environments, I randomly assign subjects to one of two candidate conjoint experiments (Sen, 2017). Half of subjects are exposed to disclosure attributes only. The other half see

these same disclosure attributes as well as other relevant political cues – partisanship, ideology, and previous experience. Randomization at the attribute level ensures unconfoundedness between those presented the full set of conjoint attributes, and those presented only the disclosure cues. Comparing the estimated marginal effects for the common set of attributes shared across both groups therefore helps illustrate how robust any effect of disclosure is to the inclusion of other relevant signals.

2.2 Experimental protocol

All participants completed two separate conjoint experiments – one choosing candidates in a hypothetical state gubernatorial election (either with or without additional attributes), and one asking subjects to consider four initiative policy proposals. In both experiments, subjects were presented with randomized information (levels) for each facet of disclosure (attributes). Table 1 provides details of all the conjoint levels across the two experiments.

The dollar amounts in the conjoint levels are intended to clearly distinguish campaign finance profiles within each relevant attribute. It is worth noting, however, that legal contribution limits to gubernatorial candidates vary across states (no such limits exist for initiative campaigns). Section C in the supporting information (SI) summarises contribution limits within each state in the sample. This variance limits the external validity of these findings - a large average donation will be implausible given some state's contribution limits. However, the scenarios were presented as hypothetical and these levels did not seem to hinder subjects' completion or comprehension of the survey. Moreover, these levels are broadly plausible even if there is some mismatch with actual state laws. Beto O'Rourke's 2018 senate campaign had receipts in excess of $70 million, and Proposition 61 (2016) in California saw opposition donations exceed $100 million.

Table 1: Conjoint attributes and levels

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

Candidate conjoint. Respondents were presented with the funding profiles of two candidates running for gubernatorial office. Half of all participants ($n = 195$) were randomly assigned to see three additional politically relevant signals: the candidates' party affiliations, their ideological position, and whether or not they have been elected to either state or federal office before. 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 the two versions of the candidate conjoint. The

value of each attribute for each candidate were randomly assigned. Figure 3 displays an example of how the information was presented to subjects in each round of the candidate conjoint experiment.[6]

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

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

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

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

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

Q56.

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

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. The policy proposals are hypothetical but constructed to appear sufficiently realistic that a subject could imagine such proposals being placed on the ballot.

Alongside the initiative title and a brief description of the proposed policy, participants were presented with the same funding table as in the candidate conjoint, and presented for both the support *and* opposition campaigns. Participants were asked to consider this

[6] A screenshot of the initiative conjoint can be found in SI Section C.

Table 2: Hypothetical initiative policies

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

information and choose whether they would vote 'for' or 'against' the proposed policy. Each participant made a total of four choices within the initiative experiment – one per issue.

Randomization procedure. The order in which the candidate and initiative conjoints were presented to each subject was randomized, as was the assignment to the two different candidate conjoints. Within the initiative conjoint component, subjects were shown each initiative issue once and the order of these issues was randomized to limit any order-effect. Finally, across all experiments, attributes were randomized with minimal restrictions to prevent implausible attribute-level combinations.[7] SI Section E presents tests verifying that these randomization 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 con-

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

joint experiments were conducted using a high-quality, U.S. specific subject pool with no communication between subjects, high attention rates, and location information known prior to the experiment. Members of the subject pool were invited to participate if they were resident in a state which used the initiative process and therefore were likely to be familiar with the process. The first round of invitations was sent to those resident in California, Washington, Oregon, Arizona, Ohio, Florida, Colorado, and Massachusetts - all states with relatively high usage of the initiative process. Further invitations were then sent to those resident in the other 17 states where some form of initiative provision is in operation. In total, 390 eligible participants completed the experiment.

Further demographic information is available in SI Section A. 46 percent of respondents identified (post-experiment) as Democrats, 30 percent as independents, and 15 percent as Republicans. The imbalance in party identification is to be expected given states on the West Coast (where initiative elections are most common) were 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,[8] weighted by the proportion of respondents per state in the experimental sample. While the lean in the experimental sample is larger, the expected lean towards the Democrats is nevertheless substantial (7.3 percentage points) suggesting that Democratic bias is to be expected. Given this study tests for the causal effects of disclosure, rather than its generalisability, this Democratic lean does not affect the validity of the inference.

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

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

experiment.

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 (without partisan, ideological, or valence cues). To recover the marginal effect of each attribute, I estimate the following logistic probability model:

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

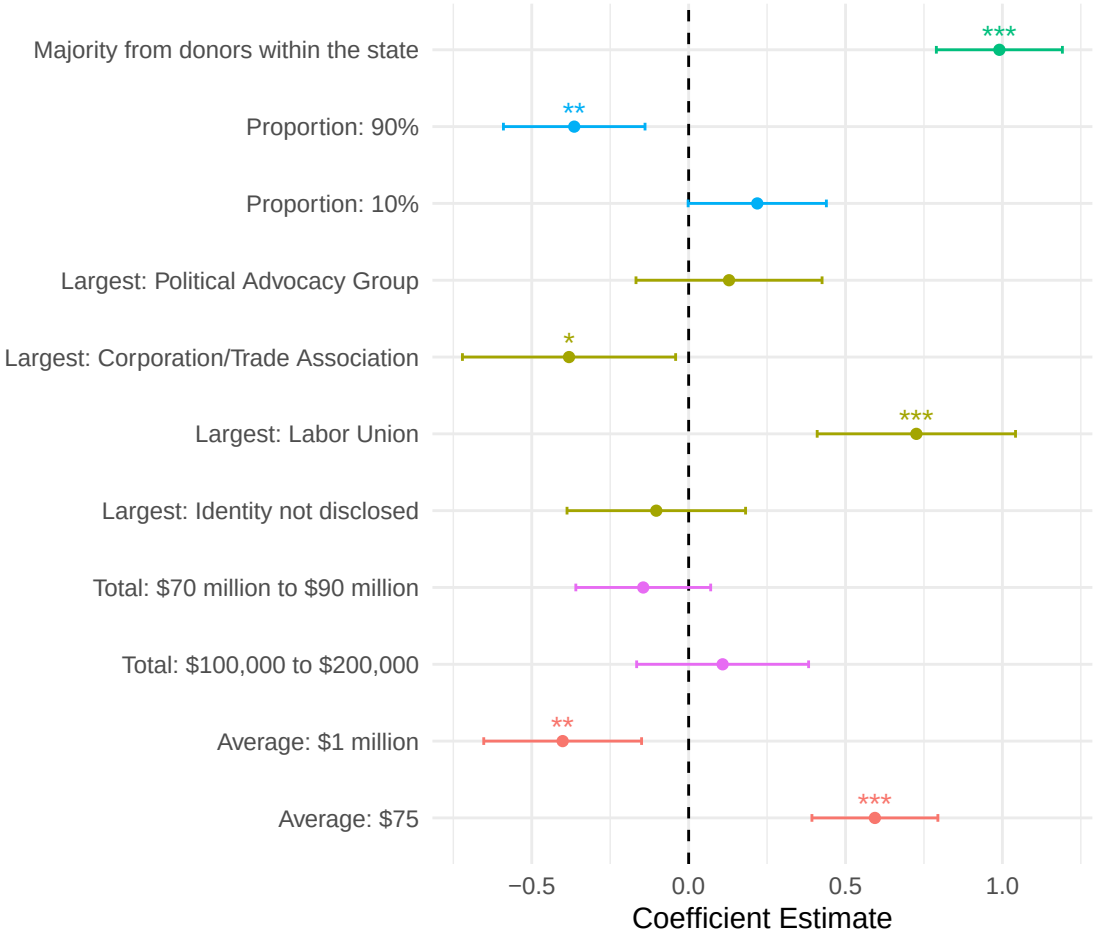
Equation 1 recovers an estimate of the additive effect for each attribute-level, relative to a baseline.[9] All standard errors are clustered at the individual-level since subjects (from which we take multiple observations) are sampled from a much wider population of interest.

Figure 4 plots the model coefficients. In this low-information environment, disclosure does 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, or where the average donation amount is high. Donations mainly from within the state, labor union funds (relative to private individuals), and lower average donation amounts (relative to the baseline $10,000) all have a positive effect on the likelihood of a candidate being chosen.

These results show that aggregate disclosure *can* influence voter decisionmaking. In the absence of other cues, subjects are averse to instances where candidates appear to be captured by a particular interest or group. In particular, voters appear to care about political

[9] An interactive theory would suggest that voters combine multiple aspects of disclosure to form an assessment of candidates. For instance, they compare both a small average donation size and the a large overall total raised and infer that this candidate is widely supported by regular voters. These sorts of interactions are plausible, but given the limited research in this area exploring the fundamental effects of disclosure, and the demanding power requirements, I defer these theoretical issues to future research.

Figure 4: Candidate conjoint results *without* additional cues.



Model coefficients are shown for the 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.

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 significant 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. I explore the causal effects by party identification in the following sections. It is nevertheless noteworthy that voters adjust their vote choice when presented with this

information. PACs did not affect vote choice which perhaps suggests that their ubiquity in the American electoral landscape has dulled their informative quality to voters.

Moreover, the "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 suggests that the presence of anonymous donors does not deter individuals from voting for a campaign. In other words, there is little evidence in this experiment of voters punishing campaigns for accepting money from obscured sources.

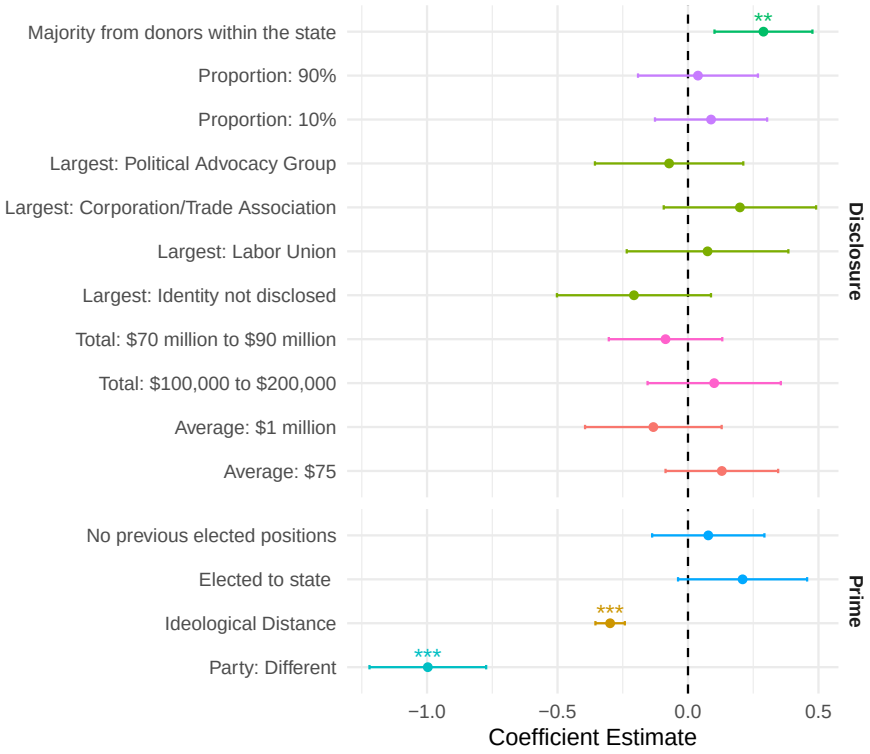
3.2 Priming subjects' perceptions of candidates' ideology and valence

Elections are rarely fought on the topic of disclosure alone. Indeed, voters typically receive signals about candidate's ideology, partisanship and valence characteristics. The first set of results demonstrate the ability of disclosure mechanisms to affect vote choice. But does disclosure have an independent effect once we control for other 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 partisanship, ideology, and previous office-holding. This model is run only on the random subset of subjects who received these additional cues. Given the explicit party and ideological primes, we should expect differential effects dependent on subjects' own partisan affiliation for these attributes. I therefore compare the revealed partisanship of the candidate to that of the subject, coding whether the partisanship is the "same" or "different". Similarly, for ideology, I project the ideological factor levels evenly between (0-10) and measure the absolute difference in ideology between candidate and subject:

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

Figure 5: Candidate conjoint results *with* partisan, ideological and previous experience attributes included.



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

Figure 5 displays the results of this model. The independent effects of all but one feature of disclosure are indistinguishable from zero once subjects' political priors are primed: only the positive effect of a majority of within-state donors has a significant effect on vote choice.

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

Figure F2 in the SI plots separate model coefficients for Republican and Democrats in the sample. Across all disclosure attributes, there are few substantive differences between Democrats and Republicans. Wide confidence intervals for the smaller Republican sample, however, suggest this model on its own is underpowered. Figure F3, similarly, reports the difference in marginal means between Democrat and Republican identifying subjects. Again, while there are large and highly significant differences for partisan and ideological primes, all differences in marginal means for the disclosure attributes are statistically insignificant.

Taken together, the results across the two candidate conjoint models (with and without partisan information) suggest that while disclosure *can* affect vote choice, it is relatively inert in more realistic contexts in which other politically relevant 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. I explore the reasons for this drop in efficacy in Section 4.

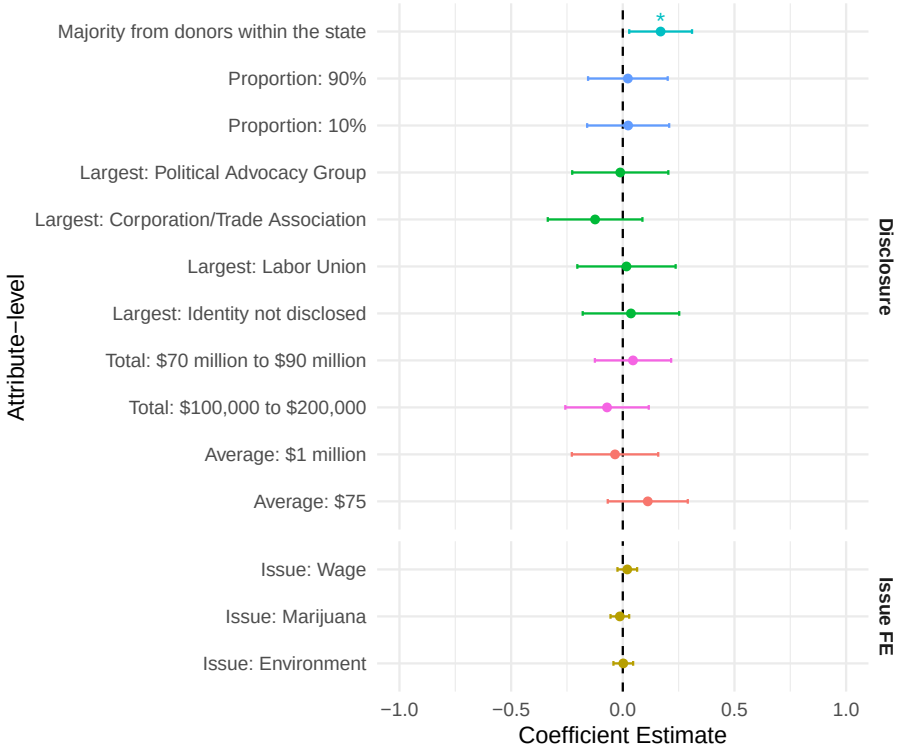
3.3 Disclosure's effect on initiative outcomes

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

Figure 6 displays the results of a logistic regression on the pooled experimental data across the four initiative topics, with issue fixed effects to control for the underlying support of each policy. The only statistically significant disclosure attribute is, again, the geographic origin of donations. This attribute's effect is robust across all three conjoint experiments in this paper. Subjects do seem to favour those campaigns that are funded predominantly by donors within their own state. This feature notwithstanding, it appears that voters'

attitudes towards initiative policies are reasonably fixed and that disclosure in general does not impact subjects' vote choice.[10]

Figure 6: Pooled initiative conjoint results.



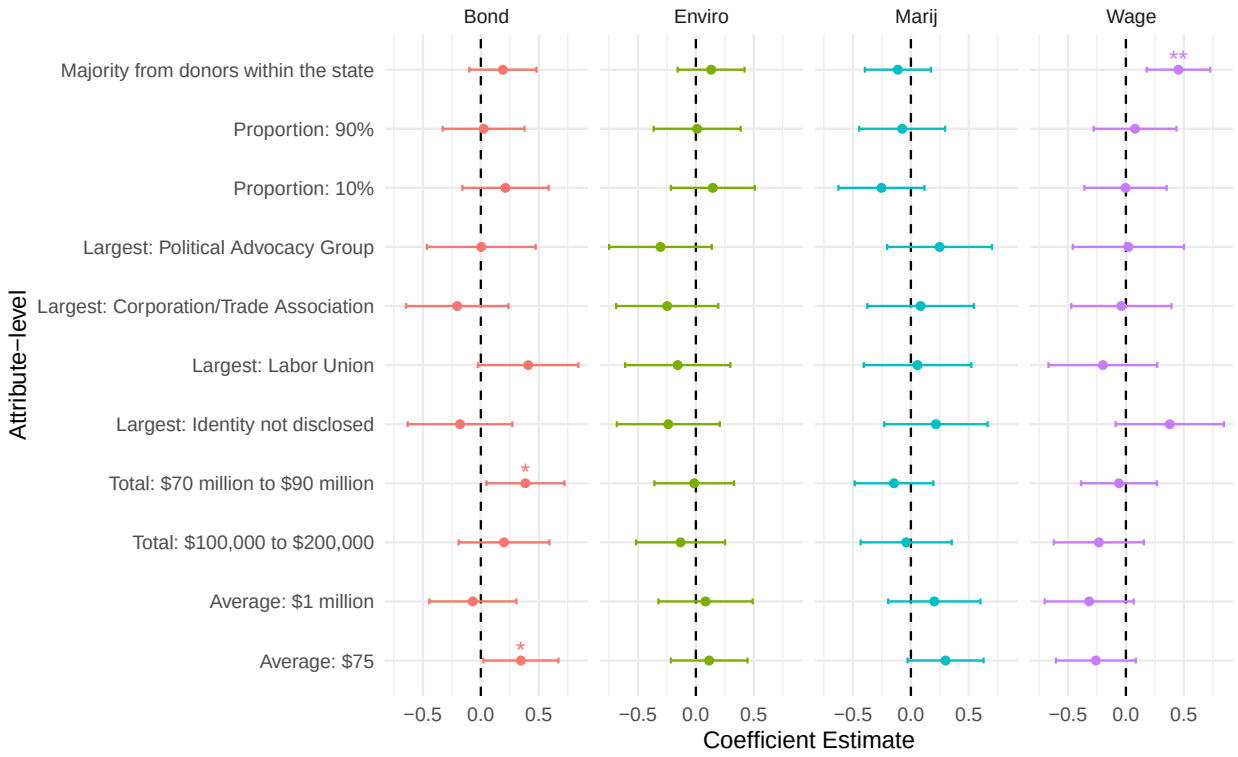
All responses across the four issues are pooled, and issue fixed effects are included to control for varying levels of support not contingent on disclosure mechanisms. Coefficients are shown with 95 percent confidence intervals, and standard errors are clustered by participant. Stars above coefficients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

It may be plausible, however, that voters use disclosure information in different ways 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 and a low average donation are statistically significant, positive predictors of support for the sewage bond issue. No attribute level is significant for either the marijuana legalization or environmental taxation issues, and only the majority of within-state donations attribute is statistically significant for the minimum wage issue. These results are suggestive that for issues that are less salient (where we might expect

[10]Further analysis of subjects' support for these four issues is reported in SI Section F.1.

Figure 7: Initiative conjoint results by issue area.



Separate logit models are run for each of the four initiative policies posed to subjects. All coefficients are shown with 95 percent confidence intervals, and standard errors are clustered by participant. Stars above coefficients indicate significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ respectively.

voters' issue preferences to be less strongly held) that disclosure can alter vote choice. However, these results are purely indicative and further research 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, 2020) I also compare the differences in marginal means for the attributes by issue. Figure F4 in the SI 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 indistinguish-

able 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 Why does the effect of disclosure disappear?

In the absence of partisan information, various attributes of disclosure have substantive effects on vote choice. But the inclusion of explicit partisan attributes within the conjoint experiment reduce the effects of disclosure to near-zero. In Section 1.3 I outlined two plausible explanations of why the effect of disclosure would not be robust once partisan information is made explicit. In this final section, I present suggestive evidence that tests how supportive the experimental data is for both explanations. In short, the results suggest that the effects of disclosure are swamped by partisan signals rather than disclosure operating indirectly through partisanship.

4.1 Information Equivalence

Recall that the informational equivalence mechanism suggests disclosure acts, in the absence of explicit partisan information, to prime voters about the partisanship of candidates. The effect drops out in contexts with explicit partisan information because voters no longer need to make inferences about candidates' partisanship from disclosure information.

How can we test this mechanism empirically? With the experimental data it is not possible to assess directly subjects' perceptions of candidates' partisanship, and therefore we cannot recover the direct effect of disclosure on partisan perceptions either.[11] That said, we can make plausible assumptions about how subjects would use these partisan signals to inform their vote choice. A simple, plausible heuristic is that subjects choose candidates

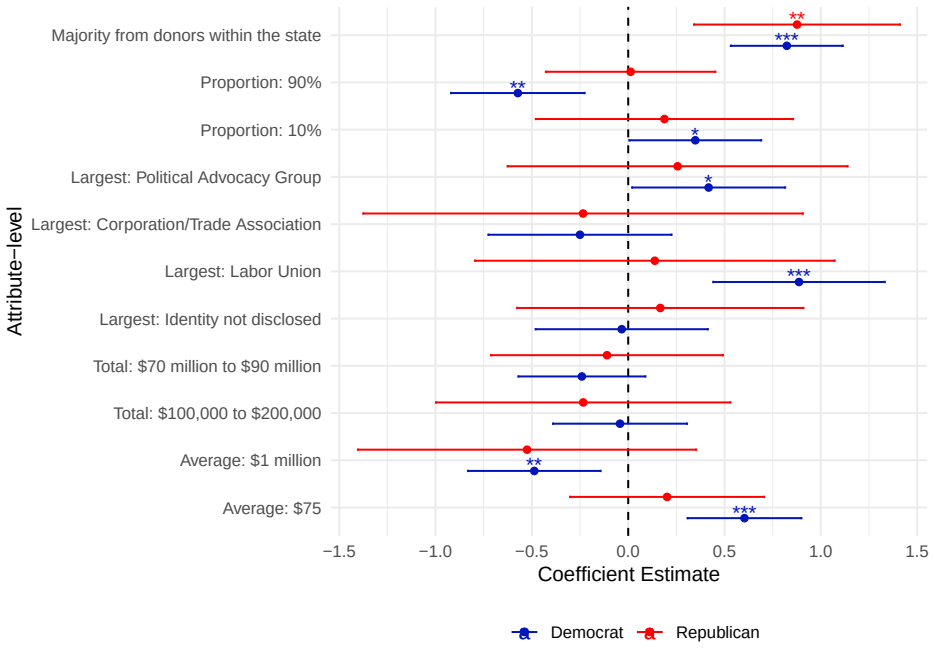
[11] Asking for perceptions of profiles' party membership would have biased how voters use the disclosure information.

who are co-partisans. If the disclosure information suggests a candidate is a Democrat, a Democrat-identifying subject should be more likely to vote for that candidate. Conversely, a Republican subject would be less likely to vote for that same candidate given the partisan information revealed by the disclosure attribute. In expectation, therefore, when partisanship is *not* directly revealed to subjects, the effects of disclosure should operate in opposite directions for Democrat and Republican subjects.

To assess this hypothesis, I run separate logistic models for Democrat- and Republican-identifying subjects exposed to the disclosure-only conjoint experiment. In this experiment, since partisanship was not explicitly primed, it is possible for disclosure to act as a partisan signal. Estimating separate coefficients for Republican and Democrat subjects enables us to test whether these effects have opposing effects dependent on partisan identification, and thus whether the experimental data is more consistent with the informational equivalence mechanism.

Figure 8 plots the coefficient estimates. As before, the usual caveats regarding subgroup analyses apply – smaller sample size (particularly for Republican subjects) increase the uncertainty surrounding the coefficient estimates. The evidence, while only suggestive, is not supportive of the informational equivalence mechanism. It appears that, across disclosure attributes, Republican and Democrat subjects adjust their voting behaviour similarly. Both groups respond positively to donations originating from within the state, low levels of capture by individual donors ('Proportion 10%') and low average donations are positive for both groups (albeit only statistically significant for Democrat subjects). And while the difference in estimated coefficients for Democrats and Republicans is larger for the labor union attribute, the estimate is still (non-significantly) positive for Republicans. Similar results using differences in marginal means are reported in SI Figure F1.

Figure 8: Conjoint results estimating separate effects dependent on subjects' partisan identity.



Each point is a logit coefficient from models estimated separately on Republican and Democrat-identifying subjects. Coefficients are shown with 95% confidence intervals, and standard errors are clustered by participant. Subjects' party identification was asked after all experimental components of the survey.

4.2 Swamping

The non-zero effects of disclosure in the first experiment do not appear to operate by priming subjects' perceptions of candidate partisanship. Nevertheless the inclusion of partisan primes causes a drop in the relative effect of disclosure on vote choice. The swamping mechanism suggests that while disclosure matters to voters in the absence of other relevant cues, the effect of revealing a candidate's partisanship is so dominant it drowns out the effect of the disclosure heuristic.

The evidence presented throughout this paper is consistent with this explanation. Disclosure is effective only when other dominant heuristics are not available. In the conjoint experiment without partisan signals, disclosure has substantive and substantial effects on vote choice. Given Republican- and Democrat-identifying subjects in this condition

respond similarly to disclosure information, this suggests that these signals operate independent of partisanship. However, when partisan signals are presented to subjects, the estimated effect of disclosure is essentially zero. This latter result is consistent with the theory that the heuristic is still operational, but its effect on *vote choice* is minimised because its impact is much weaker relative to dominant partisanship heuristics.

There are two implications of this analysis. First, the evidence is suggestive that disclosure can have a separable, informational benefit to individuals. Republican and Democratic subjects do not appear to react differently to the disclosure cues when partisan information is unavailable. Second, however, this informational benefit is not resilient to the inclusion of other relevant cues. Once highly-salient political signals like partisanship are included, subjects no long rely on disclosure-based heuristics to make their vote choice. In other words, the *direct* effect of disclosure drops out as individuals pivot to using partisan cues to inform their decisionmaking – signals that are predominant in contemporary US elections.

5 Discussion

In the absence of other relevant information, disclosure can influence vote choice. Subjects in the experiments presented in this paper appear particularly concerned about features of campaign funding profiles that indicate the extent to which campaigns are captured by narrow interests, or by out-of-state actors. Crucially, however, once subjects are primed with overt partisan and ideological cues, features that are ubiquitous to real candidate campaigns, the effects of disclosure all but disappear. Suggestive evidence comparing behaviour of Democrats and Republicans separately suggest that rather than acting to prime subjects' perceptions of candidate partisanship, the independent effect of disclosure is simply "swamped" by other, dominant signals.

One exception is the positive effect of having a majority of within-state donations, which is robust across both candidate and initiative conjoints. Voters appear averse to the influ-

ence of actors outside their state, even in the presence of partisan and policy signals. The resilience of this effect is notable and future work should consider precisely why this effect is so robust when other disclosure mechanisms are not. For instance, how might disclosure affect vote choice when salient local or state-level figures are known contributors to campaigns?

Disclosure appears to be equally ineffective in ballot initiative campaigns. This null result was true even though no additional signals beyond the policy proposal and funding information were disclosed. This finding is consistent with (although by no means conclusive) a policy rather than partisan-oriented swamping mechanism. Strong policy cues may drown out any effect that disclosure has on voters. This mechanism is harder to identify experimentally, since excluding policy signals renders subjects' decisions meaningless. Future work, therefore, may seek to develop protocols that tease out whether disclosure is more influential for some policy areas compared to others. Separately, researchers could explore whether issue salience mediates cues about valence and ideological features of initiative campaigns.

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

Consequently, the trade-off that some have suggested exists between greater exposure and voters' aversion to interest group involvement (Ashworth, 2006), does not seem to

be supported by the results of this study. The relatively limited effects of disclosure on voter behaviour observed in this study suggest policymakers should think carefully about what the goals of disclosure are. While transparency initiatives may perturb unethical behaviour by campaigns *ex ante*, disclosure does not appear to curb the influence of unrepresentative interests via voters' behavior. If policymakers wish to ensure political equality in the electoral process, and reduce the influence of donors on the viability and ultimately success of campaigns, disclosure alone appears insufficient.

References

Arceneaux, Kevin. 2010. "The Benefits of Experimental Methods for the Study of Campaign Effects." *Political Communication* 27(2):199–215.

Ashworth, Scott. 2006. "Campaign Finance and Voter Welfare with Entrenched Incumbents." *American Political Science Review* 100(1):55–68.

Briffault, Richard. 2010. "Campaign Finance Disclosure 2.0." *Election Law Journal: Rules, Politics, and Policy* 9(4):273–303.

Buckley v. Valeo 424 U.S. 1. 1976.

Carpini, Michael X. Delli and Scott Keeter. 1996. *What Americans Know about Politics and Why It Matters*. Yale University Press.

Citizens United v. Federal Election Commission, 558 U.S. 310. 2010.

Coate, Stephen. 2004. "Pareto-Improving Campaign Finance Policy." *The American Economic Review* 94(3):628–655.

Dafoe, Allan, Baobao Zhang and Devin Caughey. 2018. "Information Equivalence in Survey Experiments." *Political Analysis* 26(04):399–416.

Dowling, Conor M. and Amber Wichowsky. 2013. "Does It Matter Who's Behind the Cur-

tain? Anonymity in Political Advertising and the Effects of Campaign Finance Disclosure." *American Politics Research* 41(6):965–996.

Dowling, Conor M. and Amber Wichowsky. 2015. "Attacks without Consequence? Candidates, Parties, Groups, and the Changing Face of Negative Advertising: The Changing Face of Negative Advertising." *American Journal of Political Science* 59(1):19–36.

Dowling, Conor M. and Michael G. Miller. 2016. "Experimental Evidence on the Relationship Between Candidate Funding Sources and Voter Evaluations." *Journal of Experimental Political Science* 3(02):152–163.

First National Bank of Boston v. Bellotti, 435 U.S. 765. 1978.

Garrett, Elizabeth and Daniel A. Smith. 2005. "Veiled Political Actors and Campaign Disclosure Laws in Direct Democracy." *Election Law Journal: Rules, Politics, and Policy* 4(4):295–328.

Gerber, Elisabeth R. 1999. *The Populist Paradox: Interest Group Influence and the Promise of Direct Legislation*. Princeton University Press.

Gigerenzer, Gerd and Wolfgang Gaissmaier. 2011. "Heuristic Decision Making." *Annual Review of Psychology* 62(1):451–482.

Hainmueller, Jens, Daniel J. Hopkins and Teppei Yamamoto. 2014. "Causal Inference in Conjoint Analysis: Understanding Multidimensional Choices via Stated Preference Experiments." *Political Analysis* 22(01):1–30.

Jiang, Lear. 2019. "Disclosure's Last Stand? The Need to Clarify the "Informational Interest" Advanced By Campaign Finance Disclosure." *Columbia Law Review* 119(2):487–526.

Leeper, Thomas J, Sara B Hobolt and James Tilley. 2020. "Measuring Subgroup Preferences in Conjoint Experiments." *Political Analysis* 28(2):207–221.

Lupia, Arthur. 1994. "Shortcuts Versus Encyclopedias: Information and Voting Behavior

in California Insurance Reform Elections." *The American Political Science Review* 88(1):63–76.

Magleby, David B. 1994. "Let the Voters Decide - An Assessment of the Initiative and Referendum Process." *University of Colorado Law Review* 66:13.

Mayer, Jane. 2016. *Dark Money: The Hidden History of the Billionaires behind the Rise of the Radical Right*. First edition. ed. New York: Doubleday.

McIntyre v. Ohio Elections Commission, 514 U.S. 334. 1995.

Oklobdzija, Stan. 2019. "Public Positions, Private Giving: Dark Money and Political Donors in the Digital Age." *Research and Politics* 6(1):1–8.

Prat, Andrea, Riccardo Puglisi and James M. Snyder Jr. 2010. "Is Private Campaign Finance a Good Thing? Estimates of the Potential Informational Benefits." *Quarterly Journal of Political Science* 5(3):291–318.

Primo, David M. 2013. "Information at the Margin: Campaign Finance Disclosure Laws, Ballot Issues, and Voter Knowledge." *Election Law Journal: Rules, Politics, and Policy* 12(2):114–129.

Rhodes, Samuel C., Michael M. Franz, Erika Franklin Fowler and Travis N. Ridout. 2019. "The Role of Dark Money Disclosure on Candidate Evaluations and Viability." *Election Law Journal: Rules, Politics, and Policy* 18(2):175–190.

Ridout, Travis N., Michael M. Franz and Erika Franklin Fowler. 2015. "Sponsorship, Disclosure, and Donors: Limiting the Impact of Outside Group Ads." *Political Research Quarterly* 68(1):154–166.

Sances, Michael W. 2013. "Is Money in Politics Harming Trust in Government? Evidence from Two Survey Experiments." *Election Law Journal: Rules, Politics, and Policy* 12(1):53–73.

Sen, Maya. 2017. "How Political Signals Affect Public Support for Judicial Nominations: Evidence from a Conjoint Experiment." *Political Research Quarterly* 70(2):374–393.

Spencer, Douglas M and Alexander G Theodoridis. 2020. "The "Appearance of Corruption" Linking Public Opinion and Campaign Finance Reform." *Election Law Journal* (Available Online):1–32.

Stratmann, Thomas. 2010. Campaign Spending and Ballot Measures. In *Financing Referendum Campaigns*, ed. Karin Gilland Lutz and Simon Hug. London: Palgrave Macmillan UK.

Wood, Abby K. 2018. "Campaign Finance Disclosure." *Annual Review of Law and Social Science* 14(1):11–27.

Wood, Abby K. 2019. "Show Me the Money: Candidate Selection Based on Campaign Finance Transparency." *SSRN Electronic Journal* .
URL: *https://ssrn.com/abstract=3029095*

Supporting Information for "When do voters respond to campaign finance disclosure? Evidence from multiple types of election"

Contents

A Subject Description

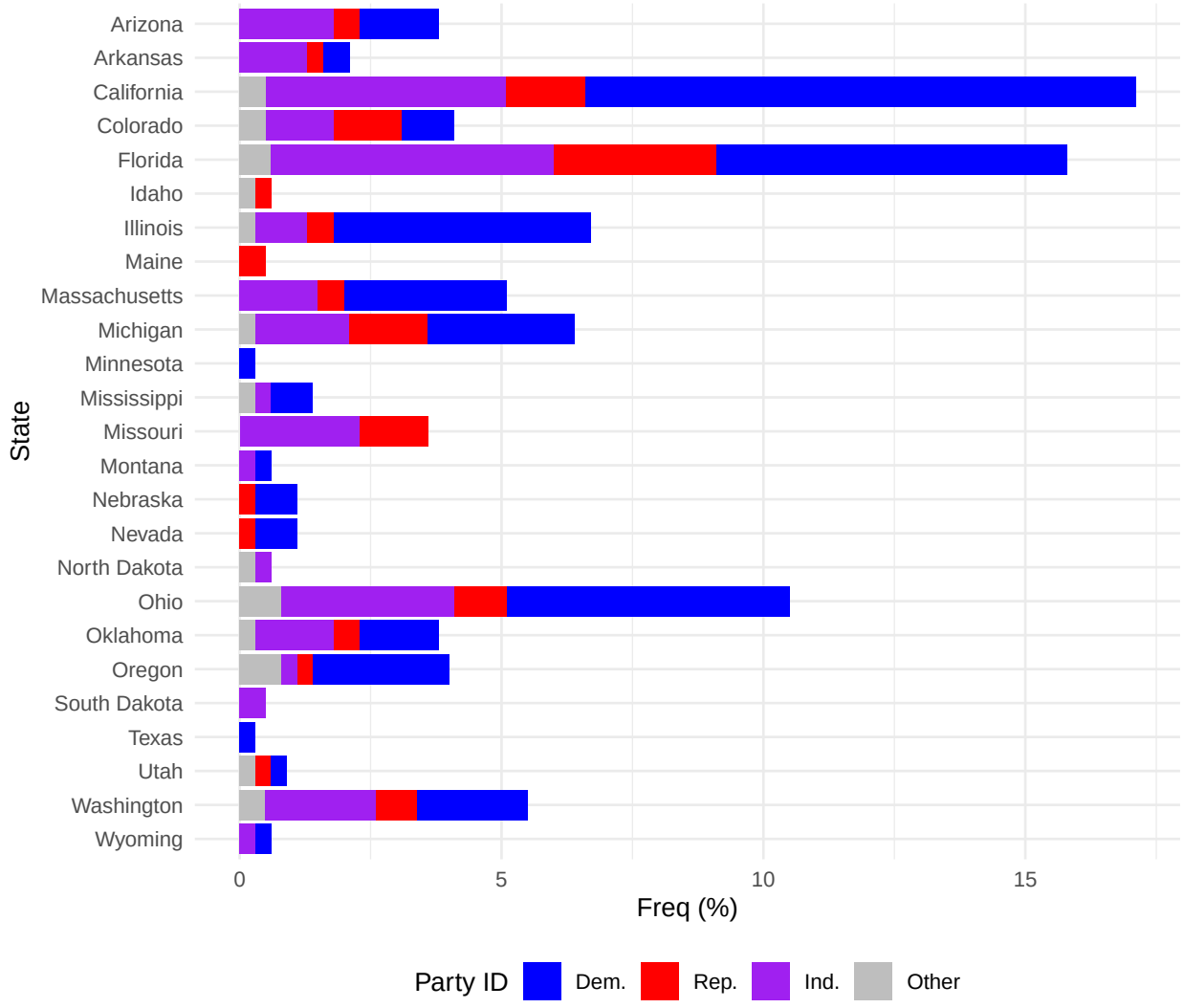
All subjects were recruited via the CESS Online US subject pool, according to the exclusion restrictions noted in the main body of the text. Responses were collected between 18th February and 8th March 2019. Table A1 outlines key demographic information about the subjects, and Figure A1 plots the frequency 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.

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

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

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

Figure A1: Frequency of subject participation by states



B Citizens United and its Implications For Disclosure

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

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

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

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

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

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

C Summary of State Campaign Finance Laws

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

[12] "Mega" PAC's can contribute $10,100
[13] $5675 for small donor committees.
[14] $5675 for small donor committees.
[15] Gubernatorial candidates with more than $250,000 independent expenditures are exempt, or if opposition candidate is self-funded (spending over $250,000.)
[16] Independent PACs can contribute up to $68,000.

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

State	Individual	PAC	Corporate	Union
Arizona	$5100	$5100[12]	Prohibited	Prohibited
Arkansas	$2700	$2700	Prohibited	Prohibited
California	$29200	$29,200	$29,200	$29,200
Colorado	$575	$575[13]	Prohibited	$575[14]
Florida	$3000	$3000	$3000	$3000
Idaho	$5000	$5000	$5000	$5000
Illinois	$5600[15]	$55,400	$11,100	$11,100
Maine	$1600	$1600	$1600	$1600
Massachusetts	$1000	$500	Prohibited	$500
Michigan	$6800	$6800[16]	Prohibited	Prohibited
Minnesota	$4000	$4000	Prohibited	$4000
Mississippi	Unlimited	Unlimited	$1000	Unlimited
Missouri	$2600	$2600	Prohibited	Prohibited
Montana	$1990	$10610	Prohibited	Prohibited
Nebraska	Unlimited	Unlimited	Unlimited	Unlimited
Nevada	$5000	$5000	$5000	$5000
North Dakota	Unlimited	Unlimited	Prohibited	Prohibited
Ohio	$12707.79	$12707.79	Prohibited	Prohibited
Oklahoma	$2700	$5000	Prohibited	Prohibited
Oregon	Unlimited	Unlimited	Unlimited	Unlimited
South Dakota	$4000	Unlimited	$4000	$4000
Texas	Unlimited	Unlimited	Prohibited	Prohibited
Utah	Unlimited	Unlimited	Unlimited	Unlimited
Washington	$2000	$2000	$2000	$2000
Wyoming	$2500	Unlimited	Prohibited	Prohibited

Example of initiative conjoint round

Figure C1: Screenshot of initiative conjoint

Initiative Title: Marijuana legalisation

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

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

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

- (●) For
- ( ) Against

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

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

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

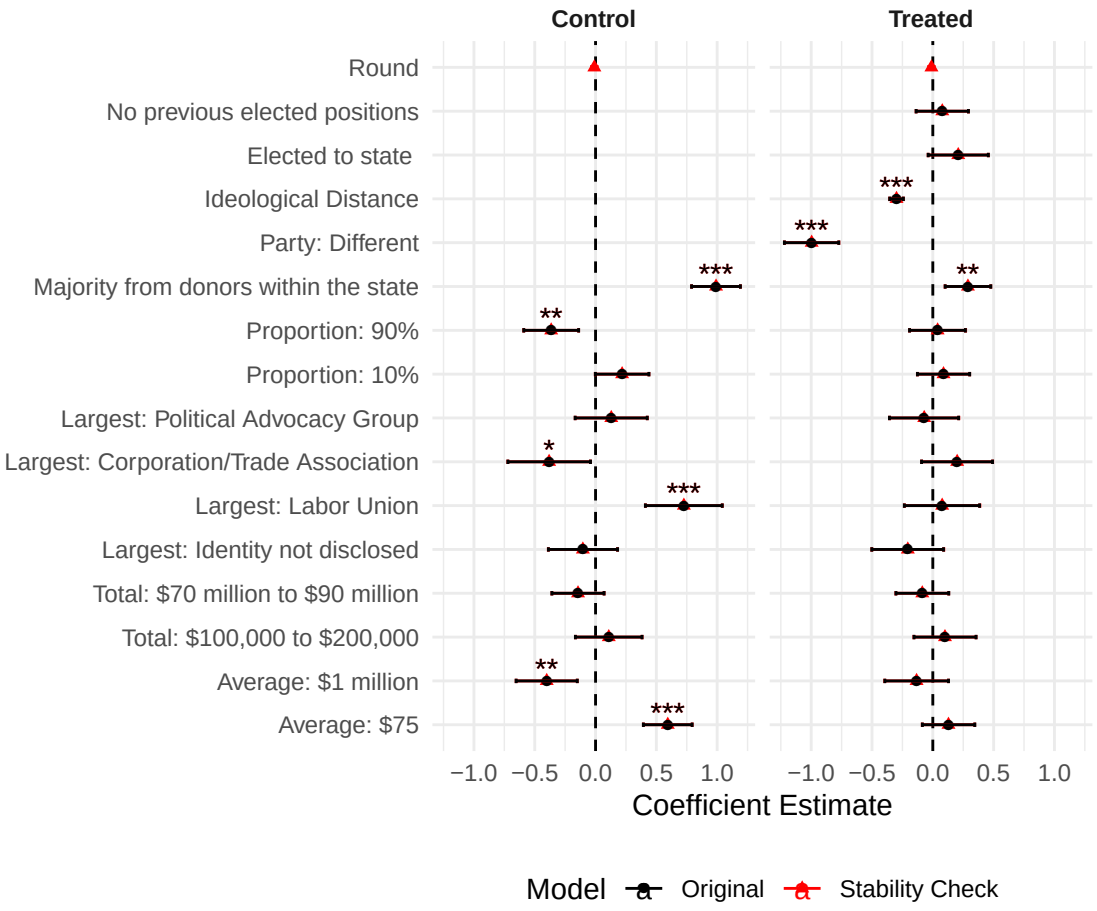
D Causal Assumptions

Stability and no carryover. In line with similar conjoint experiments about political candidates, I do not expect there to be carryover effects between rounds of the same conjoint experiment. The marginal effect of disclosing a majority of out-of-state donations, for instance, should remain stable whether it is presented in the first or last round of the experiment. To ensure this assumption holds, I reran the logistic regressions including a numeric control variable for the round the choice-profile was presented in (1-6). When this variable is included, the coefficients of the candidate conjoint attribute-levels are substantively unchanged, nor is the round variable statistically significant – suggesting that the stability assumption holds. Figure D1 demonstrates these results compared to the original models.[17]

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.

[17] 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 D1: Comparison of coefficients between models reported in the main text and models including a continuous variable of the conjoint round, to check for stability and any carryover effects. Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.



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

external validity and subject engagement outweighed the smaller benefits of including atypical profiles.[18]

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 E2 and E1 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. Given the number of models, by chance we would expect to observe some statistically significant coefficient irrespective of balance. Overall, the evidence suggests the randomisation procedures across the experiment were successful.

E Balance Tests

[18] 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.

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

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

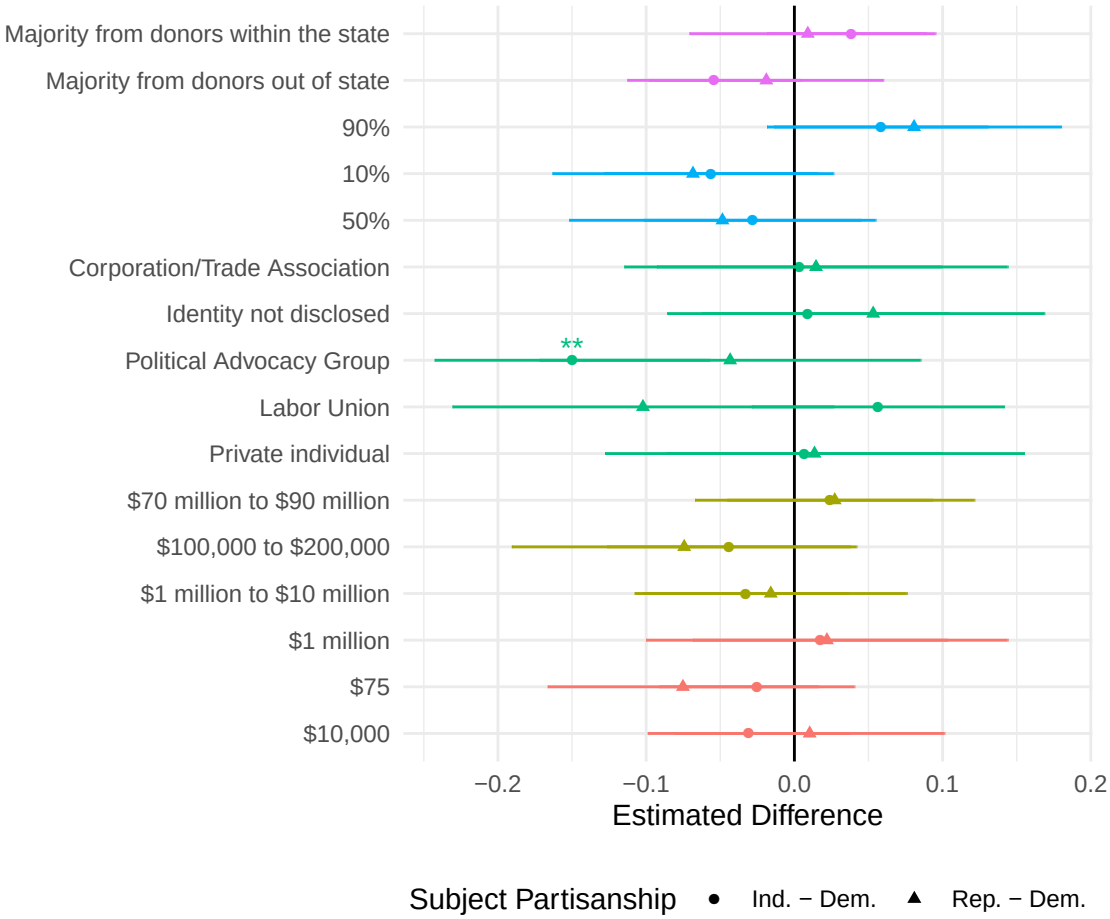
Table E2: Balance test: proportion of times each attribute-level was displayed to participants in the initiative conjoint

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

F Additional Results

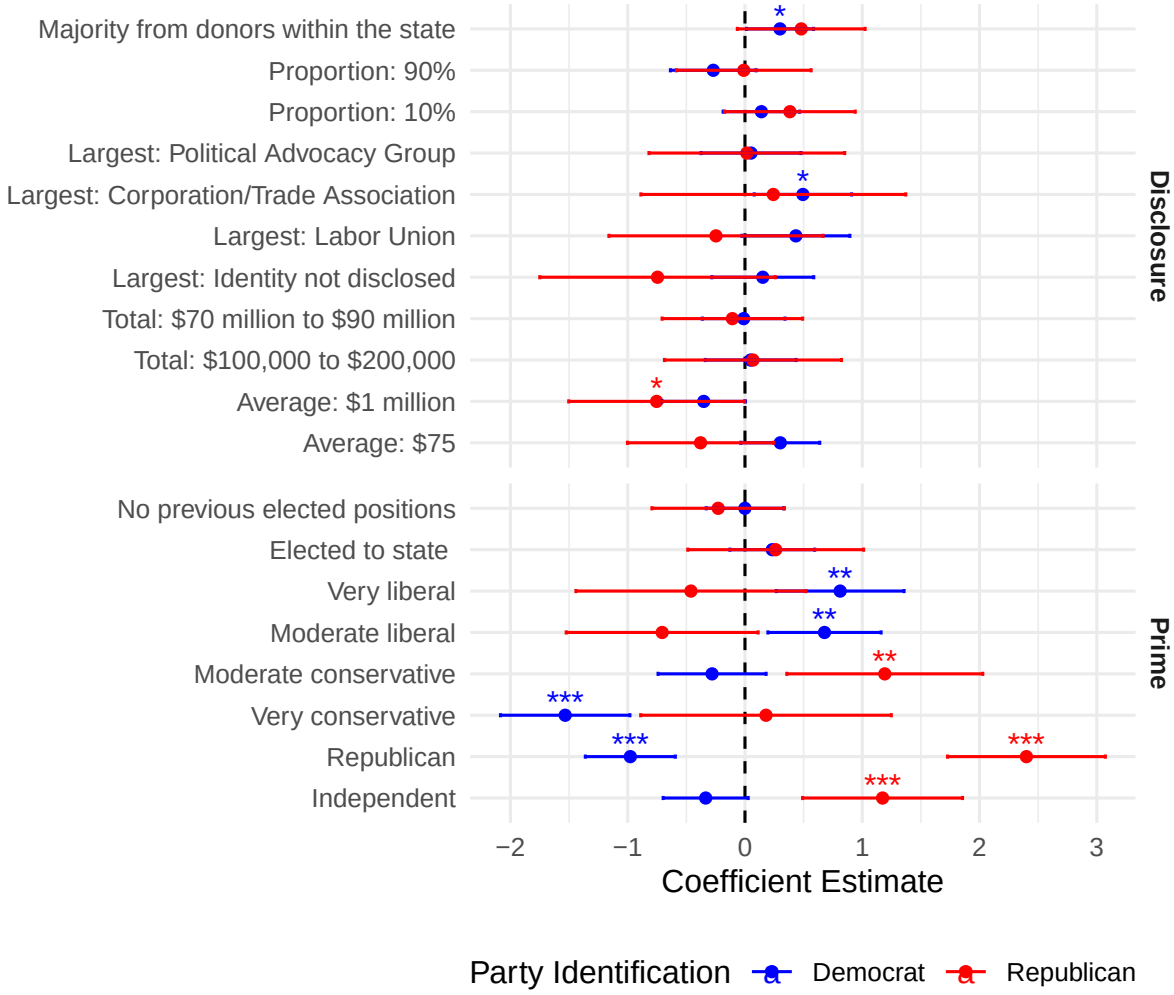
Conjoint models

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



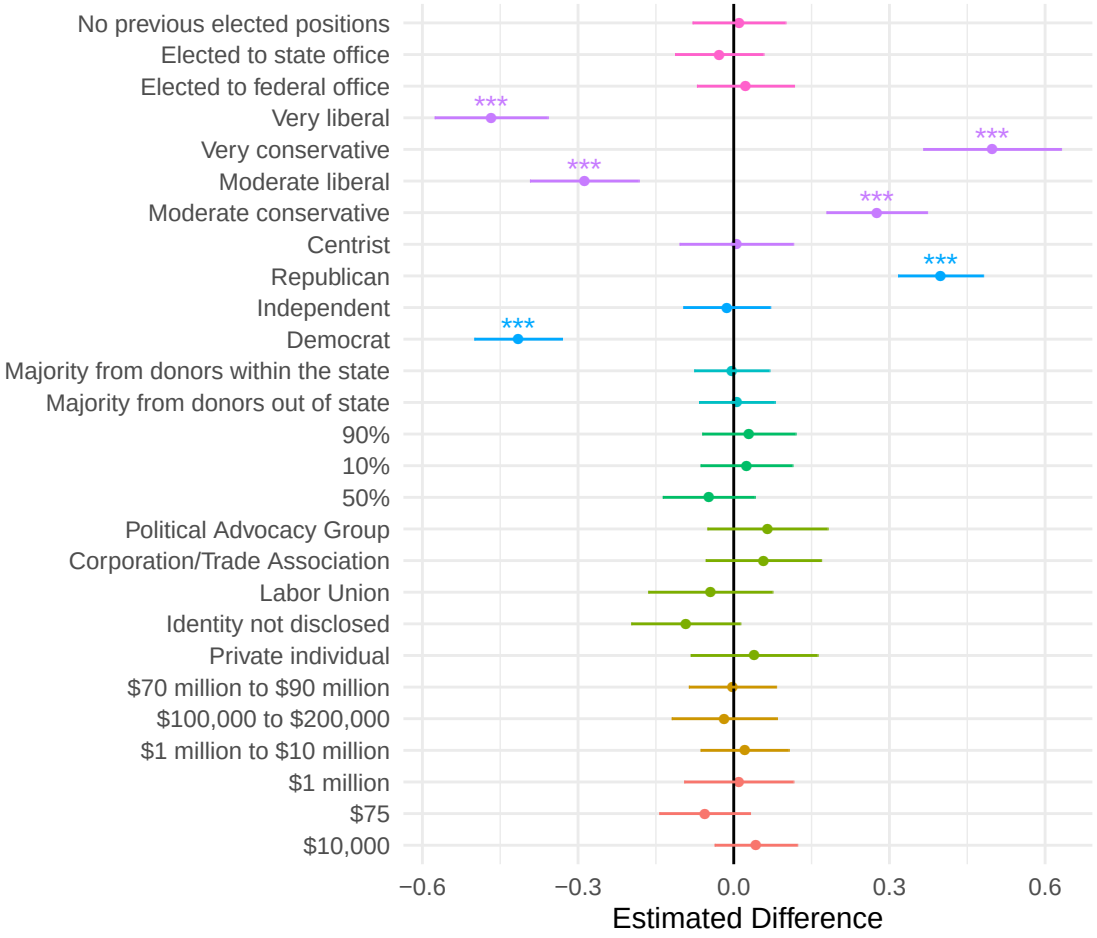
Estimated differences are shown with 95 percent confidence intervals. Stars indicate statistically significant differences at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Figure F2: Comparison of causal effects between Democratic and Republican respondents in the sample, for those exposed to additional political cues.



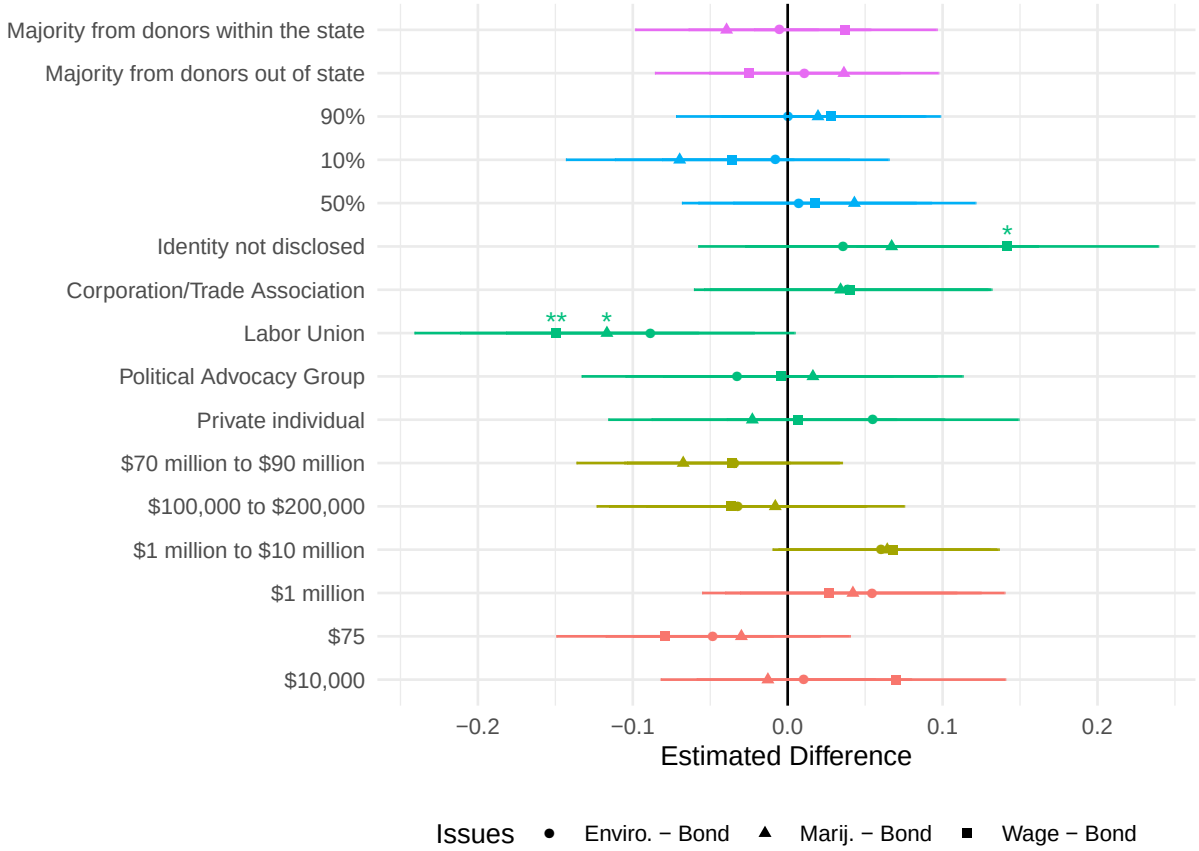
Coefficients are shown with 95 percent confidence intervals. Stars above coefficients indicate statistical significance at *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$.

Figure F3: 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 F4: 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 ***$p < 0.001$.

F.1 Subjects' Support For Hypothetical Initiative Campaigns

Table F1 shows the proportion of participants that voted for each proposal. The sewage bond, marijuana legalization and minimum wage increase initiatives all received greater than 70 percent support. Only the environmental protection initiative was a marginal race. Even without further analysis, therefore, it is clear that voters' opinions on these issues are relatively fixed (given that disclosure attribute levels are completely randomised).

Table F1: Subject support and rating of initiative campaigns

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

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

Table F1 also reports the mean approval rating of each side of the campaign (on a scale of 1-7), for each issue. Interestingly, there is some variation between subjects' approval of campaigns and their respective vote choice. While a majority voted against the 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 statistically significant) than the vote proportions would suggest.

References

Ansolabehere, Stephen, Brian F. Schaffner and Sam Luks. 2019. "Cooperative Congressional Election Study, 2018: Common Content.".

Briffault, Richard. 2011. "Nonprofits and Disclosure in the Wake of Citizens United." *Election Law Journal: Rules, Politics, and Policy* 10(4):337–361.

Citizens United v. Federal Election Commission, 558 U.S. 310. 2010.

Jiang, Lear. 2019. "Disclosure's Last Stand? The Need to Clarify the "Informational Interest" Advanced By Campaign Finance Disclosure." *Columbia Law Review* 119(2):487–526.

Mayer, Jane. 2016. *Dark Money: The Hidden History of the Billionaires behind the Rise of the Radical Right*. First edition. ed. New York: Doubleday.

Oklobdzija, Stan. 2019. "Public Positions, Private Giving: Dark Money and Political Donors in the Digital Age." *Research and Politics* 6(1):1–8.

Wood, Abby K. 2019. "Show Me the Money: Candidate Selection Based on Campaign Finance Transparency." *SSRN Electronic Journal* .
URL: *https://ssrn.com/abstract=3029095*

Wood, Abby K. and Douglas M. Spencer. 2016. "In the Shadows of Sunlight: The Effects of Transparency on State Political Campaigns." *Election Law Journal: Rules, Politics, and Policy* 15(4):302–329.