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FOCAL POINTS, BELIEFS, AND DISTRIBUTIONAL PREFERENCES: AN EXPERIMENTAL ANALYSIS

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Focal points, beliefs, and distributional preferences: An experimental analysis

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Abstract

This paper experimentally examines the interplay between focal points, beliefs, and distributional preferences. Contrary to common wisdom, there is no detectable evidence that equal splits act as salient focal points in distributional voting contexts. Participants mispredict others' preferences: they overestimate egalitarianism when Pareto efficient options are available and underestimate it when efficiency gains come at others' expense. There is a clear correlation between individuals' preferences and beliefs (*false-consensus bias*), and participants are more egalitarian when they perceive a low probability of being pivotal (*cheap fairman talk*). These findings challenge assumptions about focal points and highlight belief-driven behavior in distributional settings.

Keywords: Distributional preferences; Focal Points; Equal split; Belief elicitation; False-consensus bias.
JEL Codes: C9, D3, D63, D7, D9.

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1. INTRODUCTION

The study of social and distributional preferences is crucial for understanding economic and political behavior with redistributive consequences. These preferences play a role not only in large social contexts but also within smaller groups such as firms, committees, and organizations. As a result, a substantial and growing body of theoretical and empirical research has examined the factors shaping social and distributional preferences. While many determinants, such as other-regarding preferences, are directly related to individuals' payoffs (e.g., Fehr and Schmidt 1999; Charness and Rabin 2002), others pertain to aspects of the decision environment that are not payoff-relevant, such as the framing of the situation (e.g., List 2007; Bardsley 2008). We refer to these factors as "non-consequentialist" throughout this paper.

A prominent non-consequentialist element of the choice environment is a focal point. Consider the distributional voting game presented in Table 1, taken from Bolton and Ockenfels (2006). The equal split in Option E may act as a "payoff-based focal point", which, through its salience, encourages the choice of Option E (Engelmann and Strobel 2006, p. 1919; Isoni et al. 2022, p. 113). Of course, there are several other reasons to prefer Option E over Option S, such as inequality aversion (Fehr and Schmidt 1999). Here, we refer specifically to a *pure focality effect*, where the exact equal split (rather than a similar alternative) may *further* strengthen motives — such as inequality aversion — that encourage the choice of E (Güth et al. 2001; Galeotti et al. 2022; Isoni et al. 2022).

Another prominent non-consequentialist element in some distributional contexts is individuals' beliefs about what others will choose. We define these beliefs as non-consequentialist when from a payoff perspective, players should choose their preferred option regardless of what they believe others will do. This situation arises in the example shown in Table 1. As with focal points, although these beliefs are non-consequentialist, they can still influence distributional preferences. For instance, in the game presented in Table 1, if Member A believes that Members B and C will vote for Option E, Member A may choose to vote for E as a "cheap" way to be perceived as fair (Engelmann and Strobel 2006).

In this paper, we employ an online incentivized preregistered experiment to investigate the role of these two non-consequentialist elements in distribu-

Table 1. A distributional voting game

	Option E	Option S
Member A	130	190
Member B	130	130
Member C	130	130

Notes: Members A, B, and C vote for Options E or S, and the option with most votes is selected.

tional voting contexts. We systematically manipulate simple distributional voting games similar to the one presented in Table 1 to address three main research questions: (i) Is an equal split a salient focal point that encourages egalitarian choices? (ii) Are beliefs about others' distributional (revealed) preferences accurate? (iii) Is there a relationship between these beliefs and individuals' distributional (revealed) preferences? To study the third question, we investigate two distinct phenomena: (iii.a) whether individuals believe that others are more likely to hold distributional (revealed) preferences similar to their own (a *false-consensus bias*), and (iii.b) whether they are more likely to vote for egalitarian options when they perceive a low probability of being pivotal (*cheap fairman talk*). We examine these research questions across various distributional voting games that involve a trade-off between equality and efficiency, both with and without a Pareto efficient alternative and with different configurations of winners and losers resulting from greater efficiency.

We find no detectable evidence that the equal split acts as a salient focal point in distributional voting settings. More specifically, participants are just as likely to choose egalitarian options (over more efficient alternatives) when an equal split option is available as when an "almost-equal" or "roughly-equal" split is offered instead. This finding is surprising given the common tendency to interpret the equal split as a salient payoff-based focal point (e.g., Güth et al. 2001; Engelmann and Strobel 2006; Galeotti et al. 2019, 2022; Isoni et al. 2022).[1]

In terms of belief accuracy, participants *overestimate* the likelihood that others will choose egalitarian options when a Pareto efficient alternative is avail-

[1]There is also a large and influential literature that interprets the equal split as a payoff-based focal point in bargaining contexts where — in contrast to our setting — options are not pre-specified (e.g., Roth et al. 1981; Roth and Murnighan 1982; Herreiner and Puppe 2010; Bolton and Karagözoğlu 2016; Camerer et al. 2019).

able. In other words, they think that others prioritize equality over efficiency (or self-interest) more than they actually do in such cases. By contrast, when efficiency gains come at the expense of some players (i.e., when a non-Pareto efficient alternative is available), individuals tend to *underestimate* the likelihood that others will choose egalitarian options.

Regarding the relationship between individuals' distributional (revealed) preferences and their beliefs about others' preferences, we find two clear patterns. First, there is a statistically significant and positive correlation between the preferences and beliefs of individuals in various positions that do and do not benefit from efficiency gains. This suggests that people are prone to a false-consensus bias irrespective of their position in distributional voting contexts. Second, participants who benefit from efficiency gains are more likely to vote for egalitarian options when they perceive a low probability of being pivotal. In other words, they tend to choose egalitarian options more often when they believe it is "cheap" to do so, consistent with concerns about social image and the phenomenon of cheap fairman talk.

Finally, our setting allows us to examine how preferences for equality, Pareto efficiency, and non-Pareto efficiency respond to changes in surplus size. Previous studies have shown that many individuals are willing to violate the Pareto principle to reduce inequality (e.g., Amiel and Cowell 1994; Beckman et al. 2002; Cetre et al. 2019). We find no detectable evidence that increasing the surplus by 15% or 30% increases support for Pareto efficient options, suggesting that inequality concerns driving Pareto inefficient choices remain stable regardless of the size of the efficiency gain. However, we do observe a marginally significant increase in support for *non-Pareto* efficient options when the surplus increases. This shift is mainly driven by individuals who benefit from surplus creation, even when it imposes substantial losses on others. These findings shed light on the conditions under which surplus size influences the trade-off between equality and efficiency.

1.1 Relation to the literature

Our study contributes to the growing literature on the factors shaping social and distributional preferences (e.g., Cruces et al. 2013; Schulz et al. 2014; Kuziemko et al. 2015; Roth and Wohlfart 2018; Fehr et al. 2020; Stantcheva 2021; Hoy and

Mager 2021; Melkonyan et al. 2021; Fehr and Vollmann 2022; Hope et al. 2023; Støstad and Lobeck 2023; Casoria et al. 2024). We provide novel evidence on two prominent non-consequentialist elements in distributional voting settings: focal points and beliefs.

Regarding focal points, our findings contribute to the extensive literature investigating the role of equality as a focal point (e.g., Roth et al. 1981; Roth and Murnighan 1982; Güth et al. 2001; Herreiner and Puppe 2010; Schulz et al. 2014; Bolton and Karagözoğlu 2016; Camerer et al. 2019; Galeotti et al. 2019, 2022; Isoni et al. 2022). The studies closest to ours examine a pure focality effect, that is, whether the mere presence (and *salience*) of an equal split influences decision making. Prior evidence on this question is mixed: while Güth et al. (2001) and Galeotti et al. (2022) find support for a pure focality effect in bargaining games, Schulz et al. (2014) do not find such an effect in the dictator game. However, to our knowledge, there is no existing evidence from distributional voting settings, which differ significantly from bargaining and dictator games. For example, in bargaining settings such as the ultimatum game, the equal split may be appealing due to beliefs about second-mover behavior. We are also the first to study whether results differ when the equal split is replaced with an almost-equal or a roughly-equal split, as well as whether individuals believe others to behave according to a pure focality effect. Our findings challenge the common assumption that the equal split serves as a salient payoff-based focal point. They show that pure focality based on equality is not a significant driver of decisions in distributional voting games and that using an exact equal split — as opposed to an almost or roughly equal alternative — has a limited impact on behavior. In an exploratory (non-preregistered) analysis, we also find that participants do not generally anticipate focality to be a key driver of others' behavior in distributional voting settings. These findings provide important insights for experimental methodology and theoretical models of social preferences (e.g., Levine 1998; Fehr and Schmidt 1999; Charness and Rabin 2002; Fisman et al. 2021), as well as for policy decisions on the inclusion of equal splits in settings with distributional consequences.

Regarding beliefs, despite the large and growing experimental literature on belief elicitation, relatively few studies examine the role of beliefs in settings where they are non-consequentialist, as defined above (e.g., Aguiar et al. 2009; Iriberri and Rey-Biel 2013; Yamagishi et al. 2013; Brañas-Garza et al. 2017). A

notable exception is Iriberri and Rey-Biel (2013), who find a clear relationship between participants' preferences and their beliefs about the choices of others in modified dictator games. To the best of our knowledge, we are the first to provide evidence on non-consequentialist beliefs in distributional voting settings. Unlike dictator games, where the receiver is passive, our setting makes others' behavior more salient, although beliefs should still not theoretically influence decisions. This distinction is important because "experimental economists now generally acknowledge that the dictator game is likely to involve more experimenter demand effects (Bardsley 2008) and is less robust than other games in which subjects interact with each other (Cooper and Kagel 2010)" (Fehr and Schneider 2010, p. 1322; see also Yamagishi et al. 2013).

In our setting, we find systematic deviations between participants' beliefs and actual behavior, as reported above. This reveals a significant gap between people's perceptions of the distributional preferences of others and others' actual choices, an important factor that may shape behavior in settings with distributional consequences. In addition, we find a clear positive correlation between individuals' distributional preferences and their beliefs about others' preferences, consistent with the false consensus bias observed in other contexts (Engelmann and Strobel 2000; Iriberri and Rey-Biel 2013; Blanco et al. 2014; Brañas-Garza et al. 2017).[2] Furthermore, we show that perceived pivotality — an important topic in the literature on voting behavior (e.g., Duffy and Tavits 2008; Shayo and Harel 2012; Ginzburg et al. 2022) — appears to influence decision making in distributional voting settings through cheap fairman talk. We also contribute methodologically by proposing a precise measure of perceived pivotality that can be employed using simple belief elicitation tasks and has not to our knowledge previously been employed with incentivized belief elicitation.

Finally, we contribute to the empirical literature on the trade-off between equality and efficiency, particularly studies on "money burning" and violations of the Pareto principle in distributional settings (e.g., Amiel and Cowell 1994; Zizzo and Oswald 2001; Beckman et al. 2002; Abbink and Sadrieh 2009; Fehr 2018; Cetre et al. 2019; Galeotti et al. 2019; Masclet and Rebière 2023). We extend this literature by showing that on one hand, Pareto violations remain

[2] As with most studies, we show a positive correlation but are silent in terms of the direction of causality (cf. Bischoff and Egbert 2013). We also note that Engelmann and Strobel (2000) defined a (truly) false consensus effect as weighting own decisions more heavily than those of a randomly selected person from the same population.

stable regardless of the size of the efficiency gain and that on the other, a surplus increase boosts support for non-Pareto efficient choices. By highlighting the asymmetry between these situations, our study provides new insights into the conditions under which surplus size influences the trade-off between equality and efficiency.

The remainder of the paper is organized as follows. In Section 2 we describe the experimental design. Section 3 presents our preregistered hypotheses, and Section 4 lays out our main results. Section 5 concludes the paper.

2. EXPERIMENTAL DESIGN

We conducted an online incentivized experiment with 10 independent rounds. Participants were randomly assigned to groups of three members at the beginning of each round, maintaining their position throughout the experiment (Member A, B, or C). The groups varied each round using *perfect strangers matching*; subjects were informed about this. In each round, members voted for one of two allocations of points and predicted the choices of others. One option was always an equal, almost-equal, or roughly-equal allocation; from now on we refer to this option as the *egalitarian* option (*E*). The other option always created an efficiency gain compared to option E in terms of total payoffs. We refer to this option as the *surplus-creating* option (*S*).[3] The full set of screens used in the experiment are shown in Online Appendix G.

2.1 The forecasting stage

In each round of this stage, participants in the position of Member A were asked to indicate the fraction of participants in the positions of Member B and Member C that they believed would vote for Option E (one question for each position). Members B and C completed an equivalent task. The task was incentivized, with earnings based on the accuracy of their predictions and calculated using the following quadratic scoring rule (QSR): $200 - 0.25 \times (\text{actual votes} - \text{their answer})^2$, with a minimum payoff bounded to 0.[4]

[3]Option E (S) was referred to as Allocation A (B) during the experiment.

[4]The QSR is a widely used method for eliciting beliefs that offers stronger truth-telling incentives than other scoring rules (Schlag et al. 2015). The QSR is not incentive-compatible in risky environments, as risk-averse individuals may report biased beliefs. Harrison et al. (2017) suggest that QSR performs better when eliciting distributions rather than binary probabilities,

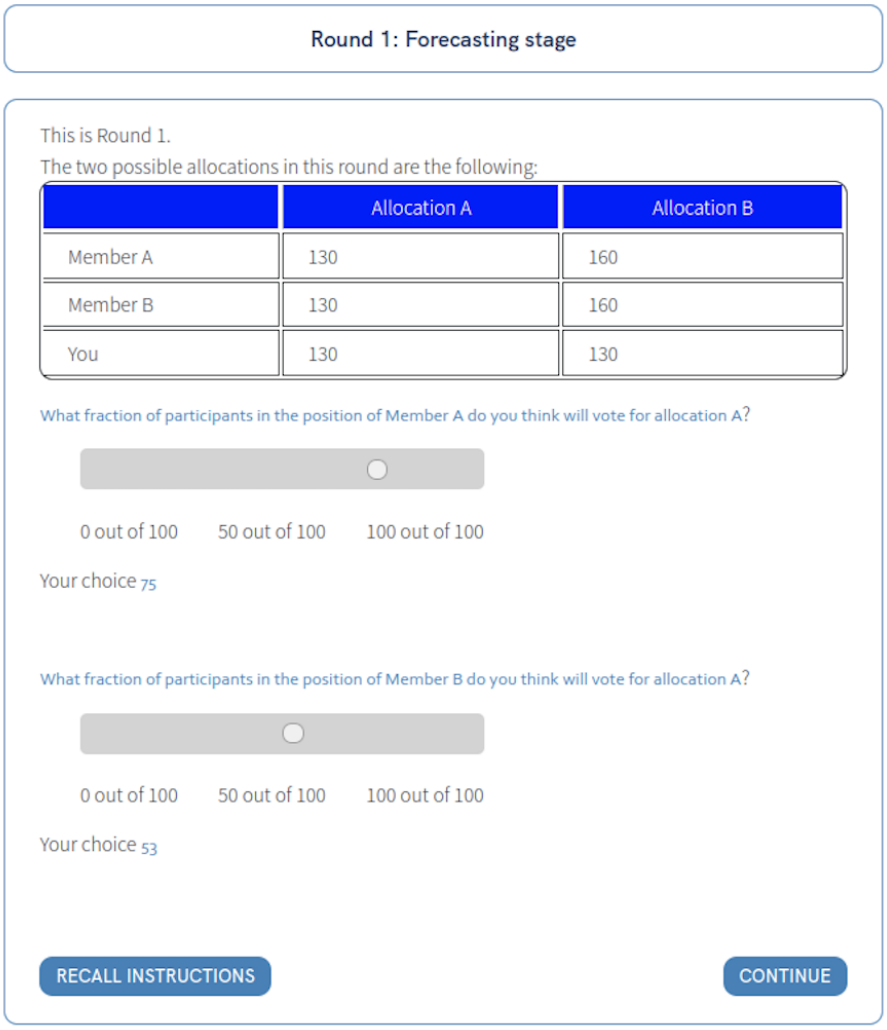


Figure 1. Forecasting screen

A screenshot of a forecasting stage is shown in Figure 1. Participants were able to use a slider without a default value and with labels that showed their chosen level on the screen. This reduces the complexity of the elicitation method (Schlag et al. 2015, pp. 482-485). To further simplify the task for participants, we asked them about probabilities as a population frequency as opposed to single-event or numerical probabilities (Schlag et al. 2015, pp. 482-485).

and paying small stakes, as we do, helps mitigate concerns about risk aversion (Schlag et al. 2015). QSR is also less complex than binarized scoring rules (Charness et al. 2021), making it better suited for online samples.

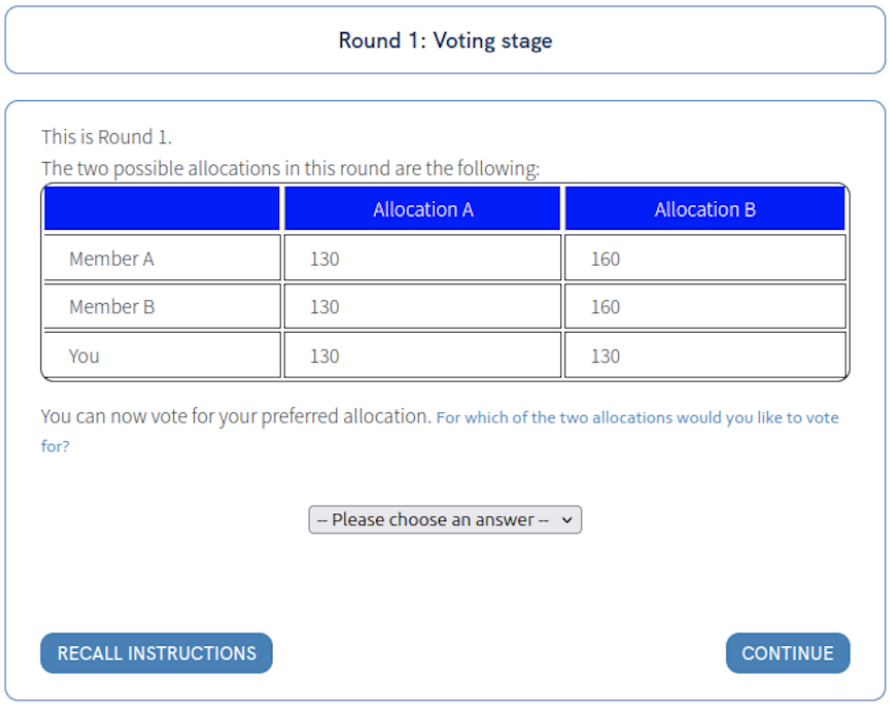


Figure 2. Voting screen

2.2 The voting stage

Following the forecasting stage, participants voted between the two allocations of points, Options E and S. Figure 2 shows a screenshot of the voting stage. Beliefs were elicited before voting in each round. A potential concern is that eliciting beliefs might influence subsequent behavior. However, the reverse is also true — actions can shape beliefs for various reasons (Schlag et al. 2015, p. 479). A common solution is to elicit beliefs at the end of the experiment, but this does not eliminate the issue of decisions influencing beliefs and is impractical in a multi-period setting like ours (Charness et al. 2021, p. 237). Moreover, existing evidence on whether beliefs influence decisions or vice versa is mixed and often contradictory (see Schlag et al. 2015 and Charness et al. 2021). Randomizing the order or implementing multiple treatments was not feasible due to budget constraints. Given these considerations, and knowing that players should theoretically choose their preferred option regardless of what they believe others will do, we opted to elicit beliefs first.

Table 2. Baseline games (payoffs in points)

	All games Option E	**Game I** Option S	**Game II** Option S	**Game III** Option S	**Game IV** Option S
Member A	130	190	270	270	160
Member B	130	130	10	90	160
Member C	130	130	170	90	130

2.3 The baseline games

We used four baseline games as shown in Table 2. Baseline Games I, II, and III replicate scenarios from Bolton and Ockenfels (2006), while Game IV has a Pareto efficient option that benefits two players (as opposed to only the "top" player in Game I). In all games, there is a trade-off between equality (Option E) and efficiency as surplus creation (Option S). It is worth noting that we have two baseline games with a "non-egalitarian" Pareto efficient alternative (Games I and IV) and two where efficiency gains come at the expense of some players (Games II and III). This distinction will feature in our analysis below.

2.4 Focal point and surplus manipulations

To investigate whether the equal split acts as a salient focal point, we introduce situations that disrupt the equal split in Option E. Table 3 presents these situations for Game I, with a1 serving as the baseline shown in Table 2. In a2, Option E becomes an almost-equal split, allowing us to test whether people choose E more often when an exact equal split is available (a1) than when it is not while payoffs for the three players remain equivalent across options (a2). This approach tests the salience interpretation of focality: that is, whether equality-driven choices, influenced by factors such as inequality aversion (Fehr and Schmidt 1999) and social image concerns (Andreoni and Bernheim 2009), are stronger when an exact equal split is available. Similar methods have been used in ultimatum and dictator games (Güth et al. 2001 and Schulz et al. 2014, respectively). We extend this approach with a3, a roughly-equal split, to determine whether a potential null result in a2 stems from the perceived proximity of the almost-equal split to the equal split.

We use a similar approach to examine how preferences for Pareto and non-

Table 3. Situations that alter the focal point and surplus (Game I)

	Option E	**Option S**
a1 (Equal split)	(130, 130, 130)	(190, 130, 130)
a2 (Almost-equal split)	(132, 129, 129)	(192, 129, 129)
a3 (Roughly-equal split)	(140, 125, 125)	(200, 125, 125)
a4 (30% surplus)	(130, 130, 130)	(250, 130, 130)
a5 (45% surplus)	(130, 130, 130)	(310, 130, 130)

Notes: (x,y,z) are payoffs for Members A, B, and C, respectively.

Pareto efficiency gains vary with the size of the surplus. In a1 of all games, Option S offers a surplus of 15% with respect to Option E. Situations a4 and a5 increase this surplus to 30% and 45%, respectively. Furthermore, these additional situations help us better evaluate the accuracy of beliefs and their relationship to distributional preferences.

We apply the same strategy for Games II, III, and IV (Table A.1 in Online Appendix A presents the payoffs for all games and situations). In the experiment, participants face all situations from two randomly selected games in a random order ($2 \times 5 = 10$). Our analysis relies on both aggregate data and data from the first round faced by participants. The aggregate analysis incorporates both within- and between-subjects comparisons, while the first-round analysis relies solely on a between-subjects comparison. Since participants are randomly assigned to different situations in the first round without prior exposure, the first-round analysis addresses potential concerns about order effects.

2.5 Payment

At the end of the experiment, the computer randomly selected one of the 10 rounds for payment for each participant. All rounds were equally likely to be selected. The computer then formed groups by matching the participants with the decisions made by two other participants in different positions in the round selected for payment for that participant. For instance, if a Member A is paid for round 8, that member will be randomly matched to the decisions made by one Member B and one Member C in the game and situation that Member A faced in round 8. Then, for each group, the computer randomly selected, with equal probability, whether the forecasting stage or the voting stage would count for

payment. In the case where the forecasting stage was selected for payment, the computer randomly selected one of the two forecasts made by the participant in that round, and payment was made using the formula explained above. In the case where the voting stage was selected for payment, the option with the most votes was selected. Participants were informed about this. After data collection, participants received a link through which they could check how their payment was calculated and the decisions of all group members in the round chosen for payment.

2.6 Procedures and sample observed characteristics

Data was collected via Prolific in February 2022, and the experimental design, empirical analysis, and hypotheses were preregistered before data collection started (https://aspredicted.org/kbxf-nrw2.pdf). Recruitment was done in a sample of nationals from the United States (US) and the United Kingdom (UK), currently living in the US/UK, with a minimum "approval rate" of 95% on Prolific.[5] We also screened to select participants with student status. We did this because we wanted to raise levels of comprehension of our experiment and for our results to be more comparable to previous related papers which predominantly use student samples. We initially planned to recruit only from the US but soon realized that given the participants' student status, reaching our target sample of 900 would be challenging. To address this, we expanded recruitment to students in the UK who were on Prolific. Therefore, in our analysis, we aggregate the data from both countries.

Our results are based on a sample of 901 participants (452 in the US and 449 in the UK). All these participants completed the experiment, and we took two additional steps to ensure high data quality. More specifically, all these participants correctly answered three comprehension questions in no more than two attempts and passed two attention checks introduced after rounds 3 and 7 of the experiment. Prospective participants who failed the comprehension questions or attention checks are not included in our sample.

The median completion time was approximately 10 minutes in both countries. Participants were paid a show-up fee of £1.30 and an average of £0.66

[5]Prolific provides the option to "reject" subjects and not pay for their participation (e.g. because of *speeding*). The approval rate is the percentage of studies for which a participant has been previously approved by the people who conduct the studies.

based on their decisions, equivalent to approximately £11.76 per hour, which is considered a "great" hourly rate according to Prolific. Participants made their choices asynchronously and were matched to other participants when the experiment ended, with payment made subsequently.

All participants in our sample were students at the time of the experiment, with most studying "sciences" (42% in the US and 40% in the UK), "humanities" (22% in the US and 18% in the UK), or "other disciplines" (22% in the US and 22% in the UK). Women comprised 68% of the subjects (70% in the US and 67% in the UK), and the mean age was 21 in both countries. Most subjects perceived the instructions of the experiment as clear or very clear (average 5.56 on a scale of 1 [very unclear] to 7 [very clear]). Table B.1 in Online Appendix B summarizes our sample observed characteristics.

3. HYPOTHESES

In this section, we report several hypotheses for observed behavior in our experiment, based on our preanalysis plan. The first two hypotheses concern potential behavioral changes across different situations:

Hypothesis 1. (Focal point hypothesis) There are more votes for the egalitarian Option E when an equal split is available than when an almost-equal (or roughly-equal) split is available.

Hypothesis 2. (Surplus gain hypothesis) There are more votes for the egalitarian Option E when a 15% surplus is available than when a 30% (or 45%) surplus is available.

As suggested by Hypothesis 1, we test differences in behavior between situation a1 (equal split) and situation a2 (almost-equal split) and between a1 and a3 (roughly-equal split). We also test a secondary hypothesis, which posits that the further the option is from the equal split, the stronger the effect of focal point salience: more specifically, we hypothesized that there are more egalitarian choices with an almost-equal split option (a2) than a roughly-equal split option (a3).

Similarly, for Hypothesis 2, we test differences in behavior between situations a1 (with 15% surplus) and a4 (30% surplus), as well as between a1 and a5 (45% surplus). We also test a monotonicity hypothesis according to which there

are more votes for the egalitarian Option E with the 30% surplus than with the 45% surplus.

Our next hypothesis concerns belief accuracy:

Hypothesis 3. (Belief accuracy hypothesis) There are no significant differences between observed voting behavior and beliefs about others' voting behavior.

In particular, we test whether there is no statistically significant difference between the proportion of subjects who actually vote for Option E and the average proportion of subjects who are believed to vote for Option E. We do this for all subjects' positions.

We also propose two hypotheses about the relationship between beliefs and decision making in distributional voting games:

Hypothesis 4. (False-consensus bias hypothesis) Voting behavior and beliefs about others' choices in the same (or similar) position are positively correlated.[6]

Hypothesis 5. (Cheap fairman talk hypothesis) Subjects who benefit from the surplus-creating Option S are more likely to vote for the egalitarian Option E when their perceived probability of being pivotal decreases.

Following our pre-analysis plan, we test these hypotheses within and across games. For Hypotheses 1 and 2, which could be influenced by order effects, we test them with aggregate data and data for round 1. We use Pearson chi-square tests and probit regressions with and without individual controls to test Hypotheses 1, 2, 4, and 5. We test Hypothesis 3 with two-sided one-sample tests of proportion. We do the latter tests per game and situation (a1 to a5) to obtain one observation per subject for each test. As preregistered, we also look at heterogeneity in terms of participants' *relative position* in the games. Since some subjects change relative position across games (e.g., Member B in Games II and IV), we divide subjects into two categories: (i) those who benefit from the surplus-creating Option S (Member A in Games I and III, Members A and C in Game II, and Members A and B in Game IV), and (ii) those who do *not* benefit from Option S (all others).

[6]We preregistered testing this hypothesis for "selfish" participants. However, that analysis seems unnecessarily restrictive, and we test the false-consensus bias for participants with various distributional preferences.

Table 4. Percentage of E votes in baseline of each game (a1)

	Game I	**Game II**	**Game III**	**Game IV**
Member A	14.29%	37.41%	32.09%	10.49%
Member B	37.08%	95.14%	89.97%	
Member C		48.55%		28.39%
Game average	29.81%	59.48%	72.06%	17.06%

* For Games I and III, Members B and C are in the same payoff position, so their results are averaged together under Member B.
$^{\diamond}$ For Game IV, Members A and B are in the same payoff position, so their results are averaged together under Member A.

4. MAIN RESULTS

Before presenting our main results, we show the patterns of behavior in our baseline games. Table 4 shows the percentage of votes for Option E in situation a1 of all games. A few patterns are worth noting. First, Games I and IV show that there is a significant minority of subjects who violate the Pareto principle to achieve equality. This is particularly the case among those who do not gain from efficiency, but there is a non-negligible number of members who do benefit from efficiency and still choose Option E in these baseline games. Second, our results show that splitting the Pareto efficiency gain between a majority increases the support for Pareto efficiency; this holds for both members who benefit and those who do not benefit from efficiency (Game I vs Game IV; $p < 0.01$ for both comparisons, Pearson chi-square tests). Third, Games II and III show that, unsurprisingly, more subjects oppose non-Pareto efficiency, with large minorities of subjects who benefit from efficiency voting for Option E in these baseline games. Finally, we note that our results for Games I to III are similar to those of Bolton and Ockenfels (2006). The largest difference is that Members B and C in Game I are more egalitarian in our experiment (37.08% E votes) than in Bolton and Ockenfels (2006), where that figure is 16.67% (see Table C.1 in Online Appendix C for a full comparison). The raw choice data for all games and situations is displayed in Table D.1 of Online Appendix D.

4.1 Is the equal split a focal point?

Figure 3 shows our main results from testing the focal point hypothesis. As the figure shows, there are only small differences between situations, and those differences are not systematic across games. None of the differences between situations a1 and a2 and between a1 and a3 is statistically significant (per game and across games; Pearson chi-square tests). Probit regressions with and without individual controls lend further support to these results against the focal point hypothesis (see Table E.1 in Online Appendix E).[7]

Order effects such as a desire for consistency across rounds could potentially explain these results. To control for this, we look at the results for round 1 where these effects cannot be at play. Once again, we find no statistically significant evidence for the equal split being a focal point (Pearson chi-square tests; see also the regression results reported in columns 7 and 9 of Table E.1 in Online Appendix E). Taken together, these results support the following finding:

Finding 1. The equal split is *not* a salient focal point in distributional voting games. Hypothesis 1 is rejected.

We now look at the secondary hypothesis that the further one is from the equal split, the stronger the effect of the focal point. We find no statistically significant difference in the choices of subjects who face the almost-equal split (a2) and the roughly-equal split (a3). This holds for Pearson chi-square tests and the Wald tests of equality of dummies for situations a2 and a3 in all regressions of Table E.1 in Online Appendix E. This result further supports the finding that the equal split is not a salient focal point in distributional voting settings.

It is also interesting to look at the results by subjects' *relative position*. For members who do not benefit from the surplus-creating Option S, we find no statistically significant differences between situations a1 and a2 and between a1 and a3 (Pearson chi-square tests per game and across games and probit regressions for all games and round 1 reported in columns 8 and 9 of Table E.1 in Online Appendix E).[8] For those who benefit from Option S, we find mixed ev-

[7] We have also conducted an exploratory (non-preregistered) analysis using equivalence tests to estimate the largest effect size we might have missed. We reject the null hypothesis of a meaningful difference between a1 and a2 (a1 and a3) for effects of 4% (3%) of E votes or greater for all games ($p < 0.5$ for both cases). Therefore, we are confident that the focality of the equal split does not influence behavior by 4% or more.

[8] For Members B and C in Game III, the difference between a1 and a2 is statistically signifi-

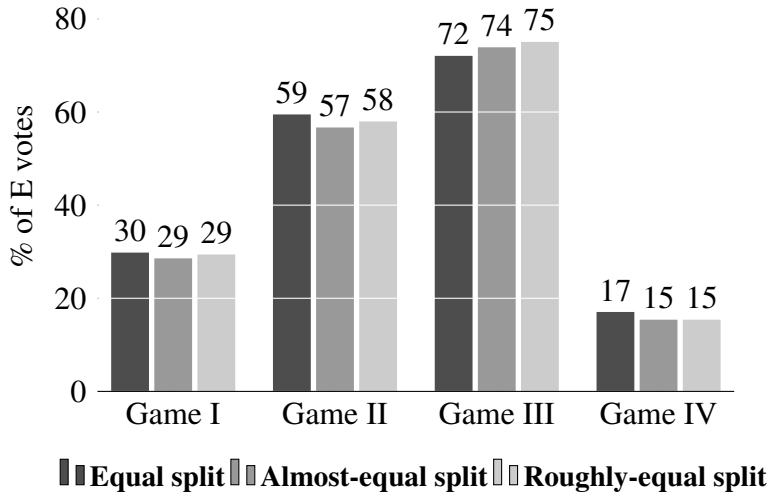


Figure 3. Percentage of E votes across equal split (a1), almost-equal split (a2), and roughly-equal split (a3)

idence. On one hand, they are statistically significantly more likely to vote for Option E in a1 than in a2 ($p = 0.033$, Wald test based on the probit regression reported in column 8 in Table E.1 of Online Appendix E). On the other, this difference is not statistically significant according to non-parametric Pearson chi-square tests; more importantly, it is not statistically significant for round 1, as reported in column 9 of Table E.1 in Online Appendix E. In addition, we find no statistically significant difference between their voting behavior in a1 and a3 (Pearson chi-square tests and probit regressions). Therefore, we conclude that there is no consistent evidence of a focal point effect for participants in these two relative positions.

Finally, we can check whether participants believed that others would behave in line with the focal point hypothesis. This analysis is exploratory, as it was not preregistered. Our findings indicate that participants generally did *not* expect behavioral differences between situations featuring an equal split (a1) and an almost-equal split (a2) (two-sided t-tests for all games, per game, and per game and relative position; see Table D.1 in Online Appendix D for raw choice data). The only exception is Game I, where participants anticipated that Members B and C would be more likely to vote for E in a1 than in a2 (50.14% vs. 46.70%; $p = 0.037$, two-sided t-test). A similar picture emerges when com-

cant ($p = 0.048$, Pearson chi-square test), but it goes in the opposite direction of the focal point hypothesis.

paring beliefs about behavior with an equal split (a1) and a roughly-equal split (a3). Again, participants only anticipated a focality effect in Game I, with statistically significant differences in beliefs about Members B and C (50.14% in a1 vs. 46.92% in a3; $p = 0.049$, two-sided t-test) and for all members (41.83% in a1 vs. 38.39% in a3; $p = 0.021$, two-sided t-test). In Game IV, we observe belief differences between a1 and a3 that are consistent with a belief in the focal point hypothesis, but they do not reach statistical significance in two-sided t-tests ($p = 0.079$ for Member C and $p = 0.078$ for all members). A similar trend is observed when considering expected behavior across all games, with a marginally significant result ($p = 0.064$, two-sided t-test). Overall, these results suggest that, with a few notable exceptions, participants do not generally expect focality to be a key driver of others' behavior in distributional voting settings.

4.2 Do Pareto inefficient choices persist with larger surplus?

In this section, we present our results showing how preferences for equality, Pareto efficiency, and non-Pareto efficiency respond to changes in surplus size. Figure 4 shows that differences between situations are small and not systematic across games. When looking at aggregate results for all games, we find no statistically significant differences for all rounds and for round 1 between a1 and a4, a1 and a5, and between a4 and a5 (Pearson chi-square tests and regressions with and without individual controls reported in Table E.2 in Online Appendix E).

Looking at results per game, we find no statistically significant effects of additional surplus for games with a Pareto efficient option (Games I and IV; Pearson chi-square tests and regressions with individual controls reported in Table E.2 in Online Appendix E). In other words, Pareto inefficient choices persist regardless of the size of the surplus.[9]

For games with non-Pareto efficient options (Games II and III), we find mixed evidence. One one hand, Pearson chi-square tests suggest that there are no statistically significant differences. On the other, we find a statistically signif-

[9]We have also conducted an exploratory (non-preregistered) analysis using equivalence tests to estimate the largest effect size we might have missed. We reject the null hypothesis of a meaningful difference between a1 and a4 (a1 and a5) for effects of 4% (5%) of E votes or greater for all games ($p < 0.5$ for both cases). We also reject the null hypothesis of a meaningful difference between a1 and a4 (a1 and a5) for effects of 3% (3%) of E votes or greater in Game I, and for effects of 5% (7%) between a1 and a4 (a1 and a5) in Game IV ($p \leq 0.5$ for all cases). Therefore, we are confident that in these games increasing the surplus by 15% (30%) does not influence behavior by 5% (7%) or more.

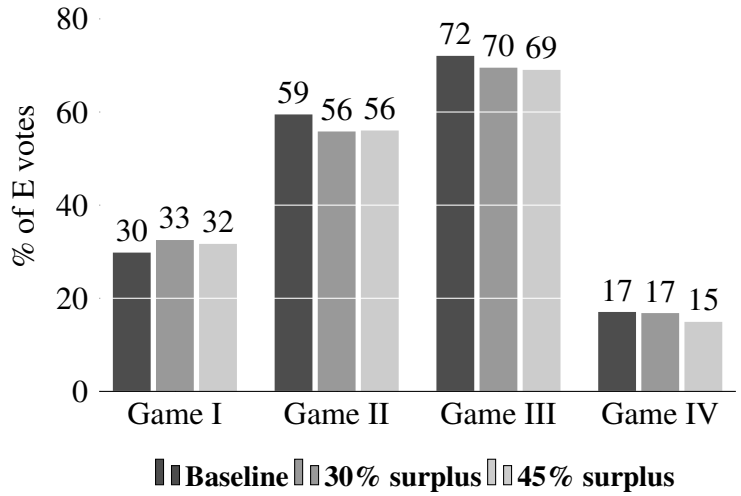


Figure 4. Percentages of E votes with baseline (a1), 30% surplus gain (a4), and 45% surplus gain (a5)

icant effect of an additional 15% surplus (a4) in Game II and marginally significant effects of an additional 30% surplus (a5) in Games II and III ($p = 0.041$, $p = 0.064$, and $p = 0.054$, respectively, regressions reported in Table E.2 in Online Appendix E). Further inspection shows that the latter effect of an additional 30% surplus gain (a5) is driven by those who benefit from Option S. More specifically, they are 2.9% to 8.2% more likely to vote for Option S in a5 than in a1 in Games II and III (columns 2 and 10 of rows 4, 6, and 8 of Table D.1 in Online Appendix D; these differences are statistically significant for Game III: $p = 0.006$, Wald test based on the regression reported in column 8 in Table E.2 of Online Appendix E when restricted to Game III). We summarize these results as follows:

Finding 2. Increasing the surplus by 15% or 30% does *not* raise support for Pareto efficient options (Games I and IV).

Finding 3. Increasing the surplus by 15% or 30% marginally increases support for non-Pareto efficient options (Games II and III), particularly from those who benefit from efficiency.

Therefore, Hypothesis 2 is partially supported.

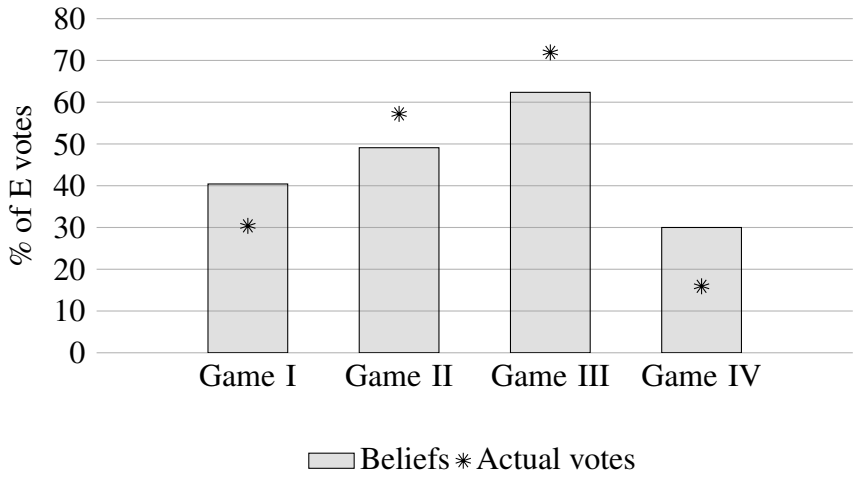


Figure 5. Beliefs about percentages of E votes and actual percentages of E votes

Notes: Data aggregated for all situations per game (a1 to a5). To calculate beliefs about one player (e.g., Member A), we average the beliefs of the players in the other positions in the game (Members B and C in the example).

4.3 Are beliefs accurate?

Figure 5 compares the actual percentage of votes for Option E with participants' beliefs about others' behavior, taking all situations for each game (Table D.1 in Online Appendix D shows raw data comparing beliefs and choices for all situations and relative positions). The figure reveals an interesting pattern. On one hand, participants *overestimate* the proportion of subjects who choose the egalitarian Option E against a non-egalitarian Pareto efficient option (Games I and IV; $p < 0.01$ for all two-sided one-sample tests of proportion per game and situation). In other words, when there are no losers from efficiency gains, participants are more likely to choose efficient allocations than others anticipate. On the other, participants *underestimate* the proportion of subjects who choose Option E against a non-Pareto efficient option (Games II and III; $p < 0.01$ for all two-sided one-sample tests of proportion per game and situation). In other words, when there are losers associated with surplus creation, participants are more egalitarian than what others anticipate. These results can be summarized as follows:

Finding 4. Subjects overestimate subjects' propensity to choose egalitarian options in games with a Pareto efficient option (Games I and IV).

Finding 5. Subjects underestimate subjects' propensity to choose egalitarian

options in games with a non-Pareto efficient option (Games II and III).

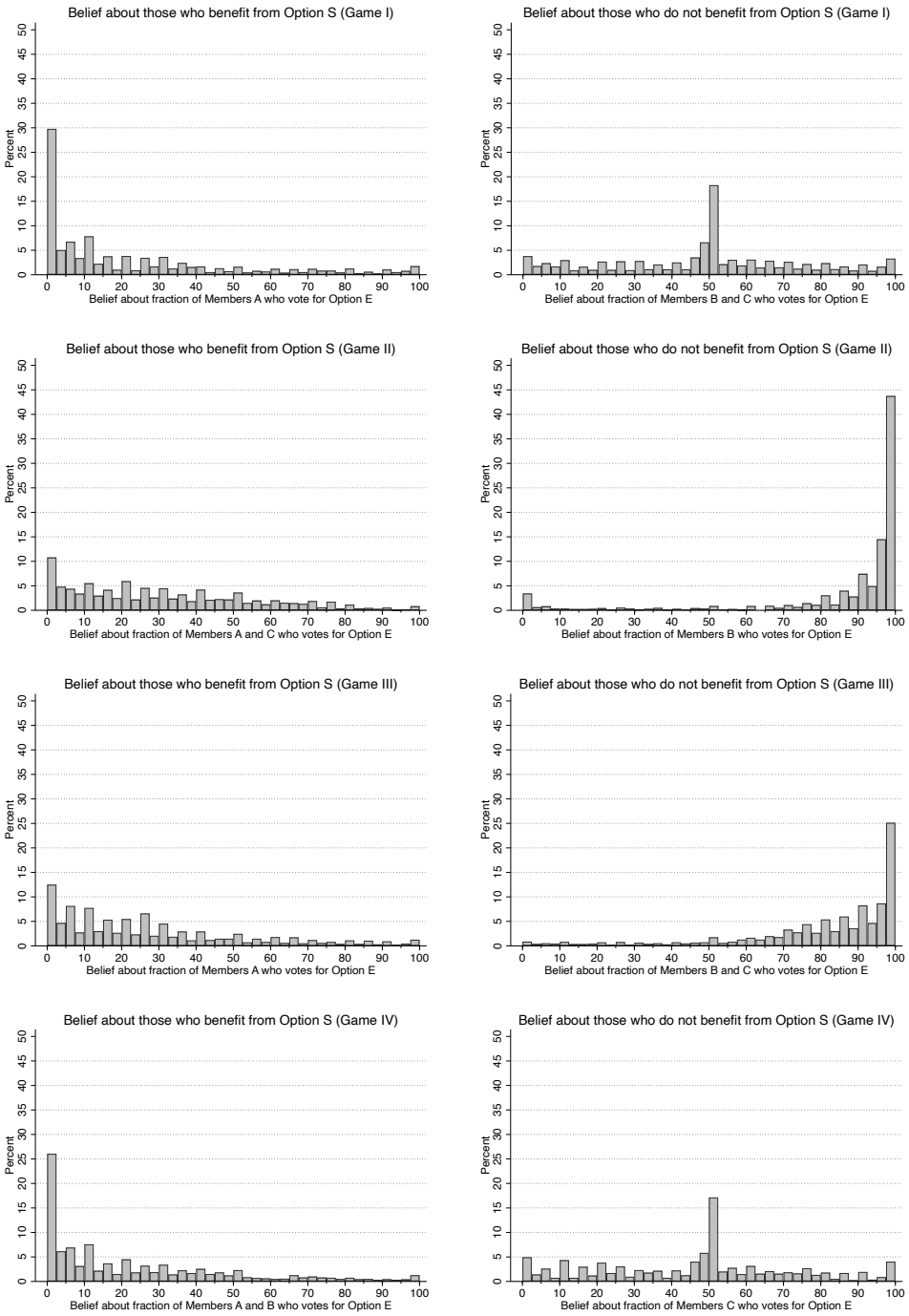
Thus, Hypothesis 3 is rejected. These findings are confirmed when we separate beliefs *about* those who benefit and those who do not benefit from Option S. More specifically, we find that subjects over- or underestimate the proportion of subjects who choose Option E in the same direction as above. The only exceptions are beliefs about Member A's behavior in Game III, for which Members B and C are accurate ($p > 0.1$ for all two-sided one-sample tests of proportion per situation), and beliefs about Member A in Game I, for which we find mixed evidence ($p = 0.1$ for two situations and $p < 0.01$ for three situations, two-sided one-sample tests of proportion per situation). All other two-sided one-sample tests of proportion per game and situation are significant at 5% (one) or at least 1% (all other cases).

What about heterogeneity in beliefs? The histograms in Figure 6 show the distribution of beliefs per game, separating (a) beliefs about those who benefit from Option S (left panes) and (b) beliefs about those who do not benefit from Option S (right panels). A few patterns are worth highlighting. First, in games with a Pareto efficient option (Games I and IV), there are clear modes in all panels. These modes suggest that a large share of participants believe that all or most others care only about their own material payoffs in these distributional voting settings. Second, in games with a non-Pareto efficient option (Games II and III), there is significant heterogeneity in beliefs about the behavior of those who benefit from efficiency. Third, in Games II and III, there is a strong consensus that 80% or more of those who do not benefit from Option S will vote for the egalitarian option that is in their monetary self-interest.[10]

4.4 How do beliefs and distributional preferences relate to each other?

In the previous subsection, we showed that beliefs about the behavior of others often overestimate or underestimate the weight individuals place on equality, efficiency, or self-interest, depending on the characteristics of the situation. In this subsection, we look at how such misperception of others' behavior may matter for preferences and behavior in situations with distributional consequences.

[10] In terms of beliefs *of* those who do or do not benefit from Option S, subjects who benefit from Option S are less accurate than subjects who do not benefit in their beliefs about others' voting behavior in Games II and III and more accurate in Game IV (see Table E.3 in Online Appendix E).



Notes: These figures report the density function of the perceived percentage of members benefiting (left) and not benefiting (right) from Option S who vote for Option E.

Figure 6. Heterogeneity in beliefs per game and relative position

More specifically, we analyze whether two effects linking preferences and beliefs that are prominent in the literature are relevant in distributional voting games: (i) false-consensus bias and (ii) cheap fairman talk.

4.4.1 False-consensus bias

False-consensus bias is a well-documented phenomenon in both psychology and economics. It refers to the tendency of individuals to believe that others are relatively more likely to share their own characteristics, preferences, or behaviors. In the context of our experiment, this means that participants are inclined to assume that others will make similar choices to their own, leading them to assign a high probability to others taking the same actions they themselves have taken.

To test for a false-consensus bias, we first construct an "egalitarian ranking" using the five within-subject decisions across situations in each game that ranks subjects on a scale from fully "egalitarian" (always vote for Option E) to fully "efficiency lover" (always vote for Option S).[11] For members who benefit from Option S, fully efficiency lover corresponds to fully "selfish". For those who do not benefit from Option S, fully egalitarian may or may not be in their monetary self-interest. We then follow Iriberri and Rey-Biel (2013) and report results on beliefs about subjects in the same (or similar) position. The main underlying reason is that a false-consensus bias is well defined when two people are in the same (or similar) position, while it is less clear when subjects are in different positions. This means that in our setting, we look at subjects who benefit from efficiency gains in Games II and IV (Members A and C in Game II and Members A and B in Game IV) and subjects who do not benefit from efficiency in Games I and III (Members B and C in Game I and Members B and C in Game III).

Table 5 reports the resulting egalitarian ranking, where 5 means always voting for Option E and 0 never voting for Option E. As shown in the table, about 80% of subjects always choose self-servingly in Games III and IV. In Games I and II, there is a bi-modal distribution of consistent choices with about 40% fully efficient lovers (self-servingly in Game II) and about 20% fully egalitarian.

Our main results on the presence of a false-consensus bias are shown in

[11] We do not use the ten within-subject decisions of each subject because (i) some subjects change relative position across games and because (ii) we would rank on the same scale subjects who made choices in games x and y and subjects who made choices in games z and w, where x,y,z, and w can take any values from I to IV.

Table 5. Egalitarian ranking: Consistency of voting for E across situations (a1 to a5)

Members:	**Game I** B & C	**Game II** A & C	**Game III** B & C	**Game IV** A & B
5	21.28%	23.12%	81.61%	3.00%
4	7.60%	7.19%	7.69%	2.62%
3	8.21%	10.31%	4.01%	2.25%
2	9.73%	9.06%	3.01%	4.12%
1	9.42%	9.69%	0.67%	10.11%
0	43.77%	40.62%	3.01%	77.90%
No. of subjects	329	320	299	267

Figure 7. The figure shows that for all games, there is a clear correlation between choice behavior (egalitarian ranking on the x-axis) and beliefs about others' voting behavior (y-axis). The observed upward-sloping trends are in line with the presence of a false-consensus bias (a flat line or downward-sloping trend would contradict the false-consensus bias hypothesis). The fact that this result holds for all games means that this bias is common across various positions in distributional voting games. The regression analysis reported in Table E.4 in Online Appendix E supports these findings. These results can be summarized as follows:

Finding 6. There is a false-consensus bias in distributional voting games. Hypothesis 4 is supported.

4.4.2 Cheap fairman talk

According to the cheap fairman talk hypothesis, subjects who benefit from the surplus-creating Option S are more likely to vote for the egalitarian option when their perceived probability of being pivotal decreases. This hypothesis is well defined for subjects who benefit from Option S. Therefore, we test it for Member A in Game I, Members A and C in Game II, Member A in Game III, and Members A and B in Game IV. To measure perceived pivotality, we calculate the following:

$$\textbf{Perceived pivotality}_A = \frac{f_B * (100 - f_C) + f_C * (100 - f_B)}{100} \tag{1}$$

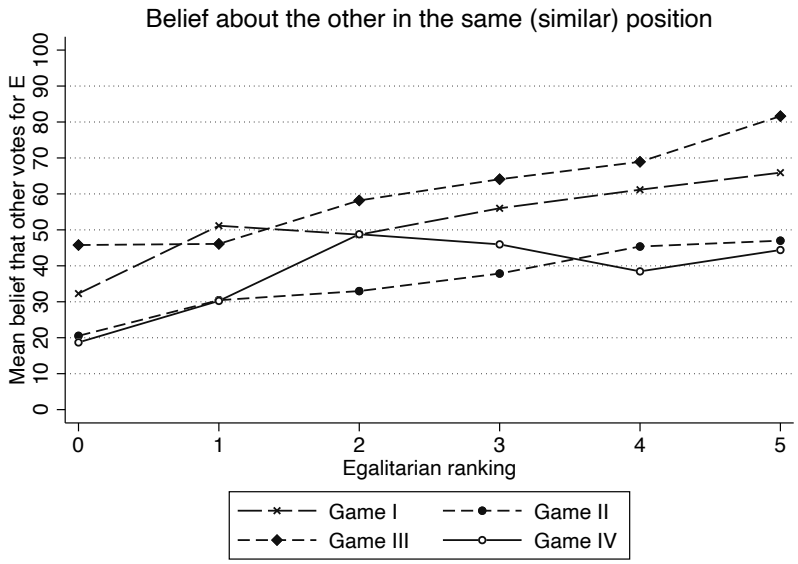


Figure 7. False-consensus bias per game

where f_B and f_C denote the proportion of Members B and C, respectively, that Member A believes will vote for Option E. Eq. (1) means that the more a participant believes that the two others will vote differently, the more a participant believes that he or she is more pivotal. To the best of our knowledge, we are the first to use this precise measure of perceived pivotality with incentivized belief elicitation.

Figure 8 shows our main results. On one hand, and contrary to the cheap fairman talk hypothesis, we find no evidence of such an effect in Games I, III, and IV ($p > 0.1$ for all tests, regressions reported in Table E.5 of Online Appendix E). On the other, we find evidence in line with this hypothesis for Game II. More specifically, we find that Members A are more likely to vote for the egalitarian option when they believe they are less pivotal ($p < 0.01$, Table E.5 of Online Appendix E). These results can be summarized as follows:

Finding 7. There is cheap fairman talk in distributional voting games, but only in particular settings (Game II). Hypothesis 5 is partially supported.

We conjecture that two potential reasons may largely explain this game-specific result. First, the average perceived probability of being pivotal is higher in Game II than in the other games, which may make participants take particular notice of their own pivotality in that game (means of 39.9, 64.9, 20.2, and 43.3

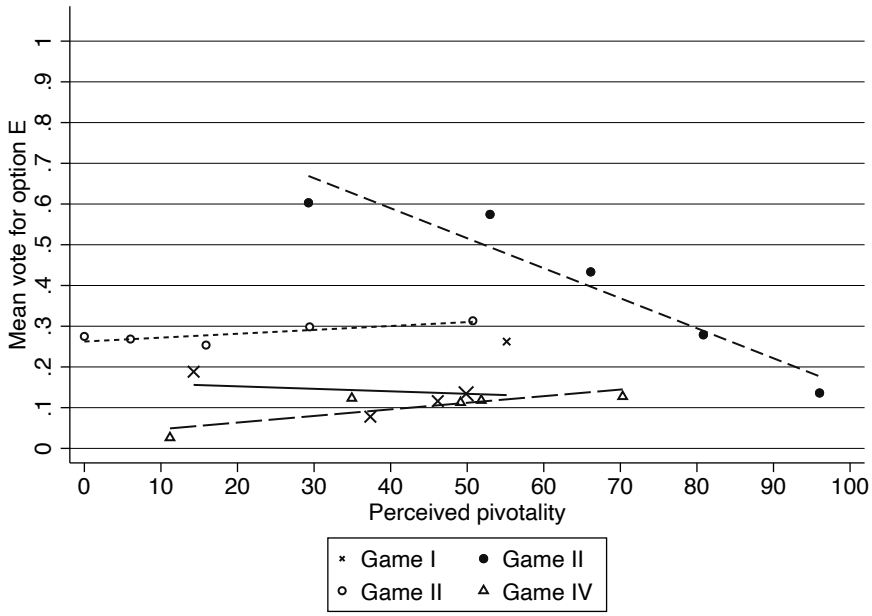


Figure 8. Cheap fairman talk per game

Notes: We grouped observations according to quintile limits. Each point represents a pair of the group average perceived pivotality and the average vote for Option E. As groups may have different sizes, the marker size varies accordingly. The quintile limits are as follows: Game I [0;30.2], [30.4;42.08], [42.2;49.02], [49.04;50], [55.02;86.96]; Game II [0;46.22], [46.36;58.58], [58.6;73], [73.04;89], [89.2;100]; Game III [0;0], [1;9.58], [10.4;22], [22.62;37.5], [38;100]; Game IV [0;23.4], [23.54;45.36], [45.44;50], [50.06;55.28], [55.32;100].

for Games I to IV, respectively). Second, choosing the surplus-creating Option S in Game II involves a very substantial loss for Member B, and the maximin motive is likely to be more salient in this game. For that reason, choosing Option E in Game II is more likely to be perceived as fair and choosing Option S is more likely to be perceived as more costly in terms of social image, prompting participants to choose the egalitarian Option E when it is less costly to do so due to lower perceived pivotality.

5. CONCLUDING REMARKS

This study contributes to the literature on distributional preferences by providing empirical evidence on the role of focal points and beliefs in distributional voting contexts. Our findings challenge the common assumption that equal splits serve as salient focal points in such settings, as we find no detectable evidence to support this hypothesis. We also find that participants' beliefs about the distributional decisions of others are often inaccurate and have systematic bi-

ases. Specifically, individuals tend to overestimate the prevalence of egalitarian choices when Pareto efficient options are available and underestimate egalitarianism when surplus creation comes at the expense of some players.

Despite these inaccuracies, beliefs are systematically related to voting behavior. Two key insights emerge from our findings. First, individuals' preferences and beliefs are positively correlated, regardless of whether they hold an advantageous or disadvantageous position. Second, participants who benefit from efficiency gains are more likely to vote for egalitarian options when they perceive a low probability of being pivotal. These results are consistent with the presence of a *false-consensus bias* and suggest that social image concerns, via *cheap fairman talk*, influence distributional voting behavior.

Our findings have important theoretical and policy implications. Theoretically, some scholars have argued that the appeal of equality may partially stem from its salience (e.g., Engelmann and Strobel 2006). If true, this would call into question the authenticity of egalitarian preferences.[12] However, our results suggest that salience-based focality is not a major driver of preferences for equality. This challenges the validity of models that rely on salient payoff-based focality to explain distributional choices. At the same time, our findings highlight the need for theories of social preferences to account for the interaction between individuals' distributional preferences and their beliefs about others' preferences. Incorporating this interplay could provide a more nuanced understanding of how people make economic and political decisions involving redistribution.

From a policy perspective, our findings suggest that using equal-split resource distributions as a salient focal point is unlikely to promote more egalitarian choices in sharing a resource within a group or an organization. While our study focuses on voting settings such as committees, we conjecture that this insight extends to other domains, including the allocation of bequests. If an equal-split division appears to be common for estate or life insurance (Fahle 2025), Francesconi et al. (2023) provide evidence that about 40% of Americans over the age of 50 do not have a will and that 20% exhibit a preference for unequal division. Our results indicate that proposing a perfectly equal distribution to these undecided people is unlikely to *further* strengthen motives — such as inequality aversion — that encourage equal divisions. Additionally, our results highlight a

[12] See, for instance, Sugden (2022) and Ferreira (2023) for recent discussions on "true" preferences and choices in behavioral economics.

pertinent challenge; individuals' beliefs about others' distributional preferences are often inaccurate, yet these beliefs influence distributional decisions. If people are more egalitarian when they believe that others share that outlook, our findings indicate that interventions aimed at improving belief accuracy — such as providing information about others' preferences (Iriberri and Rey-Biel 2013) — could have mixed effects. In contexts where a non-Pareto efficient option is available, such interventions may promote redistribution. However, in contexts where a Pareto efficient option exists, they may reduce it.

Finally, our study opens several promising avenues for future research. One direction would be to further explore alternative definitions of focality — such as the role of equality as a coordination device — in distributional voting contexts. Another relevant avenue would be to examine the relationship between people's meritocratic fairness views and their beliefs about others' distributional choices. Additionally, future research could explore the roots of belief inaccuracy and assess how interventions designed to improve belief accuracy influence redistribution and cooperation. Relatedly, while our findings suggest the presence of a false-consensus bias, we cannot fully disentangle it from potential bandwagon behavior or the influence of social norms. Future studies could address this by employing experimental designs that manipulate beliefs and track their causal influence on voting behavior (e.g., Bischoff and Egbert 2013). Such investigations could yield valuable insights into distributional decision making in both economic and political environments.

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DECLARATION OF INTERESTS

None to declare.

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ONLINE APPENDIX

Focal points, beliefs, and distributional preferences: An experimental analysis

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A. PAYOFFS OF ALL SITUATIONS

Table A.1. Payoffs (all situations)

	Option E	**Option S**
	Game I	
a1 (Equal split)	(130, 130, 130)	(190, 130, 130)
a2 (Almost-equal split)	(132, 129, 129)	(192, 129, 129)
a3 (Roughly-equal split)	(140, 125, 125)	(200, 125, 125)
a4 (15% surplus)	(130, 130, 130)	(250, 130, 130)
a5 (30% surplus)	(130, 130, 130)	(310, 130, 130)
	Game II	
a1 (Equal split)	(130, 130, 130)	(270, 10, 170)
a2 (Almost-equal split)	(132, 129, 129)	(272, 9, 169)
a3 (Roughly-equal split)	(135, 125, 125)	(280, 5, 165)
a4 (15% surplus)	(130, 130, 130)	(330, 10, 170)
a5 (30% surplus)	(130, 130, 130)	(390, 10, 170)
	Game III	
a1 (Equal split)	(130, 130, 130)	(270, 90, 90)
a2 (Almost-equal split)	(132, 129, 129)	(272, 89, 89)
a3 (Roughly-equal split)	(135, 125, 125)	(275, 85, 85)
a4 (15% surplus)	(130, 130, 130)	(330, 90, 90)
a5 (30% surplus)	(130, 130, 130)	(390, 90, 90)
	Game IV	
a1 (Equal split)	(130, 130, 130)	(160, 160, 130)
a2 (Almost-equal split)	(131, 131, 128)	(161, 161, 128)
a3 (Roughly-equal split)	(135, 135, 120)	(165, 165, 120)
a4 (15% surplus)	(130, 130, 130)	(190, 190, 130)
a5 (30% surplus)	(130, 130, 130)	(220, 220, 130)

Notes: (x, y, z) are payoffs for Members A, B, and C respectively. For a3, two competing objectives were present: (i) being sufficiently close to the equal split such that fairness and efficiency concerns do not significantly differ across a1 and a3, and (ii) maintaining efficiency stable across a1 and a3. Games I and IV give priority to (ii), while Games II and III give priority to (i) such that the maximum distance to the equal split is 5 points. Due to an operational error, the payoff for Member A in Option S of Game II in a3 was 280 instead of 275. We interpret the results with this in mind.

B. CHARACTERISTICS OF SUBJECTS

Table B.1. Characteristics of Subjects

	US	**UK**	**Total**
Age (median)	21	21	21
Female (%)	70.13	66.59	68.37
Undergraduate (%)	79.12	75.12	77.11
Field of study (%):			
Economics & management	6.86	7.57	7.21
Sciences	42.26	39.87	41.07
Law	3.54	5.35	4.44
Humanities	21.68	17.82	19.76
Math	4.20	7.13	5.66
Other	21.46	22.27	21.86
Duration in seconds (median)	597	603	599
No. of studies in Prolific (median)	388	199	284
Understanding (mean)	5.63	5.49	5.56
No. of participants	452	449	901

Notes: For gender, 3.98% and 2.00% responded "Other" in the US and UK, respectively. There are 36 missing observations for *Undergraduate* because that variable is from the Prolific database (we do not include it in our regression analysis for this reason). *Understanding* of the instructions was measured on a scale from 1 (very unclear) to 7 (very clear).

C. BASELINE COMPARISON TO BOLTON AND OCKENFELS (2006)

Table C.1. Percentage of E votes in the baseline of Games I, II, and III

	Our experiment			Bolton and Ockenfels (2006)		
	Game I	**Game II**	**Game III**	**Game I**	**Game II**	**Game III**
Member A	14.29%	37.41%	32.09%	12.50%	33.33%	20.83%
Member B	37.08%	95.14%	89.97%	16.67%	87.50%	87.50%
Member C		48.55%			37.50%	
Game average	29.81%	59.48%	72.06%	15.28%	52.78%	65.28%

Notes: For Games I and III, Members B and C are in the same payoff position, with their results averaged together under Member B. Game IV is omitted since it is novel to our study.

D. RAW BELIEFS AND CHOICE DATA

Table D.1. Beliefs about percentages of E votes and E votes per situation and position

	a1		**a2**		**a3**		**a4**		**a5**		**Average**	
	Baseline		Almost equal		Roughly equal		30% surplus		45% surplus		All situations	
	Beliefs	Choices	Beliefs	Choices	Beliefs	Choices	Beliefs	Choices	Beliefs	Choices	Beliefs	Choices
Game I												
Beliefs about Member A (b)	24.30%	14.29%	24.41%	11.04%	20.96%	15.58%	24.14%	14.29%	20.30%	14.94%	22.82%	14.03%
Beliefs about Members B & C	50.14%	37.08%	46.70%	36.78%	46.92%	35.87%	49.33%	41.03%	50.12%	39.51%	48.64%	38.05%
Round average	41.83%	29.81%	39.76%	28.57%	38.39%	29.40%	41.62%	32.51%	40.52%	31.68%	40.42%	30.39%
Game II												
Beliefs about Member A (b)	27.53%	37.41%	27.57%	34.01%	28.30%	36.05%	24.54%	30.61%	23.43%	31.97%	26.28%	34.01%
Beliefs about Member B	84.48%	95.14%	86.77%	95.14%	84.69%	95.83%	84.61%	90.97%	86.20%	93.06%	85.35%	94.03%
Beliefs about Member C (b)	34.09%	48.55%	34.12%	43.93%	33.13%	45.09%	35.35%	47.98%	33.75%	45.66%	34.09%	46.24%
Round average	49.22%	59.48%	50.04%	56.68%	49.26%	57.97%	48.64%	55.82%	48.31%	56.03%	49.10%	57.20%
Game III												
Beliefs about Member A (b)	28.30%	32.09%	29.33%	28.36%	27.74%	32.84%	25.17%	23.88%	26.72%	23.88%	27.45%	28.21%
Beliefs about Members B & C	79.35%	89.97%	80.51%	94.31%	79.23%	93.98%	79.94%	89.97%	79.88%	89.30%	79.78%	91.51%
Round average	62.36%	72.06%	63.38%	73.90%	62.39%	75.06%	61.46%	69.52%	62.18%	69.05%	62.36%	71.92%
Game IV												
Beliefs about Members A & B (b)	24.81%	10.49%	23.04%	9.74%	22.42%	10.11%	21.95%	11.61%	21.28%	8.61%	22.70%	10.11%
Beliefs about Member C	47.97%	28.39%	45.27%	25.16%	44.01%	24.52%	46.69%	25.81%	46.52%	25.81%	46.09%	25.94%
Round average	31.70%	17.06%	30.40%	15.40%	28.84%	15.40%	29.55%	16.82%	29.53%	14.93%	30.00%	15.92%
All games	46.29%	44.62%	45.89%	43.62%	44.72%	44.45%	45.37%	43.73%	45.16%	43.01%	45.49%	43.88%

Notes: The "Beliefs" columns report the average belief of the players in the other two positions about the proportion of subjects in the row position voting for Option E. The "Choices" columns report the actual choices of those members. When two players have the same relative position in a game (e.g., Members B and C in Game I), we aggregate their choice data and the beliefs about their behavior. We find no statistically significant differences suggesting the need to disaggregate their data. (b) denotes participants who benefit from Option S in each game.

E. REGRESSION ANALYSES

Table E.1. Focal points (dependent variable voting for Option E)

	(1) All games	(2) All games	(3) Game I	(4) Game II	(5) Game III	(6) Game IV	(7) Round 1	(8) All games	(9) Round 1
Almost-equal split (a2)	-0.025	-0.030	-0.037	-0.072	0.056	-0.069	-0.185	0.034	-0.279
	(0.022)	(0.027)	(0.057)	(0.045)	(0.047)	(0.073)	(0.143)	(0.041)	(0.201)
Roughly-equal split (a3)	-0.004	-0.005	-0.013	-0.039	0.093*	-0.069	-0.160	0.019	-0.284
	(0.021)	(0.026)	(0.056)	(0.043)	(0.045)	(0.064)	(0.144)	(0.038)	(0.197)
Age		0.004	0.007	0.004	-0.008	0.010	0.014	0.006	0.015
		(0.005)	(0.007)	(0.008)	(0.009)	(0.008)	(0.009)	(0.005)	(0.009)
Female		0.115	0.274*	0.048	0.162	-0.069	0.043	0.125	0.041
		(0.070)	(0.119)	(0.115)	(0.128)	(0.137)	(0.129)	(0.072)	(0.139)
Sciences & Math		-0.001	0.131	-0.016	-0.002	-0.153	-0.048	0.023	-0.023
		(0.064)	(0.105)	(0.107)	(0.119)	(0.126)	(0.118)	(0.065)	(0.124)
Duration		-0.000***	0.000***	0.000	-0.000***	0.000*	-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Prolific		-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000**	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Benefit from S								-1.233***	-1.265***
								(0.082)	(0.235)
Almost-equal split × Benefit from S								-0.144*	0.132
								(0.067)	(0.303)
Roughly-equal split × Benefit from S								-0.042	0.213
								(0.062)	(0.307)
Game		Yes					Yes	Yes	Yes
Constant	-0.135***	-0.647***	-0.860***	0.068	0.687**	-1.206***	-0.800**	-0.396**	-0.431
	(0.031)	(0.134)	(0.211)	(0.240)	(0.242)	(0.272)	(0.267)	(0.139)	(0.289)
Observations	5406	5406	1449	1392	1299	1266	545	5406	545
Pseudo R^2	0.000	0.160	0.013	0.003	0.007	0.018	0.186	0.286	0.286
p-values (a2 = a3, Wald tests)	0.315	0.328	0.652	0.440	0.451	0.997	0.858	0.691	0.977

Notes: *Significant at 5% level, **Significant at 1% level, ***Significant at 0.1% level, based on probit regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

Table E.2. Surplus gains (dependent variable voting for Option E)

	(1) All games	(2) All games	(3) Game I	(4) Game II	(5) Game III	(6) Game IV	(7) Round 1	(8) All games	(9) Round 1
30% surplus (a4)	-0.022	-0.025	0.077	-0.094*	-0.073	-0.011	0.019	0.008	0.020
	(0.022)	(0.025)	(0.054)	(0.046)	(0.044)	(0.058)	(0.137)	(0.038)	(0.201)
45% surplus (a5)	-0.041	-0.049	0.052	-0.088	-0.087	-0.086	-0.087	-0.006	-0.076
	(0.022)	(0.026)	(0.054)	(0.048)	(0.045)	(0.067)	(0.135)	(0.039)	(0.184)
Age		0.005	0.011	0.002	-0.011	0.015	0.006	0.008	0.011
		(0.005)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.005)	(0.010)
Female		0.157*	0.368**	0.060	0.203	-0.055	0.103	0.170*	0.083
		(0.070)	(0.120)	(0.113)	(0.125)	(0.139)	(0.121)	(0.073)	(0.132)
Sciences & Math		-0.025	0.104	-0.064	-0.060	-0.086	-0.157	-0.004	-0.108
		(0.065)	(0.105)	(0.105)	(0.117)	(0.128)	(0.112)	(0.066)	(0.118)
Duration		-0.000***	0.000***	0.000	-0.000***	0.000	0.000	-0.000***	0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Prolific		-0.000	-0.000	-0.000	0.000	-0.000	-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Benefit from S								-1.197***	-1.056***
								(0.081)	(0.204)
30% surplus × Benefit from S								-0.079	-0.002
								(0.060)	(0.292)
45% surplus × Benefit from S								-0.110	-0.081
								(0.064)	(0.282)
Game		Yes					Yes	Yes	Yes
Constant	-0.135***	-0.654***	-1.039***	-0.008	0.705**	-1.365***	-0.477	-0.411**	-0.199
	(0.031)	(0.136)	(0.227)	(0.240)	(0.235)	(0.271)	(0.293)	(0.140)	(0.321)
Observations	5406	5406	1449	1392	1299	1266	550	5406	550
Pseudo R^2	0.000	0.138	0.018	0.006	0.009	0.018	0.099	0.262	0.200
p-values (a4 = a5, Wald tests)	0.401	0.362	0.610	0.906	0.753	0.278	0.443	0.703	0.626

Notes: *Significant at 5% level, **Significant at 1% level, ***Significant at 0.1% level, based on probit regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

Table E.3. Accuracy per game and position (dependent variable difference between subjects' beliefs and realized frequencies)

	(1) All games	(2) All games	(3) Game I	(4) Game II	(5) Game III	(6) Game IV
Benefit from S	-1.567*	-4.008***	-1.471	-17.576***	-4.895***	7.417***
	(0.710)	(0.751)	(1.215)	(1.590)	(1.364)	(1.479)
Age		0.169**	0.158	0.188	0.240*	0.106
		(0.058)	(0.089)	(0.098)	(0.121)	(0.109)
Female		-0.816	-0.269	-2.494	-4.089*	3.830*
		(0.746)	(1.264)	(1.371)	(1.620)	(1.500)
Sciences & Math		-0.373	0.642	-1.465	-1.167	-2.017
		(0.678)	(1.139)	(1.279)	(1.429)	(1.456)
Duration		0.000***	0.000***	-0.002	0.000***	-0.000
		(0.000)	(0.000)	(0.002)	(0.000)	(0.002)
# Prolific		-0.001	-0.000	0.001	-0.002	-0.004*
		(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Game		Yes				
Situation			Yes	Yes	Yes	Yes
Constant	23.581***	21.582***	21.017***	38.230***	16.673***	17.393***
	(0.514)	(1.554)	(2.471)	(3.197)	(3.381)	(3.039)
Observations	9010	9010	2415	2320	2165	2110
R^2	0.002	0.051	0.012	0.205	0.030	0.056

Notes: *Significant at 5% level, **Significant at 1% level, ***Significant at 0.1% level, based on OLS regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

Table E.4. False-consensus bias (dependent variable egalitarian ranking)

	(1) Game I (C about B)	(2) Game I (B about C)	(3) Game II (C about A)	(4) Game II A about C)	(5) Game III (C about B)	(6) Game III (B about C)	(7) Game IV (B about A)	(8) Game IV (A about B)
Belief Member A votes E			0.043***				0.009**	
			(0.004)				(0.004)	
Belief Member B votes E	0.033***				0.016***			0.019***
	(0.004)				(0.004)			(0.004)
Belief Member C votes E		0.042***		0.033***		0.012**		
		(0.003)		(0.005)		(0.004)		
Age	0.013	-0.004	-0.009	0.030	-0.037	-0.014	0.023	-0.004
	(0.020)	(0.021)	(0.016)	(0.021)	(0.023)	(0.010)	(0.017)	(0.008)
Female	0.430	0.049	0.326	0.079	0.410	-0.212	-0.098	-0.120
	(0.276)	(0.319)	(0.290)	(0.297)	(0.236)	(0.141)	(0.231)	(0.217)
Sciences & Math	0.397	-0.037	-0.035	-0.745**	0.055	-0.084	-0.280	0.035
	(0.264)	(0.263)	(0.274)	(0.272)	(0.178)	(0.137)	(0.185)	(0.191)
Duration	0.000	0.000	0.000	-0.000	-0.000***	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Prolific	-0.000	-0.000	-0.000	0.000	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Situation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.532	0.063	0.871	0.130	3.741***	4.374***	-0.019	0.048
	(0.499)	(0.622)	(0.530)	(0.648)	(0.610)	(0.460)	(0.392)	(0.353)
Observations	890	755	865	735	760	735	700	635
R^2	0.230	0.324	0.281	0.222	0.223	0.120	0.079	0.189

Notes: *Significant at 5% level, **Significant at 1% level, ***Significant at 0.1% level, based on OLS regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

Table E.5. Cheap fairman talk (dependent variable voting for Option E)

	(1) Game I Member A	(2) Game II Member A	(3) Game II Member C	(4) Game III Member A	(5) Game IV Member A	(6) Game IV Member B
Belief pivotal	-0.004	-0.012**	-0.006	0.004	0.008	0.008
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)
Belief Member A votes E			0.023***			0.013***
			(0.004)			(0.004)
Belief Member B votes E					0.022***	
					(0.003)	
Belief Member C votes E		0.009*				
		(0.004)				
Age	0.014	0.019*	-0.004	0.012	-0.002	0.028
	(0.010)	(0.011)	(0.011)	(0.014)	(0.011)	(0.016)
Female	0.281	0.044	0.182	0.112	-0.132	-0.115
	(0.227)	(0.187)	(0.180)	(0.207)	(0.240)	(0.308)
Sciences & Math	0.309	-0.466**	-0.006	-0.143	-0.002	-0.353
	(0.200)	(0.167)	(0.168)	(0.197)	(0.223)	(0.267)
Duration	-0.000	-0.000	0.000	-0.001	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Prolific	-0.000	0.000	-0.000	-0.000	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Situation	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-1.429**	-0.138	-0.423	-0.443	-2.319***	-2.357***
	(0.502)	(0.539)	(0.462)	(0.462)	(0.413)	(0.621)
Observations	770	735	865	670	635	700
Pseudo R^2	0.028	0.120	0.184	0.025	0.192	0.123

Notes: *Significant at 5% level, **Significant at 1% level, ***Significant at 0.1% level, based on probit regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

F. MAIN RESULTS WITH RESTRICTED SAMPLE

For robustness, and as preregistered, we perform our main analysis with a subsample of participants for which we exclude (i) participants who failed one of the two attention questions, (ii) participants who failed the first attempt at two out of three comprehension questions, and (iii) participants who answered in a duration lower than half of the median duration. From the initial sample of 901 participants, we exclude 15 participants due to (i), 80 due to (ii), and 5 due to (iii). Some participants could be excluded for more than one reason (i.e., these figures count twice or more for some participants). We end up with a sample of 806 participants (402 in the US and 404 in the UK).

Focal point. Figure F.1 shows that the results with this restricted sample are very similar to those with our main sample. Table F.1 also shows results similar to those reported in the main text. The main difference is that we find a statistically significant effect in line with the focal point hypothesis for the almost-equal split across games (columns 1 and 2 of Table F.1). However, this effect is not statistically significant in round 1 and does not carry over for the roughly-equal split as expected by the focal point hypothesis. In addition, non-parametric tests show no statistically significant differences for all tests. Therefore, we conclude once again that there is no consistent evidence of a focal point effect, supporting our Finding 1.

Surplus gain. Figure F.2 shows that the results with this restricted sample are again very similar to those with our full sample. Table F.2 supports our Finding 2 (no effect of added surplus in Games I and IV) and brings added confidence to our Finding 3 (statistically significant effect of added surplus in Games II and III).

Belief accuracy. Figure F.3 shows equivalent results to our main analysis. Supporting Findings 4 and 5, we find that subjects overestimate others' propensity to choose egalitarian options in games with a Pareto efficient option (Games I and IV) and underestimate others' propensity to choose egalitarian options in games with a non-Pareto efficient option (Games II and III) ($p < 0.01$ for all two-sided one-sample tests of proportion per game and situation).

False-consensus bias. Figure F.4 and Table F.3 support our finding that there is a false-consensus bias in distributional voting games (Finding 6). There is again a statistically significant correlation of preferences and beliefs in all

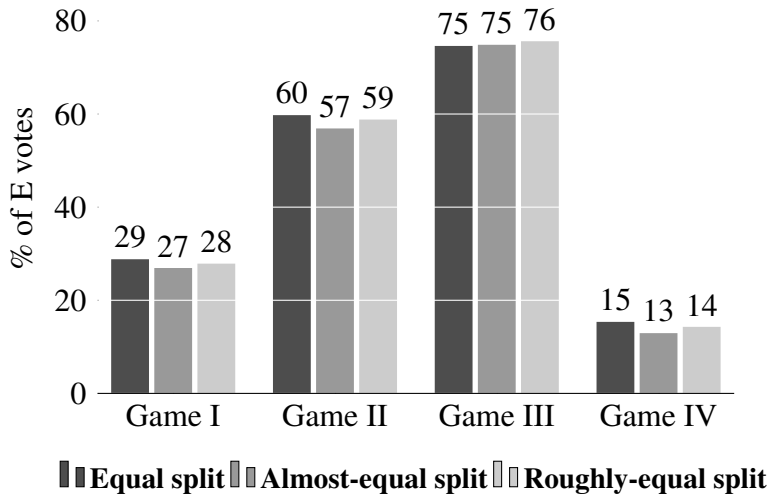

Figure F.1. % of E votes across equal split (a1), almost-equal split (a2), and roughly-equal split (a3) with restricted sample

games. The only exception with the restricted sample are the preferences and beliefs of Members B in Game IV (column 7 in Table F.3).

Cheap fairman talk. Results in Figure F.5 and Table F.4 are equivalent to those with the main sample and further support Finding 7. We find once again that there is cheap fairman talk in specific distributional voting games, in particular by Members A in Game II ($p < 0.001$, column 2 of Table F.4).

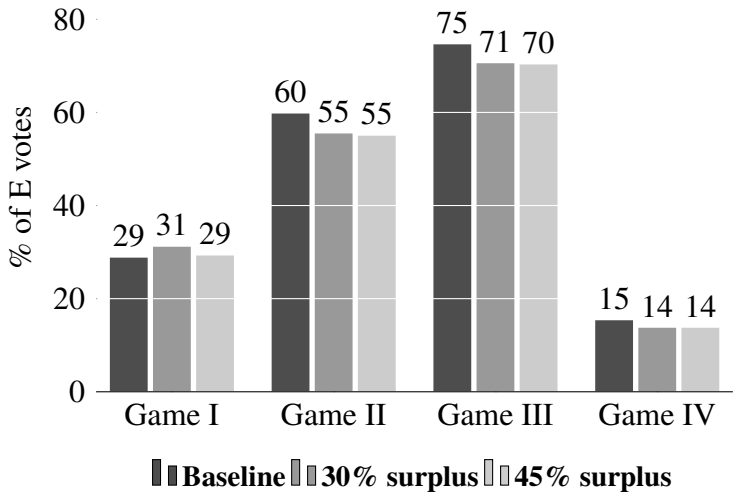

Figure F.2. % of E votes with Baseline (a1), 30% surplus gain (a4), and 45% surplus gain (a5) with restricted sample

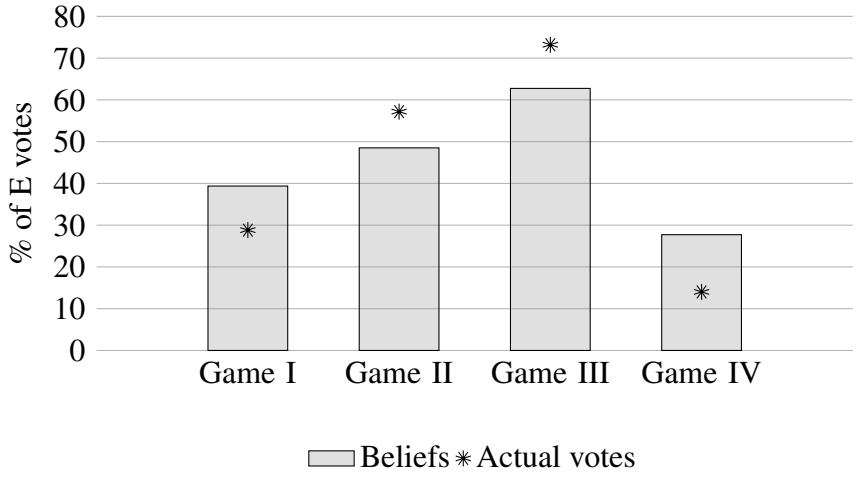

Figure F.3. Beliefs about % of E votes and actual % of E votes with restricted sample

Notes: Data aggregated for all situations per game (a1-a5). To calculate beliefs about one player (e.g., Member A), we average the beliefs of the players in the other positions in the game (Members B and C in the example).

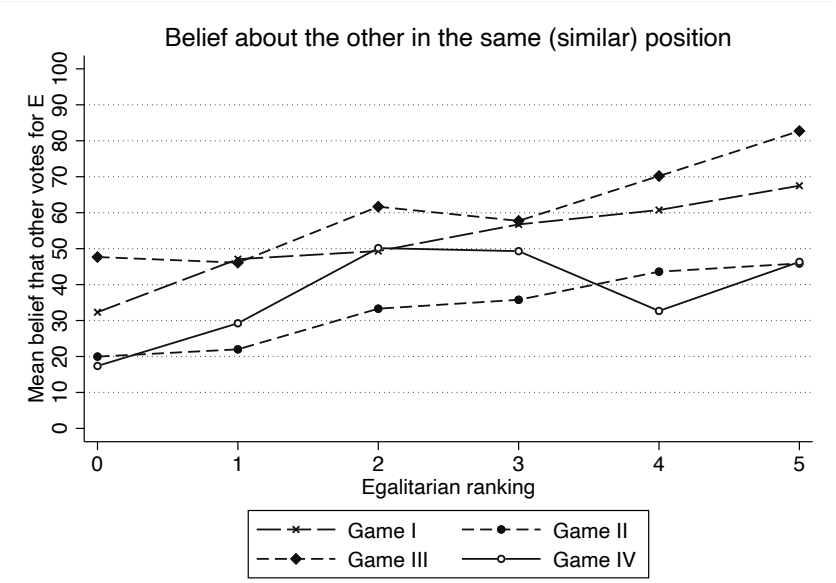

Figure F.4. False-consensus bias per game with restricted sample

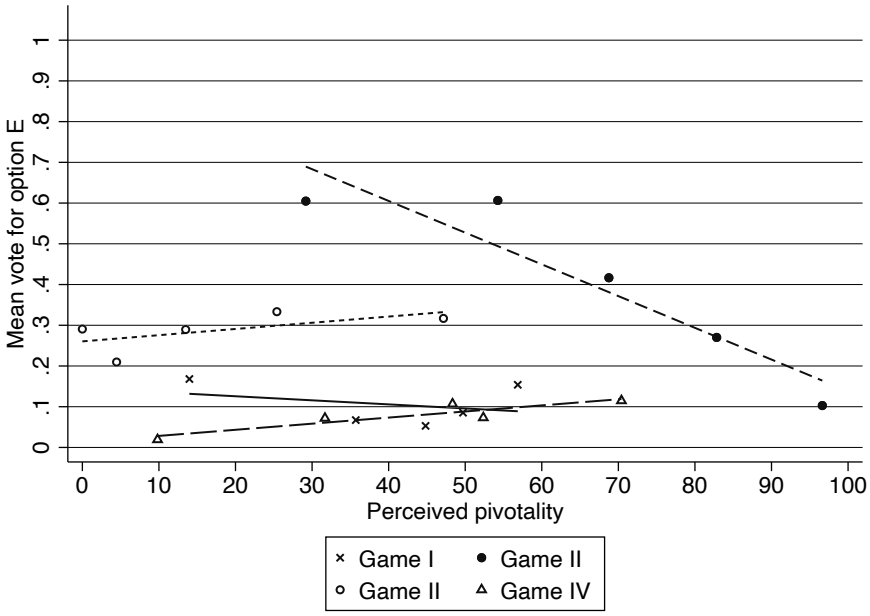

Figure F.5. Cheap fairman talk per game with restricted sample

Notes: We grouped observations according to quintile limits. Each point represents a pair of the group average perceived pivotality and the average vote for Option E. As groups may have different sizes, the marker size varies accordingly. The quintile limits are as follows: Game I [0; 28.88], [29; 41.36], [41.5; 48], [48.02; 50], [50.02;86.96]; Game II [0; 46.36], [46.64; 60.5], [60.56; 75], [75.2; 90], [90.18 ;100]; Game III [0;0], [1;9.58], [8.72; 18], [18.18; 31.76], [32;100]; Game IV [0; 20.6], [20.76 ; 41.4], [41.42 ;50], [50.06; 56.46], [56.5 ;100].

Table F.1. Focal points with restricted sample (dependent variable voting for Option E)

	(1) All games	(2) All games	(3) Game I	(4) Game II	(5) Game III	(6) Game IV	(7) Round 1	(8) All games	(9) Round 1
Almost-equal split (a2)	-0.044*	-0.055*	-0.056	-0.074	0.008	-0.114	-0.253	0.004	-0.400
	(0.022)	(0.027)	(0.060)	(0.043)	(0.046)	(0.078)	(0.154)	(0.042)	(0.213)
Roughly-equal split (a3)	-0.013	-0.016	-0.028	-0.025	0.033	-0.047	-0.149	0.017	-0.243
	(0.021)	(0.026)	(0.055)	(0.044)	(0.044)	(0.071)	(0.156)	(0.038)	(0.206)
Age		0.004	0.006	0.001	-0.008	0.014	0.014	0.006	0.016
		(0.005)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)	(0.005)	(0.010)
Female		0.139	0.249	0.077	0.227	-0.049	0.057	0.178*	0.092
		(0.076)	(0.127)	(0.123)	(0.137)	(0.153)	(0.139)	(0.078)	(0.156)
Sciences & Math		0.015	0.181	0.015	-0.031	-0.141	-0.047	0.062	-0.017
		(0.069)	(0.115)	(0.114)	(0.128)	(0.138)	(0.126)	(0.071)	(0.135)
Duration		-0.000***	0.000***	0.000	-0.000***	0.001*	-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Prolific		-0.000	-0.000	-0.000	0.000	-0.000	-0.000	-0.000*	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Benefit from S								-1.311***	-1.445***
								(0.092)	(0.259)
Almost-equal split × Benefit from S								-0.142*	0.157
								(0.070)	(0.331)
Roughly-equal split × Benefit from S								-0.062	0.120
								(0.065)	(0.337)
Game		Yes					Yes	Yes	Yes
Constant	-0.126***	-0.700***	-0.872***	0.064	0.709**	-1.554***	-0.971***	-0.482**	-0.611*
	(0.033)	(0.146)	(0.234)	(0.253)	(0.258)	(0.318)	(0.290)	(0.153)	(0.310)
Observations	4836	4836	1281	1260	1182	1113	487	4836	487
Pseudo R^2	0.000	0.181	0.015	0.005	0.010	0.028	0.219	0.317	0.342
p-values (a2 = a3, Wald tests)	0.123	0.123	0.591	0.256	0.597	0.343	0.493	0.722	0.425

Notes: *Significant at 5% level, **Significant at 1% level, ***Significant at 0.1% level, based on probit regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

Table F.2. Surplus gains with restricted sample (dependent variable voting for Option E)

	(1) All games	(2) All games	(3) Game I	(4) Game II	(5) Game III	(6) Game IV	(7) Round 1	(8) All games	(9) Round 1
30% surplus (a4)	-0.047*	-0.056*	0.069	-0.110*	-0.122**	-0.076	-0.043	-0.022	-0.074
	(0.021)	(0.025)	(0.056)	(0.046)	(0.046)	(0.057)	(0.145)	(0.040)	(0.209)
45% surplus (a5)	-0.065**	-0.078**	0.012	-0.122*	-0.130**	-0.070	-0.062	-0.033	-0.065
	(0.022)	(0.026)	(0.054)	(0.048)	(0.046)	(0.072)	(0.145)	(0.039)	(0.192)
Age		0.007	0.014	0.002	-0.009	0.018	0.008	0.009	0.015
		(0.005)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.005)	(0.010)
Female		0.167*	0.348**	0.064	0.257	-0.076	0.143	0.202*	0.141
		(0.076)	(0.130)	(0.121)	(0.132)	(0.156)	(0.131)	(0.080)	(0.145)
Sciences & Math		0.007	0.166	-0.006	-0.065	-0.070	-0.150	0.052	-0.104
		(0.070)	(0.114)	(0.112)	(0.125)	(0.141)	(0.120)	(0.072)	(0.127)
Duration		-0.000***	0.000***	0.000	-0.000***	0.001**	0.000	-0.000***	0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Prolific		-0.000	-0.000	-0.000	0.000	-0.000	-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Benefit from S								-1.262***	-1.137***
								(0.089)	(0.214)
30% surplus × Benefit from S								-0.099	0.051
								(0.060)	(0.309)
45% surplus × Benefit from S								-0.128*	-0.139
								(0.065)	(0.309)
Game		Yes					Yes	Yes	Yes
Constant	-0.126***	-0.728***	-1.135***	-0.026	0.713**	-1.704***	-0.776*	-0.521***	-0.466
	(0.033)	(0.148)	(0.250)	(0.256)	(0.250)	(0.322)	(0.314)	(0.155)	(0.337)
Observations	4836	4836	1281	1260	1182	1113	492	4836	492
Pseudo R^2	0.000	0.158	0.020	0.007	0.013	0.030	0.123	0.292	0.237
p-values (a4 = a5, Wald tests)	0.406	0.395	0.265	0.788	0.852	0.934	0.898	0.766	0.963

Notes: *Significant at 5% level, **Significant at 1% level, ***Significant at 0.1% level, based on probit regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

Table F.3. False-consensus bias with restricted sample (dependent variable egalitarian ranking)

	(1) Game I (C about B)	(2) Game I (B about C)	(3) Game II (C about A)	(4) Game II A about C)	(5) Game III (C about B)	(6) Game III (B about C)	(7) Game IV (B about A)	(8) Game IV (A about B)
Belief Member A votes E			0.046***				0.006	
			(0.005)				(0.003)	
Belief Member B votes E	0.033***				0.016***			0.018***
	(0.004)				(0.005)			(0.005)
Belief Member C votes E		0.042***		0.033***		0.005*		
		(0.003)		(0.005)		(0.002)		
Age	0.016	-0.009	-0.008	0.029	-0.035	-0.007	0.031	-0.008
	(0.022)	(0.023)	(0.016)	(0.021)	(0.023)	(0.008)	(0.018)	(0.009)
Female	0.366	0.070	0.217	0.229	0.455	0.011	-0.070	-0.055
	(0.293)	(0.337)	(0.308)	(0.316)	(0.247)	(0.122)	(0.204)	(0.235)
Sciences & Math	0.532	0.080	0.160	-0.727*	0.123	-0.021	-0.287	-0.081
	(0.290)	(0.274)	(0.284)	(0.298)	(0.191)	(0.103)	(0.173)	(0.217)
Duration	0.000	0.001	0.000	-0.000	-0.000***	-0.000	-0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Prolific	-0.000	-0.000	-0.000	0.000	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Situation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.685	-0.101	0.718	-0.035	3.630***	4.509***	-0.192	0.199
	(0.528)	(0.802)	(0.524)	(0.705)	(0.657)	(0.313)	(0.395)	(0.397)
Observations	765	680	795	645	690	685	645	540
R^2	0.237	0.337	0.301	0.202	0.240	0.054	0.085	0.169

Notes: *Significant at 5% level, **Significant at 1% level, ***Significant at 0.1% level, based on OLS regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

Table F.4. Cheap fairman talk with restricted sample (dependent variable voting for Option E)

	(1) Game I Member A	(2) Game II Member A	(3) Game II Member C	(4) Game III Member A	(5) Game IV Member A	(6) Game IV Member B
Belief pivotal	-0.008	-0.014***	-0.007	0.002	0.007	0.010
	(0.006)	(0.004)	(0.004)	(0.004)	(0.006)	(0.007)
Belief Member A votes E			0.024***			0.014**
			(0.005)			(0.004)
Belief Member B votes E					0.021***	
					(0.003)	
Belief Member C votes E		0.008				
		(0.005)				
Age	0.014	0.019	-0.004	0.016	-0.011	0.040*
	(0.012)	(0.011)	(0.011)	(0.015)	(0.018)	(0.017)
Female	0.285	0.149	0.113	0.160	-0.057	-0.074
	(0.259)	(0.204)	(0.194)	(0.224)	(0.296)	(0.291)
Sciences & Math	0.383	-0.469*	0.114	-0.158	-0.133	-0.440
	(0.218)	(0.184)	(0.177)	(0.213)	(0.265)	(0.305)
Duration	0.000	-0.000	0.000	-0.001	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Prolific	-0.000	0.000	-0.000	0.000	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Situation	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-1.404*	-0.018	-0.450	-0.464	-1.986***	-2.832***
	(0.574)	(0.541)	(0.491)	(0.496)	(0.510)	(0.699)
Observations	690	645	795	595	540	645
Pseudo R^2	0.039	0.128	0.198	0.027	0.201	0.161

Notes: *Significant at 10% level, **Significant at 5% level, ***Significant at 1% level, based on probit regressions with clustered standard errors per subject. Standard errors in parentheses. Independent variables as described in Table B.1.

G. SCREENS

In this appendix, we present screen shots of the computer interface that participants used in the experiment. For concision, we only present one round and one attention check, while subjects faced 10 rounds in a random order and two attention checks introduced after rounds 3 and 7 of the experiment. The two attention checks are equivalent. The screens for all other rounds are equivalent, with the exception of the options available.

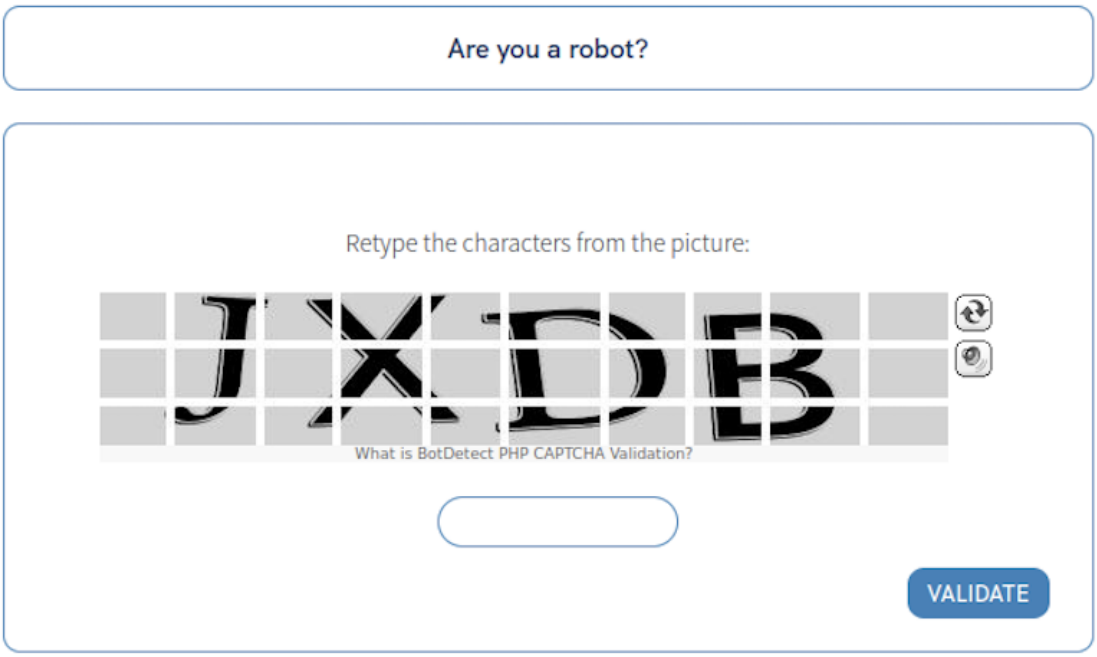

Figure G.6. Screen 1

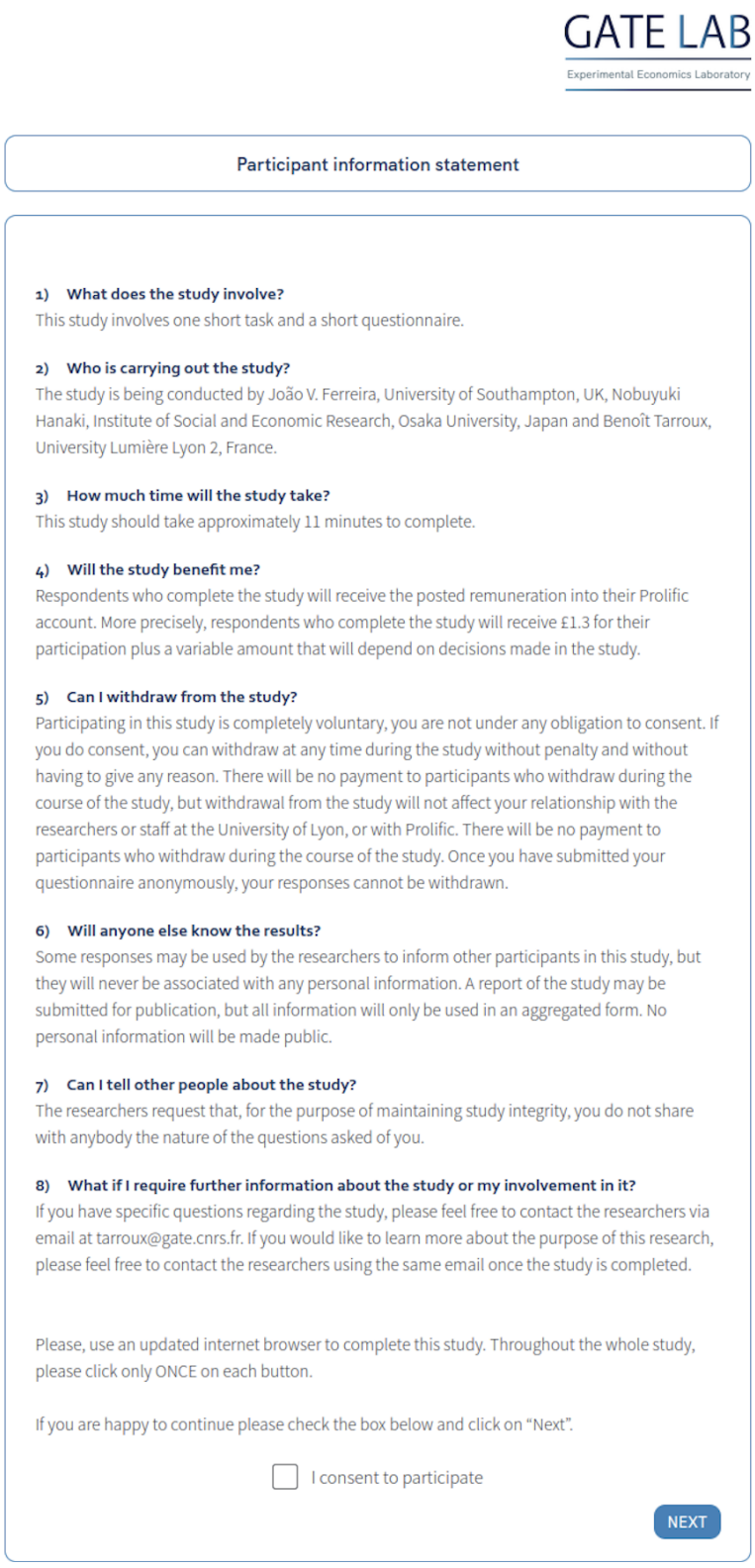

Figure G.7. Screen 2

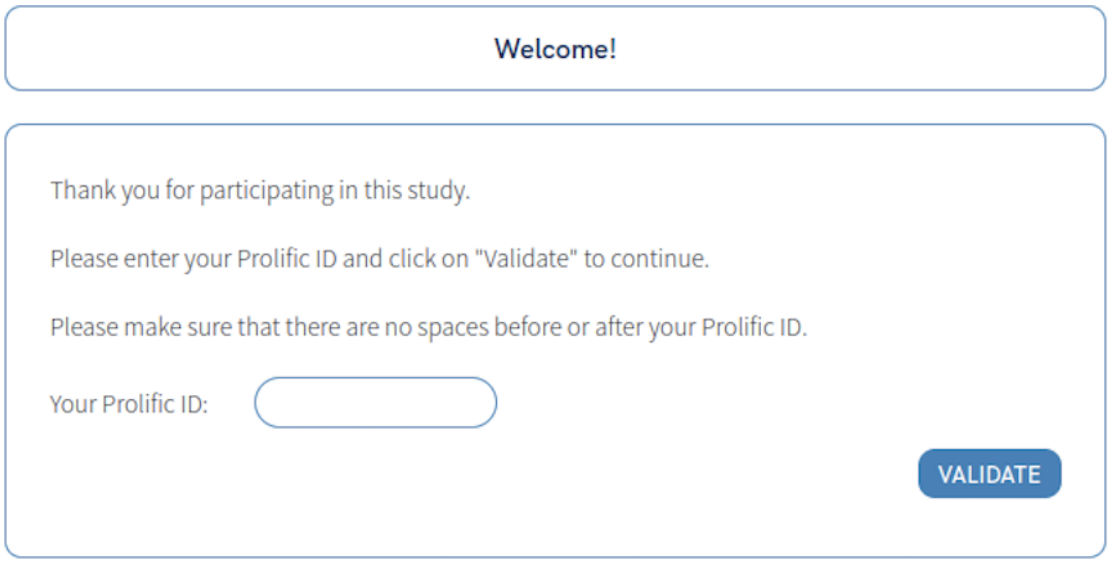

Figure G.8. Screen 3

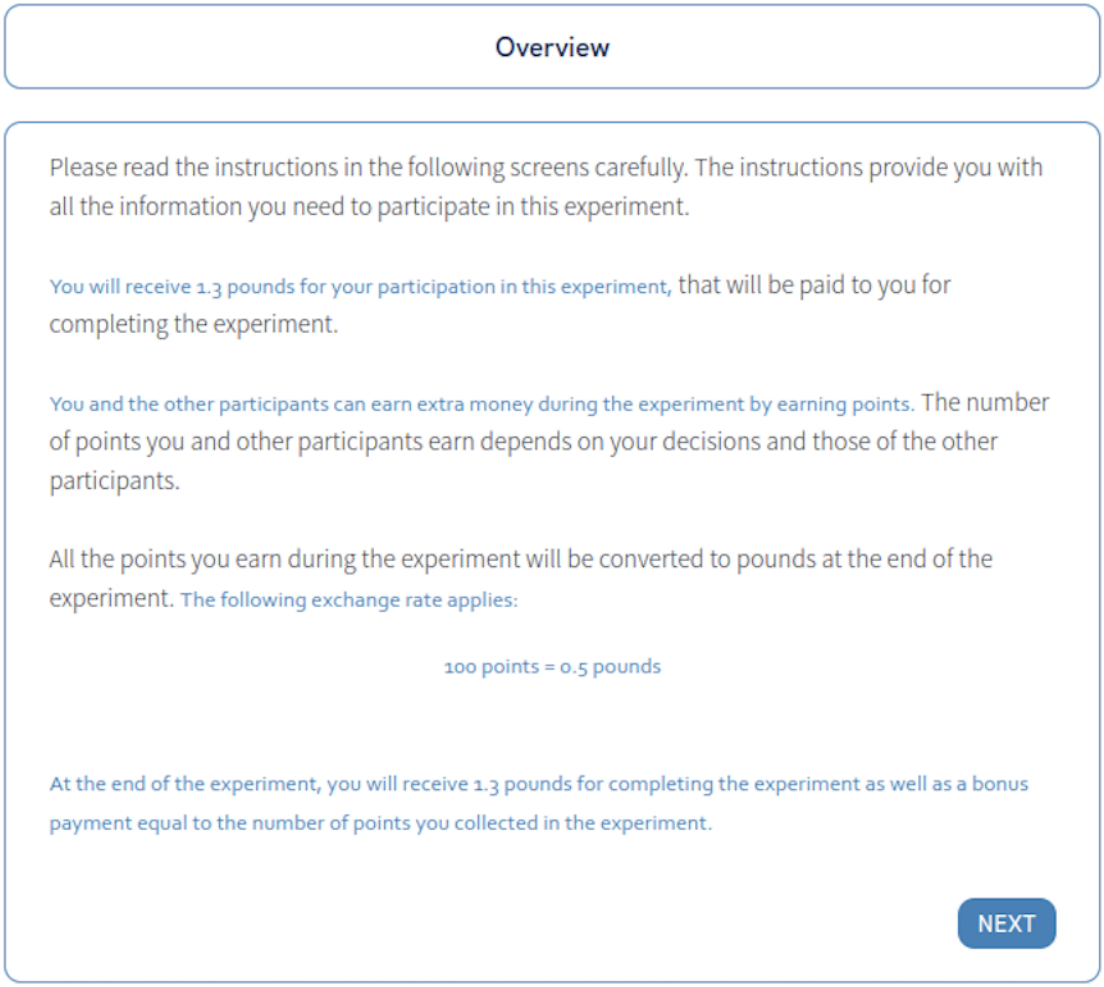

Figure G.9. Screen 4

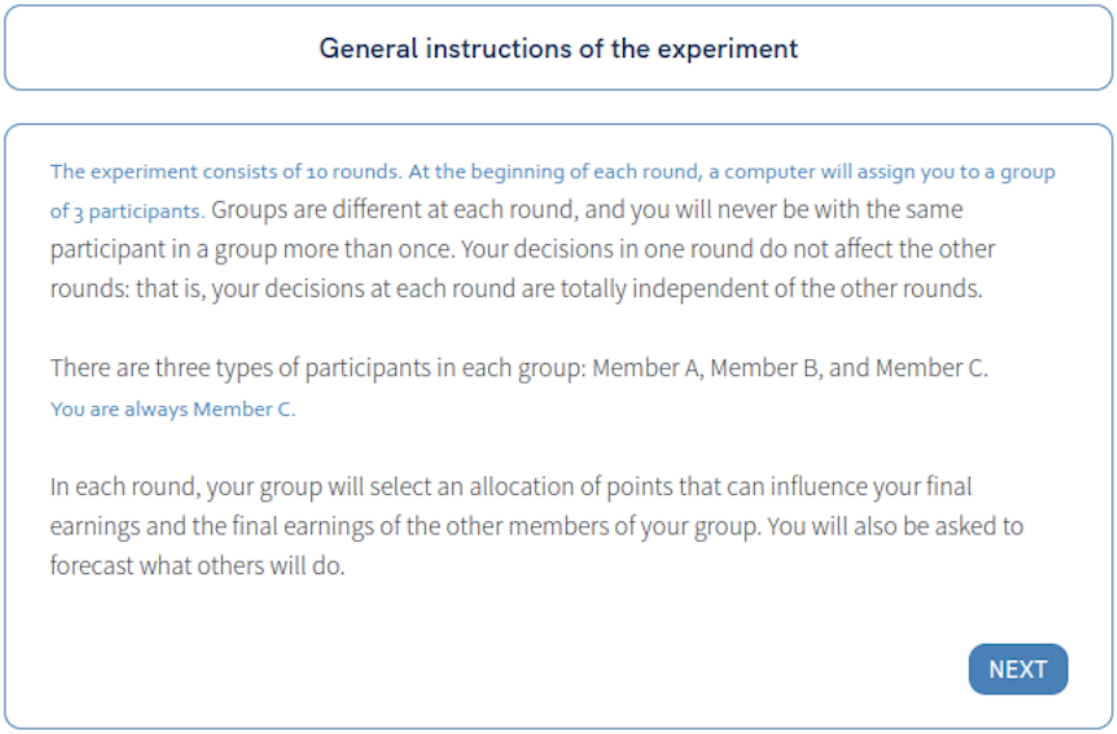

Figure G.10. Screen 5

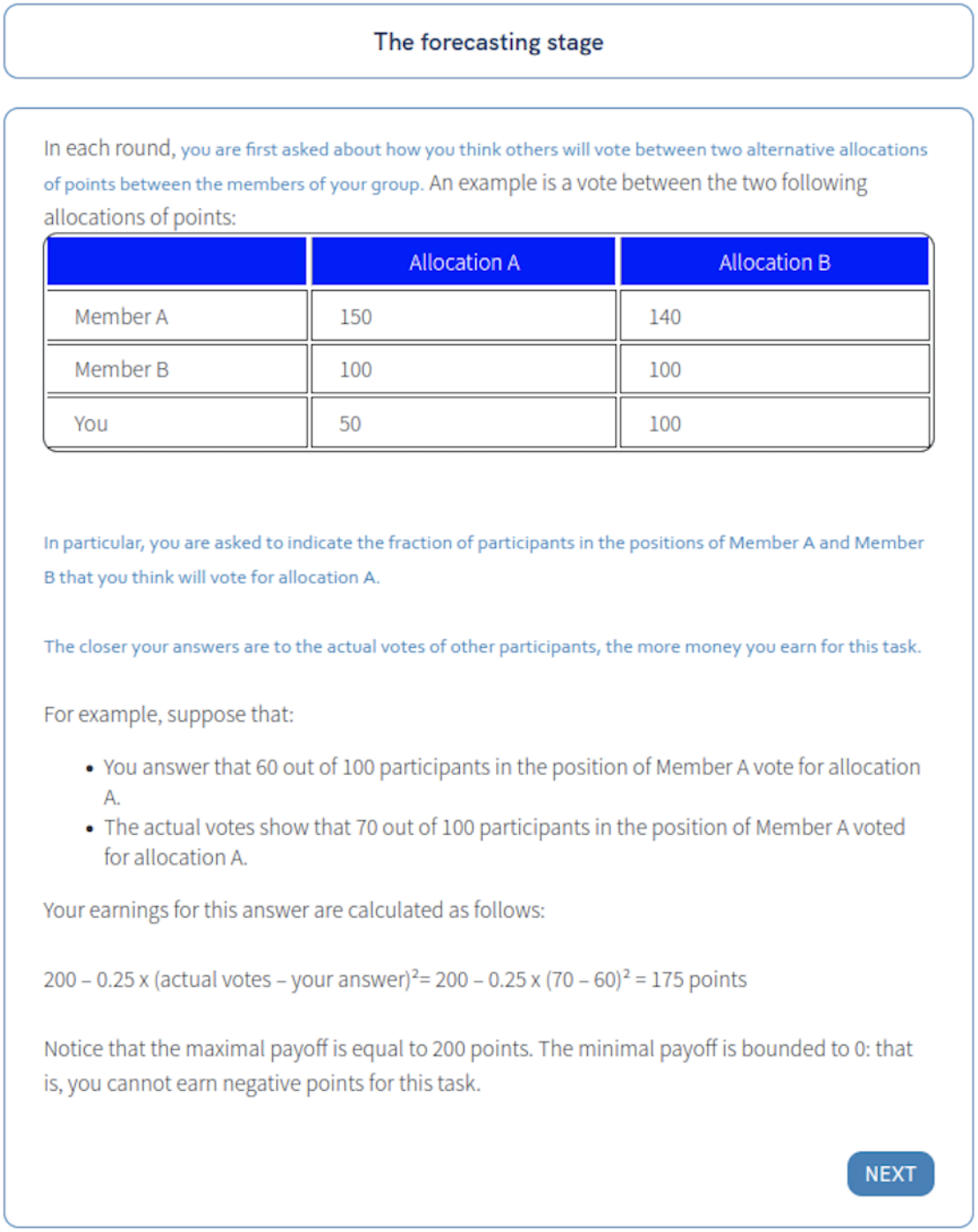

Figure G.11. Screen 6

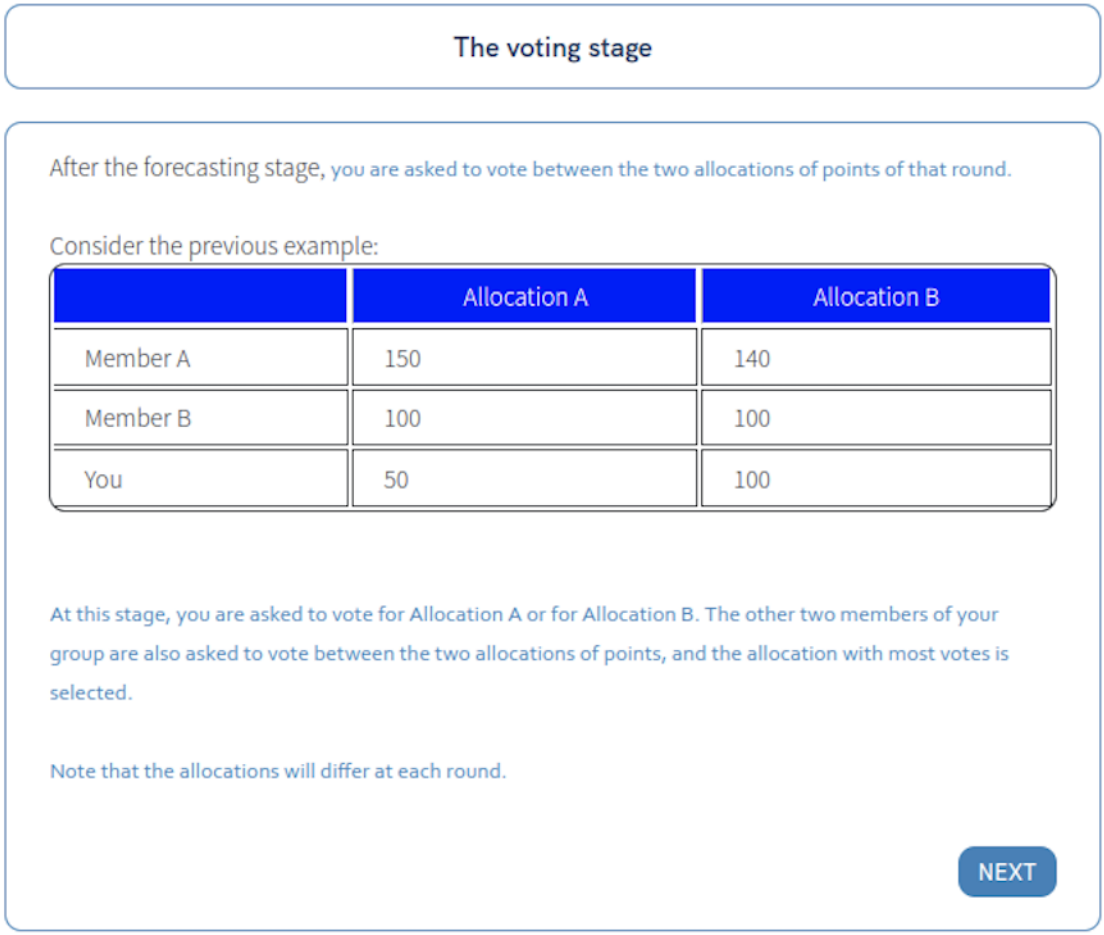

Figure G.12. Screen 7

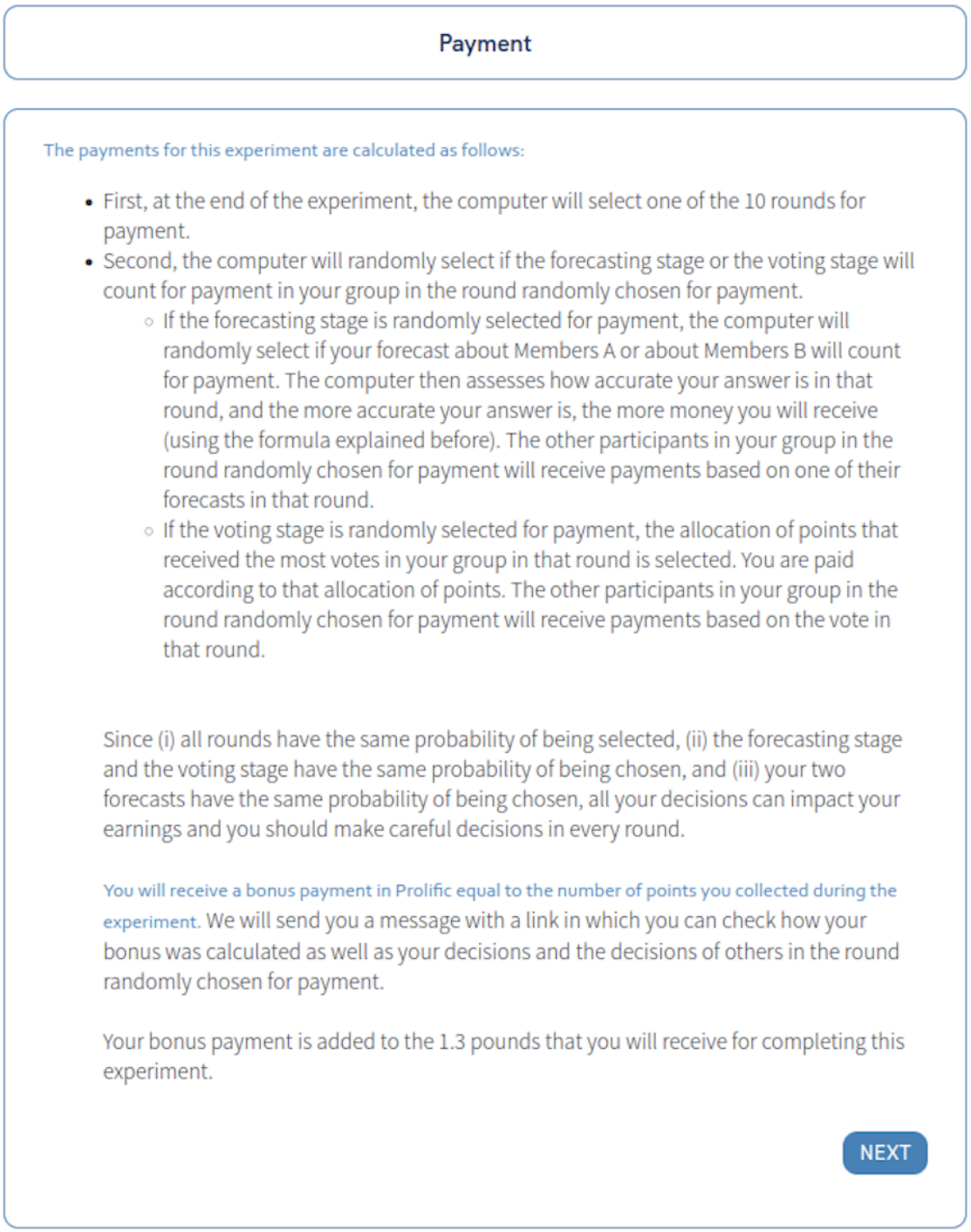

Figure G.13. Screen 8

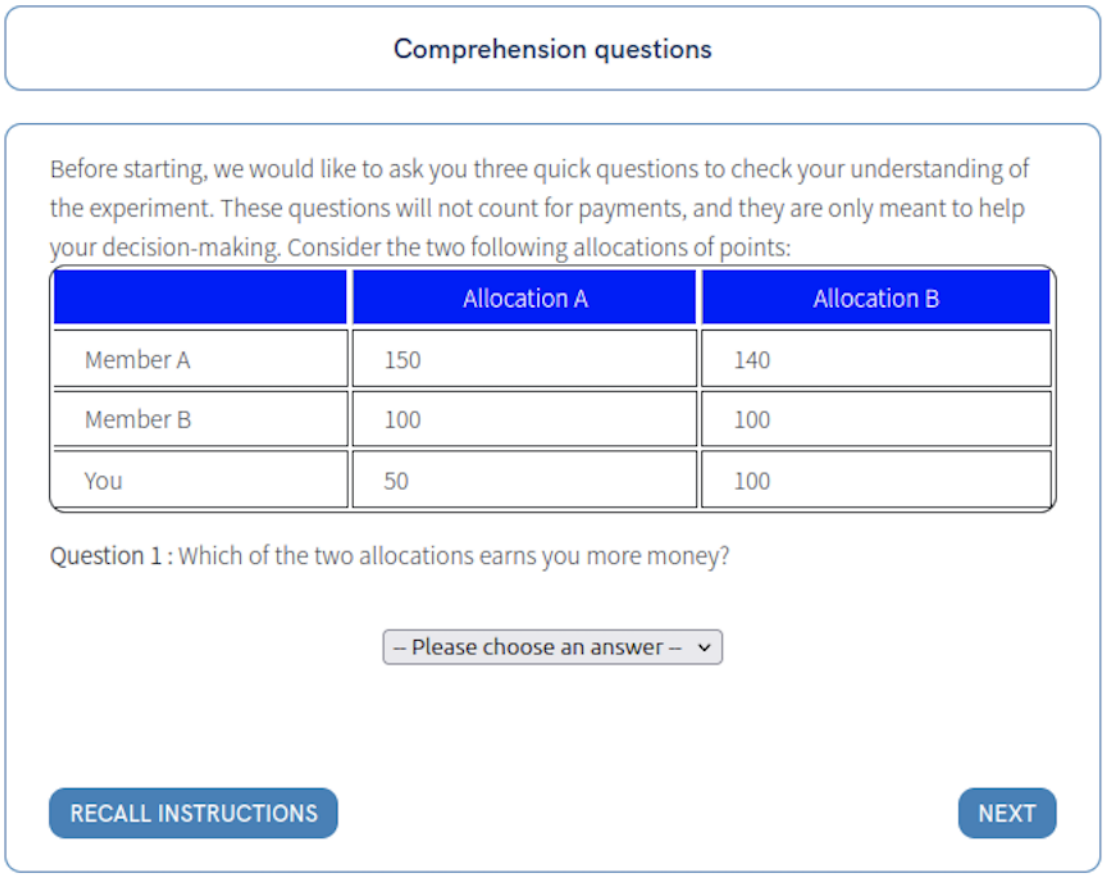

Figure G.14. Screen 9

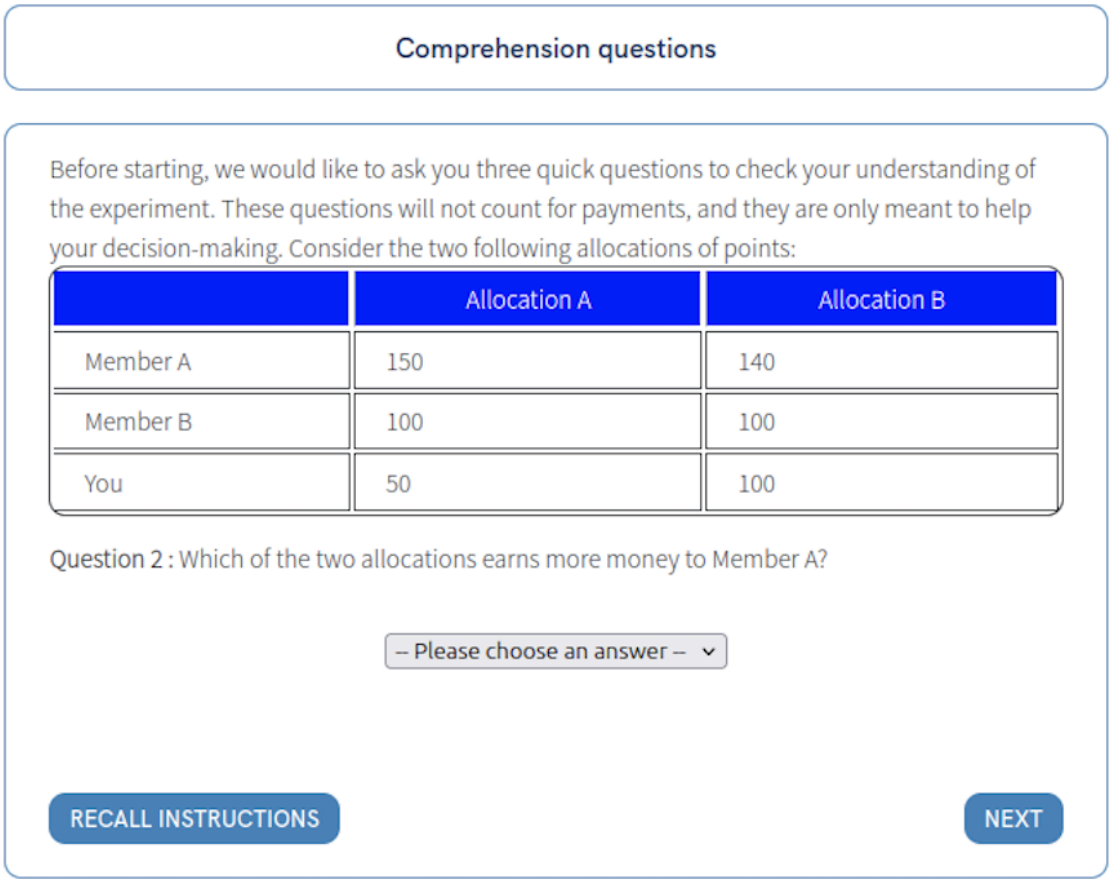

Figure G.15. Screen 10

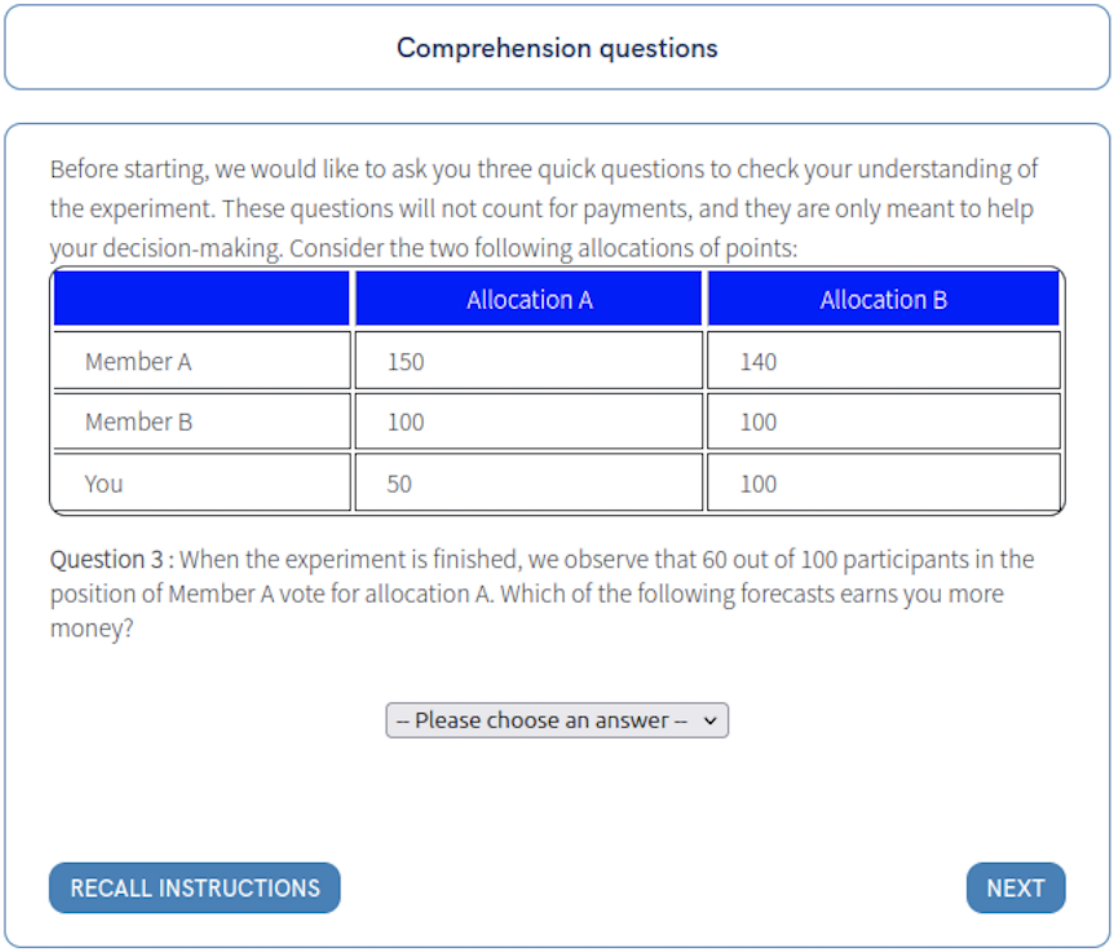

Figure G.16. Screen 11

Figure G.17. Screen 12

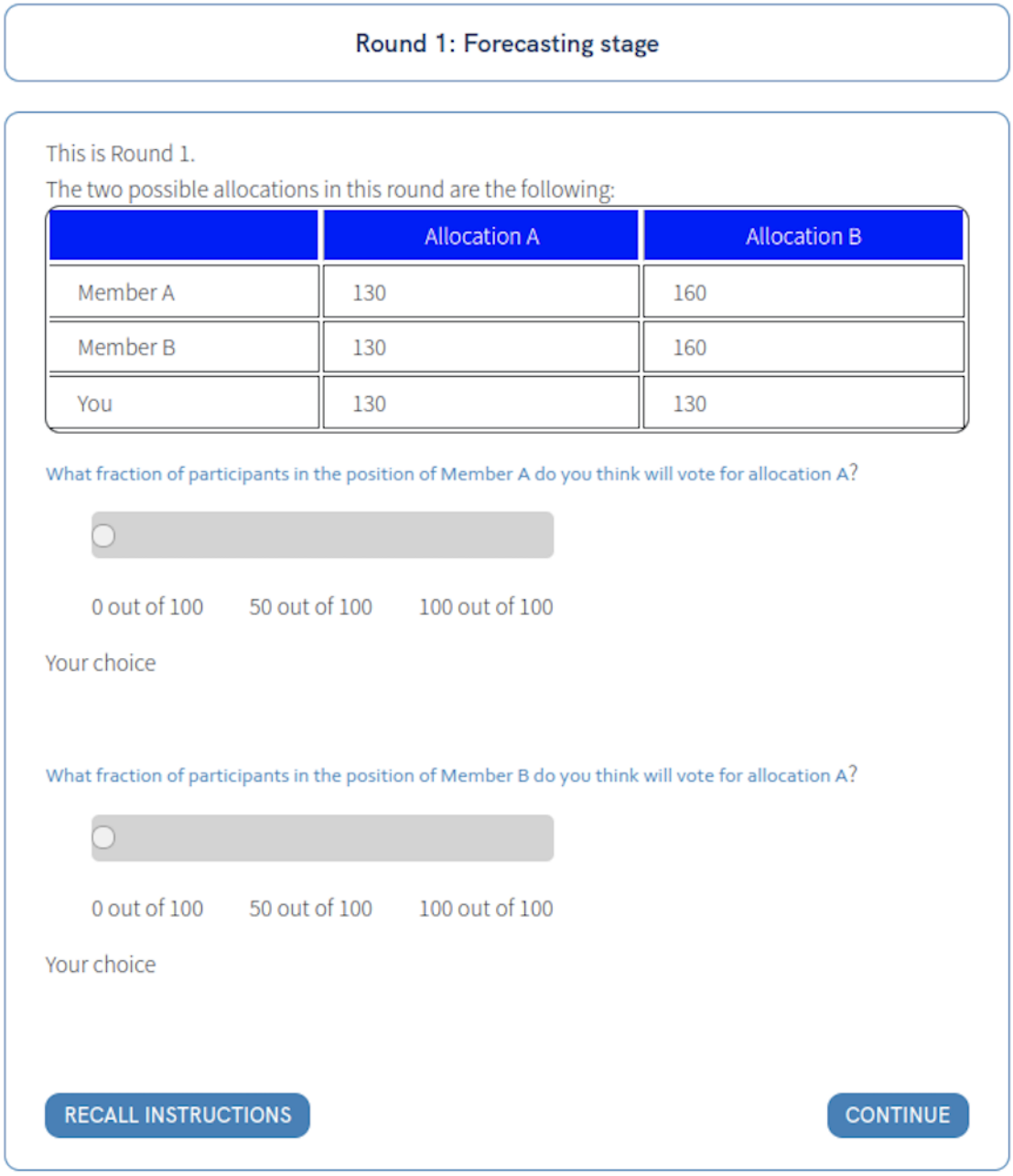

Figure G.18. Screen 13

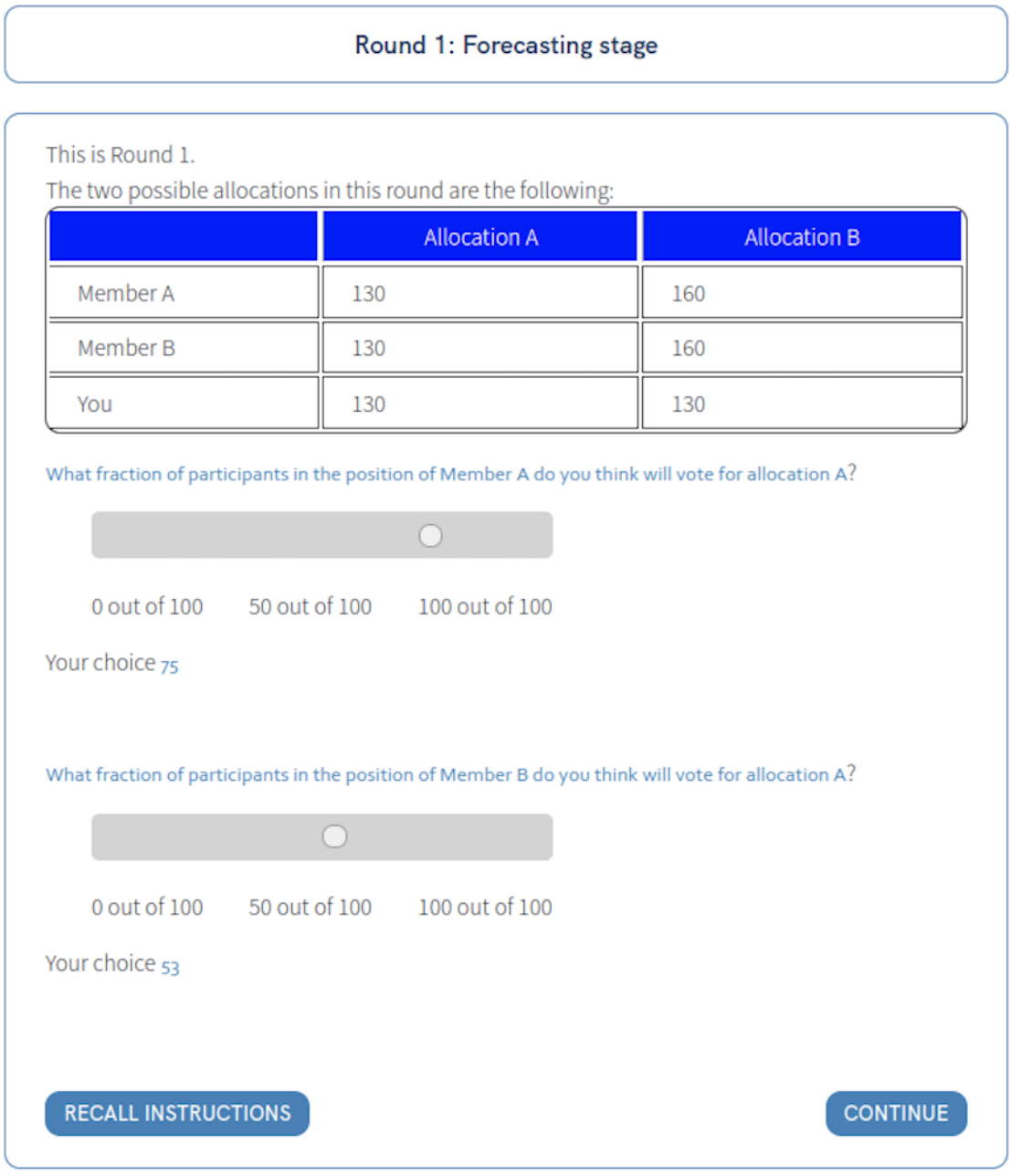

Figure G.19. Screen 14

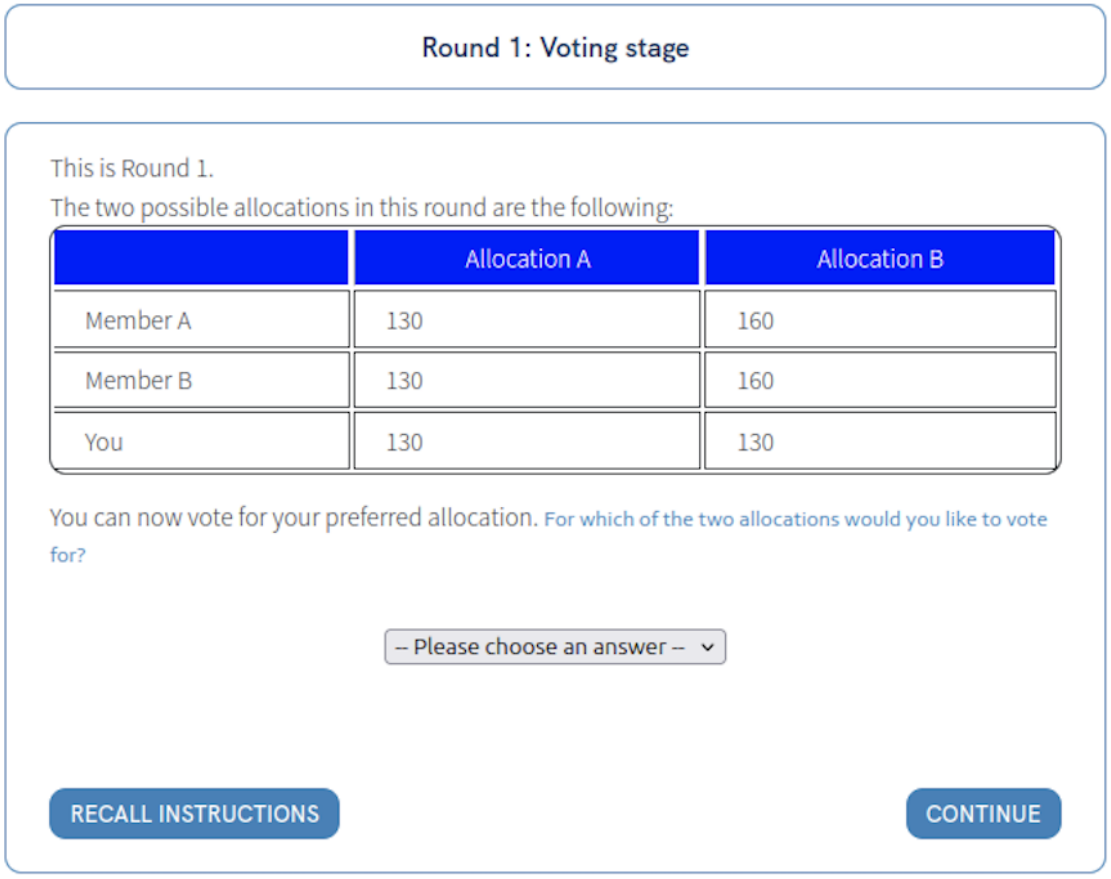

Figure G.20. Screen 15

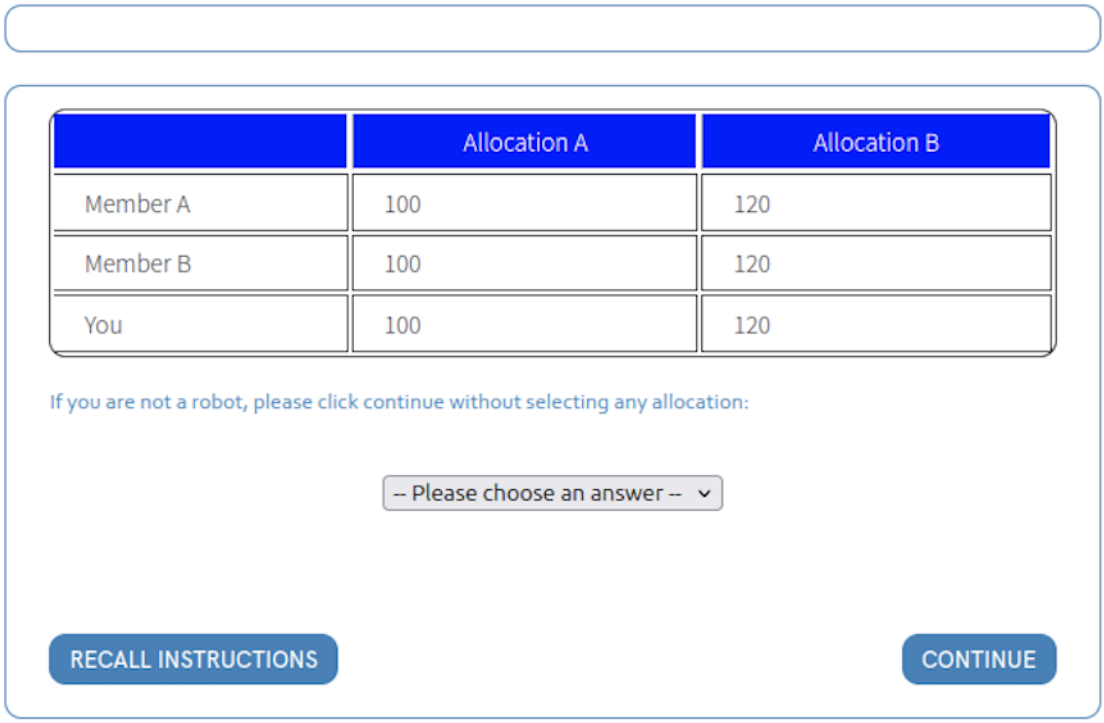

Figure G.21. Screen 16

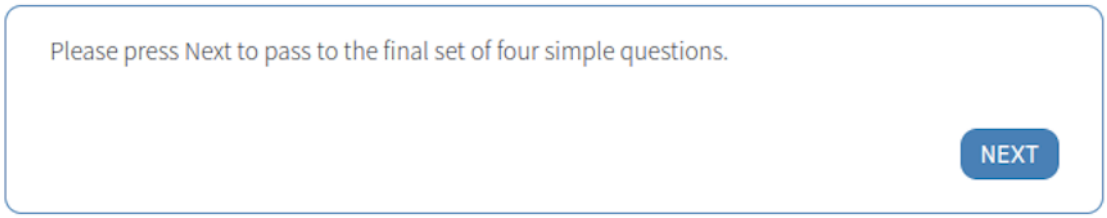

Figure G.22. Screen 17

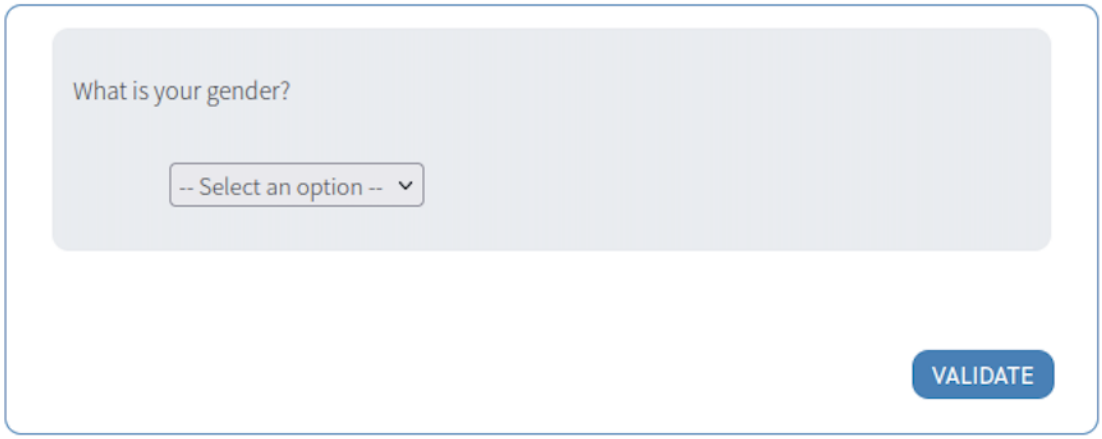

Figure G.23. Screen 18

Figure G.24. Screen 19

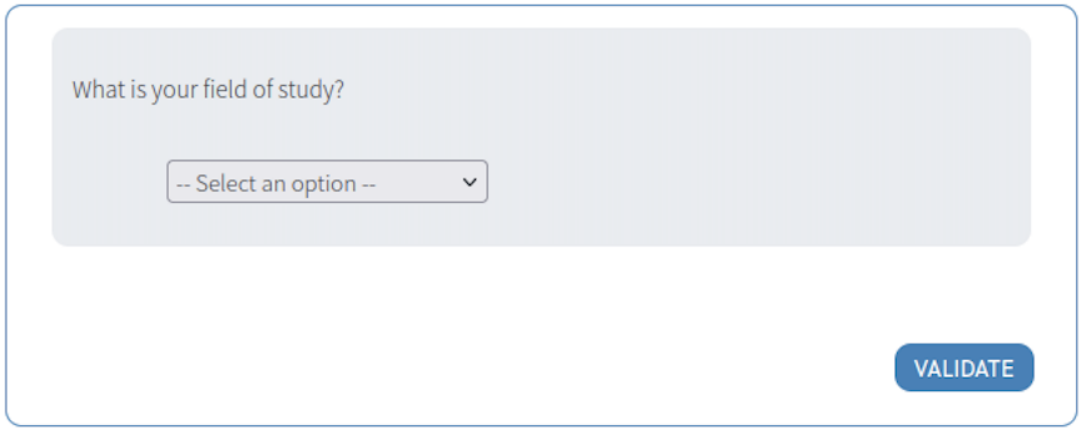

Figure G.25. Screen 20

Figure G.26. Screen 21

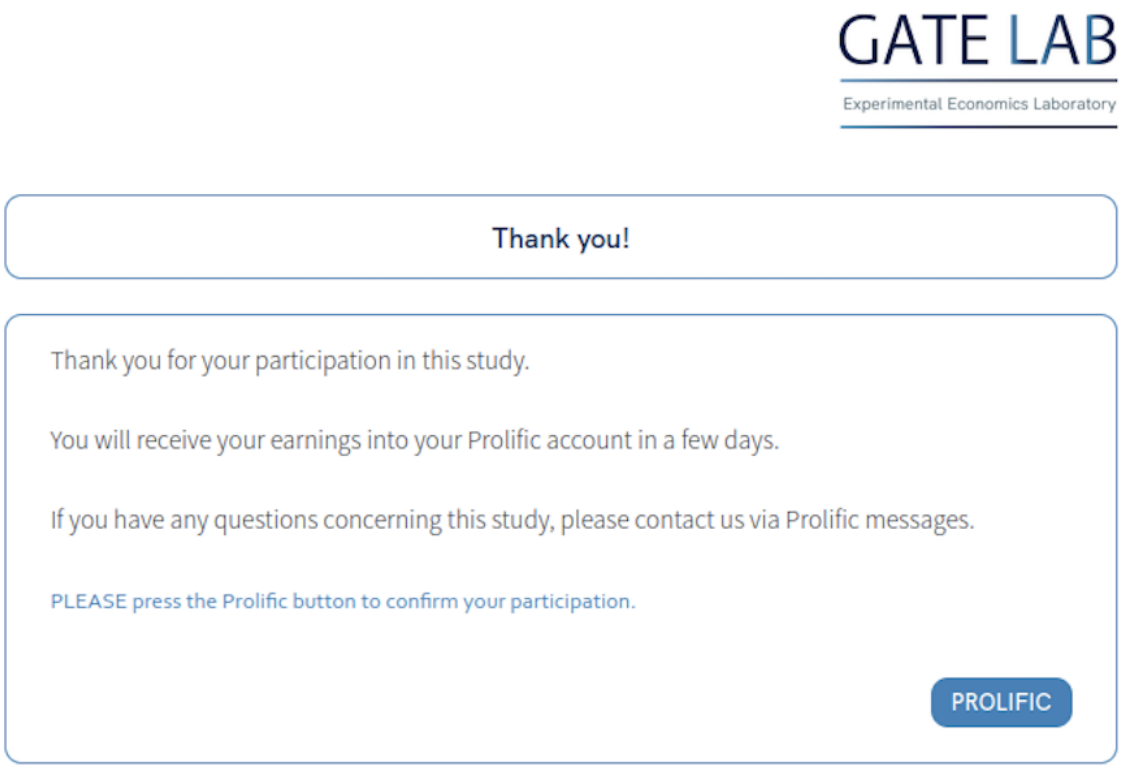

Figure G.27. Screen 22