Preferential Abstention in Conjoint Experiments

Abstract

Conjoint experiments are used to mimic political choices that people face, such as voting for public officials or selecting news stories. Conjoint designs, however, do not always mirror the real-world decision-making contexts that individuals engage in because respondents are typically forced to select one of the available options. We illustrate theoretically how offering respondents an abstention option can produce different average marginal component effects (AMCEs) relative to a forced-choice outcome. This difference depends on 1) the proportion of respondents who would rather abstain than select profiles lacking their preferred attribute-levels, and 2) those respondents' preference orderings. Then, we replicate two conjoint experiments and demonstrate how omitting a realistic abstention option can lead to substantively different estimates of the AMCEs.

1 Introduction

Social scientists frequently use conjoint experiments to study every-day decisions humans face. Conjoint experiments ask respondents to evaluate two or more alternatives that differ along a set of characteristics, often requiring participants to indicate which of the available alternatives they most prefer.[1] This "forced-choice" design has been used to measure individuals' preferences over candidates in elections (Carlson 2015), economic policies (Chilton, Milner and Tingley 2020), and news stories (Mukerjee and Yang 2020). Yet, participants may ordinarily abstain from these types of decision-making processes entirely in daily life if they find none of the choices suitable.

We demonstrate that deviating from the response options available in real-world settings is not only a concern for external validity, but also a problem in how preferences are aggregated; the average marginal component effects (AMCEs)—the workhorse estimand for conjoint studies—estimated with a forced-choice outcome can substantively differ from those we would observe if abstention was possible. We outline theoretically how the AMCEs associated with attribute-levels in a forced-choice design can be larger *or* smaller than the AMCEs for the same attribute-levels in a design allowing for abstention. Importantly, researchers cannot know how the magnitudes of any AMCEs obtained through a forced-choice design compare to the estimates that would arise from a design with an abstention option. We then replicate two conjoint experiments that used forced-

[1] We identify and hand-code the outcome measures of all conjoint experiments published in the *American Journal of Political Science*, *American Political Science Review*, and *Journal of Politics* as of June 2022 in the Supplementary Materials. Of the published conjoint studies, 88% include a forced-choice outcome. The second most common outcome measure in conjoint designs is the individual rating, whereby respondents evaluate each profile using an ordinal scale (Bansak et al. 2021). The issues we describe concerning forced-choice outcomes do not apply to rating outcomes because respondents rate individual profiles rather than choose among profiles.

choice outcomes (Funck and McCabe 2022; Mummolo 2016) to highlight empirically how including an abstention option can yield statistically and substantively different conclusions.

2 Forced-Choice Outcomes, Abstention, and AMCEs

The standard conjoint experiment asks each respondent ($i \in 1, ..., N$) to evaluate a fixed number of tasks ($k \in 1, ..., K$). Each task presents respondents with a fixed number of profiles ($j \in 1, ..., J$) that consist of randomly assigned levels for each attribute ($l \in 1, ..., L$). After viewing the profiles in each task, respondents express their preferences toward those profiles, most commonly with a “forced-choice" outcome that requires respondents to indicate the profile they most prefer.

A key assumption of the conjoint framework is that “respondents must choose one preferred profile j within each choice task k" (Hainmueller, Hopkins and Yamamoto, 2014, 7). Therefore, we assume that all respondents, if presented with the same choice task in a real-world setting, would be required to make a choice from the set of available alternatives. However, for many of the decision-making contexts to which conjoint experiments are applied, ample evidence exists that many people abstain when prompted to make a choice. For instance, voters’ decision to cast a ballot for one of the available candidates or parties in an election is far from universal, even in countries with compulsory voting (Blais 2006).

Notably, abstention is often non-random and certain *types* of respondents, as defined by their preference orderings, might be more likely to abstain. Voters’ propensity to cast

a ballot is a mixture of individual, institutional, and election-specific forces. The competitiveness of a race, individuals' education and wealth, and the costs associated with voting all impact turnout (Rosenstone and Hansen 2003). Similarly, partisans' willingness to consume news is influenced by whether their party is advantaged or disadvantaged by salient events (Kim and Kim 2021). As such, when forced-choice outcomes are employed in conjoint experiments to study contexts where abstention is common, some types of respondents may be artificially induced to make a choice when they would ordinarily abstain.[2] While this disconnect sparks concerns about the external validity of the forced-choice design, we focus on the mechanical issues that arise when we force respondents' to elicit preferences and how it may yield different estimates of the AMCEs than would manifest if respondents could abstain.[3]

2.2 Implications of Abstention on AMCEs

We adapt a stylized conjoint experiment from Abramson, Koçak and Magazinnik (2022) in which $N = 5$ voters are asked to evaluate pairs of candidates based on two attributes, each with two levels. First, whether the candidate shares the voter's partisan affiliation—Partisanship $\in$ {C=Copartisan, N=Non-copartisan}—and second, the candidate's ethical reputation—Reputation $\in$ {H=Honest, D=Dishonest}. For simplicity, we assume

[2]Including abstention options (e.g., "don't know") in outcome measures is sometimes discouraged because respondents may use these options to satisfice and conceal real opinions (Berinsky 2008; Krosnick et al. 2002). The survey questions that typically fall prey to this concern strive to measure latent concepts that most ordinary people have preferences over, such as approval of politicians and policies. However, choice-based outcomes in conjoint designs seek to measure respondents' preferences over a circumscribed set of stylized alternatives. In this context, abstention is a meaningful response option because respondents' willingness to make a selection is a prerequisite to choosing one of the available profiles, and omitting an abstention option can force respondents to express insincere preferences.

[3]Within the validity typology proposed by Egami and Hartman (2022), these conceptual concerns fall under Y-validity, or how well the outcome measures map onto the theoretical quantities of interest.

all voters prefer copartisans to non-copartisans and honest candidates to dishonest candidates. However, three voters prioritize candidates' copartisanship over their honesty, while two voters care more about candidates' ethical reputation than copartisanship. The full preference ordering of the four possible candidate types for the five voters is found in Table 1a.

Table 1: Individual and aggregate preferences over candidate profiles with differing importance of copartisanship and reputation.

(a) Voter preferences

Rank	V1	V2	V3	V4	V5
1	C, H	C, H	C, H	C, H	C, H
2	C, D	N, H	C, D	N, H	C, D
3	N, H	C, D	N, H	C, D	N, H
4	N, D	N, D	N, D	N, D	N, D

(b) Aggregate preferences

Comparison	V1	V2	V3	V4	V5	No Abstentions	Uniform Abstentions	Partisanship Abstentions
C, H vs. C, D	C, H	C, H	C, H	C, H	C, H	5,0	5,0	5,0
C, H vs. N, H	C, H	C, H	C, H	C, H	C, H	5,0	5,0	5,0
C, H vs. N, D	C, H	C, H	C, H	C, H	C, H	5,0	5.0	5,0
C, D vs. N, H	C, D	N, H	C, D	N, H	C, D	3,2	3,2	3,2
C, D vs. N, D	C, D	C, D	C, D	C, D	C, D	5,0	3,0	5,0
N, H vs. N, D	N, H	N, H	N, H	N, H	N, H	5,0	2,0	2,0

Notes: C=Copartisan, N=Non-copartisan, H=Honesty, D=Dishonest. In the three rightmost columns of Panel 1b, the first and second numbers indicate the number of voters preferring the first and second candidates in the comparison pair given respondents' ability to abstain (as indicated by the column headings).

We use the voter preference rank-orderings in Table 1a to determine the electorate's aggregate preferences for each unique combination of candidate profiles in Table 1b. The final three columns of the table provide the vote tallies for each candidate comparison

under three scenarios: when voters must vote for the candidate they prefer ("No Abstentions"); when all voters abstain if neither candidate possesses their desired level of the attribute they prioritize ("Uniform Abstentions"); and when voters 1, 3, and 5 abstain if neither candidate is a copartisan, but voters 2 and 4 always vote ("Partisanship Abstentions"). In all three scenarios, the first candidate profile wins each comparison, but some voters abstain in the final two comparisons because neither candidate has their preferred level of the attribute-level they emphasize (e.g., in the final matchup, voters 1, 3, and 5 abstain because neither candidate is a copartisan).

Finally, we use these vote tallies to calculate the AMCEs for copartisanship and honesty under each scenario in Table 2. When using a forced-choice outcome that requires all voters to cast a ballot, the AMCEs for partisanship and reputation are 0.40 and 0.35 (Table 2, Column 1). However, when voters are allowed to abstain if neither candidate in the matchup has their preferred level of the attribute that they consider more important, the AMCEs for partisanship and reputation increase to 0.50 and 0.375 (Table 2, Column 2).

To understand why the AMCEs increase under uniform abstention, focus on the matchups in which the vote tallies diverge under the forced-choice and uniform abstention scenarios. For instance, consider the comparison of $\bar{Y}(C, H; N, H) - \bar{Y}(N, H; N, H)$ (Table 2, Panel A, third row); the voters' preference for copartisans is stronger when abstention is allowed because voters who prioritize partisanship no longer cast votes for non-copartisan candidates when no copartisans are present (e.g., the $N, H; N, H$ matchup). Put differently, the respondents that prioritize candidate copartisanship no longer dilute their preference for copartisans by choosing non-copartisans when forced

Table 2: AMCEs for partisanship and reputation varying abstention options.

Panel A: Partisanship

	Forced-Choice	Uniform Abstention	Partisanship Abstentions Only
$\bar{Y}(C,H;C,H)$ - $\bar{Y}(N,H;C,H)$	2.5	2.5	2.5
$\bar{Y}(C,H;C,D)$ - $\bar{Y}(N,H;C,D)$	3	3	3
$\bar{Y}(C,H;N,H)$ - $\bar{Y}(N,H;N,H)$	2.5	4	4
$\bar{Y}(C,H;N,D)$ - $\bar{Y}(N,H;N,D)$	0	3	3
$\bar{Y}(C,D;C,H)$ - $\bar{Y}(N,D;C,H)$	0	0	0
$\bar{Y}(C,D;C,D)$ - $\bar{Y}(N,D;C,D)$	2.5	1.5	2.5
$\bar{Y}(C,D;N,H)$ - $\bar{Y}(N,D;N,H)$	3	3	3
$\bar{Y}(C,D:N,D)$ - $\bar{Y}(N,D;N,D)$	2.5	3	4
AMCE	$=\frac{16}{40}=0.40$	$=\frac{20}{40}=0.50$	$=\frac{22}{40}=0.55$

Panel B: Reputation

	Forced-Choice	Uniform Abstention	Partisanship Abstentions Only
$\bar{Y}(C,H;C,H)$ - $\bar{Y}(C,D;C,H)$	2.5	2.5	2.5
$\bar{Y}(C,H;C,D)$ - $\bar{Y}(C,D;C,D)$	2.5	3.5	2.5
$\bar{Y}(C,H;N,H)$ - $\bar{Y}(C,D;N,H)$	2	2	2
$\bar{Y}(C,H;N,D)$ - $\bar{Y}(C,D;N,D)$	0	2	0
$\bar{Y}(N,H;C,H)$ - $\bar{Y}(N,D;C,H)$	0	0	0
$\bar{Y}(N,H;C,D)$ - $\bar{Y}(N,D;C,D)$	2	2	2
$\bar{Y}(N,H;N,H)$ - $\bar{Y}(N,D;N,H)$	2.5	1	1
$\bar{Y}(N,H;N,D)$ - $\bar{Y}(N,D;N,D)$	2.5	2	1
AMCE	$=\frac{14}{40}=0.35$	$=\frac{15}{40}=0.375$	$=\frac{11}{40}=0.275$

Notes: C=Copartisan, N=Noncopartisan, H=Honest, D=Dishonest. Following Abramson, Koçak and Magazinnik (2022), we use Proposition 3 in Hainmueller, Hopkins and Yamamoto (2014, 16) to calculate the AMCEs. To do so, we first obtain the difference in the number of votes a candidate with one level of an attribute would receive compared to a candidate with the other level of that same attribute, holding the second attribute constant, when pitted against each possible candidate. We then sum these differences and normalize the sums by the product of the number of possible profiles (4), number of possible profiles with a fixed level of one of the two attributes (2) and the number of voters (5). So, the denominator is calculated as the number of unique profiles times the number of voters times the number of possible profiles with the unique levels of copartisanship and corruption (i.e., $4 \times 5 \times 2$).

to do so.

Conversely, some differences, such as $\bar{Y}(C, D; C, D) - \bar{Y}(N, D; C, D)$ (Table 2, Panel A, sixth row), become smaller when voters can abstain because voters who prioritize honesty no longer vote if no honest candidates are present. Nevertheless, for both copartisanship and honesty, because the differences increase, on average, the AMCEs increase from Column 1 to Column 2 in Table 2. Thus, when all voters abstain if neither candidate has their preferred level of the attribute they prioritize, the electorate's preference for both copartisanship and honesty become stronger than when all voters are forced to participate.

Now, consider when only voters 1, 3, and 5 abstain if neither candidate is a copartisan, but voters 2 and 4 never abstain (Table 2, Column 3). In this case, the AMCEs for partisanship and reputation move in opposite directions. While the AMCE for copartisanship increases further to 0.55, the AMCE for honesty shrinks to 0.275. The AMCE for partisanship increases because voters 2 and 4 are forced to participate and cast votes for dishonest copartisans when no honest candidates are present, thus contributing the preference for copartisans to the ultimate AMCE (e.g., the $C, D; N, D$ matchup). However, the AMCE for reputation decreases because voters 1, 3, and 5 are no longer required to vote when both candidates are non-copartisans, and do not express their preference for honest non-copartisans relative to dishonest non-copartisans (e.g., $N, H; N, D$). This example of asymmetric abstention showcases that the consequences of forced-choice outcomes in conjoint experiments are not straightforward.

To illustrate how the rate of abstention among voters with different preference orderings impacts the AMCEs, we extend our example to N-voters and $L > 2$ attributes in the

Supplementary Materials. We find that the differences in the AMCEs are based on the distribution of persons in the sample with each preference ordering and the rate of abstention among those who abstain if none of the profiles presented contain the attribute-level that they prioritize. In other words, the differences in AMCEs depend on *which type of* respondents would abstain, as described by their preference orderings, and *how many* respondents of each type would abstain.

When only one type of voter abstains, the AMCE for the attribute-level prompting abstention increases while other AMCEs decrease. However, when more than one type of voter abstains, the implications for all AMCEs hinge on the distribution of voters of each type and the abstention rate for each voter type. Importantly, we show that when we have $L > 2$ attributes, abstentions triggered by a given attribute-level can alter the AMCEs associated with levels of other attributes, such that using a forced-choice outcome can affect *all* AMCEs in a given study, not only those linked to attributes associated with abstention.

Unfortunately, researchers cannot know beforehand which types of respondents would rather abstain than select profiles lacking their preferred attribute-levels. Further, if respondents with certain preference orderings are more likely to abstain, forced-choice outcomes compel respondents to reveal preferences they would otherwise not hold or express in the real-world. Therefore, researchers cannot rely on any "rules of thumb" to speculate ex post on whether the directionality or magnitude of their AMCEs from forced-choice responses generalize to realistic contexts when abstention is possible.

3 Replication & Extension of Forced-Choice Conjoint

We demonstrate empirically how forced-choice outcomes can produce different estimates of the AMCEs relative to a choice set allowing for abstention through replicating two published conjoint experiments. The first study examines how the complexity of the information environment affects the impact of scandals on vote choice (Funck and McCabe 2022, henceforth "F-M"). The second study, described in the Supplementary Materials, explores how the topic relevance and source partisanship of news stories impact individuals' media consumption (Mummolo 2016).

In their study, F-M ask American respondents recruited through Lucid to view pairs of hypothetical candidates running for U.S. Congress. Partisanship is fixed at the task-level to mirror a general election, with one candidate randomly presented as a Democrat and the other as a Republican. For each task, respondents indicate the candidate for whom they would prefer to vote using a forced-choice outcome.

The two experimental attributes of interest for F-M's primary "Information Hypothesis" are (1) whether candidates are accused of improper behavior and (2) the amount of information provided about the candidates. First, F-M randomly assign one of six levels of "Recent news" for each candidate; three levels are neutral or positive ("No recent news", "Recently honored for public service", or "Recently celebrated wedding anniversary"), while the others implicate the candidate in a scandal ("Recently accused of sexual harassment", "Recently accused of cheating on spouse", or "Recently accused of leaking confidential information").

Second, the number of attributes provided for both candidates is randomly manipulated at the task-level. In the "Low Information" condition, respondents receive only the

candidates' party affiliation and recent news. In the "Medium" and "High Information" conditions, respondents receive randomly assigned levels of three or eight additional attributes of the candidates. In line with their "Information Hypothesis", F-M find voters are less likely to select candidates associated with scandalous allegations, but the magnitude of this penalty shrinks as the information environment's complexity increases.

We replicate F-M's experiment using 2,254 respondents from Amazon's Mechanical Turk (MTurk). We match F-M's protocol with a few modifications. First, half of our respondents were assigned to a *forced-choice arm*. These participants were required to indicate which candidate they preferred in each task. The other half of our respondents were placed in a *abstention option arm* where they could indicate their preferred candidate or abstain from making a selection. Second, after completing their 6 tasks, respondents were presented again with the first profile pair they evaluated but were provided the outcome measure from the opposite experimental arm (e.g., forced-choice arm respondents were given the option to abstain).[4]

Following F-M, we estimate our AMCEs using ordinary least squares regression with standard errors clustered by respondent. Our outcome is a binary indicator for whether a respondent selected an available profile, coded as "1", and all other profiles in the task are coded as "0". If a respondent abstained, all profiles in the task are coded as "0." As in F-M, our baseline levels of the information and recent news attributes are "Low Information" and "No recent news". We interact all non-baseline attribute-levels with a binary indicator for each respondent's outcome measure arm so we can compare directly the AMCEs

[4]We discuss how respondents' choices in the final task correspond to their choices when they evaluated the same task with the opposite outcome question in the Supplementary Materials. Among respondents in the forced-choice arm that completed the entire survey, approximately one third abstained when allowed to do so in the final task (27.5% F-M, 33.9% Mummolo).

yielded when respondents are forced to choose a candidate versus when they are allowed to abstain.

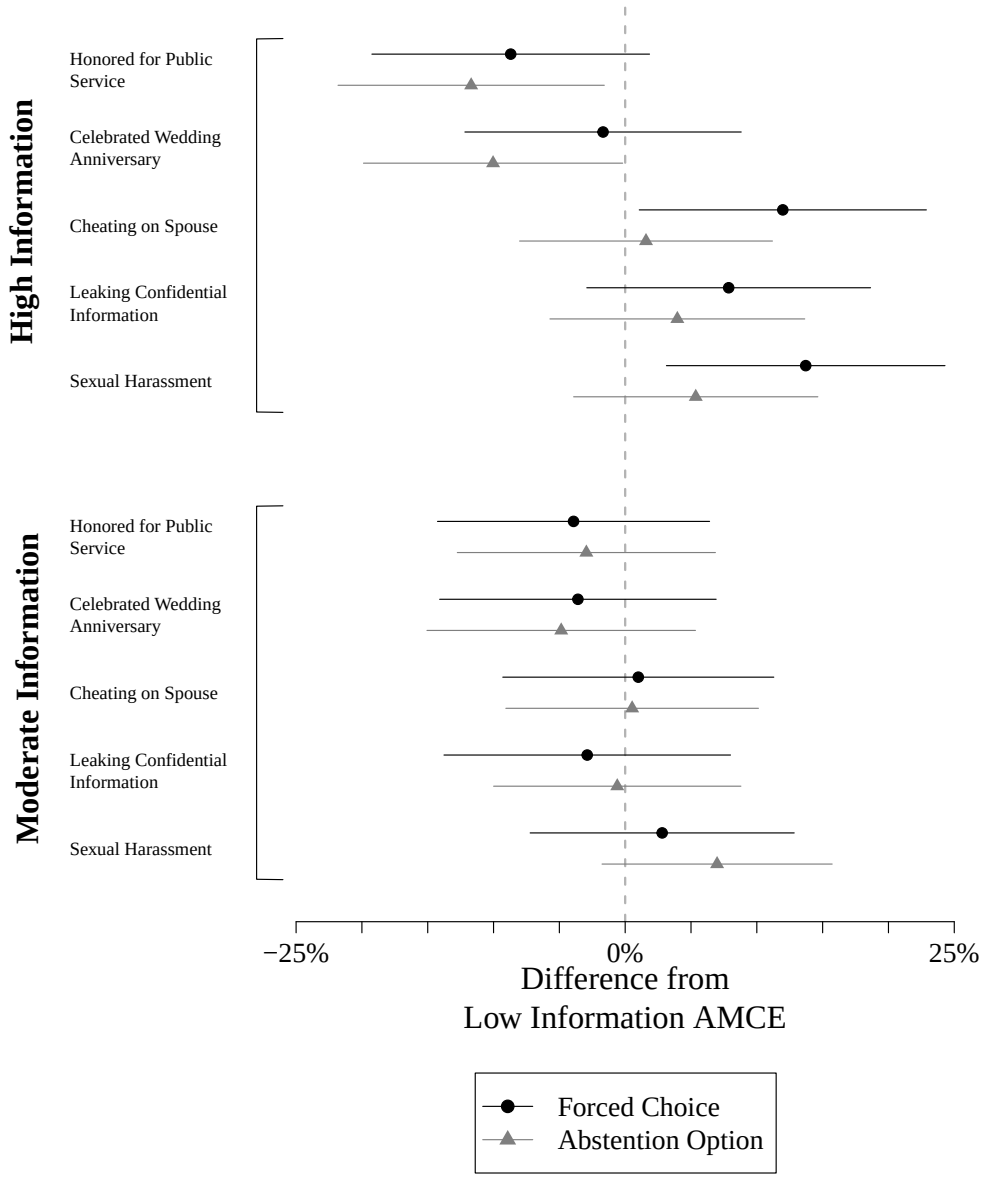
3.2 Results: Forced-Choice Design Can Provide Different AMCEs

Figure 1 displays the AMCEs for the forced-choice (black circles) and abstention option (grey triangles) arms. Specifically, since the reference level is the “No recent news" attribute in the “Low Information" condition, the points and lines in Figure 1 compare the AMCEs for the same level of the “Recent news" attribute among tasks completed in the “Moderate" or “High Information" conditions.

First, we see that all of the differences corresponding with the forced-choice and abstention-option arms lead to substantively similar conclusions when comparing the “Low" and “Moderate Information" conditions. In other words, we find no difference in how much respondents prefer any attribute between the two lowest information categories. However, distinctions emerge when comparing the “Low" and “High Information" conditions. For two of the three negative events (“Cheating on spouse" and “Sexual harassment"), our forced-choice arm replicates F-M’s finding: respondents penalize candidates less for transgressions in the “High Information" condition than in the “Low Information" condition (i.e. respondents are more likely to vote for corrupt candidates in the “High" compared to the “Low Information" condition). However, these differences do not manifest in our abstention option arm.

Further, whereas the differences in the AMCEs for the two positive news stories (“Honored for public service" and “Celebrated wedding anniversary") between the “Low" and “High Information" conditions in the forced-choice arm are not statistically

Figure 1: Effect of scandalous news and information complexity on vote choice.



Notes: This figure presents the differences in the average marginal component effect for each non-baseline "Recent News" attribute-level for conjoint tasks situated in a "Low Information" conjoint task relative to when the same attribute-level is situated in "Moderate" (lower half of figure) or "High Information" (upper half of figure). The black circles and lines represent differences and Bonferroni-adjusted 95% confidence intervals ($\alpha = \frac{0.05}{20} = 0.0025$) for each attribute-level among respondents in the forced-choice arm (Liu and Shiraito 2023). The grey triangles and lines represent those same quantities among respondents in the abstention option arm. See the Supplemental Materials for the full model summary.

distinguishable, these differences are distinguishable in the abstention option arm. Though these AMCEs are not relevant to F-M's "Information Hypothesis", which focuses only on how information environment complexity moderates the effect of scandals, the inconsistent results across the forced-choice and abstention choice arms further reinforce that researchers' selection of choice-based outcome measure can alter their substantive conclusions.

One explanation for the differing results with respect to F-M's "Information Hypothesis" that is in line with our theoretical example is that respondents who are forced to make a selection are reluctant to vote for scandalized candidates in low information environments, but are more willing to do so when additional information can obscure or compensate for scandals. Yet, when respondents can abstain, those who prioritize candidates' reputations do so when presented with scandalized candidates irrespective of other information. In the Supplementary Materials, we probe the profile- and task-level characteristics associated with abstention and find that respondents are more likely to abstain when both candidates or the candidate who shares their party affiliation are implicated in scandals. Further, we find that as the information environment becomes more complex, abstention becomes less common only if the respondent's copartisan is scandalized; otherwise, the probability of abstention increases as the information environment becomes more complex.

4 Conclusion

We have shown theoretically and empirically that forcing participants to reveal a preference when they would otherwise prefer to abstain may lead to different AMCEs than

would be recovered with more realistic outcome measures. In some contexts, such as when political elites must hire civil servants (Liu 2019) or select which lawmakers to lobby (Miller 2022), abstention is unlikely and undesirable because a choice *must* be made as a key function of the respondents' occupations. However, we advise that researchers provide an abstention option to respondents if the data generating process they wish to model naturally includes one. For example, some studies have included an abstention option to model the selection over candidates in elections (Agerberg 2020) and places to relocate (Ghosn et al. 2021).

If researchers wish to use forced-choice designs, we encourage them to be explicit about the contexts to which their findings might apply. The AMCEs recovered with a forced-choice outcome reflect the revealed preferences of respondents *when a choice is required*, which may not precisely mirror the real-world environments to which researchers wish to generalize. Alternatively, if researchers are concerned with modeling the entire decision-making process, it may be better to modify the research design to make use of an appropriate estimating procedure that accounts for task attributes that impact respondents' selection as well as preferences once a selection has been made.

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Supplementary Materials: Preferential Abstention in Conjoint Experiments

Contents

In the first portion of the Supplemental Materials (Section AI) we report the popularity of forced-choice outcomes and the contexts for which they have been used in published conjoint experiments. Second, we build on our theoretical explanation of how abstention impacts the estimates of the AMCEs. We then discuss our replication of Funck and McCabe (2021) in Section AIII, and Mummolo (2016) in Section AIV. Specifically, we provide background information about the original survey experiments, and we present the full estimates from the models discussed in the manuscript.

AI Popularity of Forced-Choice Outcomes in Conjoints

We surveyed the "top-3" journals in Political Science (American Journal of Political Science, American Political Science Review, and Journal of Politics) to capture all

published research (articles and "letters"/"notes") that use a conjoint design in an experiment as of June 2022. We conducted a Google Scholar search with the following parameters: (a) Publication name–[the name of the journal in quotation marks], and (b) Keywords – "experiment" AND "discrete choice" OR "choice based" OR "stated preference" OR "conjoint". After saving the resulting list of articles, we retrieved the PDF file of each article to assess whether the study utilized original conjoint experiments.[1] We determined that 41 of the articles identified by our search terms utilized original conjoint experiments.[2]

We then examined each article to determine the 1) types of objects that respondents were asked to evaluate, and 2) type of outcome measure that the authors utilized to evaluate the results of their conjoint experiments. These pieces of information were often clearly identified at the beginning of a paper's methods section and often included a picture of a sample conjoint task. We categorized the types of objects that respondents evaluated into the following groups:

- Candidates for elected office (type of office)
- Policies (type of policy)
- Immigration/asylum applicants
- Other

[1] One of the authors and an undergraduate research assistant independently coded each article for whether it contained an original conjoint experiment, the types of objects respondents were asked to evaluate, and the type of outcome measure the authors included. In cases where the author and research assistant disagreed on any of these three points, the other author evaluated and resolved the conflict.

[2] We excluded from our count all publications whose conjoint experiments utilized only one profile per task. We do so because these experiments cannot, by design, incorporate a forced-choice outcome.

The two most common outcome measures are "forced-choice", where respondents are required to choose one of the profiles presented, or "rating", where respondents are asked to rate each profile presented on an ordinal scale (i.e., very likely to vote for, somewhat likely, somewhat unlikely, very unlikely). A small number of studies also utilized both forced-choice and rating outcomes. Thus, studies' outcome measures were categorized into the following groups:

- Forced-choice
- Rating
- Forced-choice and rating

The results of our data collection are shown in Table A.1. Of the 41 articles, 88% (36/41) utilized a forced-choice outcome, either exclusively (56%, or 23/41) or in conjunction with a rating outcome (31%, or 13/41). The remaining studies used only a rating outcome (5%, or 2/41), or a choice-based outcome that allowed respondents to choose an option other than one of the two profiles in the task and/or abstain (7%, or 3/41). The usage of forced-choice designs appears to be driven by studies focusing on candidates for elected office (18 of the 36 studies utilizing forced-choice outcomes). Therefore, the theoretical and empirical issues we outline may be particularly problematic for a large portion of published work using conjoint experiments.

Table A.1: Conjoint experiments published in AJPS, APSR, and JOP.

	Citation	Choice Object	Outcome Measure(s)
1	Auerbach and Thachil 2018	Other (slum leaders in India)	Forced-choice
2	Auerbach and Thachil 2020	Other (slum residents seeking help)	Forced-choice
3	Arias and Blair 2022	Immigration/asylum applicants	Forced-choice & rating
4	Bakker, Schumacher and Rooduijn 2021	Candidates for US House of Representatives	Forced-choice & rating
5	Ballard-Rosa, Martin and Scheve 2017	Policies (income tax)	Forced-choice
6	Bansak, Bechtel and Margalit 2021	Policies (austerity packages)	Forced-choice & rating
7	Barnett, A. Jamal and Monroe 2021	Other (job opportunities)	Forced-choice & rating

	Citation	Choice Object	Outcome Measure(s)
8	Blumenau and Lauderdale 2022	Persuasive arguments	Choice-based (allows respondents to say both options are equally persuasive)
9	Campbell et al. 2019	Candidates for British Parliament	Forced-choice
10	Carnes and Lupu 2016	Candidates for local office in Argentina, UK, and US	Forced-choice
11	Costa 2021	Candidates for US House of Representatives	Forced-choice
12	Dill and Schubiger 2021	Policies (use of military force)	Forced-choice & rating
13	Doherty, Dowling and Miller 2019	Candidates for US state legislatures	Forced-choice
14	Eggers, Vivyan and Wagner 2018	Candidates for British Parliament	Choice-based (allows for abstention or voting for a party not displayed)

	Citation	Choice Object	Outcome Measure(s)
15	Eshima and Smith 2022	Candidates for Japanese House of Councilors	Forced-choice
16	Frederiksen 2022	Candidates for president/prime minister in five democracies	Rating
17	Ghosn et al. 2021	Other (places to return to in Syria)	Choice-based with abstention option
18	Hainmueller and Hopkins 2015	Immigration applicants	Forced-choice & rating
19	Hankinson 2018	Other (urban development projects)	Forced-choice
20	Hanretty, Lauderdale and Vivyan 2020	Candidates for British Parliament	Forced-choice
21	Henderson et al. 2022	Candidates in US House of Representatives primaries	Forced-choice & rating
22	Johns and Kölln 2020	Candidates for elected office (parties)	Forced-choice & rating

	Citation	Choice Object	Outcome Measure(s)
23	Kennedy, Waggoner and Ward 2022	Policies (algorithms for forecasting criminal recidivism)	Forced-choice & rating
24	Levy 2022	Candidates for governor in Colombia	Forced-choice & rating
25	Magni 2022	Other (welfare recipients)	Forced-choice
26	Magni and Reynolds 2021	Candidates for national legislature in UK, US, and NZ	Forced-choice
27	Mummolo 2016	Other (news stories)	Forced-choice
28	Mummolo and Nall 2017	Other (communities to live in)	Forced-choice
29	Nelson and Witko 2021	Other (job offers)	Forced-choice & rating
30	Ono and Zilis 2022	Other (judges)	Forced-choice
31	Peterson 2017	Candidates for US House of Representatives	Forced-choice

	Citation	Choice Object	Outcome Measure(s)
32	Peterson and Simonovits 2018	Candidates for US House of Representatives	Forced-choice
33	Poertner 2021	Candidates for Bolivian Plurinational Legislative Assembly	Forced-choice
34	Rodon and Sanjaume-Calvet 2020	Policies (redistribution)	Rating
35	Schneider 2020	Candidates for EU Parliament	Forced-choice & rating
36	Spater 2022	Candidates for local office in India & Other (neighbors)	Forced-choice
37	Teele, Kalla and Rosenbluth 2018	Primary candidates for local, state, and national offices in the US	Forced-choice
38	Tellez 2021	Policies (peace agreements)	Forced-choice

	Citation	Choice Object	Outcome Measure(s)
39	Tsai, Trinh and Liu 2022	Candidates for township party secretaries	Forced-choice
40	Ward 2019	Other (immigrants to live in respondents' neighborhood)	Forced-choice & rating
41	Weaver 2021	Candidates for mayor in Peru	Forced-choice

Notes: This table presents information about the choice objects and outcome measures utilized in all conjoint experiments published in the *American Political Science Review*, *American Journal of Political Science*, and *Journal of Politics* as of June 2022. For more information about each of the experiments included in this table, please refer to the original articles.

AII Theoretic Explanation of Abstention Options and AMCEs

Following from our manuscript, we consider the implications of including an abstention option in a forced-choice design for conjoint experiments. We focus on the real-world context in which voters might not participate in an election if the only candidates they could select from have undesirable characteristics. We elaborate on our theoretical illustration more extensively in this section, specifically the precise implications to the AMCEs for both 5- and N-voter elections, as well as profiles with $L > 2$ attributes.

AII.2 5-Voter Example

In our first scenario of selective abstention, we assume that voters uniformly turn out to vote and make a selection only if at least one of the candidates in the choice set possesses the characteristic they prioritize—copartisanship for voters V1, V3, and V5, and corruption for voters V2 and V4. The five voters' aggregate preferences under uniform abstention are displayed in Table A.2.

Note that in the final two comparisons (rows 5 and 6 of Table A.2a), although all voters prefer the first candidate to the second candidate, those candidates receive fewer votes because some voters would abstain if presented with those comparisons. When we then calculate the AMCEs for the two attributes in Table A.2b, we obtain different quantities than we would with no abstentions. In fact, our AMCEs moved in the same direction. The AMCE for partisanship has increased from 0.40 to 0.5, while the AMCE for corruption has decreased by 0.025 from 0.35 to 0.375. Even though the underlying preferences of the voters remained constant, the estimated AMCEs obtained in the more

Table A.2: Scenario with uniform abstention.

(a) Individual and aggregate preferences

Comparison	V1	V2	V3	V4	V5	Tally
C, H vs. C, D	C, H	C, H	C, H	C, H	C, H	5,0
C, H vs. N, H	C, H	C, H	C, H	C, H	C, H	5,0
C, H vs. N, D	C, H	C, H	C, H	C, H	C, H	5,0
C, D vs. N, H	C, D	N, H	C, D	N, H	C, D	3,2
C, D vs. N, D	C, D	-	C, D	-	C, D	3,0
N, H vs. N, D	-	N, H	-	N, H	-	2,0

(b) AMCEs for partisanship and corruption

Panel A: Partisanship

$\bar{Y}(C,H;C,H)$ - $\bar{Y}(N,H;C,H)$	2.5
$\bar{Y}(C,H;C,D)$ - $\bar{Y}(N,H;C,D)$	3
$\bar{Y}(C,H;N,H)$ - $\bar{Y}(N,H;N,H)$	4
$\bar{Y}(C,H;N,D)$ - $\bar{Y}(N,H;N,D)$	3
$\bar{Y}(C,D;C,H)$ - $\bar{Y}(N,D;C,H)$	0
$\bar{Y}(C,D;C,D)$ - $\bar{Y}(N,D;C,D)$	1.5
$\bar{Y}(C,D;N,H)$ - $\bar{Y}(N,D;N,H)$	3
$\bar{Y}(C,D;N,D)$ - $\bar{Y}(N,D;N,D)$	3
	AMCE $= \frac{20}{40} = 0.5$

Panel B: Reputation

$\bar{Y}(C,H;C,H)$ - $\bar{Y}(C,D;C,H)$	2.5
$\bar{Y}(C,H;C,D)$ - $\bar{Y}(C,D;C,D)$	3.5
$\bar{Y}(C,H;N,H)$ - $\bar{Y}(C,D;N,H)$	2
$\bar{Y}(C,H;N,D)$ - $\bar{Y}(C,D;N,D)$	2
$\bar{Y}(N,H;C,H)$ - $\bar{Y}(N,D;C,H)$	0
$\bar{Y}(N,H;C,D)$ - $\bar{Y}(N,D;C,D)$	2
$\bar{Y}(N,H;N,H)$ - $\bar{Y}(N,D;N,H)$	1
$\bar{Y}(N,H;N,D)$ - $\bar{Y}(N,D;N,D)$	2
	AMCE $= \frac{15}{40} = 0.375$

Notes: C=Copartisan, N=Non-copartisan, H=Honest, D=Dishonest.

externally valid setting are different.

Second, consider what would happen if *only* one of the two types of voters chose not to participate when neither candidate had the level of the attribute the voter weighed more heavily in their decision-making. First, we can imagine the preference orderings if those emphasizing partisanship abstained (Table A.3a). This time only the final comparison is affected by voters' abstentions relative to when all voters choose a candidate. Again, when we calculate the AMCEs for the two attributes in Table A.3b we see that our AMCEs are different; by abstaining, the strong copartisans have made the effect of copartisanship seem stronger and the effect of corruption seem weaker.

Next, we imagine what would happen if those emphasizing corruption abstained in Table A.4a. In this instance, only the second to last comparison is affected by voters' abstentions relative to when all voters choose a candidate. Now, when we calculate the AMCEs for the two attributes in Table A.4b, we see that when voters prefer honest candidates and abstain when only dirty candidates are on the ballot, the AMCE for copartisanship decreases to 0.35, but the AMCE for corruption increases to 0.45.

The first takeaway is that allowing for abstentions can increase or decrease the AMCE estimates. Second, if abstention rates are even across voter types and the number of voter types is not equal, the AMCE for the attribute more important to the larger group gets larger and the attribute more important to the smaller group gets smaller. Third, when only one type of voter abstains, the AMCE for the attribute more important for that group gets larger, while the attribute more important to the other group gets smaller (the abstaining group is no longer contributing "1"s to choices it would never choose, thus increasing the difference in marginal means for that attribute). To show how the rate

Table A.3: Scenario with strong partisan abstention.

(a) Aggregate preferences over candidate profiles.

Comparison	V1	V2	V3	V4	V5	Tally
C, H vs. C, D	C, H	C, H	C, H	C, H	C, H	5,0
C, H vs. N, H	C, H	C, H	C, H	C, H	C, H	5,0
C, H vs. N, D	C, H	C, H	C, H	C, H	C, H	5,0
C, D vs. N, H	C, D	N, H	C, D	N, H	C, D	3,2
C, D vs. N, D	C, D	C, D	C, D	C, D	C, D	5,0
N, H vs. N, D	-	N, H	-	N, H	-	2,0

(b) AMCEs for partisanship and corruption.

Panel A: Partisanship

$\bar{Y}(C,H;HP,H)$ - $\bar{Y}(N,H,HP,H)$	2.5
$\bar{Y}(C,H;HP,D)$ - $\bar{Y}(N,H,HP,D)$	3
$\bar{Y}(C,H;N,H)$ - $\bar{Y}(N,H,N,H)$	4
$\bar{Y}(C,H;N,D)$ - $\bar{Y}(N,H,N,D)$	3
$\bar{Y}(C,D;HP,H)$ - $\bar{Y}(N,D,HP,H)$	0
$\bar{Y}(C,D;HP,D)$ - $\bar{Y}(N,D,HP,D)$	2.5
$\bar{Y}(C,D;N,H)$ - $\bar{Y}(N,D,N,H)$	3
$\bar{Y}(C,D;N,D)$ - $\bar{Y}(N,D,N,D)$	4
	AMCE $= \frac{22}{40} = 0.55$

Panel B: Reputation

$\bar{Y}(C,H;HP,H)$ - $\bar{Y}(C,D;HP,H)$	2.5
$\bar{Y}(C,H;HP,D)$ - $\bar{Y}(C,D;HP,D)$	2.5
$\bar{Y}(C,H;N,H)$ - $\bar{Y}(C,D;N,H)$	2
$\bar{Y}(C,H;N,D)$ - $\bar{Y}(C,D;N,D)$	0
$\bar{Y}(N,H,HP,H)$ - $\bar{Y}(N,D,HP,H)$	0
$\bar{Y}(N,H,HP,D)$ - $\bar{Y}(N,D,HP,D)$	2
$\bar{Y}(N,H,N,H)$ - $\bar{Y}(N,D,N,H)$	1
$\bar{Y}(N,H,N,D)$ - $\bar{Y}(N,D,N,D)$	1
	AMCE $= \frac{11}{40} = 0.275$

Notes: C=Copartisan, N=Non-copartisan, H=Honest, D=Dirty.

Table A.4: Scenario with strong reputation abstention.

(a) Aggregate preferences over candidate profiles.

Comparison	V1	V2	V3	V4	V5	Tally
C, H vs. C, D	C, H	C, H	C, H	C, H	C, H	5,0
C, H vs. N, H	C, H	C, H	C, H	C, H	C, H	5,0
C, H vs. N, D	C, H	C, H	C, H	C, H	C, H	5,0
C, D vs. N, H	C, D	N, H	C, D	N, H	C, D	3,2
C, D vs. N, D	C, D	-	C, D	-	C, D	3,0
N, H vs. N, D	N, H	N, H	N, H	N, H	N, H	5,0

(b) AMCEs for partisanship and corruption.

Panel A: Partisanship

$\bar{Y}(C,H;HP,H)$ - $\bar{Y}(N,H,HP,H)$	2.5
$\bar{Y}(C,H;HP,D)$ - $\bar{Y}(N,H,HP,D)$	3
$\bar{Y}(C,H;N,H)$ - $\bar{Y}(N,H,N,H)$	2.5
$\bar{Y}(C,H;N,D)$ - $\bar{Y}(N,H,N,D)$	0
$\bar{Y}(C,D;HP,H)$ - $\bar{Y}(N,D,HP,H)$	0
$\bar{Y}(C,D;HP,D)$ - $\bar{Y}(N,D,HP,D)$	1.5
$\bar{Y}(C,D;N,H)$ - $\bar{Y}(N,D,N,H)$	3
$\bar{Y}(C,D;N,D)$ - $\bar{Y}(N,D,N,D)$	1.5
	AMCE $= \frac{14}{40} = 0.35$

Panel B: Reputation

$\bar{Y}(C,H;HP,H)$ - $\bar{Y}(C,D;HP,H)$	2.5
$\bar{Y}(C,H;HP,D)$ - $\bar{Y}(C,D;HP,D)$	3.5
$\bar{Y}(C,H;N,H)$ - $\bar{Y}(C,D;N,H)$	2
$\bar{Y}(C,H;N,D)$ - $\bar{Y}(C,D;N,D)$	2
$\bar{Y}(N,H,HP,H)$ - $\bar{Y}(N,D,HP,H)$	0
$\bar{Y}(N,H,HP,D)$ - $\bar{Y}(N,D,HP,D)$	2
$\bar{Y}(N,H,N,H)$ - $\bar{Y}(N,D,N,H)$	2.5
$\bar{Y}(N,H,N,D)$ - $\bar{Y}(N,D,N,D)$	3.5
	AMCE $= \frac{18}{40} = 0.45$

Notes: C=Copartisan, N=Noncopartisan, H=Honest, D=Dirty.

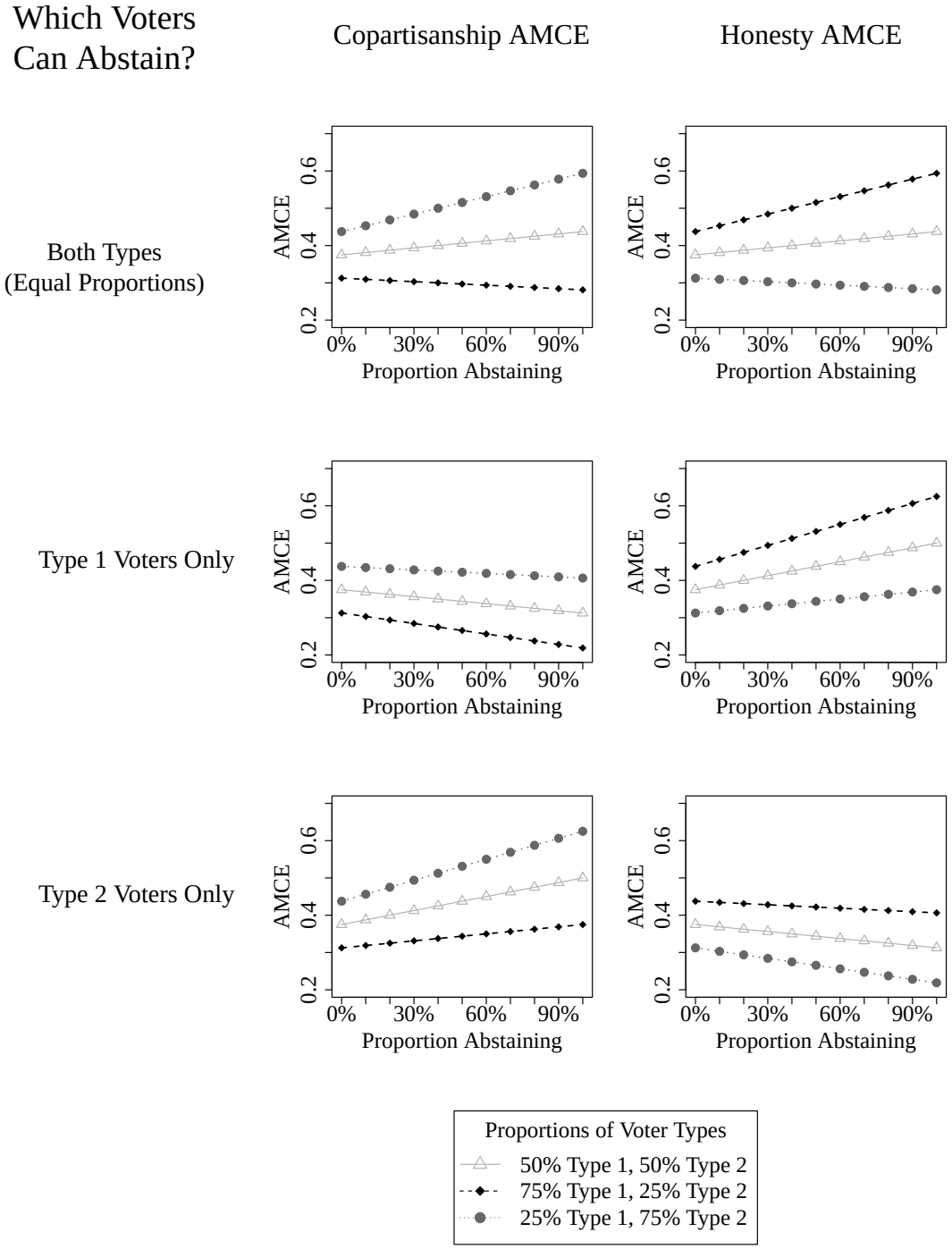
of abstention among voters with different preference orderings impacts the AMCEs, we extend our example to N-voters.

AII.3 Extension to N-Voters

In this extension, we imagine that voters are one of two types; the preference orderings of Type 1 voters mirror those of voters 2 and 4 in our 5-voter example (they prioritize reputation over partisanship), and the preference orderings of Type 2 voters match those of voters 1, 3, and 5 (emphasize partisanship over reputation). The rows of Figure A.1 indicate which types of voters are allowed to abstain if no candidate with their prioritized attribute is present, and the columns differentiate the AMCEs for partisanship (first column) and reputation (second column) under different rates of abstention identified by the x-axis of each pane. We also vary the proportions of Type 1 and Type 2 voters in the electorate, such that there can be an even number of Type 1 and Type 2 voters (light grey triangles), Type 1 voters outnumber Type 2 voters by a 3-to-1 margin (black diamonds), or Type 2 voters outnumber Type 1 voters by the same 3-to-1 margin (dark grey circles).

When abstention is symmetric, as in the top row of Figure A.1, we observe that the AMCEs for copartisanship and honesty gradually increase as the proportion of each voter type abstains if (1) no candidates with their prioritized characteristic is present and (2) the electorate includes an equal proportion of Type 1 and Type 2 voters. However, if the composition of the electorate is lopsided, such that 75% of voters are Type 1 or Type 2, then the AMCE for the attribute-level prioritized by the more common voter type increases while the AMCE for the attribute-level prioritized by the less common voter

Figure A.1: AMCEs varying voter-type and the proportion of the sample that abstains.



Notes: The AMCEs that would be obtained using a forced-choice design are those presented in each pane where the proportion of voters abstaining is 0%.

type decreases. Thus, when abstention rates are symmetric across voter types, AMCEs can be larger or smaller than those obtained using a forced-choice design depending on the prevalence of preference orderings in the sample, and those differences increase in magnitude as abstention rates increase.

When abstention is asymmetric, such as the middle row where only Type 1 voters can abstain, we observe slightly different implications of abstention on the AMCEs. The AMCE for honesty, which is the attribute-level Type 1 voters prioritize, increases as abstention rates increase, even if Type 1 voters make up only a small proportion of the population. Type 1 voters withhold votes for dishonest candidates if no honest candidates are present, thus not diluting their preference for honest candidates. Differently, Type 2 voters continue to express their preference for honest non-copartisans as compared to dishonest non-copartisans even if none of the candidates are copartisans.

Conversely, the AMCE for copartisanship decreases as abstention rates increase, even when Type 2 voters make up 75% of the electorate. While Type 1 voters do not express their preference for copartisans if no candidates are honest, Type 2 voters diminish their preference for copartisans by voting for noncopartisans when no copartisans are present. Thus, when abstention rates are asymmetric across voter types, AMCEs associated with the attribute-levels preferred by voters who abstain at higher rates are higher than those obtained through a forced-choice design. Yet, AMCEs associated with attribute-levels preferred by voters who abstain at lower rates (or not at all) are lower than those obtained through a forced-choice design. These differences, again, increase in magnitude as abstention rates increase.

Our theoretical exercise demonstrates that the estimated AMCEs can differ depending

on whether the choice-based outcome measure employed forces respondents to select a profile in each task or allows respondents to abstain. This is problematic because many contexts to which conjoint experiments are applied, such as voting for candidates for elected office, naturally allow for abstentions. As such, AMCEs based on forced-choice outcomes may often not reflect a sample's, nor a population's, aggregate preferences as expressed through real-world behavior.

AII.4 Extension to Additional Attributes

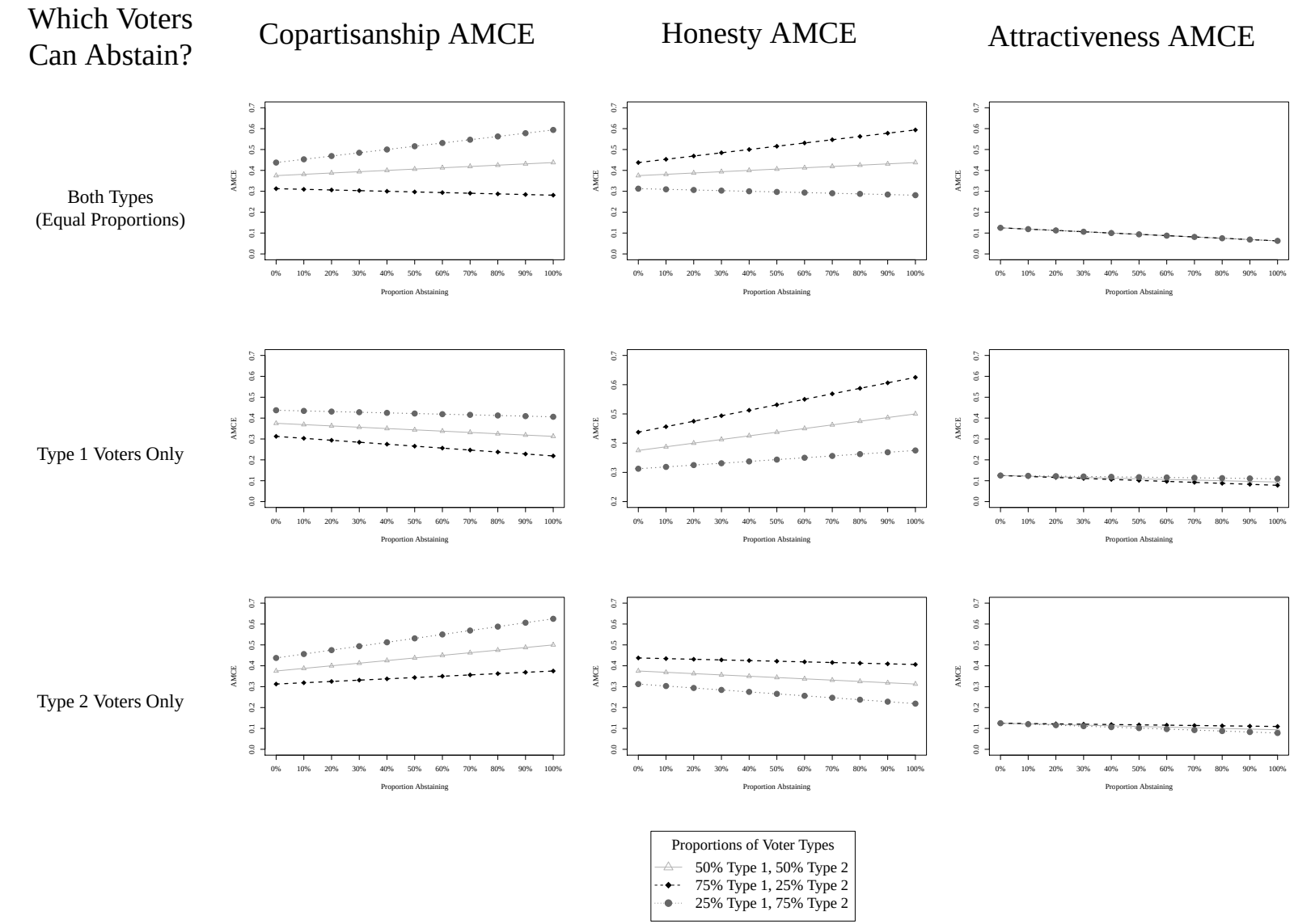
To this point, we have only considered cases where voters have preferences over only two attributes—candidates' partisanship and reputation. However, because most choice settings incorporate more than two attributes, it is important to consider how abstention prompted by specific levels of any one attribute might affect the AMCEs associated with levels of other attributes relevant to people's decision-making processes that do not prompt abstention. As such, we add a third attribute to our N-voter exercise—candidate attractiveness—and assessing how abstention prompted by candidate dishonesty and non-copartisanship influences AMCEs associated with levels of that attribute.

Our attractiveness attribute can take on one of two levels—attractive or unattractive. In our example here, all voter types prefer attractive candidates to unattractive candidates and consider attractiveness the third-most important candidate attribute.[3] Thus, voters essentially use attractiveness to break ties between candidates who have identical levels for partisanship and honesty.

Figure A.2 provides the same information as Figure A.1, the AMCEs given the type

[3]For instance, a Type 1 voter has the following preference ordering over all eight possible candidates: C, H, A; C, H, U; N, H, A; N, H, U; C, D, A; C, D, U; N, D, A; N, D, U.

Figure A.2: AMCEs varying voter-type and the proportion of the sample that abstains with three attributes.



Notes: The AMCEs that would be obtained using a forced-choice design are those presented in each pane where the proportion of voters abstaining is 0%.

of voters allowed to abstain and the composition of the electorate by voter type, with the addition of the AMCE for attractiveness relative to a baseline of unattractive (right-most column). In all cases, allowing voters to abstain on the basis of candidates' partisanship and reputation attenuates the AMCE of attractiveness, with the magnitude of that attenuation increasing as the proportion of voters abstaining increases. This is because voters do not express their preference for attractive versus unattractive candidates if both candidates in a given match-up possess a level of an attribute that prompts them to abstain, thus diminishing the magnitude of the ultimate preference for attractive versus unattractive candidates. Thus, even the AMCEs of attributes not associated with abstention decisions vary in size depending on whether participants are forced to choose from the available profiles or allowed to abstain.

AIII Replication and Extension of Funck and McCabe (2021)

In their original article, F-M provide 2,135 American respondents recruited through Lucid with two hypothetical candidates competing for U.S. Congress in an upcoming general election.[4] In each task, one candidate was randomly assigned to be a Democrat and the other was assigned to be a Republican. After viewing the candidate profiles, respondents provide their vote choice for one candidate only. Participants completed three comparison tasks for a total of six candidate profiles per respondent, and an overall number of 12,810 candidate profiles.

[4]Each recent news attribute-level (as well as those for candidates' policy stances, age, and religion) had an equal probability of appearing in every candidate profile and randomized across tasks as well as respondents, but the probability of appearance for some attribute-levels such as those concerning race, profession, and gender were adjusted to reflect the probability distribution of candidates in the real-world.

To replicate F-M, we recruited a sample of 2,254 American respondents using Amazon's Mechanical Turk (MTurk). The design of our replication (question wording, probability distribution of attribute-levels, etc.) mirrored exactly that of F-M with the following three exceptions:

1. As we describe in the main text, respondents were randomly assigned to one of two experimental arms—a *forced-choice arm*, where they were required to indicate which candidate they preferred in each task, or a *abstention option arm*, where they could indicate their preferred candidate or abstain from making a selection.

2. Respondents completed six comparison tasks in their assigned experimental arm for a total of twelve candidate profiles per respondent, and an overall number of 26,320.[5]

3. After completing their six comparison tasks, we asked respondents to complete a final, seventh task in which they were provided with the outcome measure from the opposite treatment arm. Our analyses in the main paper utilize only responses provided in the first six comparison tasks completed by each respondent, and we examine respondents' choices in this seventh task separately in this section.

To assess how the AMCEs differ across the forced-choice and abstention arms, we use ordinary least squares regression to estimate our binary indicator of whether

[5]We asked respondents to complete six comparison tasks, unlike the three F-M asked respondents to complete, for power considerations; because we wanted to compare AMCEs across experimental arms, doubling the number of tasks completed by a similar number of respondents yields a sample in each treatment arm similar in size to the full sample used by F-M.

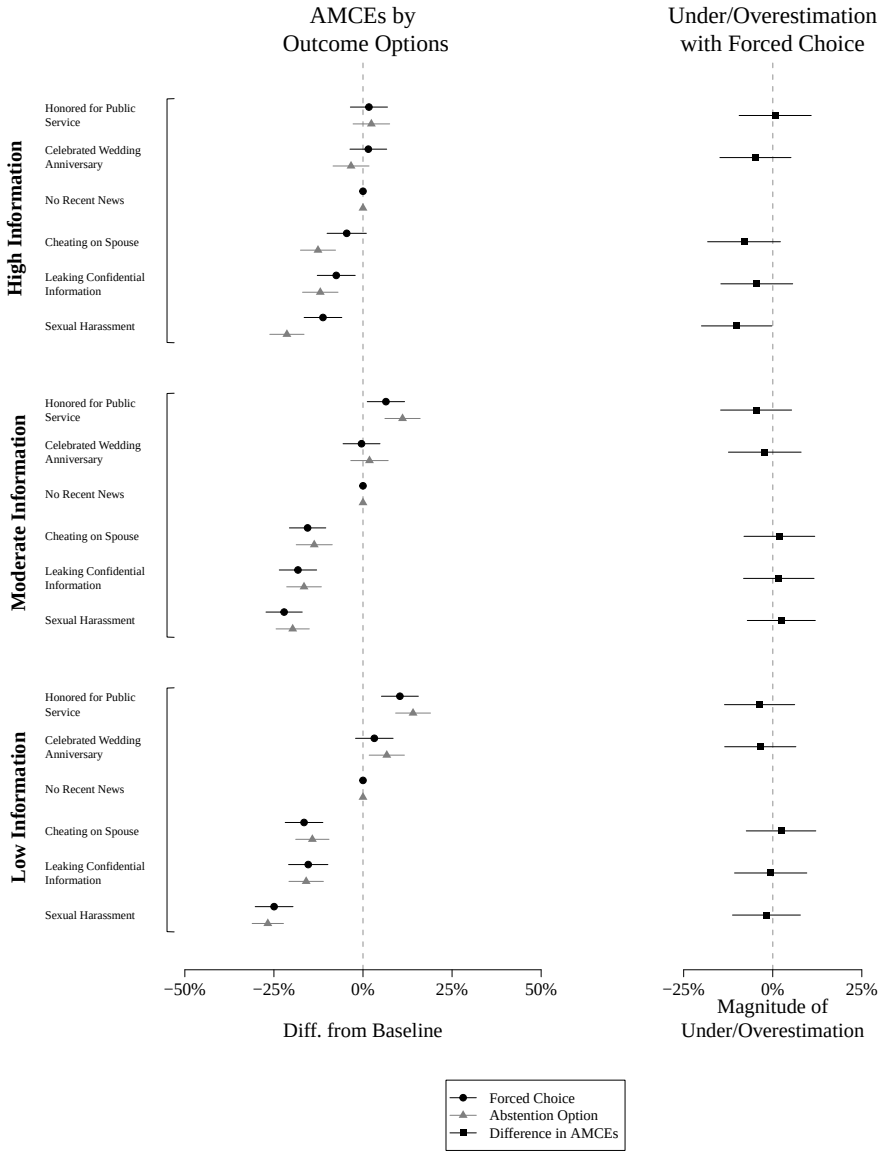
a respondent selected a profile in a given task[6] as a function of a triple interaction consisting of the following terms:

- A binary indicator for whether the respondent was in the forced-choice (0) or abstention (1) arm
- A series of binary indicators for each attribute-level of information environment complexity (excluding the reference category, Low)
- A series of binary indicators for each attribute-level of recent news (excluding the reference category, No recent news)

Table A.5 reports the estimated coefficients this model, and Figure A.3 presents visual representations of the AMCEs and the pairwise differences of each AMCE among respondents who were in the forced-choice and abstention option arms. Figure 1 in the manuscript uses the regression model reported in Table A.5 to calculate the difference between the AMCEs for each non-baseline attribute-level when situated in a low information environment conjoint task as compared to when situated in a moderate or high information environment conjoint task.

[6]If a respondent selected a profile in a given task, that profile was coded as “1" and all other profiles in the task were coded as “0." If a respondent chose to abstain in a given task, all profiles in the task were coded as “0."

Figure A.3: AMCEs for the forced-choice and abstention-option experimental arms replicating the design of Funck and McCabe (2021).



Notes: The left pane presents, by quantity of information, the average marginal component effect of each characteristic (type of scandal) on the predicted likelihood that respondents select a given candidate. The AMCEs and their corresponding 95% confidence intervals are represented by the horizontal lines. The right pane presents the differences in the magnitudes of the AMCEs for each attribute-level between the forced-choice and abstention option designs (i.e. $|AMCE_{\text{Forced}} - AMCE_{\text{Abstention}}|$) and Bonferroni-adjusted 95% confidence intervals ($\alpha = \frac{0.05}{15} = 0.00\overline{3}$).

AIII.2 When Do Respondents Abstain?

To better understand why respondents in our abstention arm in some tasks, we used ordinary least squares to regress a binary indicator for whether the respondent abstained in a given task (1) or not (0) on an interaction between the following two task and profile characteristics:

- A series of binary indicators for which candidates had Recent News attribute-levels implicating them in scandals—Neither candidate (reference category), only the candidate who shared the respondent's partisan affiliation, only the candidate who did not share the respondent's partisan affiliation, or both candidates
- A series of binary indicators for whether the task was situated in a low (reference category), medium, or high information environment

Figure A.4 indicates the predicted probability that a respondent would abstain from a task given its information environment complexity and whether and which candidates are implicated in scandals through their assigned levels of the recent news attribute. The relative magnitudes of these predicted probabilities provide several insights concerning when respondents are more likely to abstain.

First, we can see that across information environments, respondents are most likely to abstain when both candidates experience scandal. This is consistent with the notion that respondents would rather abstain than endorse a candidate with negative valence.

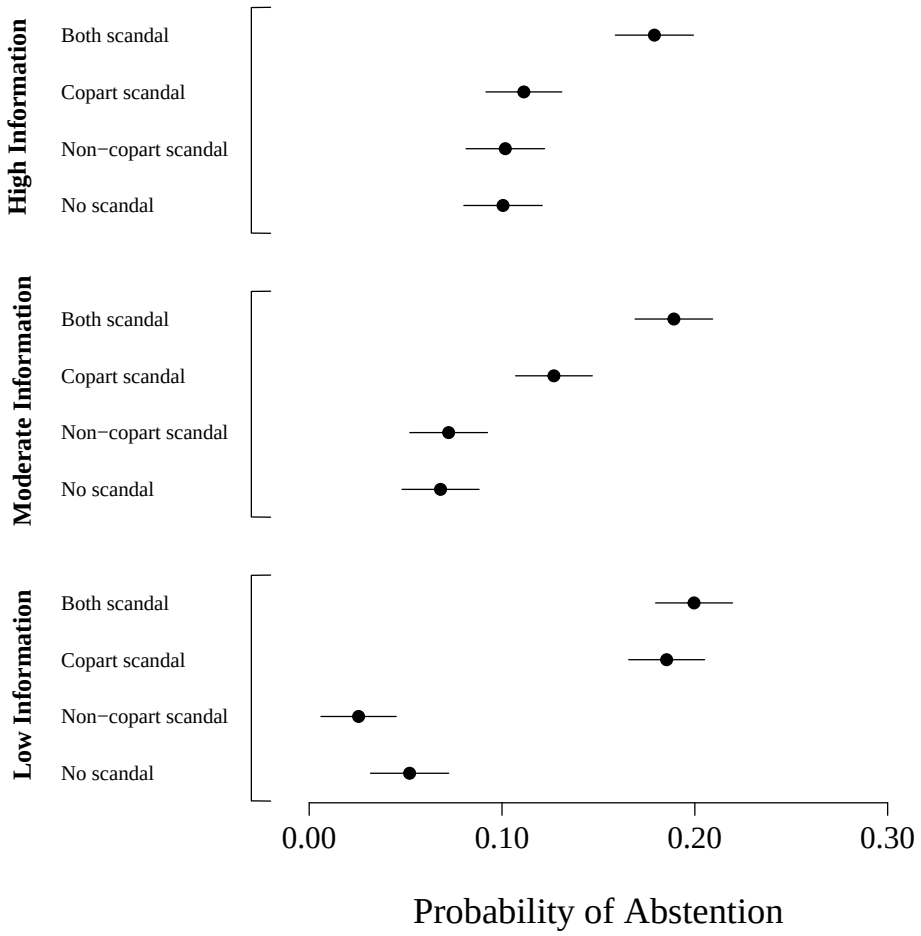
Second, respondents are more likely to abstain when only the candidate who shares their partisan affiliation experiences scandal as compared to when only the candidate who does not share their partisan affiliation or neither candidate experiences scandal,

Table A.5: Replication results testing "Information Hypothesis" of Funck and McCabe (2021).

	Forced		Abstention	
	Ref. Cat. = Low	Ref. Cat. = Medium	Ref. Cat. = Low	Ref. Cat. = Medium
Intercept	0.57*	0.58*	0.43*	0.44*
	(0.02)	(0.02)	(0.02)	(0.02)
Medium information	0.01		0.01	
	(0.02)		(0.02)	
High information	−0.04	−0.05*	0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.02)
Cheating on spouse	−0.17*	−0.16*	−0.14*	−0.14*
	(0.03)	(0.03)	(0.02)	(0.03)
Leaking confidential information	−0.15*	−0.18*	−0.16*	−0.17*
	(0.03)	(0.03)	(0.02)	(0.02)
Sexual harassment	−0.25*	−0.22*	−0.27*	−0.20*
	(0.03)	(0.03)	(0.02)	(0.02)
Wedding anniversary	0.03	−0.00	0.07*	0.02
	(0.03)	(0.03)	(0.03)	(0.03)
Honored public service	0.10*	0.06*	0.14*	0.11*
	(0.03)	(0.03)	(0.02)	(0.03)
Medium information x cheating on spouse	0.01		0.01	
	(0.04)		(0.03)	
Medium information x leaking confidential information	−0.03		−0.01	
	(0.04)		(0.03)	
Medium information x sexual harassment	0.03		0.07*	
	(0.04)		(0.03)	
Medium information x wedding anniversary	−0.04		−0.05	
	(0.04)		(0.04)	
Medium information x honored public service	−0.04		−0.03	
	(0.04)		(0.03)	
High information x cheating on spouse	0.12*	0.11*	0.02	0.01
	(0.04)	(0.04)	(0.03)	(0.04)
High information x leaking confidential information	0.08*	0.11*	0.04	0.05
	(0.04)	(0.04)	(0.03)	(0.04)
High information x sexual harassment	0.14*	0.11*	0.05	−0.02
	(0.04)	(0.04)	(0.03)	(0.03)
High information x wedding anniversary	−0.02	0.02	−0.10*	−0.05
	(0.04)	(0.04)	(0.04)	(0.04)
High information x honored public service	−0.09*	−0.05	−0.12*	−0.09*
	(0.04)	(0.04)	(0.04)	(0.04)
N	12620	8366	13700	9080

Notes: We mirror Table 1 from Funck and McCabe 2022 by estimating the same regression models using our experimental data but estimating separate models for the forced-choice and abstention arms. Standard errors are clustered by respondent, shown in parentheses, and statistical significance is indicated as $^{*}p < 0.05$.

Figure A.4: Predicted probabilities of abstention in F-M replication.



Notes: The figure plots the predicted probability that a respondent will abstain from choosing either candidate in a task given the complexity of the task's information environment and whether and which candidates have experienced scandal.

which is consistent with the notion that respondents would rather abstain than choose a candidate who does not share their partisan affiliation. However, this difference in the probabilities of abstention shrink when moving from the low to moderate and high information environments, which suggests that respondents might use additional

information about copartisan candidates to rationalize away their scandals and choose them rather than abstain.

Third, respondents are more likely to abstain as the complexity of the information environment increases. This may indicate that some respondents would rather abstain than exert the cognitive effort to absorb and consider higher levels of detail or that as more information about candidates is presented, it becomes more likely that each candidate would have at least one undesirable level of an attribute which prompts respondents to abstain.

AIII.3 Switching Experimental Arms

After completing the six tasks in their assigned experimental arm, respondents were asked to complete a seventh, final task in which the outcome measure reflected that used in the opposite experimental arm (e.g., respondents in the forced-choice arm were now given the opportunity to choose one of the candidates or to abstain). For each respondent, the attribute-levels of the two profiles and the information environment complexity for this seventh task were the same as shown in her first task, which allows us to assess how the same respondents evaluate the same set of profiles across the two different types of outcome measures.

Respondents in the abstention option arm were forced to choose one of the two candidates in this final task, even if they originally abstained; hence, the choices of abstention option arm respondents in this final task is deterministic (i.e., respondents who originally abstained must now choose one of the two profiles). However, respondents in the forced-choice arm provide a unique window on whether the same respondents would

make a different choice if allowed to abstain relative to when they are forced to choose. Of the 1,035 respondents in the forced-choice arm who completed all seven tasks (the six forced-choice tasks and the seventh abstention option task), 286 (27.6%) chose to abstain rather than choosing the candidate they originally selected when forced to do so. This descriptive evidence suggests that a fairly large proportion of respondents will choose candidates if forced to do so even though they would ultimately prefer not to make a choice.

AIV Replication and Extension of Mummulo (2016)

In Mummolo's original article, 1,059 American respondents were recruited by Survey Sampling International (SSI) that identified as Democrats or Republicans. Participants were presented with pairs of news items, and in each task, the source and headline attributes of the news items are randomly assigned. Respondents were asked to indicate which news item they would prefer to read. Participants completed twelve comparison tasks for a total of twenty-four profiles per respondent, and an overall number of 25,416 profiles.

The dimensions of the news stories that Mummolo randomly varied concern the content and the source of the news stories, specifically their "relevance" (e.g. whether a participant was a member of an "affected population") and "friendliness", which is a partisan-based comparison between respondent (Democrat or Republican) and source (MSNBC, USA Today, or Fox News). Mummolo hypothesized and showed that respondents, on average, prefer content that is relevant and friendly, though "source

reputations appear to do little to prevent consumption when topics are relevant" (2016: 771).

To replicate Mummolo, we recruited a sample of 1,553 American respondents who identified as Democrats or Republicans using Amazon's Mechanical Turk (MTurk). As in our replication of F-M, we randomly assigned respondents to a *forced-choice* or *abstention option* arm. Additionally, we again asked respondents to complete a final, thirteenth task in which they were provided with the outcome measure from the opposite treatment arm. Besides these two modifications, our replication otherwise mirrored the protocol used by Mummolo.

To assess how the AMCEs differ across the forced-choice and abstention arms, we use ordinary least squares regression with standard errors clustered by respondent. We estimate our binary indicator of whether a respondent selected a profile in a given task[7] as a function of a triple interaction consisting of the following terms:

- A binary indicator for whether the respondent was in the forced-choice (0) or abstention (1) arm
- A binary indicator for whether the news source was friendly, neutral, or unfriendly given the respondent's partisan affiliation
- A binary indicator for whether the topic of the news item is relevant to the respondent, given the respondent's membership in one of six affected publics: women, smokers, senior citizens, college students, uninsured persons or healthcare

[7]If a respondent selected a profile in a given task, that profile was coded as "1" and all other profiles in the task were coded as "0." If a respondent chose to abstain in a given task, all profiles in the task were coded as "0."

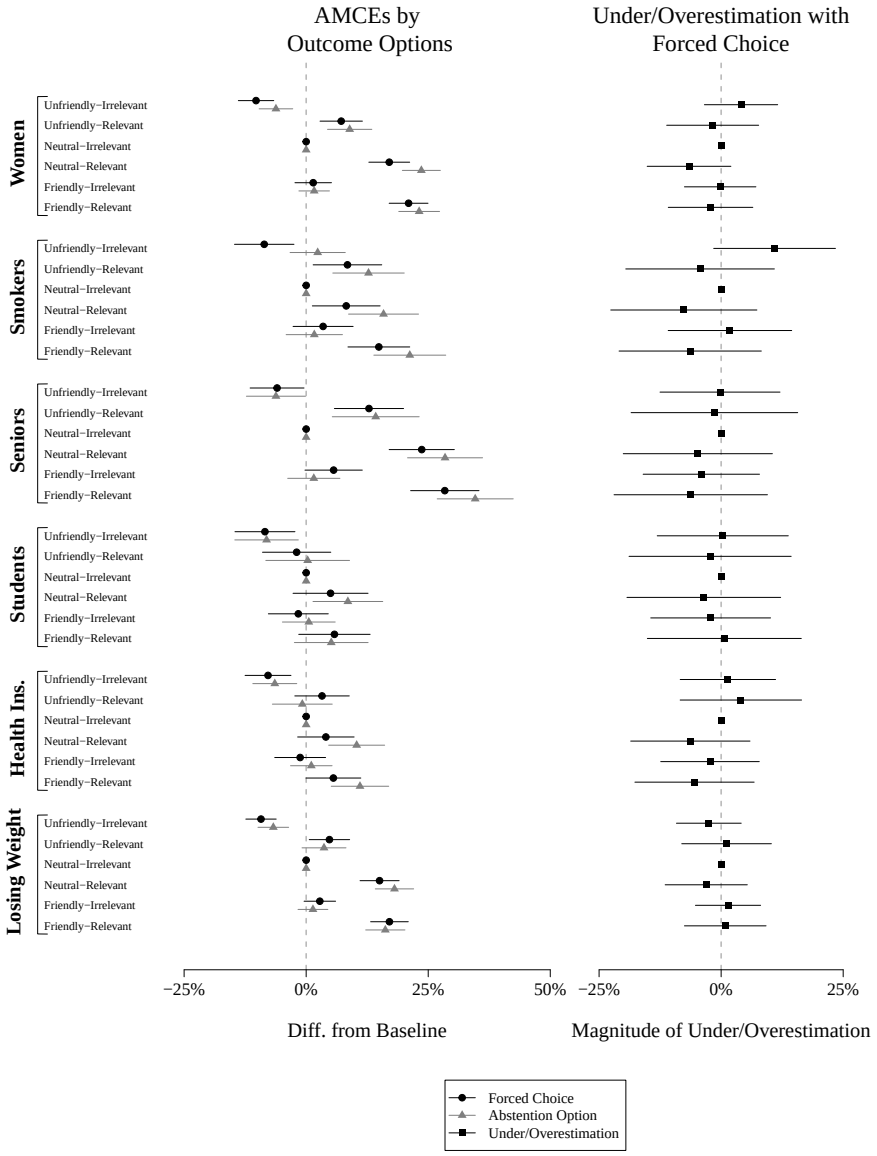
workers, and those currently trying to lose weight

Following Mummolo, we estimate separate models for respondents who are members of each affected public. AMCEs for respondents in the forced-choice and abstention option arms are shown with black circles and grey triangles, respectively, in the left pane of Figure A.5, and the differences in the magnitudes of the AMCEs across arms. Additionally, in Table A.6, we adapt Table 4 in Mummolo (2016, 770) by estimating separate models for each affected public and experimental arm.

The central finding from Mummolo is largely replicated: respondents in affected publics are more likely to select news items that are on topics relevant to them and do not come from unfriendly sources. While the AMCEs from the abstention option arm exhibit some differences in direction and magnitude compared to those in the forced-choice arm, none are statistically distinguishable at the Bonferroni-adjusted 95% confidence level.

However, some attribute-level-specific hypothesis tests yield different conclusions across treatment arms. For instance, the abstention option arm AMCEs for the Neutral-Relevant attribute-level among students and the uninsured and healthcare workers are statistically distinguishable from zero at the 95% level, but the same AMCEs from the forced-choice arm are not. In these cases, the topic relevance hypothesis presented by Mummolo 2016 enjoys more support when abstentions are allowed than when choices are forced, as in the original paper.

Figure A.5: AMCEs for the forced-choice and abstention-option experimental arms replicating the design of Mummolo (2016).



Notes: The left pane presents, by affected population, the average marginal component effect of each non-baseline attribute-level (topic relevance and source friendliness) on the probability that respondents select a given story. The AMCEs and their corresponding 95% confidence intervals are represented by the horizontal lines. The right pane presents the differences in the magnitudes of the AMCEs for each attribute-level between the forced-choice and abstention option designs (i.e. $|AMCE_{\text{Forced}} - AMCE_{\text{Abstention}}|$) and Bonferroni-adjusted 95% confidence intervals ($\alpha = \frac{0.05}{30} = 0.001\overline{6}$).

Table A.6: Adaptation of Table 4 of Mummolo 2016 by experimental arm.

	Women		Smokers		Seniors		Students		Health Ins.		Losing Weight	
	Forced	Abstention	Forced	Abstention	Forced	Abstention	Forced	Abstention	Forced	Abstention	Forced	Abstention
Intercept	0.46*	0.32*	0.47*	0.35*	0.43*	0.28*	0.51*	0.42*	0.50*	0.38*	0.48*	0.38*
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Relevant Topic	0.17*	0.24*	0.08*	0.16*	0.24*	0.28*	0.05	0.09*	0.04	0.10*	0.15*	0.18*
	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)
Friendly Source	0.01	0.02	0.03	0.02	0.06	0.02	−0.02	0.01	−0.01	0.01	0.03	0.01
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Unfriendly Source	−0.10*	−0.06*	−0.09*	0.02	−0.06*	−0.06*	−0.08*	−0.08*	−0.08*	−0.06*	−0.09*	−0.07*
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Relevant x Friendly	0.03	−0.02	0.03	0.04	−0.01	0.05	0.02	−0.04	0.03	−0.00	−0.01	−0.03
	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)
Relevant x Unfriendly	0.00	−0.08*	0.09	−0.05	−0.05	−0.08	0.01	−0.00	0.07	−0.05	−0.01	−0.08*
	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)
Num. obs.	8424	8528	2903	2796	3123	2276	2662	2586	4320	4539	10078	9647

Notes: We mirror Table 4 from Mummolo (2016: 770) by estimating the same regression models using our experimental data but estimating separate models for the forced-choice and abstention option arms. All standard errors are clustered on respondent. Statistical significance is indicated as $^{*}p < 0.05$.

AIV.2 When Do Respondents Abstain?

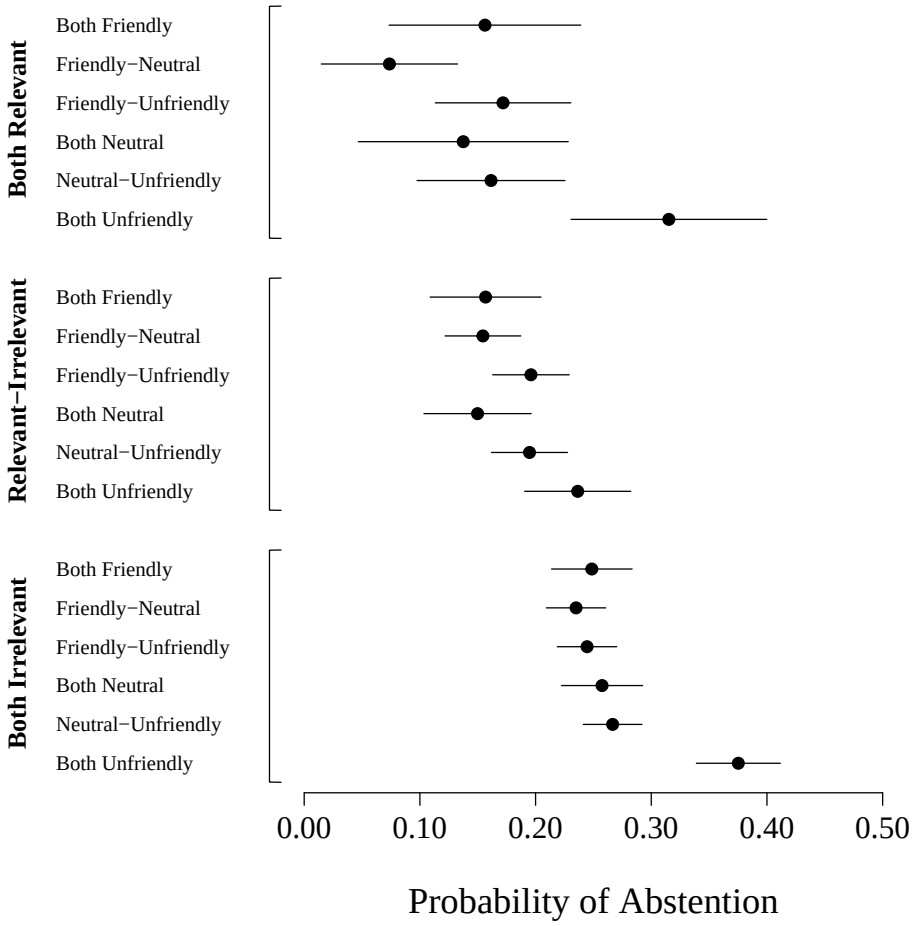
Again, to better understand why respondents in our abstention arm in some tasks, we used ordinary least squares to regress a binary indicator for whether the respondent abstained in a given task (1) or not (0) on an interaction between the following two task and profile characteristics:

- A series of binary indicators for the combination of the news items' topic relevance: whether both news items were irrelevant to the respondent (reference category); whether one news item was irrelevant and one was relevant; or whether both news items were relevant
- A series of binary indicators for the combination of the news items' source friendliness: whether both news items came from an unfriendly source (reference

category); whether one news item came from a neutral source and one news item came from an unfriendly source; whether both news items came from a neutral source; whether one news item came from a friendly source and one news item came from an unfriendly source; whether one news item came from a friendly source and one news item came from a neutral source; or whether both news items came from a friendly source

Figure A.6 indicates the predicted probability that a respondent would abstain from a task given the topic relevance and source friendliness of the presented stories. The relative magnitudes of these predicted probabilities provide several insights concerning when respondents are more likely to abstain. First, when focusing on topic relevance (the three bracketed sets), we see that respondents are most likely to abstain when both news items are irrelevant relative to when at least one of the news items is relevant. This is consistent with the notion that if presented only with news items that do not have respondents' optimal level of topic relevance, they would rather abstain than select a news item on an irrelevant topic. Second, when focusing on source friendliness (the six point estimates in each bracketed set), we see that respondents are most likely to abstain when the sources for both news items are unfriendly relative to when at least one source is neutral or friendly. This is consistent with the notion that if presented only with news items from undesirable sources, they would rather abstain than select a news item from an unfriendly source.

Figure A.6: Predicted probabilities of abstention in Mummolo replication.



Notes: The figure plots the predicted probability that a respondent will abstain from choosing either news item in a task given the topic relevance and source friendliness of the presented news items.

AIV.3 Switching Experimental Arms

After completing the twelve tasks in their assigned experimental arm, respondents were asked to complete a thirteenth, final task in which the outcome measure reflected that used in the opposite experimental arm (e.g., respondents in the forced-choice arm were now given the opportunity to choose one of the news items or to abstain). For each

respondent, we randomly selected one of the twelve tasks they already completed with their assigned outcome measure and presented them with the same profiles as in that task but with the alternative outcome measure. This allows us to assess how the same respondents evaluate the same set of profiles across the two different types of outcome measures.

Respondents in the abstention option arm were forced to choose one of the two news items in this final task, even if they originally abstained; hence, the choices of abstention option arm respondents in this final task is deterministic (i.e., respondents who originally abstained must now choose one of the two profiles). However, respondents in the forced-choice arm provide a unique window on whether the same respondents would make a different choice if allowed to abstain relative to when they are forced to choose. Of the 706 respondents in the forced-choice arm who completed all thirteen tasks (the twelve forced-choice tasks and the seventh abstention option task), 239 (33.9%) chose to abstain rather than choosing the candidate they originally selected when forced to do so. This descriptive evidence suggests that a fairly large proportion of respondents will choose news items if forced to do so even though they would ultimately prefer not to make a choice.

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