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. Theoretically, we illustrate how offering respondents an abstention option can produce average marginal component effects (AMCEs) of differing signs and magnitudes 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. Empirically, we replicate two conjoint experiments and demonstrate how omitting a realistic abstention option can lead to different AMCE estimates.

1 Introduction

Social scientists frequently use conjoint experiments to study a wide array of decisions that humans encounter. 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 outline theoretically how the average marginal component effects (AMCEs) associated with attribute-levels in a forced-choice design can differ in sign *or* magnitude relative to the AMCEs for the same attribute-levels in a design allowing for abstention. Importantly, researchers cannot know beforehand how any of the AMCEs obtained in 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-choice outcomes (Funck and McCabe 2022; Mummolo 2016) to highlight empirically how including an abstention option can yield statistically and substantively different conclusions.

[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.

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] 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 their positions on two policy issues, which serve as our *attributes*. For each issue, candidates can take one of two positions, which constitute our *levels*. First, say on abortion policy, voters can be pro-life (PL) or pro-choice (PC). Second, on tax policy, voters can support increasing (I) or decreasing (D) taxes on the upper-class. For simplicity, we assume all voters prioritize candidates' abortion policy stances to candidates' tax policy stances, and that voters' abortion and tax policy preferences are ideologically coherent. In other words, all voters preferring pro-life candidates ($PL > PC$) are conservatives and therefore also prefer candidates pledging to cut taxes ($D > I$). To start, let three of the voters be conservatives and

[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]The disconnect between preference options and preference expression also sparks concerns about the external validity of the forced-choice design. Within the validity typology proposed by Egami and Hartman (2022), these conceptual issues fall under Y-validity, or how well the outcome measures map onto the theoretical quantities of interest.

two of the voters be liberals. The full preference ordering for the four possible candidate types for the five voters is presented in Table 1a.

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 four columns of the table provide the vote tallies for each candidate comparison under different abstention scenarios: (1) when voters must vote for the candidate they prefer ("No Abstentions"); (2) when all voters abstain if neither candidate possesses their most preferred level of their prioritized attribute, abortion policy ("Uniform Abstentions"); (3)

Table 1: Individual and aggregate preferences over candidate profiles with differing policy positions.

(a) Voter preferences

Rank	V1	V2	V3	V4	V5
1	PL, D	PC, I	PL, D	PC, I	PL, D
2	PL, I	PC, D	PL, I	PC, D	PL, I
3	PC, D	PL, I	PC, D	PL, I	PC, D
4	PC, I	PL, D	PC, I	PL, D	PC, I

(b) Aggregate preferences

	Comparison	V1	V2	V3	V4	V5	No Abstentions	Uniform Abstentions	Pro-Life Abstentions	Pro-Choice Abstentions
1	PL, D vs. PC, I	PL, D	PC, I	PL, D	PC, I	PL, D	3,2	3,2	3,2	3,2
2	PL, D vs. PC, D	PL, D	PC, D	PL, D	PC, D	PL, D	3,2	3,2	3,2	3,2
3	PL, D vs. PL, I	PL, D	PL, I	PL, D	PL, I	PL, D	3,2	3,0	3,2	3,0
4	PL, I vs. PC, I	PL, I	PC, I	PL, I	PC, I	PL, I	3,2	3,2	3,2	3,2
5	PL, I vs. PC, D	PL, I	PC, D	PL, I	PC, D	PL, I	3,2	3,2	3,2	3,2
6	PC, D vs. PC, I	PC, D	PC, I	PC, D	PC, I	PC, D	3,2	0,2	0,2	3,2

Notes: PL=Pro-life, PC=Pro-choice, D=Decrease upper-class taxes, I=Increase upper-class taxes. In the four 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).

when voters 1, 3, and 5 abstain if neither candidate is pro-life, but voters 2 and 4 always vote ("Pro-life Abstentions"); and (4) when voters 2 and 4 abstain if neither candidate is pro-choice, but voters 1, 3, and 5 always vote ("Pro-choice Abstentions"). In the first scenario, the first candidate profile wins each comparison by a 3 to 2 margin, but if abstentions are allowed, the winning candidate and margin of victory differ only when both candidates share the same abortion policy position (Table 1b, Rows 3 and 6).

Finally, we use these vote tallies to calculate the AMCEs scenario in Table 2. When using a forced-choice outcome that requires all voters to cast a ballot, the AMCE for pro-life is 0.10 (Table 2a, Column 1) and the AMCE for cutting taxes is 0.05 (Table 2b, Column 1). However, when voters are allowed to abstain if neither candidate in the matchup has their preferred abortion policy stance, the AMCE for pro-life increases to 0.15, while the AMCE for cutting taxes decreases to 0.025 (Table 2, Column 2).

To understand why the AMCEs change in opposite directions under uniform abstention, focus on the matchups in which the vote tallies diverge under the forced-choice and uniform abstention scenarios. For instance, for the pro-life AMCE, consider the comparison of $\bar{Y}(PL, D; PC, I)$ - $\bar{Y}(PC, D; PC, I)$ (Table 2a, Row 4); voters' aggregate preference for pro-life candidates is stronger when abstention is allowed because pro-life voters no longer cast votes for pro-choice candidates when no pro-life candidates are present (e.g., the $PC, D; PC, I$ matchup). Put differently, voters no longer dilute their preferences over candidates' abortion policy stances by selecting candidates who do not share their own position.

Alternatively, for the cutting taxes AMCE, because decreasing taxes is linked with being pro-choice, when there is uniform abstention along the abortion dimension there

Table 2: AMCEs for abortion and tax policies varying abstention options.

(a) Pro-life

		No Abstention	Uniform Abstention	Pro-life Abstentions	Pro-choice Abstentions
1	$\bar{Y}(PL,D;PL,D)$ - $\bar{Y}(PC,D;PL,D)$	0.50	-0.50	0.50	-0.50
2	$\bar{Y}(PL,D;PL,I)$ - $\bar{Y}(PC,D;PL,I)$	1	1	1	1
3	$\bar{Y}(PL,D;PC,D)$ - $\bar{Y}(PC,D;PC,D)$	0.50	2	2	0.50
4	$\bar{Y}(PL,D;PC,I)$ - $\bar{Y}(PC,D;PC,I)$	0	3	3	0
5	$\bar{Y}(PL,I;PL,D)$ - $\bar{Y}(PC,I;PL,D)$	0	-2	0	-2
6	$\bar{Y}(PL,I;PL,I)$ - $\bar{Y}(PC,I;PL,I)$	0.50	-0.50	0.50	-0.50
7	$\bar{Y}(PL,I;PC,D)$ - $\bar{Y}(PC,I;PC,D)$	1	1	1	1
8	$\bar{Y}(PL,I:PC,I)$ - $\bar{Y}(PC,I;PC,I)$	0.50	2	2	0.50
	AMCE	$=\frac{4}{40}=0.10$	$=\frac{6}{40}=0.15$	$=\frac{10}{40}=0.25$	$=\frac{0}{40}=0.00$

(b) Cutting taxes

		No Abstention	Uniform Abstention	Pro-life Abstentions	Pro-choice Abstentions
1	$\bar{Y}(PL,D;PL,D)$ - $\bar{Y}(PL,I;PL,D)$	0.50	1.50	0.50	1.50
2	$\bar{Y}(PL,D;PL,I)$ - $\bar{Y}(PL,I;PL,I)$	0.50	1.50	0.50	1.50
3	$\bar{Y}(PL,D;PC,D)$ - $\bar{Y}(PL,I;PC,D)$	0	0	0	0
4	$\bar{Y}(PL,D;PC,I)$ - $\bar{Y}(PL,I;PC,I)$	0	0	0	0
5	$\bar{Y}(PC,D;PL,D)$ - $\bar{Y}(PC,I;PL,D)$	0	0	0	0
6	$\bar{Y}(PC,D;PL,I)$ - $\bar{Y}(PC,I;PL,I)$	0	0	0	0
7	$\bar{Y}(PC,D;PC,D)$ - $\bar{Y}(PC,I;PC,D)$	0.50	-1	-1	0.50
8	$\bar{Y}(PC,D;PC,I)$ - $\bar{Y}(PC,I;PC,I)$	0.50	-1	-1	0.50
	AMCE	$=\frac{2}{40}=0.05$	$=\frac{1}{40}=0.025$	$=\frac{-1}{40}=-0.025$	$=\frac{4}{40}=0.10$

Notes: PL=Pro-life, PC=Pro-choice, D=Decrease upper-class taxes, I=Increase upper-class taxes. 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., $4 \times 5 \times 2$).

are fewer voters that prefer to cut taxes contributing to the AMCE (Table 2a, Rows 7 and 8). For instance, in the $\bar{Y}(PC,D;PC,I)$ - $\bar{Y}(PC,I;PC,I)$ comparison, voters who prefer

a pro-life candidate will not vote if there is a pro-choice candidate, which reduces the aggregate preference toward cutting taxes.

In the final two columns of Table 2, we consider how the pro-life and cutting taxes AMCEs change when only pro-life or only pro-choice voters abstain. First, when pro-choice voters always vote but pro-life voters abstain if no candidates espouse pro-life positions, we see that the pro-life AMCE increases further to 0.25 (Table 2a, Column 3). Pro-choice voters diminish their aggregate preference for pro-choice candidates by casting votes for pro-life candidates when no pro-choice candidates are available. Yet, pro-life voters do not vote when confronted with only pro-choice candidates.

The cutting taxes AMCE, however, decreases to -0.025 indicating that the electorate's aggregate preference on tax policy is in support of candidates pledging to raise taxes on the upper-class *despite* a majority of voters preferring candidates pledging to cut those taxes. Pro-life voters no longer express their preference for pro-choice candidates pledging to cut taxes (Table 2b, Column 3, Rows 7 and 8), but pro-choice voters continue to cast ballots for pro-life candidates pledging to raise taxes. In aggregate, these expressed preferences yield a negative, rather than positive, AMCE for cutting taxes.

Finally, when only pro-choice voters abstain, we observe that the AMCE for pro-life is 0.00 even though the majority of voters prefer pro-life candidates. This quantity arises because pro-choice voters no longer vote when only pro-life candidates are present (Table 2a, Column 4, Rows 1, 5, and 6), but pro-life voters still cast ballots for pro-choice candidates when no pro-life candidates are available. Meanwhile, the AMCE for cutting taxes increases to 0.10 as pro-choice voters no longer express their preference for increasing taxes by voting for pro-life candidates pledging to raise taxes (Table 2b, Column 4,

Rows 1 and 2). As pro-life voters outnumber pro-choice voters, these abstentions increasingly boost the pro-life AMCE. These examples of uniform and asymmetric abstention showcase that the consequences of using forced-choice outcomes in conjoint experiments relative to allowing for abstention are not straightforward. Both the *magnitudes* and *signs* of AMCEs obtained under each design may substantively differ.

To illustrate how the rate of abstention among voters with different preference orderings impacts the AMCEs, we extend our example to N-voters in the Supplementary Material. 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 they prioritize. In other words, these differences depend on *which type* of respondents would abstain, as described by their preference orderings, and *how many* respondents of each type would abstain.[4]

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 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.

[4]We also discuss in the Supplementary Materials how varying other facets of our conjoint example can affect the resulting AMCEs—namely, whether different voter types have similar or different preferences between the levels of each attribute, and the degree to which they similarly prioritize attributes when rank-ordering potential candidate profiles.

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 by 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).[5]

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

[5]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

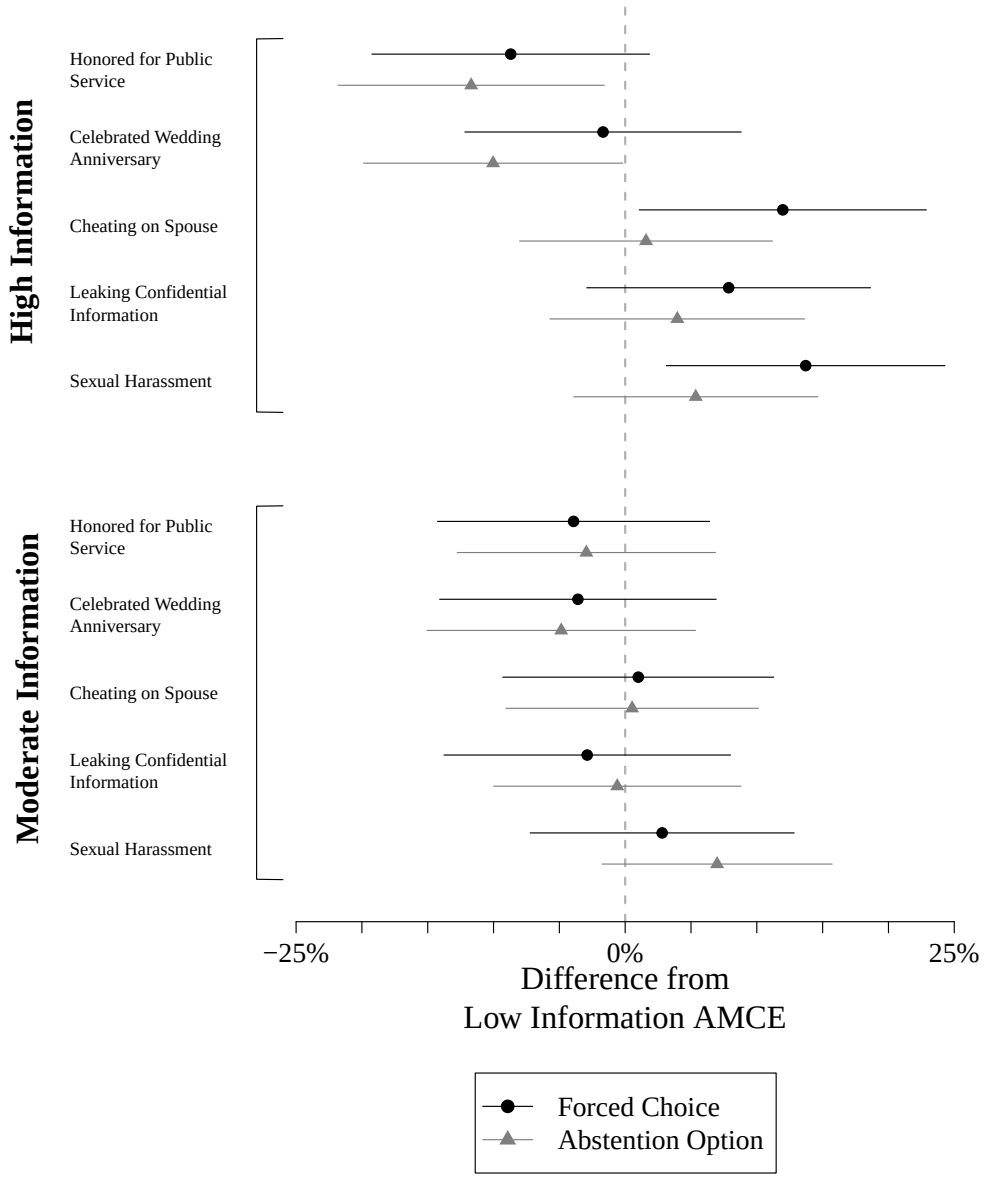
Figure 1 displays the AMCEs for the forced-choice (black circles) and abstention option (grey triangles) arms. Specifically, since the reference level is the “No recent news" attribute in the “Low Information" condition, the points and lines in Figure 1 compare the AMCEs for the same level of the “Recent 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 in each of the two arms. 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.[6]

Further, whereas the differences in the AMCEs for the two positive news stories (“Honored for public service" and “Celebrated wedding anniversary") between the “Low"

[6]While these distinctions across the experimental arms are not different from each other, they do lead to different results from the authors’ original hypothesis.

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



Notes: This figure presents the differences in the AMCE 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.

and “High Information" conditions in the forced-choice arm are not statistically 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: 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 AMCEs of different magnitudes or signs 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 locations to migrate (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' decision to select as well as preferences if a selection is made.

References

Abramson, Scott F, Korhan Koçak and Asya Magazinnik. 2022. "What Do We Learn About Voter Preferences from Conjoint Experiments." *American Journal of Political Science* .

Agerberg, Mattias. 2020. "The Lesser Evil? Corruption Voting and the Importance of Clean Alternatives." *Comparative Political Studies* 53(2):253–287.

Bansak, Kirk, Jens Hainmueller, Daniel J Hopkins and Teppei Yamamoto. 2021. Conjoint Survey Experiments. In *Advances in Experimental Political Science*. Cambridge University Press.

Berinsky, Adam J. 2008. Survey Non-Response. In *The SAGE Handbook of Public Opinion Research*, ed. Wolfgang Donsbach and Michael W. Traugott. Sage pp. 309–321.

Blais, André. 2006. "What Affects Voter Turnout?" *Annual Review of Political Science* 9:111–125.

Carlson, Elizabeth. 2015. "Ethnic Voting and Accountability in Africa: A Choice Experiment in Uganda." *World Politics* 67(2):353–385.

Chilton, Adam S, Helen V Milner and Dustin Tingley. 2020. "Reciprocity and Public Opposition to Foreign Direct Investment." *British Journal of Political Science* 50(1):129–153.

Egami, Naoki and Erin Hartman. 2022. "Elements of External Validity: Framework, Design, and Analysis." *American Political Science Review* pp. 1–19.

Funck, Amy S and Katherine T McCabe. 2022. "Partisanship, Information, and the Conditional Effects of Scandal on Voting Decisions." *Political Behavior* 44:1389–1409.

Ghosn, Faten, Tiffany S Chu, Miranda Simon, Alex Braithwaite, Michael Frith and Joanna Jandali. 2021. "The Journey Home: Violence, Anchoring, and Refugee Decisions to Return." *American Political Science Review* 115(3):982–998.

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

Kim, Jin Woo and Eunji Kim. 2021. "Temporal Selective Exposure: How Partisans Choose When to Follow Politics." *Political Behavior* 43(4):1663–1683.

Krosnick, Jon A, Allyson L Holbrook, Matthew K Berent, Richard T Carson, W Michael Hanemann, Raymond J Kopp, Robert Cameron Mitchell, Stanley Presser, Paul A Ruud and V Kerry Smith. 2002. "The Impact of 'No Opinion' Response Options on Data Quality: Non-Attitude Reduction or an Invitation to Satisfice?" *Public Opinion Quarterly* 66(3):371–403.

Liu, Guoer and Yuki Shiraito. 2023. "Multiple Hypothesis Testing in Conjoint Analysis." *Political Analysis* .

Liu, Hanzhang. 2019. "The Logic of Authoritarian Political Selection: Evidence from a Conjoint Experiment in China." *Political Science Research and Methods* 7(4):853–870.

Miller, David R. 2022. "On Whose Door to Knock? Organized Interests' Strategic Pursuit of Access to Members of Congress." *Legislative Studies Quarterly* 47(1):157–192.

Mukerjee, Subhayan and Tian Yang. 2020. "Choosing to Avoid? A Conjoint Experimental Study to Understand Selective Exposure and Avoidance on Social Media." *Political Communication* 38(3):222–240.

Mummolo, Jonathan. 2016. "News from the Other Side: How Topic Relevance Limits the Prevalence of Partisan Selective Exposure." *Journal of Politics* 78(3):763–773.

Rosenstone, Steven J and John M Hansen. 2003. *Mobilization, Participation, and Democracy in America.* Pearson Education.

Supplementary Materials: Preferential Abstention in Conjoint Experiments

Contents

In the first portion of the Supplemental Materials (Section SMI) 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 SMIII, and Mummolo (2016) in Section SMIV. Specifically, we provide background information about the original survey experiments, and we present the full estimates from the models discussed in the manuscript.

SMI 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

The two most common outcome measures are "forced-choice", where respondents are

[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.

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 SM.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 SM.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.

SMII 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.

SMII.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—pro-life (PL) for voters V1, V3, and V5, and pro-choice (PC) for voters V2 and V4. The five voters' aggregate preferences under uniform abstention are displayed in Table SM.2.

Note that in the final two comparisons (Table SM.2a, Rows 5 and 6), 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 SM.2b, we obtain different quantities than we would with no abstentions. In fact, our AMCEs moved in the opposite direction. The AMCE for pro-life has increased from 0.10 to 0.15, while the AMCE for corruption has decreased by 0.025 from 0.05 to 0.025. Even though the underlying preferences of the voters remained constant, the estimated AMCEs obtained are different.

Table SM.2: Scenario with uniform abstention.

(a) Individual and aggregate preferences

Comparison	V1	V2	V3	V4	V5	Tally
PL, D vs. PC, I	PL, D	PC, I	PL, D	PC, I	PL, D	3,2
PL, D vs. PC, D	PL, D	PC, D	PL, D	PC, D	PL, D	3,2
PL, D vs. PL, I	PL, D	-	PL, D	-	PL, D	3,0
PL, I vs. PC, I	PL, I	PC, I	PL, I	PC, I	PL, I	3,2
PL, I vs. PC, D	PL, I	PC, D	PL, I	PC, D	PL, I	3,2
PC, D vs. PC, I	-	PC, I	-	PC, I	-	0,2

(b) AMCEs for pro-life and cutting taxes

Panel A: Pro-life

$\bar{Y}(PL,D;PL,D)$ - $\bar{Y}(PC,D;PL,D)$	-0.50
$\bar{Y}(PL,D;PL,I)$ - $\bar{Y}(PC,D;PL,I)$	1
$\bar{Y}(PL,D;PC,D)$ - $\bar{Y}(PC,D;PC,D)$	2
$\bar{Y}(PL,D;PC,I)$ - $\bar{Y}(PC,D;PC,I)$	3
$\bar{Y}(PL,I;PL,D)$ - $\bar{Y}(PC,I;PL,D)$	-2
$\bar{Y}(PL,I;PL,I)$ - $\bar{Y}(PC,I;PL,I)$	-0.50
$\bar{Y}(PL,I;PC,D)$ - $\bar{Y}(PC,I;PC,D)$	1
$\bar{Y}(PL,I:PC,I)$ - $\bar{Y}(PC,I;PC,I)$	2
	AMCE = $\frac{6}{40} = 0.15$

Panel B: Cutting taxes

$\bar{Y}(PL,D;PL,D)$ - $\bar{Y}(PL,I;PL,D)$	1.50
$\bar{Y}(PL,D;PL,I)$ - $\bar{Y}(PL,I;PL,I)$	1.50
$\bar{Y}(PL,D;PC,D)$ - $\bar{Y}(PL,I;PC,D)$	0
$\bar{Y}(PL,D;PC,I)$ - $\bar{Y}(PL,I;PC,I)$	0
$\bar{Y}(PC,D;PL,D)$ - $\bar{Y}(PC,I;PL,D)$	0
$\bar{Y}(PC,D;PL,I)$ - $\bar{Y}(PC,I;PL,I)$	0
$\bar{Y}(PC,D;PC,D)$ - $\bar{Y}(PC,I;PC,D)$	-1
$\bar{Y}(PC,D;PC,I)$ - $\bar{Y}(PC,I;PC,I)$	-1
	AMCE = $\frac{1}{40} = 0.025$

Notes: PL=Pro-life, PC=Pro-choice, D=Decrease upper-class taxes, I=Increase upper-class taxes.

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 pro-life candidates abstained (Table SM.3a). This time the third and the sixth comparison are affected by voters' abstentions relative to when all voters choose a candidate. Again, when we calculate the AMCEs for the two attributes in Table SM.3b we see that our AMCEs are different; by abstaining, the pro-life voters have made the pro-life AMCE seem stronger and the effect of cutting taxes seem weaker. In fact, the AMCE for cutting taxes is now negative rather than positive, suggesting that candidates who pledge to raise taxes are now more preferred than those pledging to cut taxes *despite* the number of voters preferring tax-cutting candidates exceeding the number who prefer tax-raising candidates.

Next, we imagine what would happen if those emphasizing pro-choice candidates abstained in Table SM.4a. In this instance, only the 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 SM.4b, we see that when voters prefer pro-choice candidates and abstain when only pro-life candidates are on the ballot, the AMCE for pro-life decreases to 0.00, but the AMCE for cutting taxes increases to 0.10.

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 voters of each type is not equal, the AMCE for the attribute that is more important to the larger group becomes greater and the attribute that is more important to the smaller group gets smaller. Third, when only one type of voter abstains, the AMCE for the

Table SM.3: Scenario with pro-life abstention.

(a) Aggregate preferences over candidate profiles.

Comparison	V1	V2	V3	V4	V5	Tally
PL, D vs. PC, I	PL, D	PC, I	PL, D	PC, I	PL, D	3,2
PL, D vs. PC, D	PL, D	PC, D	PL, D	PC, D	PL, D	3,2
PL, D vs. PL, I	PL, D	PL, I	PL, D	PL, I	PL, D	3,2
PL, I vs. PC, I	PL, I	PC, I	PL, I	PC, I	PL, I	3,2
PL, I vs. PC, D	PL, I	PC, D	PL, I	PC, D	PL, I	3,2
PC, D vs. PC, I	-	PC, I	-	PC, I	-	0,2

(b) AMCEs for pro-life and cutting taxes

Panel A: Pro-life	
$\bar{Y}(PL,D;PL,D)$ - $\bar{Y}(PC,D;PL,D)$	0.50
$\bar{Y}(PL,D;PL,I)$ - $\bar{Y}(PC,D;PL,I)$	1
$\bar{Y}(PL,D;PC,D)$ - $\bar{Y}(PC,D;PC,D)$	2
$\bar{Y}(PL,D;PC,I)$ - $\bar{Y}(PC,D;PC,I)$	3
$\bar{Y}(PL,I;PL,D)$ - $\bar{Y}(PC,I;PL,D)$	0
$\bar{Y}(PL,I;PL,I)$ - $\bar{Y}(PC,I;PL,I)$	0.50
$\bar{Y}(PL,I;PC,D)$ - $\bar{Y}(PC,I;PC,D)$	1
$\bar{Y}(PL,I:PC,I)$ - $\bar{Y}(PC,I;PC,I)$	2
	AMCE $= \frac{10}{40} = 0.25$
Panel B: Cutting taxes	
$\bar{Y}(PL,D;PL,D)$ - $\bar{Y}(PL,I;PL,D)$	0.50
$\bar{Y}(PL,D;PL,I)$ - $\bar{Y}(PL,I;PL,I)$	0.50
$\bar{Y}(PL,D;PC,D)$ - $\bar{Y}(PL,I;PC,D)$	0
$\bar{Y}(PL,D;PC,I)$ - $\bar{Y}(PL,I;PC,I)$	0
$\bar{Y}(PC,D;PL,D)$ - $\bar{Y}(PC,I;PL,D)$	0
$\bar{Y}(PC,D;PL,I)$ - $\bar{Y}(PC,I;PL,I)$	0
$\bar{Y}(PC,D;PC,D)$ - $\bar{Y}(PC,I;PC,D)$	-1
$\bar{Y}(PC,D;PC,I)$ - $\bar{Y}(PC,I;PC,I)$	-1
	AMCE $= -\frac{1}{40} = -0.025$

Notes: PL=Pro-life, PC=Pro-choice, D=Decrease upper-class taxes, I=Increase upper-class taxes.

Table SM.4: Scenario with pro-choice abstention.

(a) Aggregate preferences over candidate profiles.

Comparison	V1	V2	V3	V4	V5	Tally
PL, D vs. PC, I	PL, D	PC, I	PL, D	PC, I	PL, D	3,2
PL, D vs. PC, D	PL, D	PC, D	PL, D	PC, D	PL, D	3,2
PL, D vs. PL, I	PL, D	-	PL, D	-	PL, D	3,0
PL, I vs. PC, I	PL, I	PC, I	PL, I	PC, I	PL, I	3,2
PL, I vs. PC, D	PL, I	PC, D	PL, I	PC, D	PL, I	3,2
PC, D vs. PC, I	PC, D	PC, I	PC, D	PC, I	PC, D	3,2

(b) AMCEs for pro-life and cutting taxes

Panel A: Pro-life

$\bar{Y}(PL,D;PL,D)$ - $\bar{Y}(PC,D;PL,D)$	-0.50
$\bar{Y}(PL,D;PL,I)$ - $\bar{Y}(PC,D;PL,I)$	1
$\bar{Y}(PL,D;PC,D)$ - $\bar{Y}(PC,D;PC,D)$	0.50
$\bar{Y}(PL,D;PC,I)$ - $\bar{Y}(PC,D;PC,I)$	0
$\bar{Y}(PL,I;PL,D)$ - $\bar{Y}(PC,I;PL,D)$	-2
$\bar{Y}(PL,I;PL,I)$ - $\bar{Y}(PC,I;PL,I)$	-0.50
$\bar{Y}(PL,I;PC,D)$ - $\bar{Y}(PC,I;PC,D)$	1
$\bar{Y}(PL,I:PC,I)$ - $\bar{Y}(PC,I;PC,I)$	0.50
	AMCE = $\frac{0}{40} = 0.00$

Panel B: Cutting taxes

$\bar{Y}(PL,D;PL,D)$ - $\bar{Y}(PL,I;PL,D)$	1.50
$\bar{Y}(PL,D;PL,I)$ - $\bar{Y}(PL,I;PL,I)$	1.50
$\bar{Y}(PL,D;PC,D)$ - $\bar{Y}(PL,I;PC,D)$	0
$\bar{Y}(PL,D;PC,I)$ - $\bar{Y}(PL,I;PC,I)$	0
$\bar{Y}(PC,D;PL,D)$ - $\bar{Y}(PC,I;PL,D)$	0
$\bar{Y}(PC,D;PL,I)$ - $\bar{Y}(PC,I;PL,I)$	0
$\bar{Y}(PC,D;PC,D)$ - $\bar{Y}(PC,I;PC,D)$	0.50
$\bar{Y}(PC,D;PC,I)$ - $\bar{Y}(PC,I;PC,I)$	0.50
	AMCE = $\frac{4}{40} = 0.10$

Notes: PL=Pro-life, PC=Pro-choice, D=Decrease upper-class taxes, I=Increase upper-class taxes.

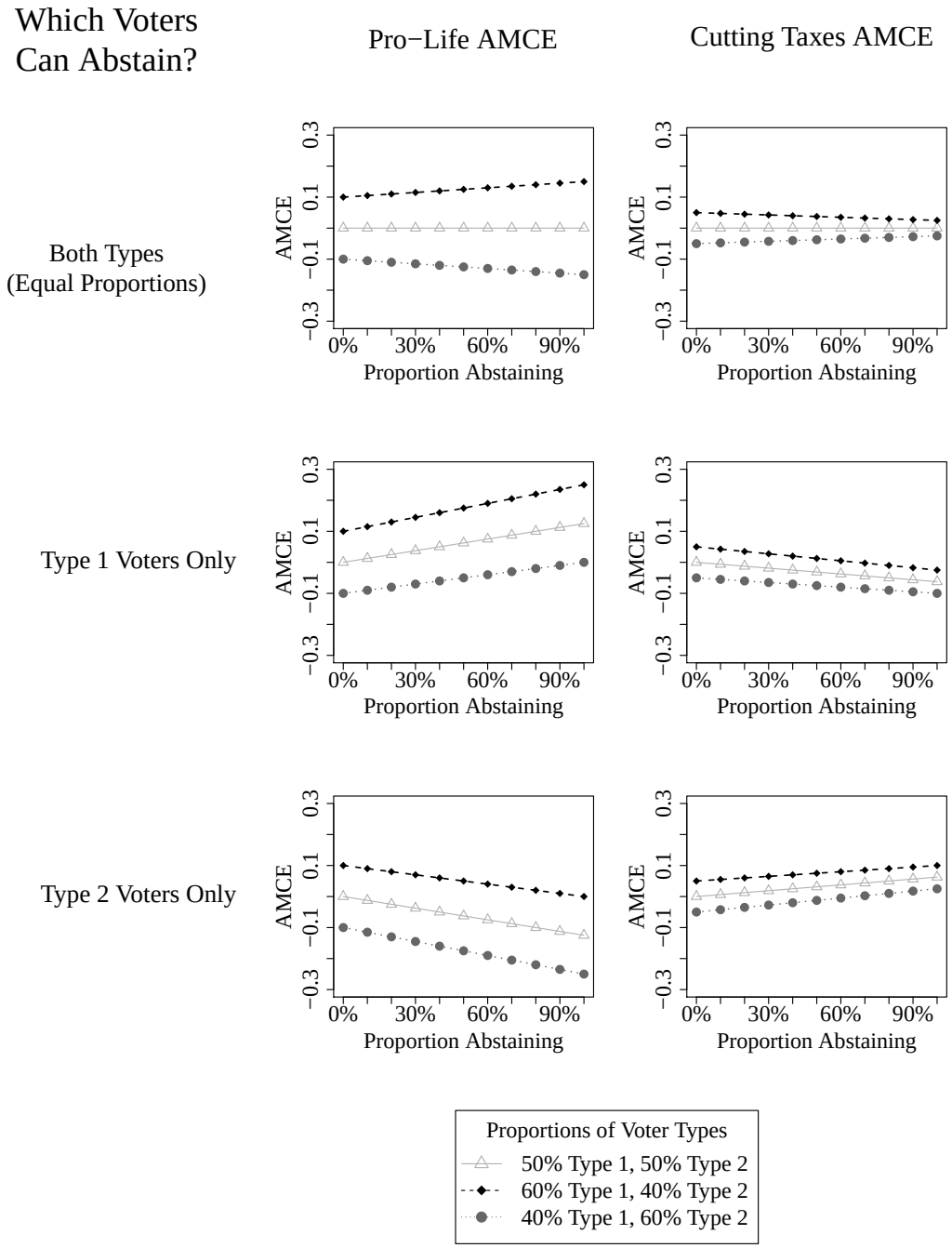
attribute that is more important for that group becomes greater, while the attribute that is more important to the other group becomes smaller (i.e. the abstaining group is no longer contributing "1"s to choices it would never choose, thus increasing the difference in marginal means for that attribute). To show how the rate of abstention among voters with different preference orderings impacts the AMCEs, we extend our example to N-voters.

SMII.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 1, 3, and and 5 in our 5-voter example and the preference orderings of Type 2 voters match those of voters 2 and 4. The rows of Figure SM.1 indicate which types of voters are allowed to abstain if no candidate with their prioritized level of the abortion policy attribute is present, and the columns differentiate the AMCEs for pro-life (first column) and cutting taxes (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).

Given these conditions, we examine in Figure SM.1 how changes in the rate of abstention for one or both types of voters affects our AMCEs of interest. We observe in the first row of Figure SM.1 that when both types of voters abstain in equal proportions and the proportion of voters types is not equal, that the magnitude of the AMCE associated with the attribute prompting abstention increases as abstension increases.

Figure SM.1: Scenario: Heterogeneous preferences for both attributes and identical prioritization.



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%.

Yet, the magnitude of the AMCE associated with an attribute that does not prompt abstention attenuates toward zero. Differently, in the second and third rows of Figure SM.1, only pro-life and pro-choice voters can abstain if neither candidate has their preferred position on abortion policy. It appears that as abstention increases, the AMCE for pro-life candidates moves toward the preferences of those abstaining, while the AMCE for candidates pledging to cut taxes moves away from the preferences of abstaining voters (e.g., in the second row, as more pro-life voters abstain, the AMCE for pro-life increases while the AMCE for cutting taxes decreases). Importantly, these shifts in AMCEs lead to differences in not only magnitude but also directionality, as the AMCE for cutting taxes changes signs when the voter type abstaining is more common and the abstention rate exceeds 70%. For instance, in the bottom right pane, when the voter composition is 40% Type 1 and 60% Type 2 and Type 2 voters abstain at a rate of greater than 70%, the AMCE for candidates pledging to cut taxes changes from negative to positive.

It is important to note that while this N-voter example accounts for shifts in the rate of abstention among different voter types, we have fixed two other facets of the conjoint setting. First, we have assumed that both types of voters have heterogeneous preferences over the two attributes; for each attribute, Type 1 and Type 2 voters prefer different levels (e.g., Type 1 voters prefer pro-life candidates, Type 2 voters prefer pro-choice candidates). Second, we have assumed that both types of voters have the same priorities over the included attributes; both Type 1 and Type 2 voters prioritize candidates' abortion policy positions over their tax policy positions. In additional simulations, we explore the implications that varying these facets can have on the resulting AMCEs. In brief, we

find that the rate of abstention affects the magnitude of the AMCEs recovered and that sign-switching is possible across the range of abstention rates so long as both voter types do not have homogeneous preferences for all attributes.

Our theoretical exercise demonstrates that the estimated AMCEs can differ in both magntitude and sign depending on whether the choice-based outcome measure employed forces respondents to select a profile in each task. 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.

SMIII 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.[3] 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.

To replicate F-M, we recruited a sample of 2,254 American respondents using

[3]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.

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.[4]

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

[4]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 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).

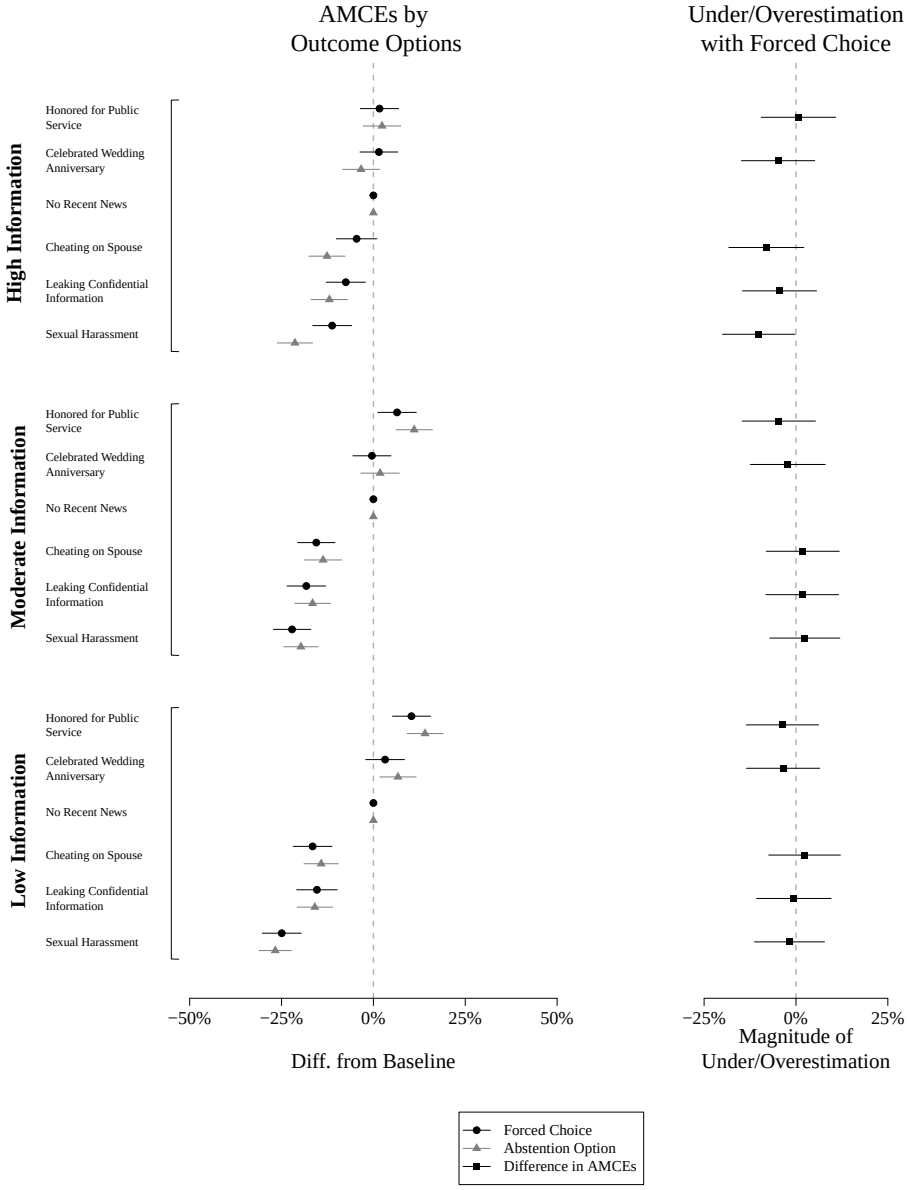
Table SM.5 reports the estimated coefficients this model, and Figure SM.2 presents visual representations of the AMCEs and the pairwise differences of each AMCE among respondents who were in the forced-choice and abstention option arms. Figure 1 in the manuscript uses the regression model reported in Table SM.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.

Table SM.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 SM.2: 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 ($\alpha = \frac{0.05}{15} = 0.00\overline{3}$).

SMIII.2 When Do Respondents Abstain?

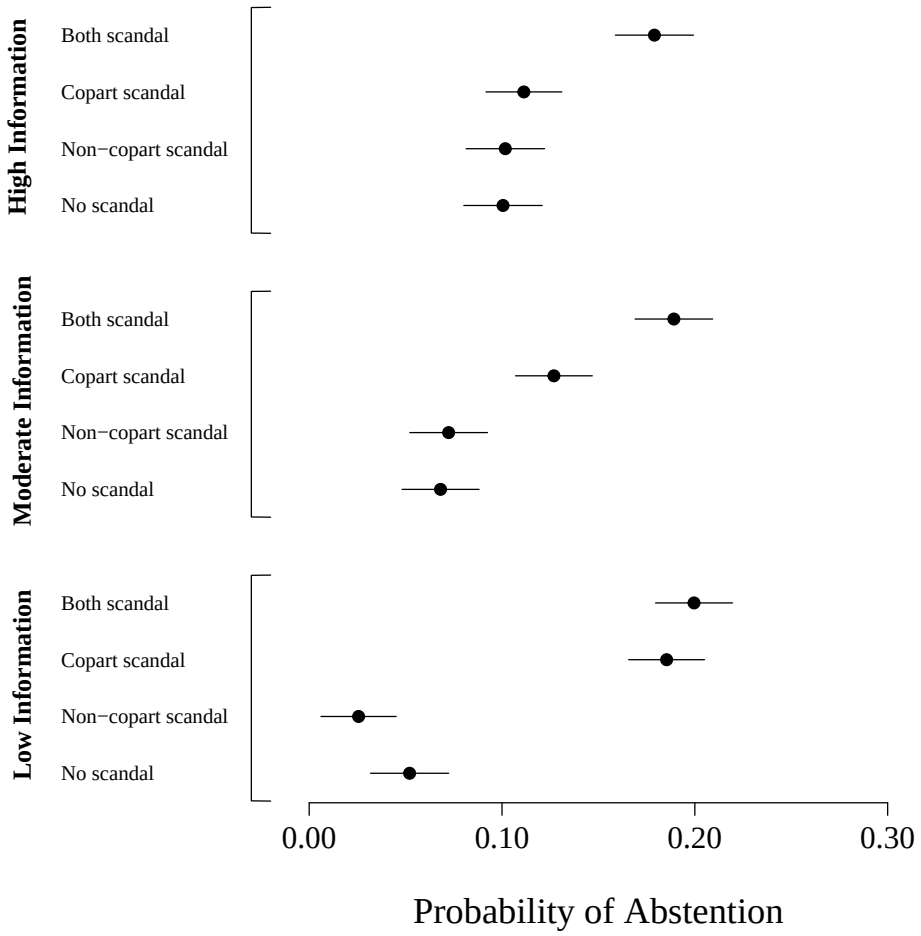
To better understand why respondents abstain during some tasks, we used ordinary least squares to regress a binary indicator for whether the respondent abstained in a given task (1) or not (0) on an interaction between the following two task and profile characteristics:

- 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 SM.3 indicates the predicted probability that a respondent would abstain from a task given its information environment complexity as well as whether and which candidates are implicated in scandals. 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 in comparison to when only the candidate who does not share their partisan affiliation or neither candidate experiences scandal. This is consistent with the notion that respondents would rather abstain than choose

Figure SM.3: 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.

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.

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 consider higher levels of detail. Conversely, 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.

SMIII.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 the 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.

SMIV Replication and Extension of Mummulo (2016)

In Mummolo's original article, 1,059 American respondents that identified as Democrats or Republicans were recruited by Survey Sampling International (SSI). 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

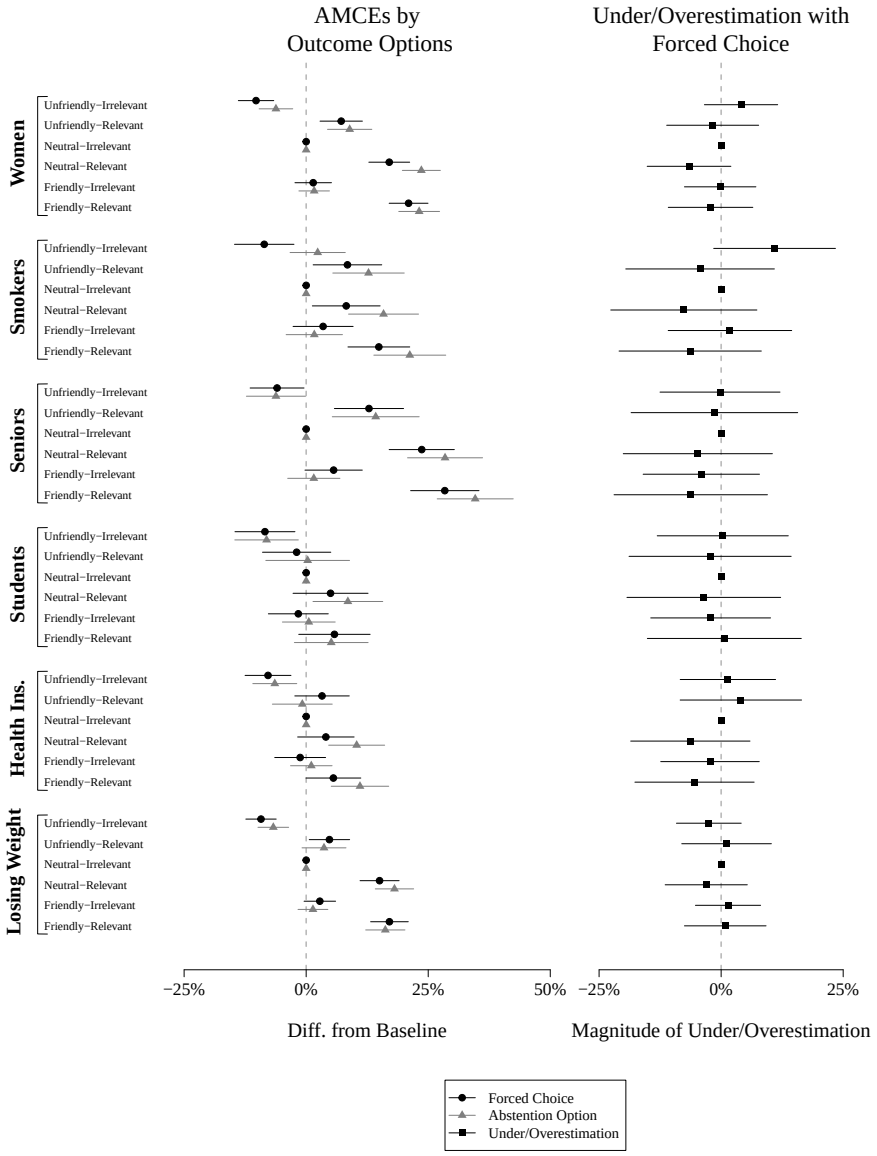
identified as Democrats or Republicans using Amazon's Mechanical Turk (MTurk). As in our replication of F-M, we randomly assigned respondents to a *forced-choice* or *abstention option* arm. Additionally, we again asked respondents to complete a final, thirteenth task in which they were provided with 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 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 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 SM.4, and the differences in the magnitudes of the AMCEs across arms.

Figure SM.4: 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 ($\alpha = \frac{0.05}{30} = 0.001\overline{6}$).

Additionally, in Table SM.6, we adapt Table 4 in Mummolo (2016, 770) by estimating separate models for each affected public and experimental arm.

Table SM.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 $^{*}p < 0.05$.

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.

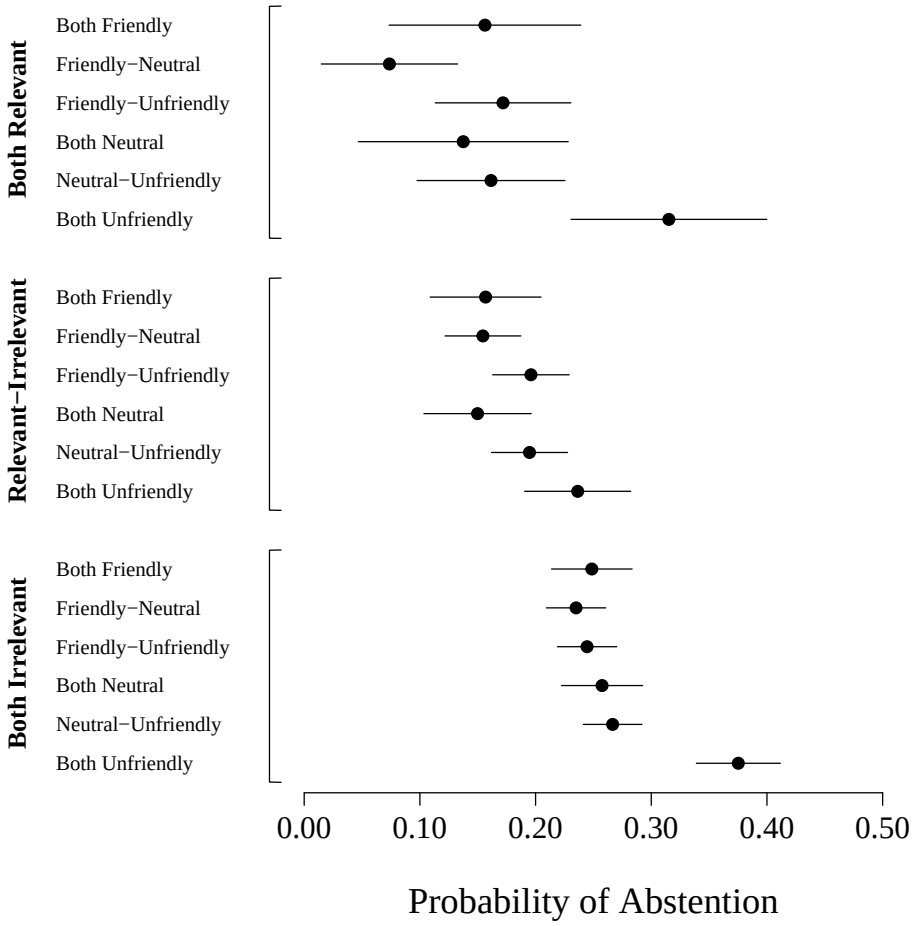
SMIV.2 When Do Respondents Abstain?

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 SM.5 indicates the predicted probability that a respondent would abstain from a task given the topic relevance and source friendliness of the presented stories. The

Figure SM.5: 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.

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. In other words, if presented only with news items from undesirable sources, respondents would rather abstain than select a news item from an unfriendly source.

SMIV.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.

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.

References

Arias, Sabrina B and Christopher W Blair. 2022. "Changing Tides: Public Attitudes on Climate Migration." *Journal of Politics* 84(1):560–567.

Auerbach, Adam Michael and Tariq Thachil. 2018. "How Clients Select Brokers: Competition and Choice in India's Slums." *American Political Science Review* 112(4):775–791.

Auerbach, Adam Michael and Tariq Thachil. 2020. "Cultivating Clients: Reputation, Responsiveness, and Ethnic Indifference in India's Slums." *American Journal of Political Science* 64(3):471–487.

Bakker, Bert N, Gijs Schumacher and Matthijs Rooduijn. 2021. "The Populist Appeal: Personality and Antiestablishment Communication." *Journal of Politics* 83(2):589–601.

Ballard-Rosa, Cameron, Lucy Martin and Kenneth Scheve. 2017. "The Structure of American Income Tax Policy Preferences." *Journal of Politics* 79(1):1–16.

Bansak, Kirk, Michael M Bechtel and Yotam Margalit. 2021. "Why Austerity? The Mass Politics of a Contested Policy." *American Political Science Review* 115(2):486–505.

Barnett, Carolyn, Amaney A. Jamal and Steve L Monroe. 2021. "Earned Income and Women's Segmented Empowerment: Experimental Evidence from Jordan." *American Journal of Political Science* 65(4):954–970.

Blumenau, Jack and Benjamin E Lauderdale. 2022. "The Variable Persuasiveness of Political Rhetoric." *American Journal of Political Science* .

Campbell, Rosie, Philip Cowley, Nick Vivyan and Markus Wagner. 2019. "Why Friends and Neighbors? Explaining the Electoral Appeal of Local Roots." *Journal of Politics* 81(3):937–951.

Carnes, Nicholas and Noam Lupu. 2016. "Do Voters Dislike Working-Class Candidates? Voter Biases and the Descriptive Underrepresentation of the Working Class." *American Political Science Review* 110(4):832–844.

Costa, Mia. 2021. "Ideology, Not Affect: What Americans Want from Political Representation." *American Journal of Political Science* 65(2):342–358.

Dill, Janina and Livia I Schubiger. 2021. "Attitudes Toward the Use of Force: Instrumental Imperatives, Moral Principles, and International Law." *American Journal of Political Science* 65(3):612–633.

Doherty, David, Conor M Dowling and Michael G Miller. 2019. "Do Local Party Chairs Think Women and Minority Candidates Can Win? Evidence from a Conjoint Experiment." *Journal of Politics* 81(4):1282–1297.

Eggers, Andrew C, Nick Vivyan and Markus Wagner. 2018. "Corruption, Accountability, and Gender: Do Female Politicians Face Higher Standards in Public Life?" *Journal of Politics* 80(1):321–326.

Eshima, Shusei and Daniel M Smith. 2022. "Just a Number? Voter Evaluations of Age in Candidate-Choice Experiments." *Journal of Politics* .

Frederiksen, Kristian Vrede Skaaning. 2022. "Does Competence Make Citizens Tolerate Undemocratic Behavior?" *American Political Science Review* .

Funck, Amy S and Katherine T McCabe. 2022. "Partisanship, Information, and the Conditional Effects of Scandal on Voting Decisions." *Political Behavior* 44:1389–1409.

Ghosn, Faten, Tiffany S Chu, Miranda Simon, Alex Braithwaite, Michael Frith and Joanna Jandali. 2021. "The Journey Home: Violence, Anchoring, and Refugee Decisions to Return." *American Political Science Review* 115(3):982–998.

Hainmueller, Jens and Daniel J Hopkins. 2015. "The Hidden American Immigration Consensus: A Conjoint Analysis of Attitudes Toward Immigrants." *American Journal of Political Science* 59(3):529–548.

Hankinson, Michael. 2018. "When Do Renters Behave Like Homeowners? High Rent, Price Anxiety, and NIMBYism." *American Political Science Review* 112(3):473–493.

Hanretty, Chris, Benjamin E Lauderdale and Nick Vivyan. 2020. "A Choice-Based Measure of Issue Importance in the Electorate." *American Journal of Political Science* 64(3):519–535.

Henderson, John A, Geoffrey Sheagley, Stephen N Goggin, Logan Dancey and Alexander G Theodoridis. 2022. "Primary Divisions: How Voters Evaluate Policy and Group Differences in Intraparty Contests." *Journal of Politics* .

Johns, Robert and Ann-Kristin Kölln. 2020. "Moderation and Competence: How a Party's Ideological Position Shapes its Valence Reputation." *American Journal of Political Science* 64(3):649–663.

Kennedy, Ryan P, Philip D Waggoner and Matthew M Ward. 2022. "Trust in Public Policy Algorithms." *Journal of Politics* 84(2):1132–1148.

Levy, Gabriella. 2022. "Evaluations of Violence at the Polls: Civilian Victimization and Support for Perpetrators after War." *Journal of Politics* 84(2):783–797.

Magni, Gabriele. 2022. "Boundaries of Solidarity: Immigrants, Economic Contributions, and Welfare Attitudes." *American Journal of Political Science* .

Magni, Gabriele and Andrew Reynolds. 2021. "Voter Preferences and the Political Underrepresentation of Minority Groups: Lesbian, Gay, and Transgender Candidates in Advanced Democracies." *Journal of Politics* 83(4):1199–1215.

Mummolo, Jonathan. 2016. "News from the Other Side: How Topic Relevance Limits the Prevalence of Partisan Selective Exposure." *Journal of Politics* 78(3):763–773.

Mummolo, Jonathan and Clayton Nall. 2017. "Why Partisans Do Not Sort: The Constraints on Political Segregation." *Journal of Politics* 79(1):45–59.

Nelson, Michael J and Christopher Witko. 2021. "The Economic Costs of Democratic Backsliding? Backsliding and State Location Preferences of US Job-Seekers.".

Ono, Yoshikuni and Michael A Zilis. 2022. "Ascriptive Characteristics and Perceptions of Impropriety in the Rule of Law: Race, Gender, and Public Assessments of Whether Judges Can Be Impartial." *American Journal of Political Science* 66(1):43–58.

Peterson, Erik. 2017. "The Role of the Information Environment in Partisan Voting." *Journal of Politics* 79(4):1191–1204.

Peterson, Erik and Gabor Simonovits. 2018. "The Electoral Consequences of Issue Frames." *Journal of Politics* 80(4):1283–1296.

Poertner, Mathias. 2021. "The Organizational Voter: Support for New Parties in Young Democracies." *American Journal of Political Science* 65(3):634–651.

Rodon, Toni and Marc Sanjaume-Calvet. 2020. "How Fair Is It? An Experimental Study of Perceived Fairness of Distributive Policies." *Journal of Politics* 82(1):384–391.

Schneider, Christina J. 2020. "Public Commitments as Signals of Responsiveness in the European Union." *Journal of Politics* 82(1):329–344.

Spater, Jeremy. 2022. "Exposure and Preferences: Evidence from Indian Slums." *American Journal of Political Science* 66(2):302–317.

Teele, Dawn Langan, Joshua Kalla and Frances Rosenbluth. 2018. "The ties that double bind: social roles and women's underrepresentation in politics." *American Political Science Review* 112(3):525–541.

Tellez, Juan Fernando. 2021. "Peace Without Impunity: Worldview in the Settlement of Civil Wars." *Journal of Politics* 83(4):1322–1336.

Tsai, Lily L, Minh Trinh and Shiyao Liu. 2022. "What Makes Anticorruption Punishment Popular? Individual-Level Evidence from China." *Journal of Politics* 84(1):602–606.

Ward, Dalston G. 2019. "Public Attitudes Toward Young Immigrant Men." *American Political Science Review* 113(1):264–269.

Weaver, Julie Anne. 2021. "Electoral Dis-Connection: The Limits of Reelection in Contexts of Weak Accountability." *Journal of Politics* 83(4):1462–1477.