The Limitations of Using Forced Choice in Electoral Conjoint Experiments*

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Abstract

Political scientists increasingly rely on conjoint experiments to understand voters' decisions in electoral studies. However, in implementing these experiments, researchers often require participants to make a forced choice between hypothetical candidates in a target election, neglecting the real-world options for voters to abstain or cast a protest (blank or null) vote. This mismatch between reality and design can lead to misclassification errors and external validity bias, even when unbiased conjoint estimators are used. We identify and analyze the source of bias induced by forced-choice design using existing conjoint data from published articles. To overcome the limitations of using forced choices in electoral conjoint experiments, we propose a design-based approach — an unforced-choice design — and evaluate the effectiveness with an original randomized experiment that embeds two candidate conjoint analyses and randomizes the typical and unforced-choice designs. By incorporating substantive knowledge about specific contexts, we provide a practical guide to help researchers avoid potential pitfalls and improve their estimations.

Keywords: Conjoint Experiment, Voter Preferences, Forced Choice, Bias

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1 Introduction

Understanding how voters choose between competing candidates who vary on multiple characteristics is a fundamental question in the study of elections and political behavior. In recent years, conjoint analysis has been increasingly used to grasp such complex decision-making process in different levels of elections in various contexts (e.g., Franchino and Zucchini, 2015; Carnes and Lupu, 2016; Mares and Visconti, 2020; Horiuchi et al., 2021). Unlike survey experiments that typically use a single vignette for manipulation, conjoint experiments allow researchers to decompose the overall treatment effect and disentangle the effect of one treatment component from those of others (Hainmueller et al., 2014). This experiment technique provides a more nuanced way to understand voters' electoral preferences. In a typical application of conjoint analysis in electoral studies, respondents are presented with a pair of hypothetical candidates with randomly selected attributes and then are asked to choose their preferred candidate and/or rate each profile.

While conjoint experiments allow researchers to decompose a single candidate profile into multiple components, most designs require participants to make a forced choice between candidates in a single task (i.e., choosing one over the other) (Agerberg, 2020). In real-world elections, where voters have many more options like abstention and protest voting, the forced-choice design may introduce biases by compelling participants to make choices they might otherwise avoid. This deviation from real-world choices may generate measurement errors and undermine the gold standard of conjoint analysis, which aims to more accurately reflect the decision-making process and voting experience of real-world elections (Hainmueller et al., 2014; de la Cuesta et al., 2022).[1] In response to these concerns, this paper adopts a design-based approach to improve electoral conjoint analysis by proposing a more realistic, unforced-choice design for better studying voters' voting decisions.

[1] We conducted a survey of articles employing conjoint designs to elicit voter choices between hypothetical candidates across different levels of elections. Specifically, we focused on articles published by ten political science journals, namely AJPS, APSR, BJPS, CPS, Elect.Stud., JOP, PolBeh, POQ, PSMR, and WP, spanning the period from 2014 to 2023. Out of the 72 articles identified, four of them utilized an unforced choice design. The list is available in Appendix A.

We argue that the typical forced-choice conjoint design unintentionally makes two underlying assumptions: (1) that non-voters are not present and (2) that respondents have no tendency to cast an indifferent or a protest vote. Therefore, this design may compel respondents (eligible voters) to vote in hypothetical elections within a voluntary voting system, even when they would prefer to abstain. Meanwhile, it fails to mimic the real options available to voters, such as submitting a protest (null or blank) ballot or voting "None of the Above" (NOTA).[2] Using existing conjoint data from published articles, we demonstrate that these two assumptions are likely to be violated. First, we find that a significant proportion of respondents recruited for electoral conjoint experiments have abstained from voting at least once in real life, despite being eligible. Second, we observe that around half of respondents might have a tendency to cast an indifferent vote at least once if such an option were available. This suggests that forced-choice conjoint experiments may compel respondents to make artificial trade-offs that do not align with their real-world voting behavior.

To assess the patterns of enforcing choices on respondents, we first formally evaluate the design-induced biases — specifically, misclassification errors and external validity bias — and then implement an original, preregistered randomized experiment. In this experiment, we randomized participants into either the typical forced-choice design or our proposed unforced-choice design, embedding two replicated candidate conjoint studies. We replicate both the data collection and analysis from conjoint studies of Presidential candidates by Hainmueller et al. (2014) and Congressional candidates by Peterson (2017), while assigning respondents to either the forced-choice design or the unforced-choice design. Respondents assigned to the typical forced-choice design are presented with one forced-choice outcome question with two options: Candidate 1 and Candidate 2, and two unforced-rating outcome questions in each task. In contrast, in the unforced-choice design, we make the choice-based question an unforced response and provide two additional options that better mirror the range of one's voting choices: "If I only have these two candidates, I will cast a blank/null vote" and "If I only have these two candidates, I will abstain," while making

[2] Countries and subnational units like India, Ukraine, Bulgaria, Thailand, Colombia, and the U.S. state of Nevada offer the NOTA option. The French equivalent, 'vote blanc,' and 'vote blanco' in some Latin American countries, serve a similar purpose. See Alvarez et al. (2018) and Plescia et al. (2023) for more details.

the rating-based question a forced response.

In the two electoral conjoint studies we replicated, approximately 50% of the respondents opted to cast at least one protest (blank/null) vote or abstain at least once when these options were available. Our findings indicate that implementing the unforced-choice conjoint design leads to significant changes in the reported estimates of Average Marginal Component Effects (AMCEs). Specifically, the estimates of AMCEs either increase, decrease, change directions, or alter their statistical significance compared to those obtained using the forced-choice candidate conjoint design. For example, in the replicated conjoint study of Presidential candidates based on Hainmueller et al. (2014), we observe that several statistically significant differences, at the 95% confidence level, in AMCE estimates for various attribute levels, such as Age (45, 52, 68 vs. 36) and Income ($54K, $65K, $210K vs. $32K), when comparing between respondents assigned to our proposed unforced-choice conjoint design with those assigned to the typical forced-choice design. Similarly, in our replication of Peterson (2017), we identified several differing results, significant at the 90% confidence level. These variations arise because the forced-choice design can introduce unpredictable biases in both magnitude and direction, making them difficult to anticipate. We conclude with a practical guide to help researchers avoid potential pitfalls and improve their methodological practices.

Overall, this paper contributes to the evolving literature on conjoint analysis in political methodology by addressing the design-induced limitations of forced-choice experiments. While recent advancements have focused on distinct conjoint estimands (e.g., Leeper et al., 2020; Abramson et al., 2022; Zhirkov, 2022; Ganter, 2023), estimate interpretation (Abramson et al., 2022; Bansak et al., 2023), response quality (Bansak et al., 2018; Horiuchi et al., 2021; Clayton et al., 2023; Kane and Costa, 2024), and external validity (de la Cuesta et al., 2022), our study shifts the focus toward the design aspects of conjoint outcomes. Specifically, we propose an unforced-choice conjoint design that integrates substantive knowledge of the voting options available in target elections. This approach enhances the realism and applicability of electoral conjoint studies by improving the extrapolation of experimental findings to real-world contexts. While this paper concentrates on electoral studies, our proposed design has broader applicability and can be adapted for other research areas — such as immigration policy preferences or employment decision-making — where decision-makers are not restricted to making binary forced choices in each task.

2 Mismatch Between Reality and Design in Electoral Conjoint Experiments

Most existing electoral conjoint analyses present either-or choices between two hypothetical candidates to respondents in a target election of interest. This design implicitly assumes that respondents have only two options in any target elections: vote for Candidate A or Candidate B. However, in real-world elections, voters have more choices and do not have to trade off between available candidates. In a target election with voluntary voting rules, eligible voters can choose to abstain from voting. Similarly, in a target election with either voluntary or mandatory voting rules, voters can submit a protest (invalid or blank) ballot or cast a "None of the Above" vote in certain countries when they are not satisfied with the current political system or either candidate. We argue that the options provided in forced-choice electoral conjoint analyses do not sufficiently reflect the choices respondents (eligible voters) have in reality. Table 1 illustrates the mismatch in voting choices available for respondents between the reality and the forced-choice design in a target election with voluntary voting rules.

Table 1: Choice Mismatch Between Target Elections and Forced-Choice Conjoint Design

Eligible Voters (Respondents)	**Target Election**	**Forced-Choice**
Voters	Candidate A, Candidate B, or a protest vote	Candidate A or Candidate B
Non-voters	Abstention	Candidate A or Candidate B

2.1 Abstention as an Electoral Choice

Not all eligible individuals take part in electoral activities. The decision to vote or abstain is often an individual choice, and voters are not selected randomly (Bernhagen and Marsh, 2007).

The literature on electoral behavior reveals that non-voters demonstrate a distinct political profile in comparison to their participatory counterparts, regular voters (e.g., Adams and Merrill, 2003; Bernhagen and Marsh, 2007; Battaglini et al., 2008; Bølstad et al., 2013; Fowler, 2015; Kawai et al., 2021; Koch et al., 2023). For instance, meta-analysis of individual-level turnout research supports systematic differences between non-voters and regular voters in sociodemographic factors like age and education, as well as participatory elements such as mobilization, party identification, political interest, and political knowledge (Smets and van Ham, 2013).

Existing evidence suggests that if all eligible voters had participated, electoral outcomes might differ, highlighting the impact of counterfactual preference aggregation (Bernhagen and Marsh, 2007; Kawai et al., 2021). This reasoning can be applied to a forced-choice conjoint experiment, where a nationally representative sample, including respondents who would choose to opt out, would mimic a scenario akin to full voter turnout, assuming every eligible voter cast their vote. As a result, since the presumed missing vote choices of non-voter respondents are gathered and aggregated without scrutiny, the standard conjoint design may potentially yield biased estimates of the counterfactual vote choices.

Are respondents who abstained from voting in the real-world elections actually recruited for candidate conjoint experiments? Answering this question is challenging because most existing candidate conjoint studies do not differentiate respondents based on their voting history or offer an abstention option allowing respondents to opt out. However, Horiuchi et al. (2020) utilize post-conjoint questions regarding respondents' voting history, allowing for a more detailed identification of non-voter participants in their sample.[3] Out of the total sample of 2,200 respondents, approximately 600 fully exercised their voting rights, while more than 370 respondents almost never participated in voting despite being eligible. The remaining 1,200 respondents skipped voting one or more times. Using the existing conjoint data collected by Horiuchi et al. (2020), we re-estimate the AMCEs in the study by examining two subsamples: one consisting of respondents

[3] In Q7.8, respondents are presented with the following inquiry: "How often have you participated in voting since you got the right to vote? Have you voted in all elections, most elections, some elections, or never?" Options: (1) Voted in all elections (2) Voted in most elections (3) Voted in several elections (4) I almost never vote. Note: The original question is in Japanese, and we have provided an English translation for the sake of the illustration.

who reported always voting in past elections, and the other comprising those who have almost never voted, despite eligible.

In Figure 1, our results indicate that in a forced-choice conjoint experiment, non-voter respondents exhibit even stronger or weaker preferences for different candidates' attributes than their regular counterparts. For example, for the sample of respondents who have actively participated in all elections, past experience in government does not appear to be a strong predictor of which candidate is more likely to be selected. However, for respondents who have abstained from almost all past elections, past experience in government is valued much more highly than it is by active voters in the experiment.

More importantly, in a real election with voluntary voting rules, the preferences of non-voters are not accounted for and thus do not contribute to the final electoral results. A forced-choice design, however, aggregates and reflects the preferences of non-voter respondents, which would otherwise remain unobserved. Given that scholars typically use conjoint experiments to analyze and predict the average likelihood of certain characteristics being selected for candidates, our results indicate that including non-voters in forced-choice conjoint analyses — especially those with stronger or weaker preferences than regular voters — could potentially introduce unpredictable biases into the estimates of various attributes.

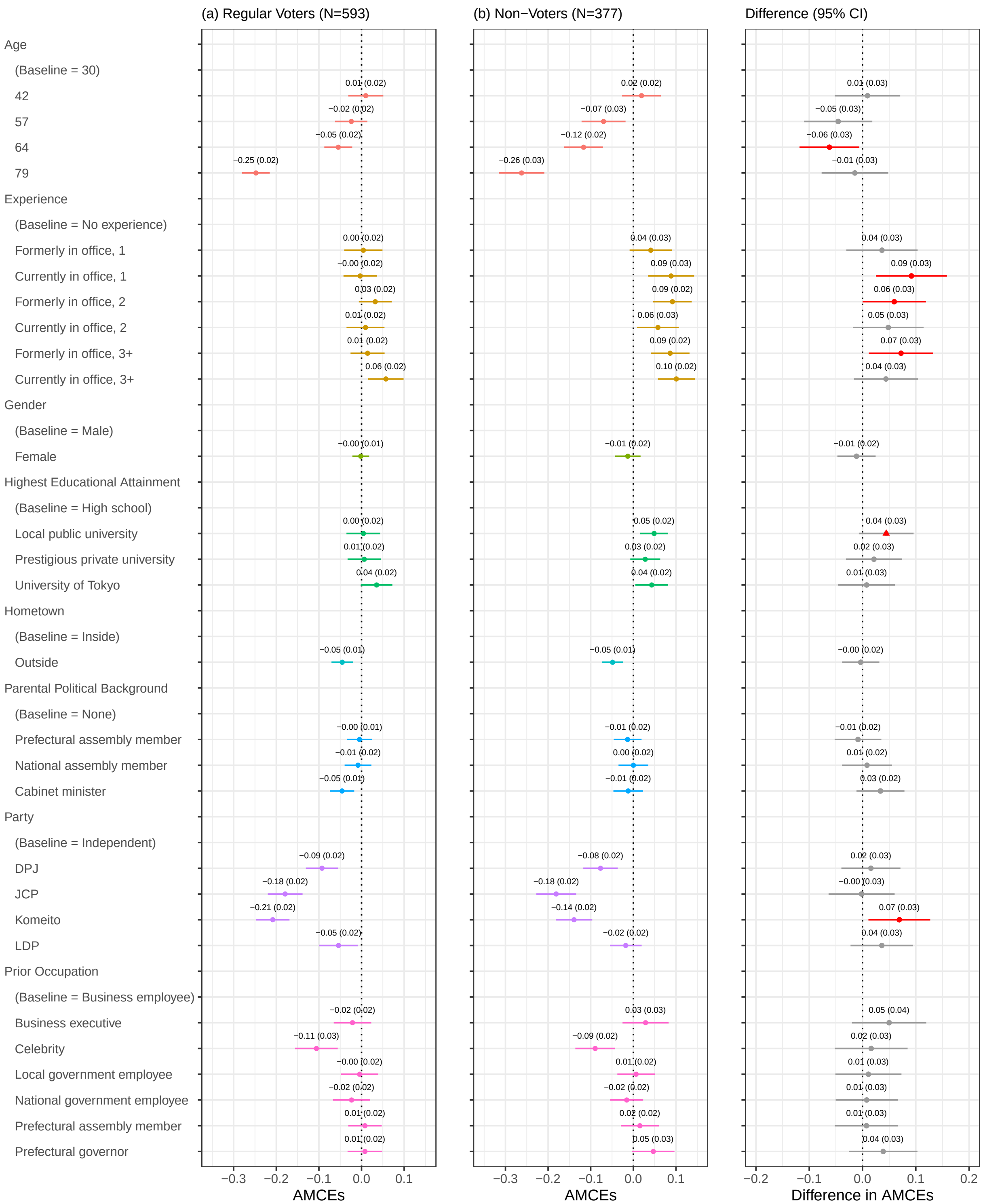

Figure 1: Replication of the Horiuchi et al. (2020) experiment using the subsamples of regular and non-voters. Note: N refers to the number of respondents in each subsample. The rightmost panel shows the differences in AMCEs between regular and non-voter subgroups for each attribute level, in which horizontal bars represent 95% confidence intervals robust to clustering at the respondent level. The red dots with red bars indicate significance at the 95% level, whereas the red triangles with gray bars indicate significance only at the 90% level. The gray dots with gray bars indicate significance at neither conventional level.

2.2 Protest Votes as an Electoral Choice

Conventional forced-choice conjoint analyses require respondents (a sample of eligible voters) to make a trade-off between available candidates. However, in real-world elections, voters do not have to make such a trade-off. If they support neither candidate or are indifferent between them, they can choose not to participate in the election at all, as discussed in the previous subsection. Alternatively, they can participate by deliberately submitting a blank ballot or an invalid ballot paper — a protest vote.

Technically, it is challenging to detect when a respondent makes a trade-off choice in a forced-choice design. However, by using rating-based responses from existing electoral conjoint analyses, we can approximate the trade-off choices respondents likely made. Researchers use rating-based responses to capture respondents' preference levels. If a respondent assigns the same ratings to profiles in a particular comparison task, it is plausible to speculate that they are making a trade-off choice in a forced-choice conjoint design because they equally prefer both profiles but must choose one over the other.

We examine pairs of profiles that received identical rating scores from respondents in the experiments by Hainmueller et al. (2014) and Kirkland and Coppock (2018). Figure 2 shows the proportion of respondents assigning identical ratings to various numbers of tasks in each experiment, and it illustrates that almost 40% of respondents assigned identical ratings to at least one task. It is worth noting that this proportion could be a conservative estimate; if respondents intentionally rate profiles to align with their forced-choice responses for self-consistency, the proportion could be even higher. However, if these respondents were given an unforced-choice design, they might not necessarily make such trade-off choices; they could choose to abstain or cast a blank vote if they prefer not to make a trade-off choice.

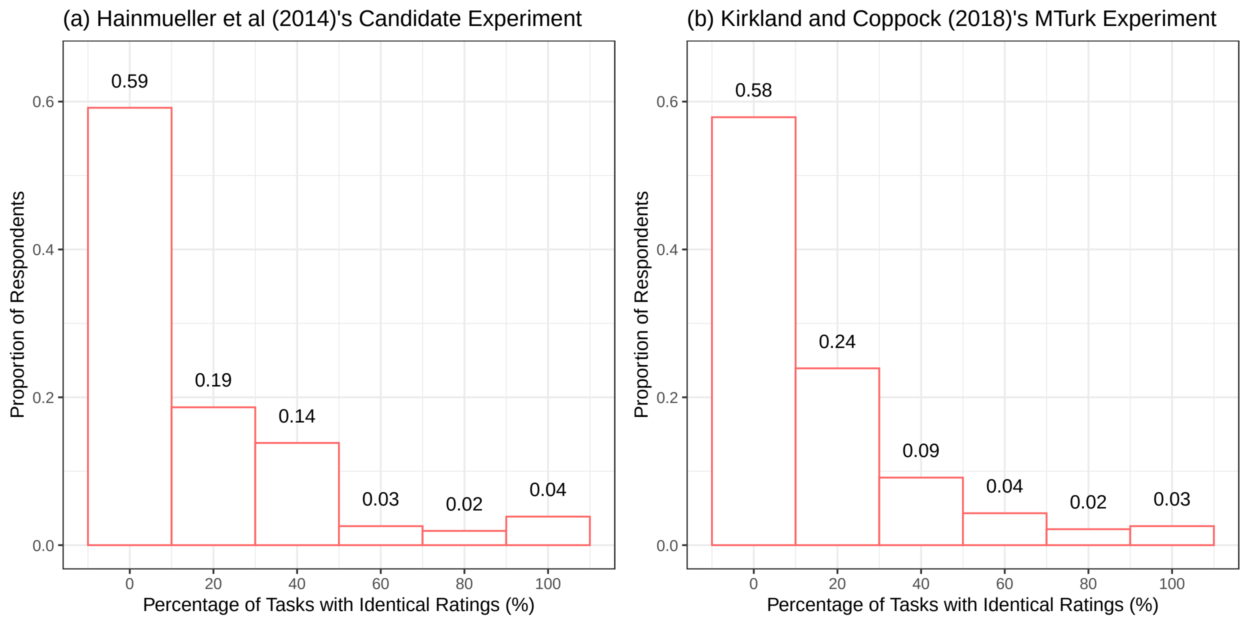

Figure 2: Proportion of Respondents Assigning Identical Ratings to Tasks in Hainmueller et al. (2014)'s and Kirkland and Coppock (2018)'s Experiments

3 Design-Induced Bias

In this section, we examine how the discrepancy between voting choices in conjoint designs and those in real-world elections introduces bias and can lead to potential misinterpretations, starting with toy examples and progressing to formal analyses.

3.1 Identifying Bias in Forced-Choice Design with Toy Examples

To illustrate, we present two toy examples that demonstrate how the AMCE aggregates individual preferences and translates into the key substantive quantity of interest — change in vote shares.[4] For simplicity, consider an electorate of ten eligible voters whose preferences we aim to study in a target election that a conjoint experiment is designed to resemble and simulate, where respondents are tasked with comparing two candidate profiles. In the first example, shown in Table 2, Voter 1, who intends to cast a protest vote and holds no genuine preference between the

[4] Bansak et al. (2023) show how the AMCE substantively has a straightforward, meaningful interpretation: the effect of a change in an attribute on a candidate's or party's expected vote share.

candidates, is compelled to choose in the observed scenario. This forced selection results in a misclassification problem, as the voter's response is recorded as a legitimate vote for Candidate t_b rather than reflecting their actual preference to neither candidates.

Table 2: Toy Example 1. This table presents a toy example demonstrating the voting behavior of an electorate of ten eligible voters for two candidates, t_a and t_b, with 100% turnout. In the observed scenario, all voters cast valid votes, although Voter 1 was forced to vote. However, in the counterfactual scenario, Voter 1, who is compelled to vote in the initial scenario, opts to cast a protest vote instead, resulting in only 90% valid votes. The highlighted cells indicate the change in voting behavior for Voter 1.

	Observed			Counterfactual		
	Turnout=100% Valid Votes=100%			Turnout=100% Valid Votes=90%		
Eligible Voter i	$Y_i(t_a)$	$Y_i(t_b)$	π_i	$\tilde{Y}_i(t_a)$	$\tilde{Y}_i(t_b)$	$\tilde{\pi}_i$
1	0	1	-1	0	0	0
2	1	0	1	1	0	1
3	1	0	1	1	0	1
4	0	1	-1	0	1	-1
5	0	1	-1	0	1	-1
6	1	0	1	1	0	1
7	0	1	-1	0	1	-1
8	1	0	1	1	0	1
9	1	0	1	1	0	1
10	1	0	1	1	0	1
Counts	6	4	2	6	3	3

$$\begin{aligned} E[Y_i(t_a) - Y_i(t_b)] &= E[Y_i(t_a)] - E[Y_i(t_b)] \\ &= \frac{6}{10} - \frac{4}{10} = \frac{1}{5} \\ E[\tilde{Y}_i(t_a) - \tilde{Y}_i(t_b)] &= E[\tilde{Y}_i(t_a)] - E[\tilde{Y}_i(t_b)] \\ &= \frac{6}{10} - \frac{3}{10} = \frac{3}{10} \end{aligned} \tag{1}$$

Mathematically, the AMCE is calculated as the difference in expected vote share between candidates when an attribute changes.[5] In this toy example, the equation $E[Y_i(t_a) - Y_i(t_b)] = \frac{6}{10} - \frac{4}{10} = \frac{1}{5}$ represents the observed effect of attribute changes on vote share when Voter 1 is compelled to make a choice. However, due to the misclassification of Voter 1's forced vote for Candidate B,

[5] Leeper et al. (2020) and Abramson et al. (2023) suggest that the AMCE estimand involves both direct and indirect comparisons between two features. For simplicity, we assume that the attribute of interest between Candidates t_a and t_b has only two features, thereby eliminating indirect comparisons. However, the logic still applies in cases where indirect comparisons are present.

the counterfactual vote share difference $E[\tilde{Y}_i(t_a) - \tilde{Y}_i(t_b)] = \frac{6}{10} - \frac{3}{10} = \frac{3}{10}$ implies that the forced-choice scenario underestimates the impact of Candidate t_a 's feature. This misinterpretation skews the AMCE, creating the illusion that Candidate t_a's feature have a smaller impact on the expected vote share than they actually do.

Similarly, consider an example where abstention plays a crucial role. In the second toy example, illustrated in Table 3, Voter 1 would have chosen to abstain, representing a significant form of political behavior—disengagement or indifference toward the candidates. In an actual (counterfactual) election, this abstention would exclude Voter 1 from the final vote count. However, in a forced-choice design, Voter 1 is compelled to participate despite their preference to abstain. This forced participation introduces external validity bias, as the inclusion of forced-choice votes that would otherwise be abstentions makes the study population differ from the target population. The study population includes respondents who would not have voted, thus diverging from the target population of eligible voters who actually turn out.

Table 3: Toy Example 2. This table presents a toy example of voting behavior among an electorate of ten eligible voters for two candidates, with a focus on the impact of compelling Voter 1 to vote or allowing them to abstain. The observed scenario shows 100% turnout and valid votes, while the counterfactual scenario reflects a 90% turnout where Voter 1 abstains. The highlighted cells indicate the change in Voter 1's behavior.

	Observed			Counterfactual		
	Turnout=100% Valid Votes=100%			Turnout=90% Valid Votes=100%		
Eligible Voter i	$Y_i(t_a)$	$Y_i(t_b)$	π_i	$\tilde{Y}_i(t_a)$	$\tilde{Y}_i(t_b)$	$\tilde{\pi}_i$
1	0	1	-1	-	-	-
2	1	0	1	1	0	1
3	1	0	1	1	0	1
4	0	1	-1	0	1	-1
5	0	1	-1	0	1	-1
6	1	0	1	1	0	1
7	0	1	-1	0	1	-1
8	1	0	1	1	0	1
9	1	0	1	1	0	1
10	1	0	1	1	0	1
Counts	6	4	2	6	3	3

$$
\begin{aligned}
E[Y_i(t_a) - Y_i(t_b)] &= E[Y_i(t_a)] - E[Y_i(t_b)] \\
&= \frac{6}{10} - \frac{4}{10} = \frac{1}{5} \\
E[\tilde{Y}_i(t_a) - \tilde{Y}_i(t_b)] &= E[\tilde{Y}_i(t_a)] - E[\tilde{Y}_i(t_b)] \\
&= \frac{6}{9} - \frac{3}{9} = \frac{1}{3}
\end{aligned}
\tag{2}
$$

Table 3 illustrates that Voter 1's preference to abstain is not reflected, leading to biased estimates of difference in vote shares. The correct calculation for the true difference in vote share between Candidate A and Candidate B, when abstention is allowed, is $E[\tilde{Y}_i(t_a) - \tilde{Y}_i(t_b)] = \frac{6}{9} - \frac{3}{9} = \frac{1}{3}$, which accurately reflects the preferences of the nine active voters. However, under the forced-choice design, where Voter 1 is compelled to choose, the calculation assumes all ten eligible voters participated, resulting in $E[Y_i(t_a) - Y_i(t_b)] = \frac{6}{10} - \frac{4}{10} = \frac{1}{5}$. This forced inclusion underestimates the true impact of the attribute change by assuming full turnout, leading to a distortion in the AMCE estimates and a misrepresentation of the dynamics of electoral choices.

To sum up the insights from the two toy examples, the forced-choice design in electoral conjoint experiments introduces two critical types of design-induced bias: misclassification and external validity bias, as illustrated in Figure 3. Misclassification arises when respondents, who would have cast a protest vote, are forced to select a candidate, resulting in their responses being inaccurately recorded as genuine preferences for one of the presented profiles. External validity bias arises when individuals who would abstain from voting in a real election are compelled to participate in the experiment. This forced inclusion skews the analysis by incorporating respondents who do not represent the target population of actual voters, thereby distorting the perceived level of candidate support and reducing the generalizability of the findings.

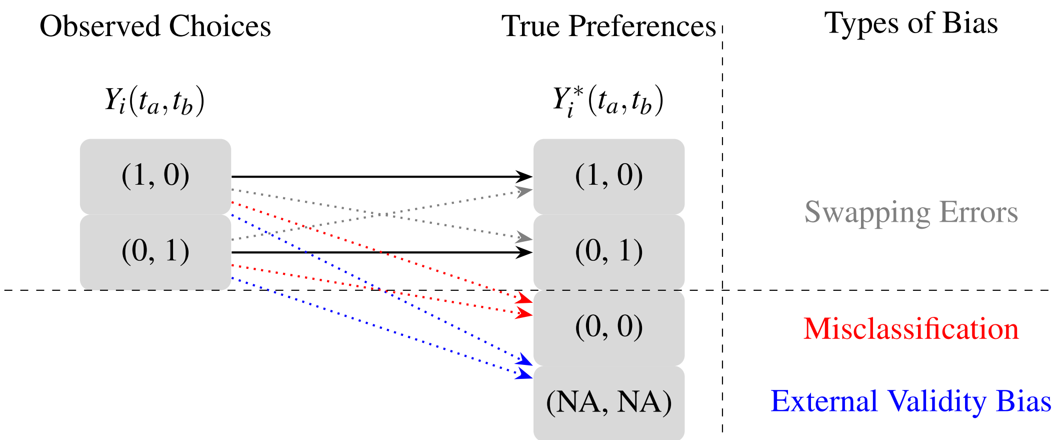

Figure 3: Types of Bias in Forced-Choice Electoral Conjoint Experiments. This figure illustrates the different types of biases that can arise in forced-choice electoral conjoint experiments when observed choices $Y_i(t_a, t_b)$ differ from true preferences $Y_i^*(t_a, t_b)$. Respondents are asked to choose between two candidates or profiles, but their true preferences may not always align with the observed responses.

In contrast to recent studies such as Clayton et al. (2023) and Kane and Costa (2024), which investigate swapping errors where respondents may unintentionally switch their choices due to inattentiveness, our primary focus is on biases that are inherent to the design itself. These design-induced biases persist even when respondents are fully attentive and when conjoint estimators remain unbiased. In the sections that follow, we formally analyze how misclassification and external validity biases can significantly distort AMCE estimates, leading to misinterpretations of the true impact of candidate attributes on the probability of being chosen.

3.2 Misclassification

The misclassification bias can be simply understood as a measurement error issue in choice-based outcome variables in conjoint experiment. A common misconception is that measurement error in a dependent variable does not lead to bias; however, this holds true only for classical measurement error. Misclassification of a binary variable constitutes non-classical measurement error, which inherently introduces bias (Meyer and Mittag, 2017; Shu and Yi, 2019). Since the key outcome variable in an electoral conjoint experiment is binary, let's denote the true binary choice-based outcome as Y^*, where $Y_i^* \in \{0,1\}$ for each profile i in a paired electoral conjoint experiment, where each profile has multiple attributes and each attribute can take on different levels.

As Hainmueller et al. (2014) introduced, the most commonly used quantities of interest are linear combinations of what we refer to as the profile-level AMCE, using the profile as the unit of analysis. Equivalently, the profile-level AMCE can be expressed as the difference between two profile-level marginal means (MM) (Leeper et al., 2020). Similarly, the estimator, profile-level MM, can be nonparametrically estimated via linear regression. The linear estimation model can take the following form:

$$Y_i^* = \beta_0 + \sum_j \beta_j^{(k)} D_j^{(k)} + \varepsilon_i \tag{3}$$

where $\beta_j^{(k)}$ represents the true MM of attribute j's level k on the outcome; $D_j^{(k)}$ is an indicator variable that equals 1 if profile i, randomized generated by design, has attribute j at level k , and 0 for all other levels of j; and ε_i is the error term clustered at the respondent level, assumed to have mean 0 and be independent of $D_j^{(k)}$ due to randomization.

Now, let's consider the scenario where the binary outcome Y^* (the true voting choice) is misclassified into the observed binary outcome Y. Let the observed outcome be denoted as Y (the mismeasured voting choice), subject to design-induced measurement error. We define the probabilities of false negatives and false positives misclassification conditional on the true response and the attribute level $D_j^{(k)}$ as:

$$P(Y_i = 0 | Y_i^* = 1, D_j^{(k)}) = p_{10} \tag{4}$$

$$P(Y_i = 1 | Y_i^* = 0, D_j^{(k)}) = p_{01} \tag{5}$$

The probabilities of no misclassification are:

$$P(Y_i = 1 | Y_i^* = 1, D_j^{(k)}) = 1 - p_{10} \tag{6}$$

$$P(Y_i = 0 | Y_i^* = 0, D_j^{(k)}) = 1 - p_{01} \tag{7}$$

With misclassification, according to the potential outcome framework in causal inference, the observed outcomes Y_i given attribute level $D_j^{(k)}$ are realized as:

$$\begin{aligned}
MM_j^k = \mathbb{E}(Y_i | D_j^{(k)} = 1) &= P(Y_i = 1 | D_j^{(k)} = 1) \\
&= P(Y_i = 1, Y_i^* = 1 | D_j^{(k)} = 1) + P(Y_i = 1, Y_i^* = 0 | D_j^{(k)} = 1) \\
&= (1 - p_{10}) P(Y_i^* = 1 | D_j^{(k)} = 1) + p_{01} P(Y_i^* = 0 | D_j^{(k)} = 1) \\
&= (1 - p_{10})(\beta_0 + \beta_j^{(k)}) + p_{01}(1 - \beta_0 - \beta_j^{(k)})
\end{aligned} \tag{8}$$

For the reference level $D_j^{(k')}$, the outcomes are derived similarly:

$$\begin{aligned}
MM_j^{k'} = \mathbb{E}(Y_i | D_j^{(k')} = 1) &= P(Y_i = 1 | D_j^{(k')} = 1) \\
&= P(Y_i = 1, Y_i^* = 1 | D_j^{(k')} = 1) + P(Y_i = 1, Y_i^* = 0 | D_j^{(k')} = 1) \\
&= (1 - p_{10})(\beta_0 + \beta_j^{(k')}) + p_{01}(1 - \beta_0 - \beta_j^{(k')})
\end{aligned} \tag{9}$$

As the AMCE is causally defined as the difference in the population probabilities of choosing the J profiles that would result if a respondent were shown the profiles with attribute j's k level as opposed to k' level, we can compute the difference as such:

$$\begin{aligned}
AMCE_j^{kk'} &= MM_j^k - MM_j^{k'} \\
&= \mathbb{E}(Y_i | D_j^{(k)} = 1) - \mathbb{E}(Y_i | D_j^{(k')} = 1) \\
&= P(Y_i = 1 | D_j^{(k)} = 1) - P(Y_i = 1 | D_j^{(k')} = 1) \\
&= (1 - p_{10})(\beta_j^{(k)} - \beta_j^{(k')}) - p_{01}(\beta_j^{(k)} - \beta_j^{(k')}) \\
&= (1 - p_{10} - p_{01})(\beta_j^{(k)} - \beta_j^{(k')})
\end{aligned} \tag{10}$$

Thus, as shown in Equation (10), with design-induced misclassification, the estimated AMCE is biased by a factor of $(1 - p_{10} - p_{01})$. The true AMCE, which would have been $\beta_j^{(k)} - \beta_j^{(k')}$, is scaled by this misclassification factor. If there is no misclassification, i.e., $p_{10} = 0$ and $p_{01} = 0$, then the estimated AMCE is equal to the true AMCE because $(1 - p_{10} - p_{01}) = 1$. However, if misclassification occurs, the estimates will be biased in three scenarios. Specifically:

- If $0 < p_{10} + p_{01} < 1$, the magnitude of the estimated AMCE will be reduced, and the bias will be downward and proportional to the sum of the misclassification probabilities.
- If $p_{10} + p_{01} = 1$, the estimate is entirely attenuated, leading to no observed effect (a completely biased estimate).
- If $p_{10} + p_{01} > 1$, the AMCE estimate could even flip sign, resulting in a completely opposite interpretation of the effect.

3.3 External Validity Bias

External validity bias arises when the target sample, which represents the target population, differs from the study population defined by enrollment processes and specific inclusion or exclusion criteria (Degtiar and Rose, 2023). As a result, the study sample may have characteristics that differ from those of the target sample, leading to potential biases in generalizing findings to the broader target population. In this subsection, we discuss even with unbiased conjoint estimators and without misclassification problems, how differences in voter behavior (i.e., abstention) between the study sample and the target population in a conjoint experiment can introduce external validity bias.

For simplicity, we assume that the distribution of profile characteristics, $D_j^{(k)}$, (such as attributes of the candidates or policy profiles shown in the experiment) is the same between the two populations, but the behavioral characteristic of interest—voter turnout—differs between the study population and the target population. In an electoral conjoint study, the target population, P_T , consists of all eligible voters who actually turn out, while the study population, P_S, includes all eligible voters, regardless of whether they turn out or not.

The true AMCE for the target population of voters who turned out is denoted as:

$$\text{AMCE}_j^{(kk')}(P_T) = \mathbb{E}_{P_T}[Y_i|D_j^{(k)} = 1] - \mathbb{E}_{P_T}[Y_i|D_j^{(k')} = 1] \tag{11}$$

Whereas, the AMCE for the study population, which includes both those who may turn out and those who may not, is:

$$\text{AMCE}_j^{(kk')}(P_S) = \mathbb{E}_{P_S}[Y_i|D_j^{(k)} = 1] - \mathbb{E}_{P_S}[Y_i|D_j^{(k')} = 1] \tag{12}$$

We suggest external validity bias arises when the AMCE calculated using the study population differs from the AMCE in the target population. Mathematically, the bias can be expressed as:

$$\text{External Validity Bias}_j^{(kk')} = \text{AMCE}_j^{(kk')}(P_S) - \text{AMCE}_j^{(kk')}(P_T) \tag{13}$$

Let's assume that the outcome Y_i in the conjoint experiment (e.g., vote choice) depends not only on the profile attributes $D_j^{(k)}$ but also on whether or not an individual turns out to vote in each pair of comparison. Let:

- $T_i = 1$ denote that individual prefers to turn out to vote in evaluating profile i.
- $T_i = 0$ denote that individual prefers not to turn out to vote in evaluating profile i.

We assume that preferences (reflected in Y_i) differ between those who turn out and those who do not, but the distribution of profile attributes $D_j^{(k)}$ is the same between the study and target populations.

The study population, P_S, includes both voters $(T_i = 1)$ and non-voters $(T_i = 0)$. Therefore, the expected outcome in the study population is a weighted average of the outcomes for voters and non-voters. These weights are the probabilities $\Pr(T_i = 1|P_S)$ (the probability of being a voter in the study population) and $\Pr(T_i = 0|P_S)$ (the probability of being a non-voter).

$$\begin{aligned}
\text{AMCE}_j^{(kk')}(P_S) &= \Big(\mathbb{E}_{P_S}[Y_i|D_j^{(k)} = 1] - \mathbb{E}_{P_S}[Y_i|D_j^{(k')} = 1]\Big) \\
&= \Pr(T_i = 1|P_S) \cdot \Big(\mathbb{E}[Y_i|D_j^{(k)} = 1, T_i = 1] - \mathbb{E}[Y_i|D_j^{(k')} = 1, T_i = 1]\Big) \\
&\quad + \Pr(T_i = 0|P_S) \cdot \Big(\mathbb{E}[Y_i|D_j^{(k)} = 1, T_i = 0] - \mathbb{E}[Y_i|D_j^{(k')} = 1, T_i = 0]\Big)
\end{aligned} \tag{14}$$

The target population only includes voters $(T_i = 1)$. Therefore, the AMCE in the target population is based solely on the expected outcome for voters:

$$\text{AMCE}_j^{(kk')}(P_T) = \mathbb{E}[Y_i|D_j^{(k)} = 1, T_i = 1] - \mathbb{E}[Y_i|D_j^{(k\prime)} = 1, T_i = 1] \tag{15}$$

We can now substitute Equation (13) with Equations (14) and (15). Notice that the voter terms $\Pr(T_i = 1|P_S)$ can partially cancel out with the corresponding voter terms in the target population. The remaining bias is driven by the non-voter terms.

$$\begin{aligned}\text{External Validity Bias}_j^{(kk')} &= \text{AMCE}_j^{(kk')}(P_S) - \text{AMCE}_j^{(kk')}(P_T) \\ &= (\Pr(T_i = 1|P_S) - 1) \cdot \Big(\mathbb{E}[Y_i|D_j^{(k)} = 1, T_i = 1] - \mathbb{E}[Y_i|D_j^{(k')} = 1, T_i = 1]\Big) + \\ &\Pr(T_i = 0|P_S) \cdot \Big(\mathbb{E}[Y_i|D_j^{(k)} = 1, T_i = 0] - \mathbb{E}[Y_i|D_j^{(k')} = 1, T_i = 0]\Big)\end{aligned} \tag{16}$$

Equation (16) shows that the external validity bias depends on two factors: 1. The proportion of non-voters in the study population, $\Pr(T_i = 0|P_S)$. 2. The mean difference in preferences (i.e., the expected outcomes) between voters and non-voters for each attribute level. In essence, if non-voters have different mean preferences compared to voters, this will introduce bias when trying to generalize the AMCE from the study population to the target population. Intuitively, the more non-voters there are in the study population (sample), the larger the potential bias.

4 The Proposed Approach: An Unforced-Choice Design

To mitigate the potential biases discussed above, we propose an unforced-choice design to better capture respondents' true voting preferences and improve measurement and external validity. Specifically, we suggest that voting choices in an electoral conjoint experiment should be designed to parallel those in real elections under study. Take, for example, an electoral conjoint experiment where the election of interest involves two candidates and operates under a voluntary voting system. Our proposed unforced-choice design makes two main improvements to the typical forced-choice conjoint design.

First, if the target election follows a voluntary voting rule, researchers should ensure that the choice-based outcome questions are not set as forced-response questions, which require respondents to answer before they can continue the survey. This forced-response requirement does not mirror the voluntary voting experience of the target election; on the contrary, it compels respondents to make choices they otherwise would not necessarily make.

Second, both *a protest vote for neither candidate* and *abstention* should be listed as additional options alongside the choices to vote for either candidate: "*Candidate 1*" and "*Candidate 2*." For example, the two newly added options could be framed as: "*If I only have these two candidates, I will cast a blank/null vote*" and "*If I only have these two candidates, I will abstain*," respectively. In regard to how to code the choice-based outcome variables, which now have four options, we recommend the following rules:

1. In line with the typical forced-choice conjoint design, if either Option "*Candidate 1*" or "*Candidate 2*" is selected, the choice-based outcome for the corresponding profile will be coded as 1, and the other profile, not being selected, will be coded as 0.

2. If a respondent chooses Option "*If I only have these two candidates, I will cast a blank/null vote*", we recommend to re-code the choice-based outcome variables for the two profiles being compared as 0. This mirrors the real-world situation where a blank or null vote contributes to overall turnout statistics but does not affect the distribution of votes among the candidates, as it is considered an invalid vote.

3. If a respondent chooses Option "*If I only have these two candidates, I will abstain*" or skips the question, we suggest coding the choice-based outcome variables as missing values. This reflects the real-world situation where the voting preferences of non-voters cannot be observed.[6] By coding abstentions as missing, the analysis sample can be restricted to

[6]We caution that this should not be considered an attrition problem because the "non-responses" are intentional and reflect a specific behavior relevant to the study—choosing not to vote. Unlike attrition, which typically involves unplanned loss of data that can bias results, coding abstentions as missing values accurately captures the behavior of individuals who opt out of making a choice, thereby maintaining the validity of the data regarding observed voter behavior.

respondents who would choose to turn out, aligning the sample more closely with the target population.

Overall, our proposed unforced-choice design offers several advantages over the forced-choice design for more accurately reflecting the true preferences of all types of voters (respondents) in electoral conjoint experiments. It is compatible with commonly used conjoint estimators, such as AMCE and MM, and requires fewer assumptions about respondents' voting behaviors. It enables both non-voters and regular voters to select voting options that better represent their genuine preferences for electoral candidates.[7] To support our proposed approach, we supplement our analysis with simulated studies using bootstrap methods, detailed in Appendix B.

5 Evidence from a Randomized Experiment

5.1 Experiment Design

To better evaluate bias patterns in forced-choice candidate conjoint designs, we pre-registered and conducted a randomized experiment embedding two conjoint experiments within two distinct design frameworks. Specifically, respondents were assigned to one of two conjoint designs: the typical electoral conjoint design featuring forced-choice evaluations (the control group), or our proposed design that allows unforced-choice evaluations (the treatment group). Both designs generate profiles similarly, but differ in the configuration of the evaluation questions that followed the profiles. We fielded the randomized experiment with a U.S. sample of 2,704 respondents from Cloud Research in June 2024, ensuring national representation by setting quotas based on key demographic characteristics like age (18 and older), gender, ethnicity, and region.

We started the survey with a battery of questions about respondents' past voting history and future voting propensity, demographic characteristics, and political attitudes. After that, we randomly

[7] By distinguishing between different choices rather than merely screening respondents based on their past voting records, we aim to avoid assuming that respondents who did not abstain in past elections will not abstain in simulated conjoint experiments, or that those who have always abstained will continue to do so.

assigned respondents to one of the two conjoint designs. We asked respondents to evaluate eight pairs of hypothetical presidential candidates as well as eight pairs of hypothetical congressional candidates, with the order of exposure to presidential or congressional candidates randomized for each respondent. The evaluation process in each task assignment involves three questions to capture outcomes: a choice-based question and two rating-based questions, rating their likelihood of voting for Candidates 1 and 2 on a 1-7 scale. For respondents assigned to the typical candidate conjoint design (the control group), the configuration of the evaluation questions follows the paradigm in Hainmueller et al. (2014), which is commonly adopted by the existing literature. Specifically, the choice-based questions require a forced-response choice between two hypothetical candidates, meaning respondents must choose one profile over the other to proceed to the next task, whereas the rating-based questions allow for an unforced-choice response, permitting respondents to skip questions and continue.

For respondents assigned to the treatment group, the evaluation questions feature a configuration with two innovations we proposed. First, rather than requiring respondents to choose between candidates in each pair of profiles alone, we provide two additional options: casting a blank/null vote and abstaining from voting. These options are specifically framed as "If I only have these two candidates, I will cast a blank/null vote." and "If I only have these two candidates, I will abstain." We believe that including these options can more accurately reflect the range of voting choices and preferences that voters have in real-world elections.

Second, in contrast to the control design, the treatment design modifies the response requirements for the evaluation questions. Here, respondents are not obliged to make a forced choice; they can opt to skip questions and proceed if they choose not to answer. Conversely, the rating-based questions in the treatment design do require a forced-choice response, ensuring that participants make a selection in order to continue. This adjustment benefits the study by enabling deeper insights into the unobservable decision-making processes of respondents, examining how they navigate and make trade-offs between options. For instance, if a respondent assigns identical scores to each profile in a pair, linking their rating-based responses to their choice-based actions could help infer the trade-offs made during their decision-making process.

To ensure comparability in our design and findings, we replicated the profile setups used by Hainmueller et al. (2014) and Peterson (2017) when designing the hypothetical profiles for presidential and congressional candidates in the U.S. We selected these two studies because they both investigate the context of U.S. politics. Specifically, the candidate conjoint design proposed by Hainmueller et al. (2014) has been established as the benchmark in this area, while Peterson (2017) utilizes a similar design that is applicable to studying congressional elections.

Panel (a)

Please carefully review the two candidates for US President detailed below. Which of the following two people do you think is more desirable as President of the United States?

	Candidate 1	**Candidate 2**
Religion	Evangelical Protestant	Mainline Protestant
Profession	High School Teacher	Farmer
Age	75	68
Annual Income	\$54,000	\$210,000
Race/Ethnicity	Caucasian	Black
Gender	Male	Male
Military Service	Served in U.S. military	No military service
College Education	BA from small college	BA from Baptist college

Panel (b)	
Control Evaluation Questions	Configuration
Which of these candidates would you vote to be **President of the United States**? ❏ Candidate 1 ❏ Candidate 2	(forced-response)
On a scale from 1 to 7, where 1 indicates that you would never support this candidate, and 7 indicates that you would always support this candidate, where would you place **Candidate 1**? **Never Support** … **Definitely Support** **1** ○ **2** ○ **3** ○ **4** ○ **5** ○ **6** ○ **7** ○	(unforced-response)
On a scale from 1 to 7, where 1 indicates that you would never support this candidate, and 7 indicates that you would always support this candidate, where would you place **Candidate 2**? **Never Support** … **Definitely Support** **1** ○ **2** ○ **3** ○ **4** ○ **5** ○ **6** ○ **7** ○	(unforced-response)
Treatment Evaluation Questions	Configuration
Which of these candidates would you vote to be **President of the United States**? ❏ Candidate 1 ❏ Candidate 2 ❏ If I only have these two candidates, I will cast a blank/null vote. ❏ If I only have these two candidates, I will abstain.	(unforced-response)
On a scale from 1 to 7, where 1 indicates that you would never support this candidate, and 7 indicates that you would always support this candidate, where would you place **Candidate 1**? **Never Support** … **Definitely Support** **1** ○ **2** ○ **3** ○ **4** ○ **5** ○ **6** ○ **7** ○	(forced-response)
On a scale from 1 to 7, where 1 indicates that you would never support this candidate, and 7 indicates that you would always support this candidate, where would you place **Candidate 2**? **Never Support** … **Definitely Support** **1** ○ **2** ○ **3** ○ **4** ○ **5** ○ **6** ○ **7** ○	(forced-response)

Figure 4: Experimental Design: Presidential candidate conjoint study replicated based on Hainmueller et al. (2014). Panel (a) illustrates an example of profiles being compared in a task, while Panel (b) shows the questions that respondents in the control and treatment groups receive, respectively. Profiles and questions are presented on the same screen in each task for respondents.

5.2 Estimation via Linear Regression

We define a sample of respondents indexed by i (i=1,..., N), and each respondent evaluates a predefined number of profiles indexed by j (j=1,..., J). The outcome variable y_{ij}, which is part of the vector (y_{i1}, ..., y_{iJ}) represents either choices or ratings (on a scale from 0 to 7) given by respondent i to the profile j presented. Profiles are described in terms of attributes indexed by k (k=1,..., K). $\mathbf{X}_{ik,j}$ is the vector of profile attributes, each element of which is a *factor variable* describing a certain attribute value presented to respondent i in profile j. The variable D_i is a binary variable that indicates the type of conjoint design randomly assigned at the individual level. It takes the value of 1 if the treatment design is assigned, and 0 otherwise. Then, the regression takes the following form:

$$y_{ij} = \sum_{k=1}^{K} \alpha_k \mathbf{X}_{ik,j} + \beta D_i + \sum_{k=1}^{K} \lambda_k \mathbf{X}_{ik,j} \times D_i + \varepsilon_{ij} \tag{17}$$

where α_k, β, and λ_k are regression parameters to be estimated, and ε_{ij} is the respondent-profile-specific error term. We are particularly interested in the size and significance of the estimate for parameter λ_k. This indicates the size and direction of the AMCEs observed from the treatment design different to the control design. We code y_{ij} based on the design assignments. For respondent i who received the typical electoral conjoint design (control design), y_{ij} is coded as 1 if profile j is selected and 0 if it is not. In contrast, for respondent i who received our proposed electoral conjoint design (treatment design), y_{ij} is coded as follows: if respondent i selects either Candidate A or B, the chosen profile is coded as 1 and the unchosen profile as 0. If respondent i casts a protest vote, both profiles are coded as 0. If abstention is chosen or the question is skipped, both profiles are coded as missing values.

5.3 Results

Overall, our two conjoint studies show that around half of the respondents choose to cast a protest (blank/null) vote or abstain at least once when these options are available. After implementing the proposed conjoint design, the reported estimates of AMCEs either increase, decrease, change signs, or alter their statistical significance compared to those obtained using the forced-choice candidate conjoint design.

5.3.1 Conjoint Study 1

In Conjoint Study 1, we replicate the profile setups of hypothetical candidates running for U.S. president used by Hainmueller et al. (2014). Respondents in the treatment group receive two additional options: casting a blank/null vote and abstaining, and they are also able to skip the choice-based question. In contrast, the control group can only select between Candidate 1 and Candidate 2 without the chance to skip the question. Among the 1,351 respondents assigned to the treatment design, 391 cast at least one protest (blank/null) vote, 424 abstained from voting at least once, and 671 (around half) chose either a protest vote or abstained at least once.[8]

We further look into the differences between AMCEs obtained from the two designs. Figure 5 shows our results. The left panel of Figure 5 presents the resulting AMCEs under the forced-choice (control) design, the middle panel presents those under the treatment design, and and the right panel presents the point estimates of the differences in AMCEs for each attribute level between the two experimental designs, along with their 95 percent confidence intervals based on standard errors robust to clustering at the respondent level.

Some remarkable differences in AMCEs are found in the age and income attributes. For example, the estimated AMCE for Age 45 (vs. Age 36) is 0.03 (with a clustered standard error of 0.01) when respondents are assigned to the control design. However, the AMCE for the same attribute

[8] As we allow respondents in the treatment design to skip choice-based questions, 43 respondents skipped at least one choice-based question throughout the survey. If we consider this number, more than half, 689, chose either a protest vote, abstained, or refused to answer at least once.

drops to -0.01 (0.01) and becomes statistically insignificant when respondents are assigned to the treatment design. This difference indicates a substantial design effect on a single attribute level. Similarly, the pattern holds for many other attribute levels, such as Age 52 and Age 68 (vs. Age 36) and Income $54K, $65K, and $210K (vs. $32K) at the 95% significance level. This suggests that a politician's income is not a strong predictor of voter selection.

It is important to note that even though some attributes show no significant treatment effects, the treatment design can still substantively challenge our interpretation of the results. For instance, some AMCEs that are statistically insignificant in the control design become significant in the treatment design, such as Catholic (vs. No Religion), and vice versa, such as, Income $5.1M (vs. $32K). This finding further confirms that there is a high chance that results from typical forced-choice conjoint designs in the study of electoral preferences might be challenged if we adopt a more conservative design that allows respondents (voters) to abstain or cast a protest vote, as they do in the real world.

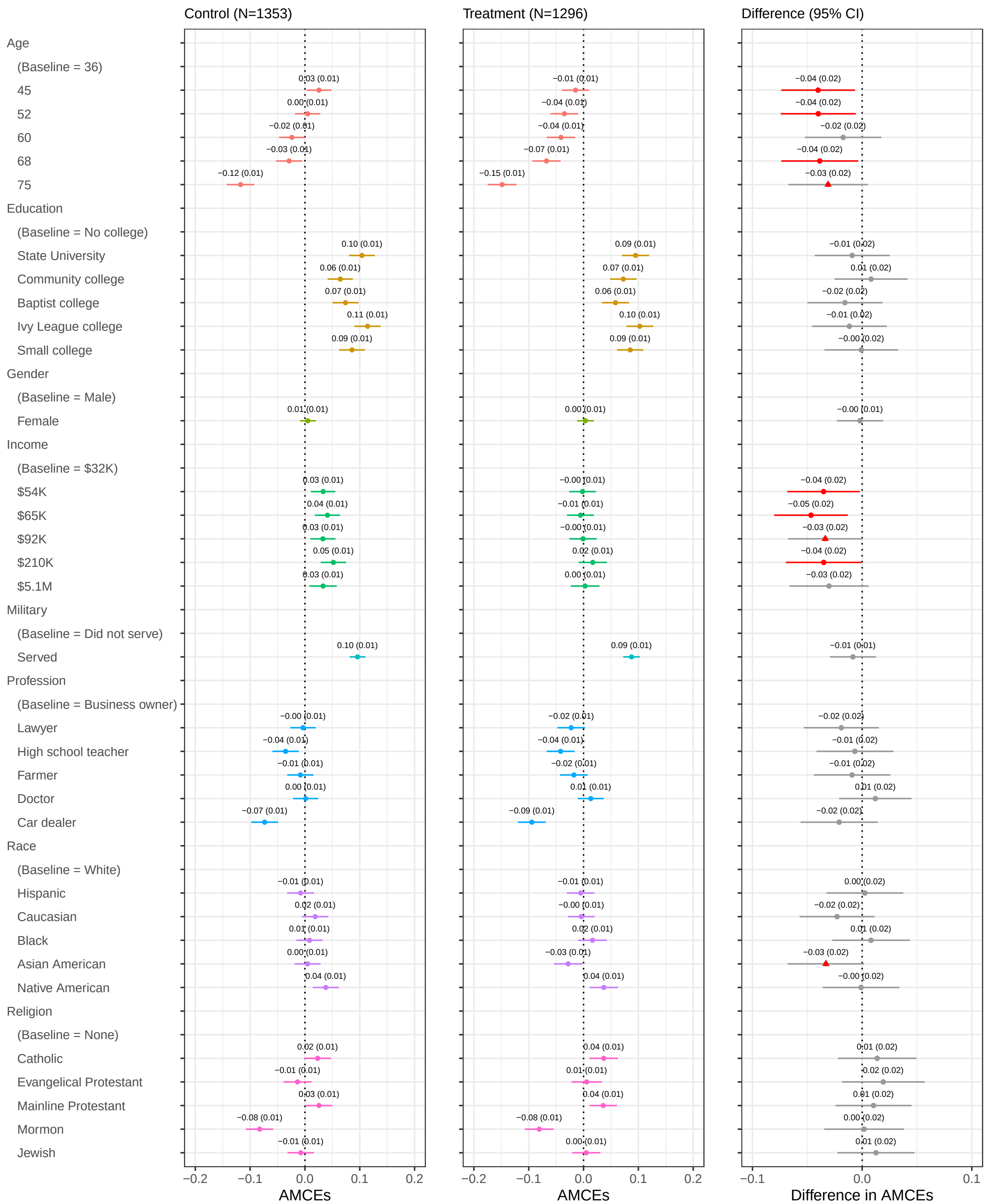

Figure 5: AMCEs of Presidential candidate attributes, by design: The left and middle panels show the results for the Presidential candidates under the control and treatment designs. The rightmost panel shows the differences in AMCEs for each attribute level, in which horizontal bars represent 95% confidence intervals robust to clustering at the respondent level. The red dots with red bars indicate significance at the 95% level, whereas the red triangles with gray bars indicate significance only at the 90% level. The gray dots with gray bars indicate significance at neither conventional level.

5.3.2 Conjoint Study 2

For Conjoint Study 2, we replicate the profile setups of hypothetical candidates running for the House of Representatives as used by Peterson (2017). Although respondents complete both studies in a random order, they remain with the same treatment assignment throughout the entire survey. Similar to our findings in Conjoint Study 1, a significant proportion of respondents opted for additional choices, such as casting a protest vote or abstaining, when these options were available. Of the 1,351 respondents assigned to the treatment design, 427 cast at least one protest (blank/null) vote, 479 abstained from voting at least once, and more than half, 741, either cast a protest vote or abstained at least once.

The differences in estimated AMCEs exhibit a pattern similar to those found in Study 1, while the findings of Study 2 are mostly significant at the 90% confidence level. The differences in AMCEs between the treatment and control designs vary between -0.03 and 0.02. Nonetheless, if we shift our attention to the findings, we can observe that many estimates for attribute levels alter their statistical significance compared to those obtained using the forced-choice candidate conjoint design. For example, using the forced-choice design, researchers might conclude that, holding everything else equal, Congressional candidates aged 46 have a higher chance of being selected, while candidates aged 74 are at a disadvantage compared to their counterparts aged 28. Nonetheless, results from the treatment design indicate that there is no particular age advantage in terms of being chosen as a Representative.

Similarly, by relying on the forced-choice design, scholars can also reach completely different conclusions about the impact of family status on a candidate's chances of winning an election. For instance, findings from the typical candidate design suggest that candidates who have been married (married or divorced) enjoy a higher chance of being selected than those who have never been married. However, findings from our proposed design question this conclusion by showing that family status does not play a significant role in voters' preferences. Conversely, the treatment design suggests that female candidates have an advantage over male candidates when all else is equal, a pattern not statistically significant in the control group.

To sum up, these findings collectively suggest that the design of evaluation questions in candidate conjoint studies can substantially influence the estimated AMCEs and, consequently, the conclusions drawn about voters' electoral preferences. The use of an unforced-choice design that more closely mirrors real-world voting options, such as allowing respondents to abstain or cast a protest (blank or null) vote, may provide more accurate insights into voter behavior.

As a note, readers might observe larger differences between the treated and control conditions when comparing the presidential and congressional conjoint experiments. A plausible explanation for this pattern could be that the presidential candidate conjoint included multiple relevant attributes, while the congressional candidate conjoint featured one particularly salient attribute for all types of voters (i.e., abortion). However, since researchers cannot predict in advance which attribute will primarily drive the results, we advise caution and recommend always including alternative options, such as abstaining or casting null/blank ballots, for respondents.

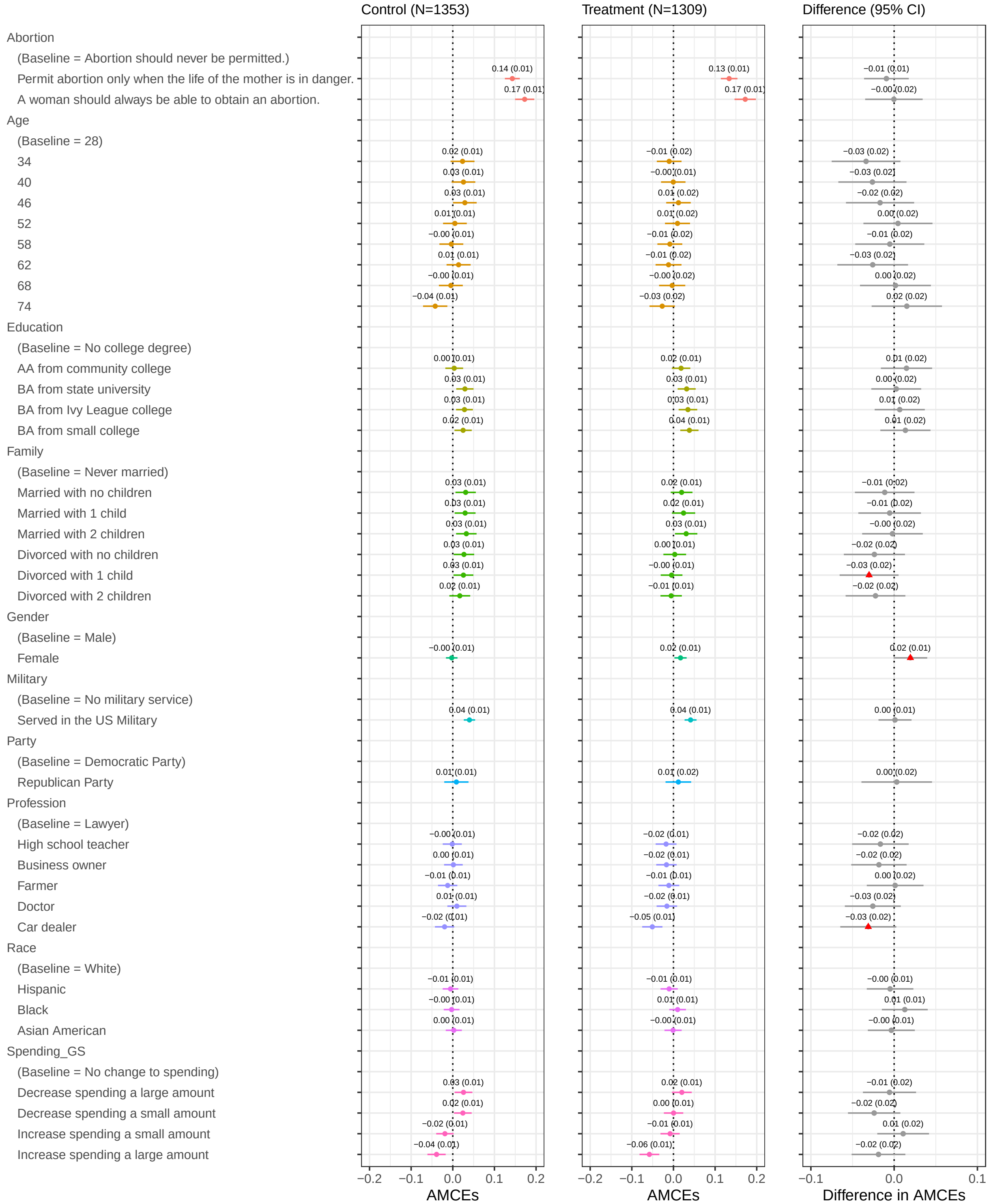

Figure 6: AMCEs of Congressional candidate attributes, by design: The left and middle panels show the results for the Congressional candidates under the control and treatment designs. The rightmost panel shows the differences in AMCEs for each attribute level, in which horizontal bars represent 95% confidence intervals robust to clustering at the respondent level. The red dots with red bars indicate significance at the 95% level, whereas the red triangles with gray bars indicate significance only at the 90% level.The gray dots with gray bars indicate significance at neither conventional level.

6 Practice Guidelines

Political scientists increasingly rely on conjoint experiments to understand voters' decisions in electoral studies. However, when designing these experiments, researchers often require participants to make a forced choice between hypothetical candidates, ignoring real-world options for voters to abstain or cast a protest (blank or null) vote. This discrepancy between experimental design and real-world scenarios can lead to misclassification and external validity biases, introducing unpredictable distortions that may lead to underestimated or overestimated effects, sign changes, or altered statistical significance in the estimates.

To reduce potential biases, researchers should design voting choices in electoral conjoint experiments to closely mirror those in the target elections under study. In this section, we make three practical suggestions.

1. During the research design phase, researchers should ensure that the voting options to be offered in conjoint experiments realistically reflect the voting choices available to voters, whether in a mandatory or voluntary voting system, based on substantive knowledge of the specific context. They should carefully decide on the number and configuration of options, such as forced-choice or unforced-choice, to align with the real-world voting environment. For instance, if examining electoral preferences in a target election with two candidates in a country with voluntary voting rules, researchers could include additional options like a protest (blank or null) vote and abstention, while disabling the forced-response requirement for choice-based outcome questions, as demonstrated in Section 4. Consider including options such as "None of the Above" or excluding the option to abstain as needed, depending on the electoral system being studied.

2. Regardless of whether the target election operates under a voluntary or mandatory voting system, we recommend researchers to consider setting the rating-based questions as forced-response questions. Since we suggest leaving the choice-based questions as unforced-

response in target elections with voluntary voting rules, there might be instances where respondents do not select any of the four options: Candidate 1, Candidate 2, abstention, or a protest vote. We interpret respondents skipping the questions as either a lack of cooperation in the survey or a mistake. Assuming respondents tend to choose the profile with higher preference, using forced-response requirements in rating-based questions can help researchers infer their plausible voting options, allow for robustness checks, and make the conjoint design more flexible.

3. It is recommended to add pre-treatment questions asking about respondents' past voting records and future voting propensity. Although adding the abstention option allows respondents who consistently abstain from voting in real-world elections to opt out if they prefer, pre-treatment questions assessing their overall voting participation can allow researchers to identify the group of consistent non-voters. This allows for robustness tests to determine if the conclusions hold when including or excluding them from the sample.

Additionally, researchers can incorporate our practical recommendations alongside recent advancements in the literature on conjoint analyses. For instance, using the real-world target profile distribution suggested by de la Cuesta et al. (2022) instead of a uniform distribution can improve external validity. Moreover, accounting for intra-respondent reliability and applying the design-based bias correction approach proposed by Clayton et al. (2023) can help mitigate measurement errors caused by respondent inattentiveness. Combining these strategies can contribute to more accurate and generalizable findings in electoral studies.

7 Concluding Remarks

Elections are a key component of democracies (Dahl, 1971). They provide a method for choosing leaders (Schumpeter, 1942), regulate political competition, and enable the peaceful resolution of political conflicts (Przeworski, 1991). As a result, understanding how voters make electoral decisions has become a crucial endeavor in the study of democratic politics (Campbell et al., 1980;

Fiorina, 1981; Achen and Bartels, 2016). However, researching this topic is challenging because traditional approaches often fail to consider that electoral choices are based on multidimensional decisions and do not accurately capture real-world decision-making.

Conjoint experiments for studying electoral choices allow researchers to directly address these two limitations. Researchers can study voters' multidimensional preferences by evaluating the impact of multiple candidate attributes on voting decisions simultaneously (Hainmueller et al., 2014; Bansak et al., 2023). Additionally, these methods have been validated against behavioral benchmarks, enhancing their external validity (Hainmueller et al., 2015).

Despite these advances, electoral conjoint experiments often require respondents to make forced voting choices that may not accurately reflect their valid preferences in the elections of interest, leading to measurement errors. People can abstain, or vote null or blank in an election. However, these options cannot be captured by traditional forced-choice conjoint experiments.

Through theoretical, simulated, and experimental evidence, we demonstrate that these errors in forced-choice electoral conjoint designs can introduce significant and unpredictable biases – misclassification errors and external validity bias — in estimating both descriptive and causal effects.

To address these issues, we propose an unforced-choice design that better mirrors the actual voting choices available in the target elections, informed by substantive knowledge. We provided two additional options for the choice question: "If I only have these two candidates, I will cast a blank/null vote" and "If I only have these two candidates, I will abstain," making the question unforced. When analyzing the data, both profiles are coded as 0 when respondents cast a protest vote or as missing values when they abstain or skip the question.

Although this paper primarily focuses on applying conjoint analysis to electoral studies, the unforced-choice design is highly adaptable to various settings and contexts. It can be extended to other research topics, such as immigration and hiring preferences, where decision-makers are not compelled to make a choice in every task; thus, forcing a choice could introduce bias into the estimation.

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A List of Published Articles Using Conjoint Analysis in Electoral Studies

Paper	Journal	Context of Study	Forced-Choice Design
Thomas et al (2024)	AJPS	India	Y
Greene (2022)	AJPS	Mexico	Y
Spater (2022)	AJPS	India	Y
Costa (2021)	AJPS	US	Y
Frederiksen (2022)	APSR	Many	Y
Teele, Kalla and Rosenbluth (2018)	APSR	US	Y
Carnes and Lupu (2016)	APSR	US, UK, and Argentina	Y
Rains and Wibbels (2023)	BJPS	India	Y
Loewen and Rheault (2021)	BJPS	US and Canada	Y
Campbell et al (2019)	BJPS	UK	Y
Schuler (Forthcoming)	CPS	Vietnam	Y
Laterzo (2024)	CPS	Argentina and Brazil	Y
Ventura, Ley and Cantú (2023)	CPS	Mexico	Y
Hankla et al (2023)	CPS	India	Y
Singh (2022)	CPS	Argentina	Y
Chou et al (2021)	CPS	Germany	Y
Carter (2021)	CPS	Peru	Y
Clayton et al (2019)	CPS	Malawi	Y
Agerberg (2019)	CPS	Spain	N
Arceneaux and Wielen (2023)	Elect. Stud.	US	Y
Busby (2022)	Elect. Stud.	US	Y
Kim (2021)	Elect. Stud.	Kenya	Y
Kang et al (2021)	Elect. Stud.	Australia	Y
Foulon and Reyes-Housholder (2021)	Elect. Stud.	Uruguay, Argentina and Chile	Y
Shockley and Gengler (2020)	Elect. Stud.	Qatar	Y
Vivyan et al (2020)	Elect. Stud.	Austria, Germany and UK	Y

Continued on next page

Paper	Journal	Context of Study	Forced-Choice Design
Badas and Stauffer (2019)	Elect. Stud.	US	Y
Arnesen, Duell and Johannesson (2019)	Elect. Stud.	Norway	Y
Gift and Lastra-Anadón (2018)	Elect. Stud.	US	Y
Matsuo and Lee (2018)	Elect. Stud.	UK	Y
Aguilar, Cunow and Desposato (2015)	Elect. Stud.	Brazil	Y
Frederiksen (2024)	JOP	US, UK, Czech, Mexico, and South Korea	Y
Driscoll and Nelson (2023)	JOP	US	Y
Portmann (2022)	JOP	Switzerland	Y
Henderson et al (2022)	JOP	US	Y
Eshima and Smith (2022)	JOP	Japan	Y
Weaver (2021)	JOP	Peru	Y
Magni and Reynolds (2021)	JOP	US, UK, and New Zealand	Y
Bakker, Schumacher and Rooduijn (2021)	JOP	US	Y
Schneider (2020)	JOP	Germany	Y
Chauchard, Klašnja and Harish (2019)	JOP	India	Y
Campbell et al (2019)	JOP	UK	Y
Peterson and Simonovits (2018)	JOP	US	Y
Eggers, Vivyan and Wagner (2018)	JOP	UK	N
Peterson (2017)	JOP	US	Y
Robinson (2023)	PolBeh	US	Y
Wood (2023)	PolBeh	US	Y
Visconti (2022)	PolBeh	Chile	Y
Magni and Reynolds (2022)	PolBeh	US, UK, and New Zealand	Y
Manento and Testa (2022)	PolBeh	US	Y
Rehmert (2022)	PolBeh	Germany	Y
Funck and McCabe (2022)	PolBeh	US	Y
Neuner and Wratil (2022)	PolBeh	Germany	Y
Saha and Weeks (2022)	PolBeh	US and UK	Y
Martin and Blinder (2021)	PolBeh	UK	Y
Rosenzweig (2021)	PolBeh	Kenya	Y
Blackman and Jackson (2021)	PolBeh	Tusnia	Y

Continued on next page

Paper	**Journal**	**Context of Study**	**Forced-Choice Design**
Mummolo, Peterson and Westwood (2021)	PolBeh	US	Y
Crowder-Meyer et al (2020)	PolBeh	US	Y
Leeper and Robison (2020)	PolBeh	US	Y
Ono and Burden (2019)	PolBeh	US	Y
Kirkland and Coppock (2018)	PolBeh	US	Y
Sances (2018)	PolBeh	US	Y
DeMora et al (2022)	POQ	US	Y
Lehrer, Stöckle and Juhl (2024)	PSRM	Germany	N
Dai and Kustov (2023)	PSRM	US	Y
Erlich and Beauvais (2023)	PSRM	Ukraine	N
Ono and Yamada (2020)	PSRM	Japan	Y
Mares and Visconti (2020)	PSRM	Romania	Y
Horiuchi, Smith and Yamamoto (2020)	PSRM	Japan	Y
Franchino and Zucchini (2015)	PSRM	Italy	Y
Carlson (2015)	WP	Uganda	Y

B Simulating Respondents' Voting Behavior in Forced-Choice Conjoint Analyses

In this section, we simulate plausible voting behaviors of respondents under both the typical and proposed designs, evaluating the potential of our proposed design to improve how we conduct electoral conjoint experiments.

B.1 Forced-Choice Design and Forced Voting Decisions

When an electoral conjoint experiment compels respondents to choose between two candidate profiles, they may approach the task strategically, particularly when they do not have a strong preference for either profile. Consider a nationally representative sample that includes respondents who have never participated in real-world elections and those who occasionally abstain from voting despite being eligible. What plausible decision-making process might these respondents follow if researchers deprive them of the option to abstain or cast a protest (blank or null) vote, forcing them to choose between two candidates?

For respondents who are eligible voters but consistently abstain from real-world voting, one possible coping approach when forced to choose is to appear *cooperative*. They might carefully evaluate each profile based on certain criteria in a way they would not in real-world elections. This behavior would not bias inference as long as, on average, they exhibit voting preferences similar to those of regular voters. However, existing studies have shown that the political preferences of non-voters and regular voters are distinct. If such respondents answer in a socially desirable way by heavily valuing or devaluing certain attributes, such as being more or less educated, experienced, younger or older, or holding certain policy positions, this could distort the aggregated preferences in an unpredictable manner. Consequently, the measurement error bias can either be upward or downward.

They might also approach the task by selecting between two candidates in a completely *random* manner, similar to flipping a coin. At first glance, this might seem inconsequential: if respondents whose true preference is to abstain are forced to randomly select between two candidates, their random choices would not cause an estimation problem when we are interested in the difference in aggregated preferences between the two profiles, given that measurement errors introduced by random choices can be canceled out by the law of large numbers. However, since the estimators of interest, AMCEs, are calculated by the weighted average of differences in means, we are essentially interested in the average preference, that is, which candidates with certain attributes are more likely to be selected on average. Forcing respondents who would prefer to abstain to make random voting choices introduces downward biases. This is because, in a forced-choice conjoint analysis, the difference in aggregated preferences between candidates is averaged over an entire sample that includes everyone, even though in reality, non-voters should never be counted.

Likewise, although regular voters actively engage in elections, this does not necessarily mean they always vote exclusively for one candidate or another. In reality, they might prefer to cast a protest vote (blank or null) or abstain as needed. However, under a forced-choice design, when they do not have a strong preference for either profile and are compelled to choose between the two, they might make a *random* choice or a *trade-off* choice by considering some second-order attributes to cope with the conjoint experiments.[1] This type of decision-making is likely when they find both candidates unattractive or indifferent. The estimates can be biased if respondents who would prefer to abstain are forced to make either a random or trade-off choice, or if respondents who would prefer to cast a protest vote are forced to make a random or a trade-off choice.

B.2 Simulation Studies

We now present bootstrap simulation studies to demonstrate how the typical forced-choice conjoint design can introduce biases while the proposed unforced-choice design improves estimation by reducing design-induced biases. To do this, we first assume that each respondent (voter) has a randomly drawn utility function, which is linear and additive. This function aggregates multiple dimensions into a composite criterion using the information provided by each candidate profile, defined by eight attributes with varying number of levels:[2]

Religion: l_1 ={Catholic, Protestant, None}

Education: l_2 ={State university, Small college, Community college, Ivy League college, No college}

Profession: l_3 ={Lawyer, High school teacher, Business owner, Farmer, Doctor, Car dealer}

Income: l_4 ={32K, 54K, 65K, 92K, 210K, 5.1M}

Race: l_5 ={Black, Asian American, Native American, Hispanic, Caucasian}

Gender: l_6 ={Male, Female}

[1]By secondary attributes, we mean that if respondents do not have a strong preference for either profile and are forced to make a choice, they might select certain attributes they care about more as the main criteria to decide which candidate they slightly prefer in the second order.

[2]These attributes and their values do not imply any substantive meaning in the simulation studies; they are used solely to aid in readability. We utilize the primary attributes and values from Hainmueller et al. (2014). However, we have omitted a few values due to memory constraints and the size limitations of the RStudio environment we are using.

Military Service: l_7 ={Served, Did not serve}

Age: l_8 ={45, 52, 60, 68, 75}

This produces 54,000 unique combinations of attribute levels, representing distinct candidates. Let each simulated respondent be indexed by $i \in \{1,...,N\}$. As defined, each conjoint profile is composed of eight attributes represented by the corresponding $\{l_1,...,l_8\} \in L$ factors, where each factor l has a total of D_l levels. For example, $D_1 = 3$ and $D_2 = 5$ represent the first and second attributes, *Religion* and *Education*, varying with three and five levels respectively. Formally, our latent utility function of respondent i for profile j is defined as follows:

$$U_{j(i)} = \sum_{l=1}^{L} \alpha_{L(i)} * \mathbf{X}_j + \varepsilon_{j(i)} \tag{1}$$

where $\mathbf{X}_j$ is a vector of D_l dummy variables for the levels of attributes contained in profile j. The coefficient $\alpha_{L(i)}$ denotes the specific utility value respondent i earns from any profiles containing a certain level of factor l. All the coefficients are randomly drawn at the individual level from a normally distributed population with predefined means and standard deviations, detailed in Table A3 in Appendix C.

We assume there are two types of eligible voters (respondents) in the experiment: regular voters and non-voters. These groups have distinct utility functions with different population means, reflecting their unique political preferences, as detailed in Section B.1. However, we do not assume that each respondent always chooses profile j when its latent utility is higher than that of the other profile j' in comparison. In some cases, some respondents might not be interested in politics or might find both candidates unattractive. As a result, they could be indifferent to both candidates and might choose randomly or based on other criteria.

Respondents are complex decision-makers, and a forced-choice design can compel them to make voting decisions that deviate from their unobserved true preferences in various ways. It is challenging to determine the degree and direction of measurement error bias introduced by a specific type of deviation when various forced decisions are mixed together. However, simulation tools allow us to fully model and customize respondent behaviors. Therefore, we analyze each type of forced voting decision separately as an ideal case to gain a deeper understanding of how a forced-choice design introduces measurement error biases. Table A2 summarizes the scenarios in which a forced-choice conjoint design might introduce measurement error biases.

We use a simulated sample of 1,001 respondents (eligible voters) for each simulation scenario. To evaluate the difference between forced-choice and unforced-choice electoral conjoint designs in estimates, we generate randomized conjoint pairs of profiles and simulate the choices of each of the 1,001 voters under both designs for every scenario. Following standard practices employed by

Table A2: Simulated Conjoint Scenarios

Scenarios	Type of Voters	True Preference	Forced-Choice Design	
			Observed Choice	Decision-Making
1	Non-Voters	Abstention	Candidate A or B	Random
				Cooperative
	Regular Voters	Candidate A	Candidate A	=
		Candidate B	Candidate B	=
2	Regular Voters	Candidate A	Candidate A	=
		Candidate B	Candidate B	=
		Abstention	Candidate A or B	Random
				Trade-Off
		Protest Vote	Candidate A or B	Random
				Trade-Off

applied researchers, we generate 10 pairs of profiles for each voter. For simplicity, in all scenarios and for all profiles in all pairs, we assume no interactions between attributes and independent uniform distributions of all the levels, ensuring that all 54,000 candidate profiles are equally likely, and all possible pairings between any two profiles are equally likely. Finally, using bootstrapping method, we iterate the simulated conjoint analysis for each simulation scenario under both designs 100 times to allow for a more robust evaluation of the statistical properties of our estimation procedure.

B.2.1 Simulation Study 1

In the first scenario, we assume a nationally representative sample where regular voters constitute 70% and non-voters make up 30%. We further assume that regular voters have clear preferences between each pair of candidates, always choosing the one with the highest utility. In contrast, non-voters either choose randomly or select a candidate in a socially desirable manner by heavily valuing or devaluing certain attributes, such as education, age, and military experience, under the forced-choice design. However, under an unforced-choice conjoint design, these non-voters would choose to abstain. To examine how forcing non-voters to vote introduces measurement error bias, we first assume that all non-voters only choose randomly, and then we consider the scenario where they select a candidate only in the socially desirable way.

We generate 100 random samples from bootstrapping for each decision-making processes non-voters and regular voters might follow in either forced- or unforced-choice conjoint experiments and compute the sample means and sampling distributions. The sampling distributions that we computed provide valuable insights into estimating average preference about which candidates with certain attributes are more likely to be selected. Since the AMCEs are unbiased estimators, the sampling distributions are centered around the true average preference of the population (voters). The spread of the sampling distribution indicates the amount of variability induced by sampling 100 simulated conjoint analyses.

In Figure A1, the left panel compares the mean estimates based on the simulated conjoint data in which non-voters who would prefer to abstain randomly select between candidates with those based on the unforced-choice data where abstention is allowed. Similarly, the right panel compares the mean estimates based on the simulated conjoint data in which non-voters who would prefer to abstain are forced to choose candidates in a socially desirable manner with those based on the unforced-choice data where abstention is available.

As can be seen from Figure A1, if respondents whose true preference is to abstain are forced to randomly select between two candidates, the forced-choice conjoint design tends to generate parameter estimates with smaller magnitudes — downward biases. In contrast, if respondents whose true preference is to abstain are compelled to choose candidates in a socially desirable manner, the forced-choice conjoint design tends to introduce either downward or upward biases, depending on which attributes they might value or devalue more. For example, in our simulation, we specifically assume and set up the socially desirable voting criteria such that, when forced to choose candidates, cooperative abstainers devalue candidates aged 52 and 68, while they largely value candidates educated at Ivy League colleges and those with military service. The results from the right panel confirm that using a forced-choice design causes upward and downward biases in the estimates of those attribute levels, to different degrees, where these biased selection criteria are assumed.

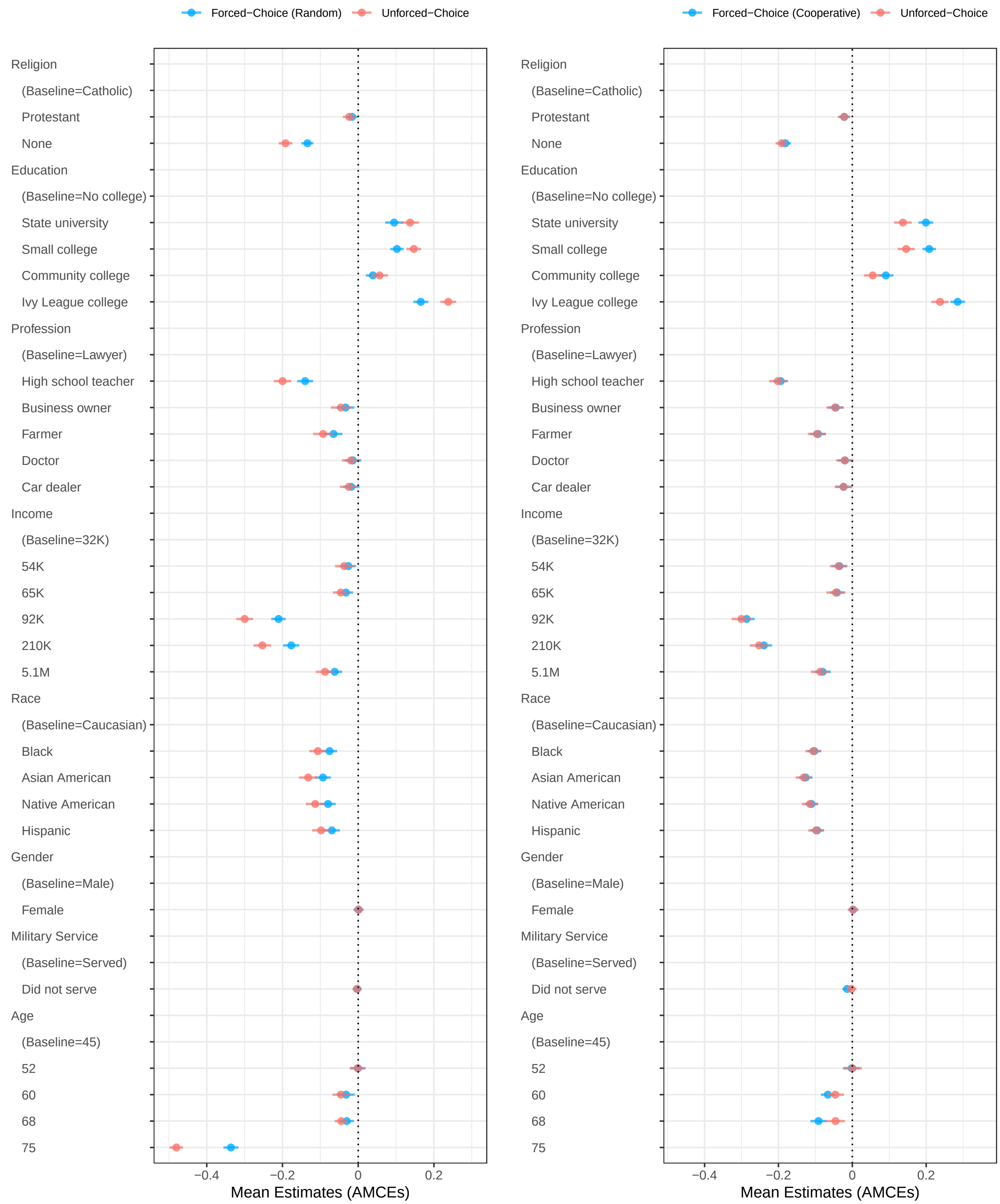

Figure A1: Simulated Choice-Based Mean Estimates in Scenario One. The estimated values based on the simulated forced-choice and unforced-choice conjoint data are represented by the blue and red dots and bars, respectively. All the dots show the mean estimates across all 100 simulated conjoint data sets from bootstrapping, and the bars denote ± 1.96 standard deviations and the minimum and maximum of the estimates.

B.2.2 Simulation Study 2

In the second scenario, we assume there are no voters who consistently abstain from voting; instead, all are regular voters. We further assume that a half of these regular voters possess clear preferences, always choosing the candidate with the highest utility and avoiding null votes or abstention under any design. The remaining half might prefer to abstain or cast a protest vote if such options are available when presented with a pair of candidates delivering utilities lower than the median candidate according to their own utility functions. We use this scenario to mirror the real-world cases where some people do not vote for the "lesser of two evils." We assume that when forced to make a decision under the forced-choice design, they might choose to vote randomly or make a trade-off decision by selecting the candidate with the highest utility based on the secondary attributes they care about most, such as profession and age.

In the following simulations, we first examine scenarios where half of the regular voters' true preferences are to abstain, but they must vote either completely randomly or in line with their secondary attribute-based preferences when both profiles have low utilities. We then explore cases where the same group's true preference is to cast a protest vote. Similar to Section B.2.1, we generate 100 random samples and compute the sample means and sampling distributions for each case. Figures A2 and A3 present the results for each case, respectively.

As shown in the left panel of Figure A2, when half of the regular voters' true preferences are to abstain but they vote randomly when confronted with both profiles having low utilities, it results in downward bias for the AMCE estimates. This bias is particularly evident for those attribute levels where respondents have a stronger preference compared to the reference level. However, if voters base their decisions on secondary attributes when both profiles have low utilities, the estimates for these secondary attributes and for those attribute levels where respondents do not have a strong preference compared to the reference level are less likely to be biased, while other estimates show varying degrees of bias.

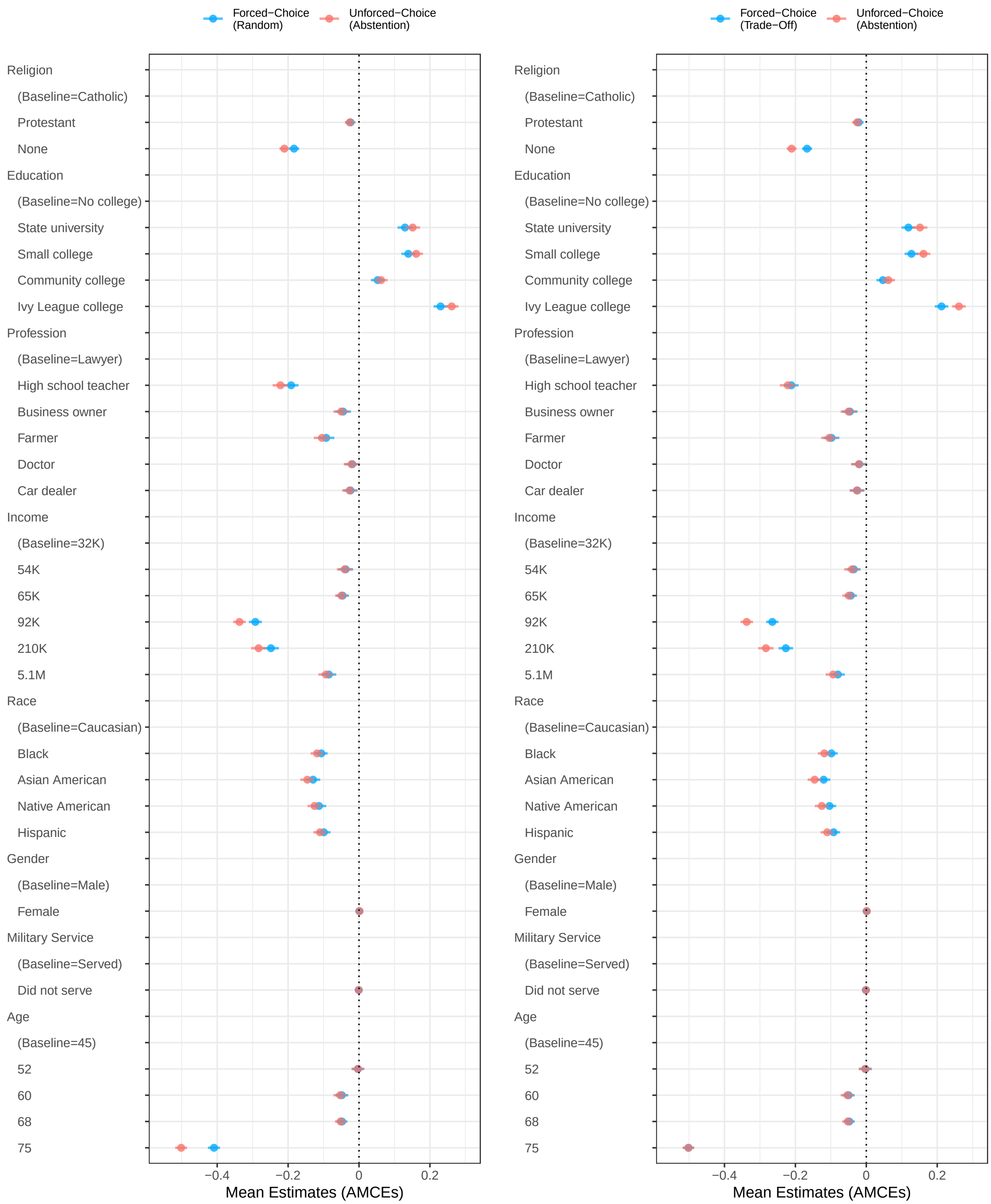

Figure A2: Simulated choice-based mean estimates in Scenario Two, where half of the respondents' true voting preference is to abstain. The estimated values based on the simulated forced-choice and unforced-choice conjoint data are represented by the blue and red dots and bars, respectively. All the dots show the mean estimates across all 100 simulated conjoint data sets from bootstrapping, and the bars denote ±1.96 standard deviations and the minimum and maximum of the estimates.

In the right panel of Figure A2, we simulate a scenario where half of the regular voters' true preference is to abstain, but they end up voting based on second-order attributes, such as profession and age, when faced with profiles offering low utility. Given our assumption that some respondents prioritize profession and age as second-order attributes, we observe that the estimates of AMCEs for these attributes remain largely unbiased. However, for other attributes, especially those where respondents exhibit a stronger preference compared to the reference level, there is a noticeable bias.

In the left panel of Figure A3, we consider a scenario in which half of the regular voters genuinely prefer to cast a protest vote, but they end up voting randomly when both profiles present low utilities. This randomness leads to a downward bias in the AMCE estimates, especially for attribute levels where respondents demonstrate stronger preferences compared to the baseline. Similar to the abstention scenario, this bias is more significant for attributes that voters prioritize heavily in their decision-making process.

In the right panel of Figure A3, we simulate a situation where half of the voters intend to cast a protest vote but instead rely on secondary attributes, such as profession and age, when neither profile provides significant utility. In this case, the estimates of AMCEs for these secondary attributes are mostly unbiased, reflecting the voters' focus on these specific factors. However, for attributes where respondents hold more pronounced preferences compared to the baseline, we observe varying degrees of bias, with some estimates deviating significantly from the true preferences, either downward or upward.

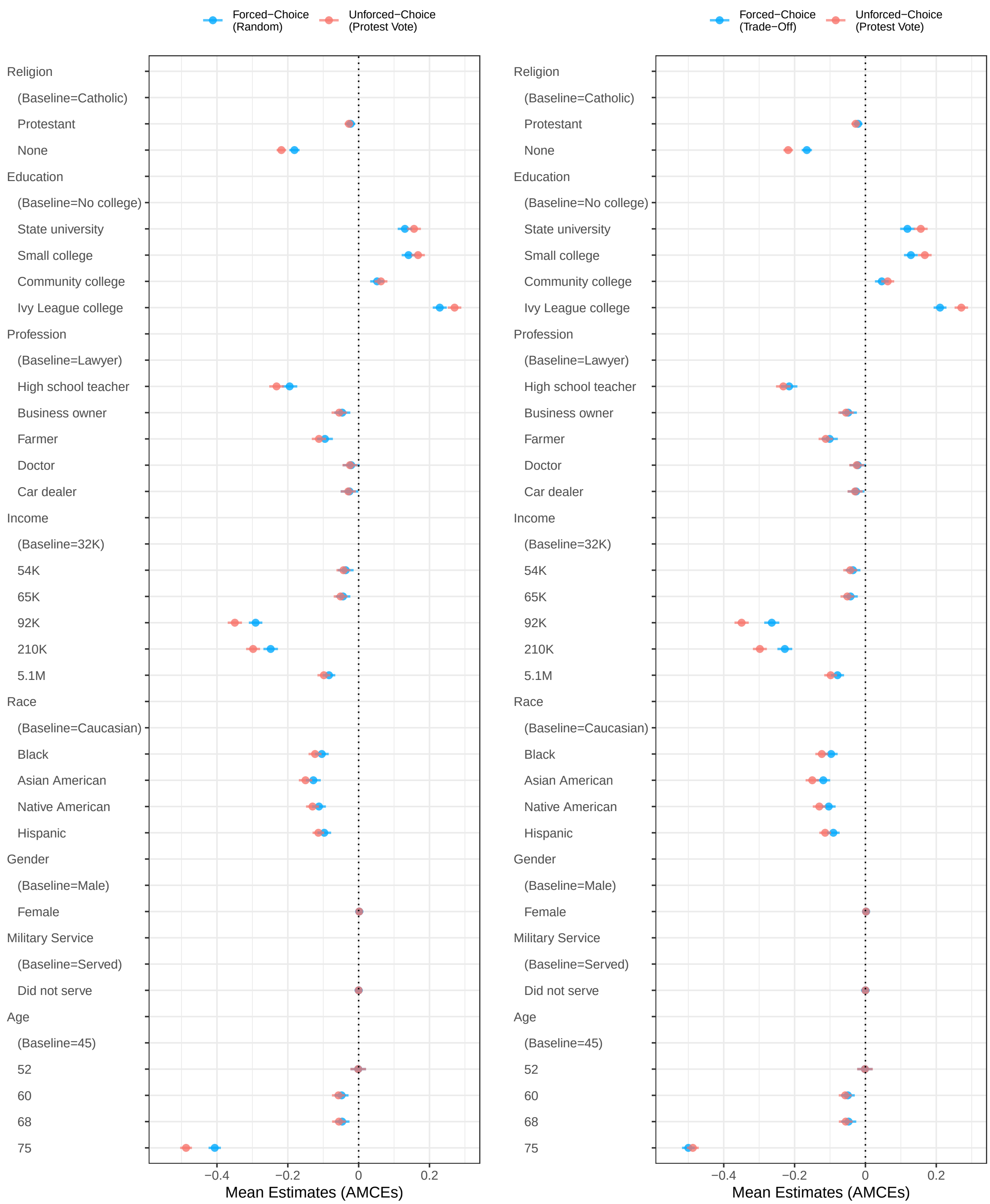

Figure A3: Simulated choice-based mean estimates in Scenario Two, where half of the respondents' true voting preference is to to cast a protest vote. The estimated values based on the simulated forced-choice and unforced-choice conjoint data are represented by the blue and red dots and bars, respectively. All the dots show the mean estimates across all 100 simulated conjoint data sets, and the bars denote ± 1.96 standard deviations and the minimum and maximum of the estimates.

C Simulation Set-Up

Table A3: Simulation Data-Generating Process: Parameters

	Regular Function	Socially Desirable Function
Religion-Catholic	$\alpha_{11(i)} \sim N(\mu_{11}, 1)$	same
-Protestant	$\alpha_{12(i)} \sim N(\mu_{12}, 1)$	same
-None	$\alpha_{13(i)} \sim N(\mu_{13}, 1)$	same
Education-No college	$\alpha_{21(i)} \sim N(\mu_{21}, 1)$	$\alpha_{21(i)} \sim N(\mu_2 \times -2, 1)$
-State university	$\alpha_{22(i)} \sim N(\mu_{22}, 1)$	same
-Small college	$\alpha_{23(i)} \sim N(\mu_{23}, 1)$	same
-Community college	$\alpha_{24(i)} \sim N(\mu_{24}, 1)$	$\alpha_{24(i)} \sim N(\mu_{24} \times 1.5, 1)$
-Ivy League college	$\alpha_{25(i)} \sim N(\mu_{25}, 1)$	$\alpha_{25(i)} \sim N(\mu_{25} \times 2, 1)$
Profession-Lawyer	$\alpha_{31(i)} \sim N(\mu_{31}, 1)$	same
-High school teacher	$\alpha_{32(i)} \sim N(\mu_{32}, 1)$	same
-Business owner	$\alpha_{33(i)} \sim N(\mu_{33}, 1)$	same
-Farmer	$\alpha_{34(i)} \sim N(\mu_{34}, 1)$	same
-Doctor	$\alpha_{35(i)} \sim N(\mu_{35}, 1)$	same
-Car dealer	$\alpha_{36(i)} \sim N(\mu_{36}, 1)$	same
Income-32K	$\alpha_{41(i)} \sim N(\mu_{41}, 1)$	same
-54K	$\alpha_{42(i)} \sim N(\mu_{42}, 1)$	same
-65K	$\alpha_{43(i)} \sim N(\mu_{43}, 1)$	same
-92K	$\alpha_{44(i)} \sim N(\mu_{44}, 1)$	same
-210K	$\alpha_{45(i)} \sim N(\mu_{45}, 1)$	same
-5.1M	$\alpha_{46(i)} \sim N(\mu_{46}, 1)$	same
Race-Black	$\alpha_{51(i)} \sim N(\mu_{51}, 1)$	same
-Asian American	$\alpha_{52(i)} \sim N(\mu_{52}, 1)$	same
-Native American	$\alpha_{53(i)} \sim N(\mu_{53}, 1)$	same
-Hispanic	$\alpha_{54(i)} \sim N(\mu_{54}, 1)$	same
-Caucasian	$\alpha_{55(i)} \sim N(\mu_{55}, 1)$	same
Gender-Male	$\alpha_{61(i)} \sim N(\mu_{61}, 1)$	same
-Female	$\alpha_{62(i)} \sim N(\mu_{62}, 1)$	same
Military Service-Did not Serve	$\alpha_{71(i)} \sim N(\mu_{71}, 1)$	same
-Served	$\alpha_{72(i)} \sim N(\mu_{72}, 1)$	$\alpha_{72(i)} \sim N(\mu_{72} \times 2, 1)$
Age-45	$\alpha_{81(i)} \sim N(\mu_{81}, 1)$	same
-52	$\alpha_{82(i)} \sim N(\mu_{82}, 1)$	same
-60	$\alpha_{83(i)} \sim N(\mu_{83}, 1)$	$\alpha_{83(i)} \sim N(\mu_{83} \times 0, 1)$
-68	$\alpha_{84(i)} \sim N(\mu_{84}, 1)$	$\alpha_{84(i)} \sim N(\mu_{84} \times -1, 1)$
-75	$\alpha_{85(i)} \sim N(\mu_{85}, 1)$	same
	$\varepsilon_{j(1)} \sim N(0, 1)$	same

where,

- μ_{1i} is a sequence of intervals from -3 to 8.
- μ_{2i} is a sequence of intervals from -5 to 8.

- μ_{3i} is a sequence of intervals from 0 to 6.
- μ_{4i} is a sequence of intervals from -4 to 5.
- μ_{5i} is a sequence of intervals from 0 to 8.
- μ_{6i} are two identical random numbers drawn from integers between 2 and 10.
- μ_{7i} is a sequence of intervals from 0 to 5.
- μ_{8i} is a sequence of decreasing intervals from -8 to 8.