

By chance or by choice? Biased attribution of others' outcomes*

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Abstract

Decision makers in positions of power often make unobserved choices under risk and uncertainty. What inferences are made about their choices when only outcomes are observed? Using a laboratory experiment, we investigate attribution biases in the evaluation of outcomes. Decision makers face a trade-off between maximizing their own payoff and those of other individuals. We show that attribution biases exist in the evaluation of good outcomes and decision makers receive too little credit for their successes. Importantly, the biases tend to be driven by subjects who make the selfish choice themselves when placed in the role of the decision maker.

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1 Introduction

In many environments, the determinants of outcomes are not observable. Decision makers (DMs) make unobserved choices under risk and uncertainty, and outcomes are determined by a combination of their choices and luck. For instance, a firm's profits are driven by both the business strategies taken by its managers and the macroeconomic factors that are beyond their control. How are outcomes evaluated in these situations? Are there systematic biases in the attribution of outcomes to the DMs' choices versus luck? Such beliefs can play an important role in determining incentives in many environments. For example, biases in the subjective evaluation of managers' performance in firms may affect their career prospects.¹

The goal of this paper is to examine the beliefs that others hold about the choices made by DMs. We focus on a context where the choices DMs make under risk affect their own payoffs as well as those of other individuals. DMs face trade-offs between maximizing their own payoffs and those of the other individuals. For example, a firm's decision to engage in a production technology could have positive (e.g., R&D) or negative (e.g., pollution) implications for the rest of society. Consequently, DMs' choices depend on their social preferences. We consider how individuals who are affected by the choices of the DMs form inferences about the DMs based only on observable outcomes.

Specifically, we conduct a laboratory experiment where individuals are divided into groups and one group member is assigned to be the DM in each group. The DM makes an investment choice on behalf of the group. S/he chooses between two investment options with binary outcomes. The outcome to the group depends on both the DM's choice, which is unobservable to the other group members, and luck. A high investment leads to a higher probability of the good outcome for the group but comes at a higher private cost to the DM. Hence, one can also think of the high investment decision as a costly effort choice made by the DM that increases the group's surplus at a personal cost. Using this design, we examine the group members' initial beliefs about the DM's type, and how

¹Within the policy domain, redistribution decisions may be driven by beliefs about the determinants of income (Alesina and Angeletos, 2005; Rey-Biel et al., forthcoming). Misattribution of determinants have also been shown to affect consumer choice (Haggag et al., 2018).

these beliefs are updated after observing the outcome of the DM's decision.

Standard economic theory assumes that the process of belief updating will be based on unbiased beliefs formed according to Bayes' rule. However, studies have revealed significant deviations from Bayes' rule across different contexts (see, e.g., Tversky and Kahneman, 1974; Grether, 1980; Eil and Rao, 2011). Identifying these deviations allows us to have a more realistic understanding of the decision-making process.

Accordingly, our research aims in the paper are threefold. First, we study biases in the way prior beliefs are treated. That is, we ask whether group members suffer from base-rate neglect (i.e., put too little weight on their prior beliefs) or confirmatory bias (i.e., put too much weight on their prior beliefs) relative to a Bayesian.² Second, we examine whether, relative to a Bayesian, group members respond too little or too much to new information about the choice made by the DM. Responding too little, for example, would imply that they believe luck plays a bigger role in determining outcomes. Third, we explore whether group members treat good and bad outcomes asymmetrically. For example, if they respond more to bad outcomes than to good outcomes, this implies that they believe that the DM's decision plays a larger role in bad outcomes while luck plays a bigger role in good outcomes.

Our results reveal that group members consistently suffer from base-rate neglect. This implies, for example, that members who are initially more optimistic about the likelihood that the DM made a high investment decision tend to over-update their beliefs about the DM's behavior when they observe a bad outcome. After controlling for base-rate neglect, we find that on average, members under-respond to good outcomes and attribute them more to luck as compared to a Bayesian. Their response to bad outcomes is similar to a Bayesian. This asymmetry in the way good and bad outcomes are treated is statistically significant. It implies that members on average attribute good outcomes more to luck and bad outcomes more to the DM's choice. As a result, DMs get too little credit for their successes.

²See, for example, Kahneman and Tversky (1973), Nisbett and Borgida (1975) and Tversky and Kahneman (1982) on base rate neglect, and Lord et al. (1979), Darley and Gross (1983), Plous (1991) and Rabin and Schrag (1999) on confirmatory bias.

These results have implications for behavior in environments where social preferences matter. For example, if DMs in such environments feel they will not receive sufficient credit for good outcomes, they may act less generously.

Interestingly, we uncover a link between decisions made as DMs and attribution biases. Specifically, those subjects who make a lower investment choice as DMs are also more likely to attribute others' good outcomes to luck. Hence, the asymmetry we identify in the evaluation of good and bad outcomes is driven by those individuals who make the less altruistic choice for the group.

We also consider different ways in which the DM is selected. We provide a simple theoretical framework that allows us to form predictions about the relationship between the way the DM is selected and members' initial beliefs. In line with our theoretical predictions, group members take into account the way their DM is selected while forming their initial beliefs about the DM's type. As a result, members are, for example, more likely to believe that a group-appointed DM will act in the group's interest as compared to a randomly-appointed DM. However, the biases that characterize the belief-updating process do not tend to depend on the way the DM is selected. That is, after controlling for these initial beliefs, they attribute good outcomes more to luck and bad outcomes more to the DM's choice regardless of how the DM has been selected.

Our paper contributes to the literature on biases in belief updating and information processing. Studies in experimental economics have analyzed this topic by focusing mainly on ego-related beliefs, i.e., beliefs about one's own ability or physical attributes where one's ego can play a big role in shaping their beliefs (Eil and Rao, 2011; Ertac, 2011; Grossman and Owens, 2012; Möbius et al., 2014; Coutts, 2018).³ Both Eil and Rao (2011) and Möbius et al. (2014) find evidence of asymmetric updating, where agents are more responsive to good news than to bad news about their own performance in an IQ test or a beauty task. Grossman and Owens (2012) find no evidence of asymmetry while Ertac (2011) and Coutts (2018) find that individuals tend to overweigh bad news.⁴

³The related literature in psychology has mainly focused on self-serving biases in the attribution of own versus others' outcomes (see, e.g., Miller and Ross, 1975). See also, e.g., Deffains et al. (2016) who study how self-serving attribution biases affect redistribution decisions.

⁴Consistent with Grossman and Owens (2012), Barron (2018) also finds no evidence of asymmetry in

Our novelty in relation to this literature is that we focus on the evaluation of others' outcomes. We show that good and bad outcomes are treated asymmetrically in this case also, and find that attribution biases exist in the case of good outcomes only. Moreover, our findings reveal that individuals' evaluations of others' outcomes tend to be influenced by their own behavior.

Similar to us, the literature on outcome biases, using a principal-agent framework, also focuses on the evaluation of others' outcomes. In contrast to us, this literature assumes that all determinants of outcomes are fully observable (hence, principals do not have to form beliefs about the agents' decisions). It is shown that good and bad outcomes are treated differently in this environment also (see, e.g., Charness and Levine, 2007; Gurdal et al., 2013; Brownback and Kuhn, 2018). Our research complements this literature by considering the (arguably more common) setup where determinants of outcomes are not observable.

Our paper is also related to the literature which uses observational data to investigate how individuals respond to others' favorable and unfavorable outcomes in contexts such as redistribution (Alesina and Angeletos, 2005), CEO compensation (Bertrand and Mullainathan, 2001; Leone et al., 2006), political elections (Wolfers, 2007; Cole et al., 2012), medical referrals (Sarsons, 2017), and soccer (Gauriot and Page, forthcoming).⁵ We differ from this literature by specifically focusing on beliefs about others' decisions and biases in updating behavior. Examining biases in the belief-updating process using observational data is challenging given that reliable data on subjective beliefs is often unavailable. Moreover, deriving the theoretical Bayesian benchmark using observational data is not possible since the DM's decisions are often unobservable to the researcher. The laboratory environment gives us the opportunity to examine attribution biases in a controlled setting with an objective signal generating process.

Finally, we complement the literature that examines the role of social preferences under risk. Studies on this topic consider, for example, how individuals behave in the

an environment where individuals update their beliefs about the composition of an urn. Coutts (2018) also considers other contexts such as weather forecasts.

⁵See also Palfrey and Wang (2012), who study the responsiveness of prices to signals in asset markets.

context of a dictator game or charitable giving when their decisions lead to risky or uncertain outcomes for the recipients (Brock et al., 2013; Cappelen et al., 2013; Exley, 2015). We consider a similar environment since the group members are passive players whose payoffs are determined by the DMs' choices under risk, and the DMs' choices depend on their social preferences. Importantly, we contribute to this literature by focusing on how the group members evaluate the outcomes of the DMs.

The paper proceeds as follows. After explaining the details of our experimental design in Section 2, we present the theoretical framework and discuss our hypotheses in Section 3. Section 4 presents our estimation strategy and the results. Section 5 concludes.

2 Experimental Design

2.1 Overview

Figure 1 presents an overview of the experiment. The main task in the experiment is the investment task, which we explain in Section 2.2. Prior to the investment task, subjects play the dictator game in groups of two. Each subject is given 300 Experimental Currency Units (ECU) and asked to allocate this endowment between themselves and their matched partner. Both subjects within the pair make allocation decisions as the dictator. They are told that one of the decisions will be randomly chosen at the end of the experiment to determine the final allocation of the given endowment within each pair. Once subjects play the dictator game, they receive instructions for the investment task.⁶

2.2 Investment task

The experiment features a within-subject treatment design, where subjects play six repeated rounds of the investment task. In each round, subjects are re-matched to a new group with two other individuals (perfect stranger matching). Within each group, there is a DM who makes an unobservable investment decision on behalf of the group. In

⁶The instructions can be found in Appendix A.

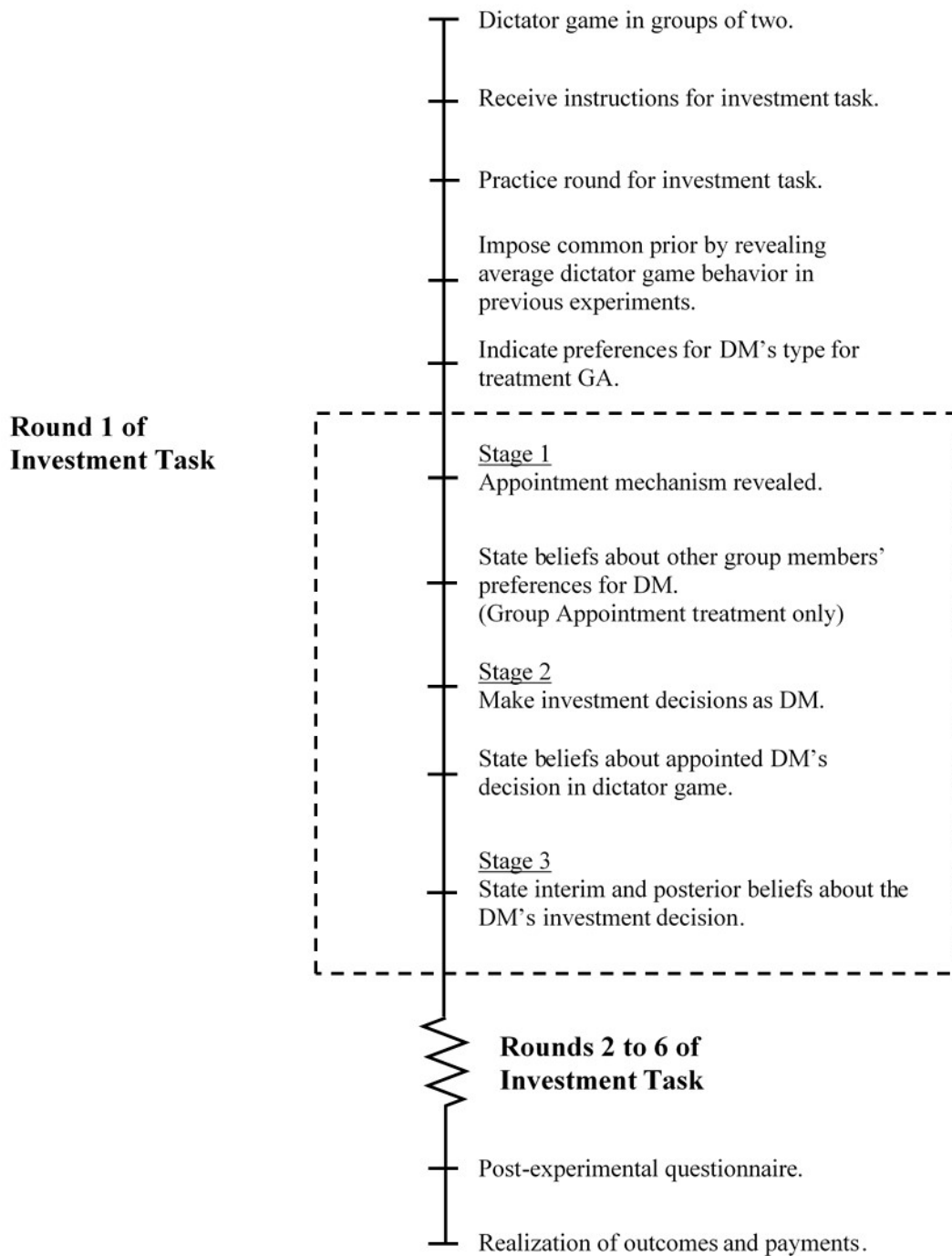


Figure 1: Overview of experiment

the experiment, to make things less abstract for the subjects, we label the DM as the leader.⁷ The group members can only observe the outcome of the DM's decision. The key variables of interest are the members' beliefs about the DM's investment decision.

Decisions are elicited using a strategy method. Hence, in each round, all subjects make their investment decisions assuming that they have been assigned to be the DM, and then state their beliefs about their DM's investment decision assuming that someone else in the group has been assigned to be the DM. This allows us to analyze whether beliefs are affected by individuals' own decisions. No feedback is given during the entire experiment. Subjects are informed whether they were assigned the role of the DM at the end of the experiment.

As shown in Figure 1, each round of the investment task consists of three stages, which we now explain in detail.

Stage 1: Appointment of DM We consider four mechanisms of appointing the DM. At the beginning of each round, subjects are informed which mechanism will be employed in that round (although they are not told who the DM is). The appointment mechanism varies across the six rounds.

In reality, DMs are appointed in a variety of ways. Considering different appointment mechanisms allows us to generate variation in members' beliefs, and to examine whether their updating behavior depend on the way the DM has been appointed. For example, it may be the case that members may be more likely to blame DMs for their failures if the DM is not appointed by the group.

In three of the appointment mechanisms, the DM is appointed exogenously. In the random assignment mechanism (treatment RA), each individual has an equal chance of being appointed as the DM. In the low assignment and high assignment mechanisms (treatments LA and HA), the group member who allocated the least and the highest amount to their matched partner in the dictator game is appointed as the DM, respec-

⁷Leadership is a natural way to frame the experiment given that leaders often make decisions under risk and uncertainty that affects the payoffs of others. This definition of leadership is similar to that in Ertac and Gurdal (2012).

tively.⁸

The fourth mechanism is the group appointment mechanism (treatment GA). Before beginning the first round of the investment task, each group member is asked to indicate whether they prefer: (i) to appoint the member who allocated the lowest amount to their matched partner in the dictator game; (ii) to appoint the member who allocated the highest amount to their matched partner in the dictator game; or (iii) to randomly select one member to be the DM. The stated preferences are used to appoint the DM in the following way. The computer randomly picks one group member and uses his/her decision to appoint one of the *other* two group members to be the DM. This ensures that there is no scope for strategic behavior in that subjects are unable to influence their probability of being the DM through their decisions.⁹ This is especially important in our set-up because, as explained later, there is a clear advantage to being the DM.

Given how the DM is appointed in treatment GA, the beliefs that subjects hold about their group members' preferences may influence their beliefs about the DM's decision in this treatment. Hence, the subjects are also asked to state their beliefs about the other two group members' preferences. They are paid an additional 10 ECU if both of their guesses are correct.

Stage 2: DM's investment decision In the second stage of the investment task, each subject is asked to make an investment decision on behalf of the group, assuming that s/he has been appointed to be the DM. The subject's decision in a given round is only implemented if s/he is appointed to be the DM for the group in that round.

The DM is given an individual endowment of 300 ECU and chooses between two investment options that will affect the payoffs of all the group members. The two investment options, given in Figure 2, are: (i) Investment X, which corresponds to a high effort level; and (ii) Investment Y, which corresponds to a low effort level. Both investment options yield the same high return if they succeed and the same low return if they fail.

⁸The details of the investment task are revealed to the subjects after they have made their decisions in the dictator game. This ensures that any strategic behavior in the dictator game is minimized. It is important to note that the actual decision of each subject in the dictator game is never revealed to their group members in the investment task at any point during the experiment.

⁹See, e.g., Galeotti and Zizzo (2018), for a similar protocol.

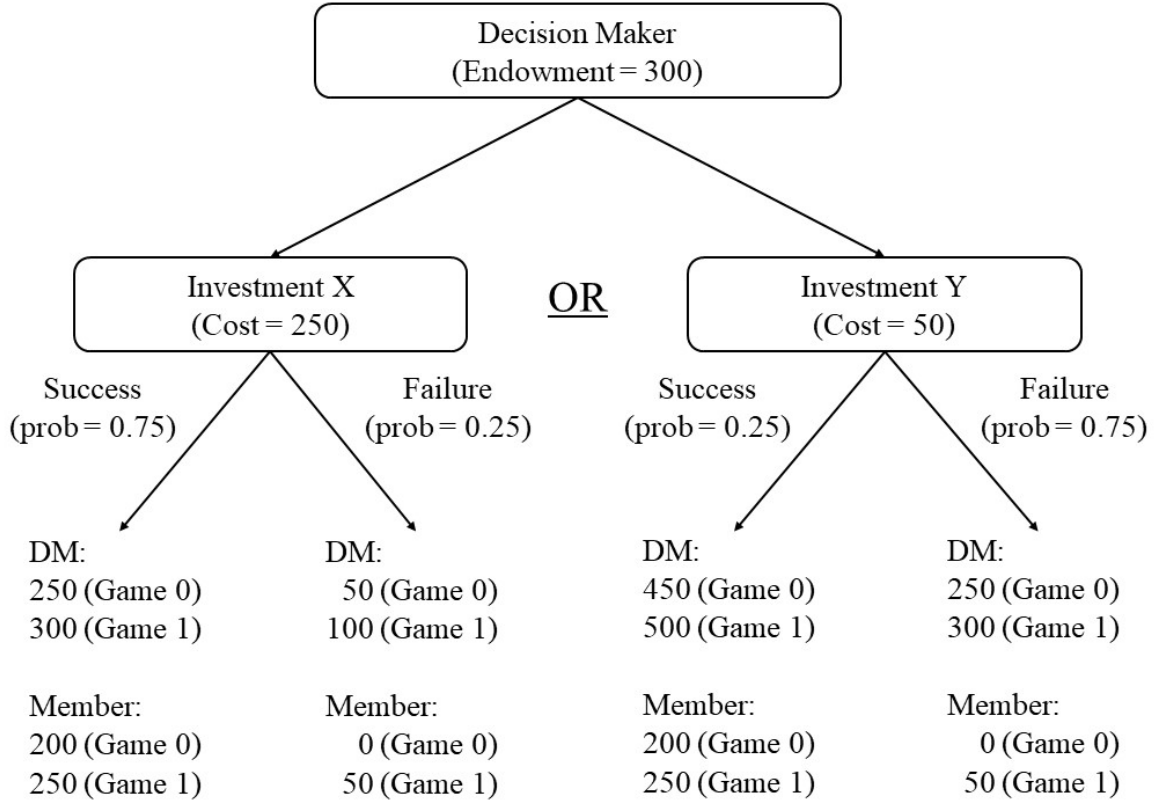


Figure 2: Investment task

However, they differ in their probability of success/failure, and in their cost to the DM. Investment X succeeds with a probability of 0.75 and costs the DM 250 ECU, while Investment Y succeeds with a probability of 0.25 and costs the DM 50 ECU. Subjects are informed that the DM's investment decision will not be revealed to the group members. They only learn the outcome of the investment in the round randomly chosen for payment at the end of the experiment.

The returns from the two investment options are assumed to take the following values. In Game 1, the investment provides a return of 750 ECU if it succeeds and 150 ECU if it fails. Note that the subjects' investment decisions as the DM and their beliefs as members about the DM's investment decision may be sensitive to the returns associated with the investment options. For instance, some subjects may be averse to choosing Investment Y if the members will receive a payoff of zero in case of failure. For this reason, we also consider Game 0, where the investment provides a return of 600 ECU if it succeeds and 0 ECU if it fails.

The return from the investment is distributed evenly between the DM and the two group members. The amount determines each group member’s final payoff, except for the DM’s. The DM’s final payoff is equal to the sum of the endowment and the share of the return from the investment minus the cost of investment. These final payoffs are given at the bottom of Figure 2.

Stage 3: Elicitation of beliefs of group members In the third stage of the investment task, subjects are asked to state their beliefs on the likelihood that the DM of their group has chosen Investment X (i.e., high effort), assuming that they have not been assigned to be the DM. We elicit two sets of beliefs from each subject. First, each subject is asked to state their unconditional belief that the DM has chosen Investment X. Given that the subjects form these beliefs after being informed of the appointment mechanism, we refer to these unconditional beliefs as the members’ *interim* beliefs. Second, each subject is asked to state their belief conditional on observing whether the investment has succeeded or failed. We refer to these beliefs as the members’ *posterior* beliefs.

We elicit beliefs on two separate screens.¹⁰ On the first screen, each subject is asked what they think is the likelihood that the DM has chosen Investment X. We elicit beliefs in the form of frequencies rather than probabilities. Previous studies have found that subjects perform better in terms of Bayesian updating and additivity when beliefs are elicited as a population frequency.¹¹ When stating their beliefs, subjects are required to enter an integer number between 0 and 100.

On the second screen, subjects are asked to state what they think is the likelihood that the DM has chosen Investment X conditional on an outcome. Specifically, they are asked to state two posterior beliefs, one assuming that the investment chosen by the DM has succeeded and a second one assuming that the investment chosen by the DM has failed. On this screen, we provide the subjects with the interim belief they have stated

¹⁰Screenshots of the decision screens can be found in Appendix B.

¹¹Specifically, Gigerenzer and Hoffrage (1995) find that subjects are more capable of performing Bayesian updating when probabilities are presented in the form of frequencies. Price (1998) finds that subjects are less likely to report extreme values in their beliefs when the questions are framed as relative frequencies. Schlag et al. (2015) argue that these findings point to the advantage of eliciting beliefs as frequencies rather than as probabilities.

in the previous screen, and ask them to consider whether their posterior beliefs are the same as or different from their interim belief. However, we do not impose any restrictions on their posterior beliefs. The group members can state any belief they want, regardless of what their interim beliefs are.

Subjects are paid for either their interim belief or their posterior beliefs. Beliefs are incentivized using the binarized scoring rule (BSR). We use the BSR because it incentivizes truth-telling independent of the subjects' risk preferences (Hossain and Okui, 2013). It is a modified version of the quadratic scoring rule with a binary lottery procedure, where the distance between a subject's belief report and the DM's investment decision determines the probability of receiving a fixed amount (10 ECU in this case). The further the subject's reported belief is from the DM's investment decision, the lower the probability of receiving the fixed payment is.¹²

In addition to the belief questions stated above, at the beginning of the third stage, subjects are also asked what they think the DM transferred to their matched partner in the dictator game. The options given are: (i) 0 ECU; (ii) 1-50 ECU; (iii) 51-150 ECU; (iv) 151-200 ECU; (v) 201-250 ECU; and (vi) 251-300 ECU. They are again asked to answer this question under the assumption that they have not been appointed to be the DM, and are paid an additional 10 ECU if their guess is correct.

2.3 Procedures and payment

The experiments were conducted in the Experimental Economics Laboratory at the University of Melbourne (E^2MU) and programmed using z-Tree (Fischbacher, 2007). We ran 10 sessions with 24 to 30 subjects in each session. A total of 282 Australian citizens were recruited using ORSEE (Greiner, 2015) to participate in the experiment.¹³ Each session lasted between 90 and 120 minutes.

¹²Specifically, for a given belief report $r \in [0, 100]$, the group member receives 10 ECU with probability $1 - [I(e = e_H) - \frac{r}{100}]^2$, where $I(e = e_H)$ is an indicator variable that equals 1 if the DM chose e_H (Investment X) and 0 otherwise.

¹³10 subjects are dropped from the analysis. Two subjects had prior experience with the experiment, while eight subjects had misreported their citizenship on the recruitment system and indicated in the questionnaire that they have lived in Australia for less than two years. Hence, data from 272 subjects are used for the analysis.

Table 1: Order of treatments for each experiment session

Session	# subjects	Round #						
		Practice	1	2	3	4	5	6
1, 5	60	GA0	LA0	HA0	RA1	LA1	HA1	GA1
2, 6	60	GA0	HA0	LA0	RA1	HA1	LA1	GA1
3, 8	54	GA0	LA0	HA0	GA1	LA1	HA1	RA1
4, 7	54	GA0	HA0	LA0	GA1	HA1	LA1	RA1
9, 10	54	GA0	LA0	HA0	LA1	HA1	GA1	RA1

To ensure that the subjects fully understood the tasks, the experimenter verbally summarized the instructions after the subjects finished reading the printed instructions. Subjects completed a set of control questions and participated in a practice round using treatment GA and Game 0 before beginning the actual investment task. To reduce experimental fatigue, subjects participated in six paid rounds of the investment task. We implemented all four appointment mechanisms for Game 1, which allows us to study the subjects' behavior across different mechanisms using the same set of parameters. For Game 0, we implemented treatments LA and HA only since the theoretical difference in interim beliefs between these two treatments is the greatest (as explained in detail in the next section).

The order between treatments was changed to control for potential order effects. However, since our main focus is the treatments associated with Game 1, Game 0 was always implemented in Rounds 1 and 2 while Game 1 was always implemented in Rounds 3 to 6. Table 1 summarizes the order of the treatments in each session. In each cell of the table, the first two letters denote the appointment mechanism, while the Arabic numeral at the end denotes the game faced by the subjects in the corresponding round within the session.¹⁴

At the end of the experiment, subjects were invited to complete a brief questionnaire which included demographic questions, questions about their decisions during the experiment, and an incentivized one-shot risk game (Gneezy and Potters, 1997) to elicit their

¹⁴For example, a cell that states "RA1" means that the DM was appointed randomly for that round (treatment RA), and the subjects played Game 1.

risk preferences. Subjects were paid for either the dictator game or the investment task. If they were paid for the investment task, then we paid them for their decisions in one of the six rounds. For the chosen round, a DM was appointed according to the corresponding treatment and the DM was paid only for their investment decision. The other two members were paid for their DM's decision as well as their stated beliefs. Earnings were converted to cash at the conclusion of the session at the rate 10 ECU = 1 AUD. Overall, subjects earned between \$10 and \$76, with the mean earnings being \$34.07. Subjects' earnings also included a show-up fee of \$10.

3 Theoretical Framework

In this section, we provide a simple theoretical framework to evaluate how beliefs will be formed under the different appointment mechanisms.

3.1 Environment and payoffs

Players maximize expected utility and are differentiated based on their other-regarding preferences. Let $\beta_i \in [0, 1]$ denote the type of player i . It is a private draw from a distribution $F(\beta)$ with density $f(\beta)$. $F(\beta)$ is common knowledge.

Players are randomly assigned to groups of size $N > 2$. One player in each group is assigned to be the DM. The DM makes an effort choice $e \in \{e_L, e_H\}$ at cost $c \in \{c_L, c_H\}$ which is deducted from an initial endowment ω that the DM receives. Assume that $\omega \geq c_H > c_L > 0$. There are two possible team outputs, $Q \in \{Q_L, Q_H\}$, where $Q_H > Q_L$, and the DM's effort choice determines the probability with which each output level will be realized. A high effort choice leads to the high output level with a higher probability, but it costs more to the DM. Specifically, a high effort choice e_H leads to an output Q_H with probability p , where $p \in (0.5, 1)$, while a low effort choice e_L leads to an output Q_H with probability $1 - p$.

We assume that the realized outcome is equally shared between the group members although the cost of effort is a private cost solely borne by the DM. Hence, for a given

outcome Q , each member in the group receives $\frac{Q}{N}$ and the utility of the DM is given by

$$U = u\left(\frac{Q}{N} + \omega - c\right) + \beta \cdot \sum_j v_j\left(\frac{Q}{N}\right). \quad (1)$$

$u(\cdot)$ and $v_j(\cdot)$ are twice differentiable utility functions with $u'(\cdot) > 0$ and $v_j'(\cdot) > 0$. $u(\cdot)$ represents the direct utility the DM receives from his/her own monetary payoff while $v_j(\cdot)$ is the utility member j receives from his/her own monetary payoff. β determines the weight the DM puts on the utilities of the other group members.¹⁵

3.2 DM's effort choice

The DM maximizes his/her expected utility and chooses e_H over e_L if $EU(e_H) \geq EU(e_L)$ or

$$\begin{aligned} & p \left[u\left(\frac{Q_H}{N} + \omega - c_H\right) + \beta \sum_j v_j\left(\frac{Q_H}{N}\right) \right] + (1-p) \left[u\left(\frac{Q_L}{N} + \omega - c_H\right) + \beta \sum_j v_j\left(\frac{Q_L}{N}\right) \right] \\ & \geq (1-p) \left[u\left(\frac{Q_H}{N} + \omega - c_L\right) + \beta \sum_j v_j\left(\frac{Q_H}{N}\right) \right] + p \left[u\left(\frac{Q_L}{N} + \omega - c_L\right) + \beta \sum_j v_j\left(\frac{Q_L}{N}\right) \right]. \end{aligned} \quad (2)$$

In the experimental design, we refer to e_H and e_L as Investment X and Investment Y, respectively. The choice of parameters in Game 0 and Game 1 are $N = 3$, $\omega = 300$, $p = 0.75$, $Q_H = 750$ (Game 1) or 600 (Game 0), $Q_L = 150$ (Game 1) or 0 (Game 0), $c_H = 250$, and $c_L = 50$. Given these parameter choices, if $\beta = 0$, then the DM only cares about his/her own payoff and chooses e_L since $EU(e_H) - EU(e_L) = p[u(\frac{Q_H}{N} + \omega - c_H) - u(\frac{Q_L}{N} + \omega - c_L)] + (1-p)[u(\frac{Q_L}{N} + \omega - c_H) - u(\frac{Q_H}{N} + \omega - c_L)] < 0$.¹⁶ For $\beta > 0$, (2) holds if

$$\beta \geq \beta^* \equiv \frac{\left[p \left[u\left(\frac{Q_H}{N} + \omega - c_H\right) - u\left(\frac{Q_L}{N} + \omega - c_L\right) \right] + (1-p) \left[u\left(\frac{Q_L}{N} + \omega - c_H\right) - u\left(\frac{Q_H}{N} + \omega - c_L\right) \right] \right]}{(1-2p) \sum_j \left[v_j\left(\frac{Q_H}{N}\right) - v_j\left(\frac{Q_L}{N}\right) \right]}.$$

Intuitively, the DM chooses high effort if s/he cares sufficiently about the payoffs of the other group members.¹⁷ In the experiment, subjects' decisions in the dictator game provide a proxy for their types (β_i). We use the dictator game since it is widely used in the literature to measure social preferences. To impose a common prior about the

¹⁵See Rotemberg (2014) for a survey of models of social preferences used in the literature.

¹⁶Note that $\frac{Q_H}{N} + \omega - c_L > \frac{Q_L}{N} + \omega - c_L = \frac{Q_H}{N} + \omega - c_H > \frac{Q_L}{N} + \omega - c_H$.

¹⁷For instance, under the assumption of risk neutrality, $\beta^* = \frac{1}{2}$.

distribution of types, as indicated in Figure 1, subjects were informed at the beginning of the investment task of the average giving behavior observed in previous experiments.¹⁸

3.3 Information and beliefs

Members' interim beliefs. We first consider the members' interim beliefs about their DM's type after observing the appointment mechanism. Specifically, we are interested in each member's belief that the DM is of type $\beta \geq \beta^*$, which corresponds to the likelihood that the DM chooses e_H over e_L . We denote member i 's interim belief after observing appointment mechanism $\Psi \in \{RA, LA, HA, GA\}$ as μ_i^Ψ .

Our first testable prediction is about the ranking of the members' interim beliefs under the different appointment mechanisms.

Hypothesis 1: $\mu_i^{LA} \leq \mu_i^{RA} \leq \mu_i^{GA} \leq \mu_i^{HA}$.

The proof is in Appendix C. In treatment GA, all players prefer to have the highest type appointed as the DM. This is because all group members want the DM to choose e_H which maximizes their expected payoffs. Although this implies that the beliefs under treatments GA and HA should be the same, the difference stated in the hypothesis is due to the implementation strategy we follow in treatment GA. Specifically, the highest type in the group will not necessarily be appointed as the DM under treatment GA if his/her appointment decision is randomly picked to be implemented. Hence, $\mu_i^{GA} \leq \mu_i^{HA}$.

Hypothesis 1 is based on the assumption that individuals use a common prior (the induced prior in the experiment) when forming their interim beliefs. However, evidence suggests that people tend to believe that others behave or think in a way similar to themselves (referred to as the consensus effect).¹⁹ This implies that their prior may also be influenced by their own type, and as a result, there may be a relationship between their decisions as DMs and their beliefs about the DM.²⁰ Since we employ a strategy

¹⁸Subjects were told that in previous experiments, (i) about 80% of participants transferred a positive amount to their matched partner, and (ii) for those who transferred, the average transfer was about 40% of their endowment. These statistics were obtained using data from pilot experiments ($N = 192$).

¹⁹See, e.g., Ross et al. (1977), Marks and Miller (1987), and Engelmann and Strobel (2000, 2012).

²⁰For example, a subject who chooses e_H may be more likely to believe that their DM has also chosen e_H .

method in our experiment, our design allows us to investigate this.

Members' posterior beliefs. We next consider how members update their beliefs about their DM's type after observing the outcome. The outcome $Q \in \{Q_L, Q_H\}$ is a signal that members receive about the DM's type. Note that $Pr(Q_L|\beta < \beta^*) = Pr(Q_H|\beta \geq \beta^*) = p$ and $Pr(Q_L|\beta \geq \beta^*) = Pr(Q_H|\beta < \beta^*) = 1 - p$.

Assuming Bayesian updating, we denote the unbiased posterior belief of group member i , given a signal Q , as $\phi_i^\Psi|_Q$. Specifically, suppose the members receive a signal $Q = Q_H$. Using Bayes' rule, member i 's posterior belief is given by

$$\phi_i^\Psi|_{Q_H} = \frac{\mu_i^\Psi \cdot Pr(Q_H|\beta \geq \beta^*)}{Pr(Q_H)} = \frac{\mu_i^\Psi p}{\mu_i^\Psi p + (1 - \mu_i^\Psi)(1 - p)}.$$

$\phi_i^\Psi|_{Q_L}$ is defined in a similar way.

Expressing the posterior beliefs in terms of a log likelihood ratio, we have

$$\log\left(\frac{\phi_i^\Psi|_{Q_H}}{1 - \phi_i^\Psi|_{Q_H}}\right) = \log\left(\frac{\mu_i^\Psi}{1 - \mu_i^\Psi}\right) + \log\left(\frac{p}{1 - p}\right), \quad (3)$$

and

$$\log\left(\frac{\phi_i^\Psi|_{Q_L}}{1 - \phi_i^\Psi|_{Q_L}}\right) = \log\left(\frac{\mu_i^\Psi}{1 - \mu_i^\Psi}\right) + \log\left(\frac{1 - p}{p}\right). \quad (4)$$

By letting $\text{logit}(x) \equiv \log\left(\frac{x}{1-x}\right)$, we can jointly express (3) and (4) as

$$\text{logit}(\phi_i^\Psi|_Q) = \text{logit}(\mu_i^\Psi) + I(Q = Q_H) \cdot \text{logit}(p) + I(Q = Q_L) \cdot \text{logit}(1 - p), \quad (5)$$

where $I(\cdot)$ is an indicator function.

We test the null hypothesis that the members will be unbiased (i.e., Bayesian) when they update their beliefs. This allows us to determine any kind of biases they may have in their belief updating process. Moreover, our design allows us to examine whether the members' updating behavior depends on the appointment mechanism. We can observe, for example, whether being appointed by the group (treatment GA) has an impact on the way the members update their beliefs about the DM. In summary, we hypothesize

that:

Hypothesis 2:

- (i) Group members behave like Bayesian agents when updating their beliefs about the DM.
- (ii) Group members behave like Bayesian agents under all of the appointment mechanisms.

We explain the econometric framework that we use to test Hypothesis 2 empirically in Section 4.3.2.

4 Results

Since there were no interactions between the group members during the experiment and no feedback was given to the subjects from the previous rounds, our unit of observation is at the subject level. For the main analyses in this paper, we pool data from the Game 0 and Game 1 treatments.²¹ For robustness, we show in Appendix D.1 that the main conclusions do not change qualitatively when we consider the Game 1 treatments only.

4.1 The dictator game as a proxy for an individual's type

We conjecture in Section 3 that subjects' behavior in the dictator game is a proxy for their type. That is, subjects who transfer more of their endowment to their matched partner in the dictator game are more likely to choose e_H when they are in the role of the DM. As our testable hypotheses depend on this relationship between the DM's type and their effort choice, we first examine if it holds.

Figure 3 presents the distribution of subjects' decisions in the dictator game against their effort choices across different appointment mechanisms. Because the subjects only participate in the dictator game once, the distribution of transfers are the same across

²¹Hence, data from treatments LA0 and LA1 are pooled together as treatment LA, while data from treatments HA0 and HA1 are pooled together as treatment HA.

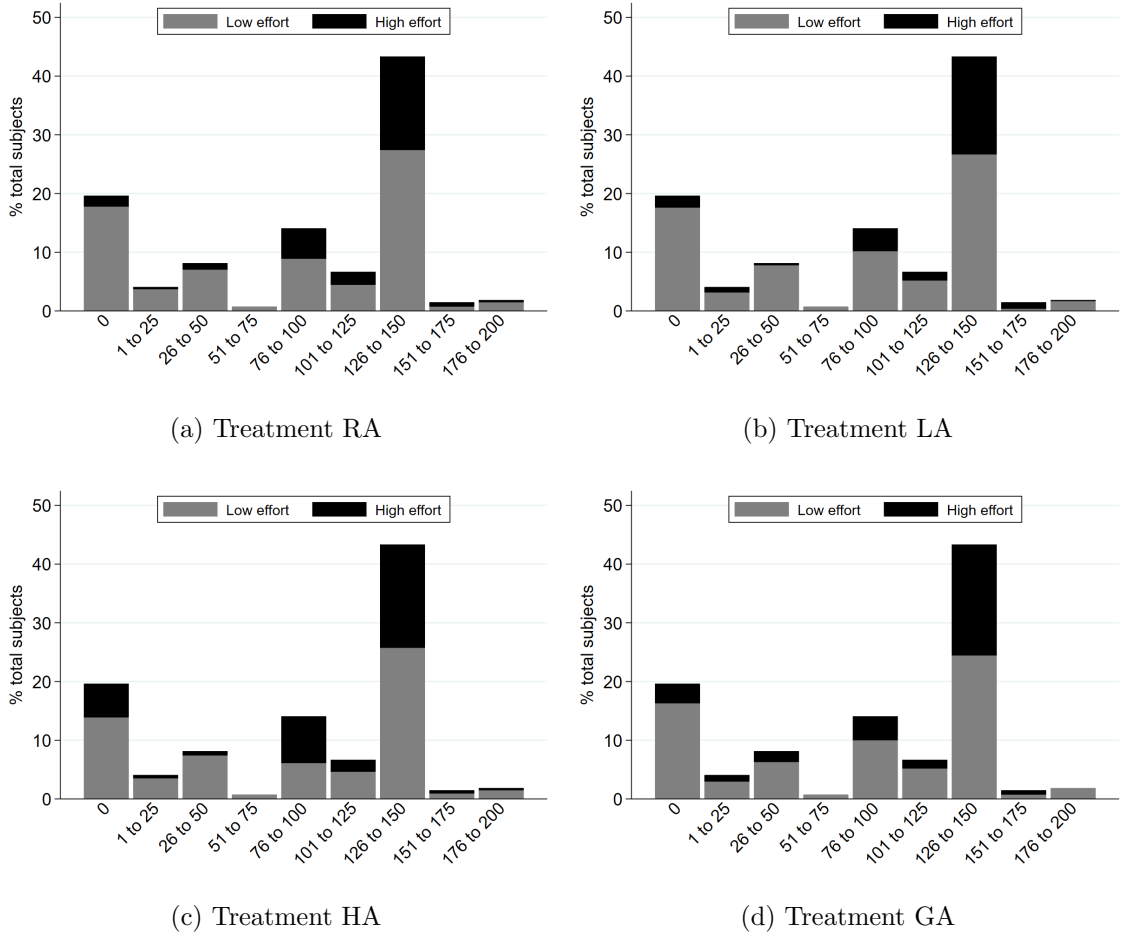


Figure 3: DM's effort choice in Game 1 against dictator game behavior

the different treatments. Within each panel in Figure 3, the black bars represent the proportion of DMs who choose high effort (e_H) while the gray bars represent the DMs who choose low effort (e_L).

A clear pattern that emerges is that DMs who are more altruistic in the dictator game are also the ones who are more likely to choose the investment option that is in the interest of the group (i.e., high effort). This pattern is consistent across the different appointment mechanisms.²² The correlation between the DM's behavior in the dictator game and their effort choice are statistically significantly positive in all treatments. The Spearman's rank correlation coefficients and corresponding p-values are: (i) RA: 0.233, p-value < 0.001; (ii) LA: 0.262, p-value < 0.001; (iii) HA: 0.096, p-value = 0.025; and

²²Interestingly, there is a higher proportion of individuals who transferred nothing to their matched partner in the dictator game, but who chose high effort as DMs in treatment HA compared to the other treatments. This may be because these individuals believe that they are unlikely to be appointed as the DM in this treatment and therefore think that their effort choice is less likely to be implemented.

Table 2: Regression of DM's effort choice

Variables	Dependent variable: =1 if DM chooses e_H (1)
% endowment transferred in DG	0.004*** (0.001)
% endowment invested in RG	-0.001 (0.001)
Treatment LA	-0.044* (0.026)
Treatment HA	0.046 (0.029)
Treatment GA	0.040 (0.029)
Game 1	-0.067*** (0.022)
Order Effects	Y
Observations	1,632

Marginal effects of probit model reported. Robust standard errors in parentheses. Standard errors are clustered at the subject level. DG: Dictator Game; RG: Risk Game.
*** p<0.01, ** p<0.05, * p<0.10.

(iv) GA: 0.183, p-value = 0.003.

Table 2 presents marginal-effects estimates from a probit model for the relationship between the subjects' decisions as DMs in the investment task and their dictator game behavior. In the regression analysis, we control for order effects, the subjects' behavior in the risk game, the appointment mechanisms, and Game 1. The estimates in the table suggest that there exists a statistically significant and positive relationship between the DM's decision in the dictator game and their decision to choose high effort in the investment task (p-value < 0.001). A DM who transfers 1% more of their endowment to their matched partner in the dictator game is 1.2% more likely to choose e_H in the investment task on average. In addition, consistent with our expectations about the DM's behavior between the Game 0 and Game 1 treatments, we observe that subjects

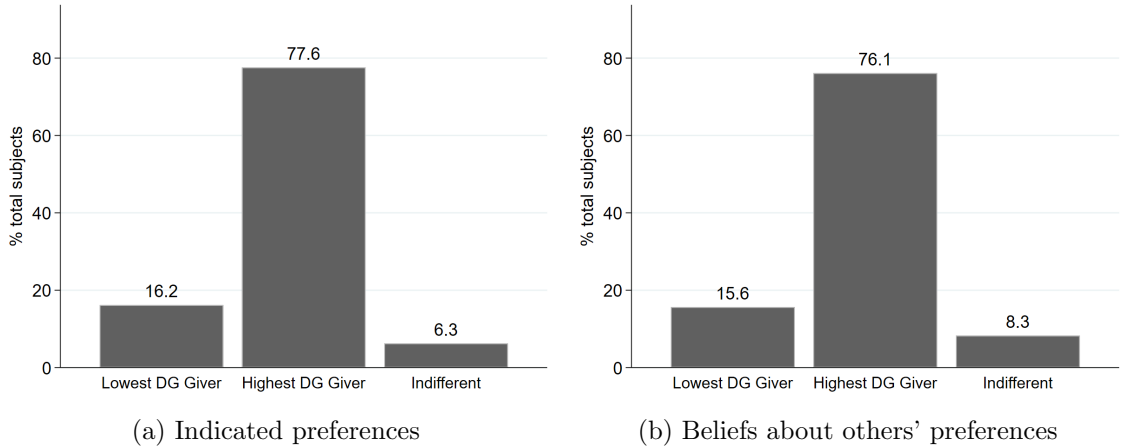


Figure 4: Preferences for DM under Treatment GA

are 19.8% less likely to choose e_H in Game 1 on average, and this effect is statistically significant (p-value = 0.002).²³

The established link between dictator game behavior and effort choices implies that subjects' preferences in treatment GA should be for the highest type to be appointed as the DM. Figure 4 presents the subjects' preferences for their DM's type under treatment GA (panel a) and their beliefs about the preferences of the other group members (panel b). The majority of the subjects (77.6%) prefer to have the individual who made the highest transfer in the dictator game to be the DM of their group. Moreover, the majority of the subjects (76.1%) believe that the other members of their group prefer to appoint the individual who made the highest transfer as the DM.

4.2 Analysis of interim beliefs

We next examine the members' interim beliefs after they observe the appointment mechanism but prior to observing the DM's outcomes. In all of our analyses, belief is a variable that takes an integer value in $[0, 100]$, where a higher belief implies that the member thinks the DM is more likely to have chosen high effort (e_H). Figure 5 presents the distributions of the members' interim beliefs by treatment. In each panel, the dashed line

²³We also elicited members' beliefs about their DM's behavior in the dictator game under each appointment mechanism. We find that there is a positive relationship between group members' interim beliefs and their reports of how much the DM has transferred in the dictator game. This further shows that the subjects regard the dictator game as a predictor of an individual's likelihood of choosing high effort as a DM.

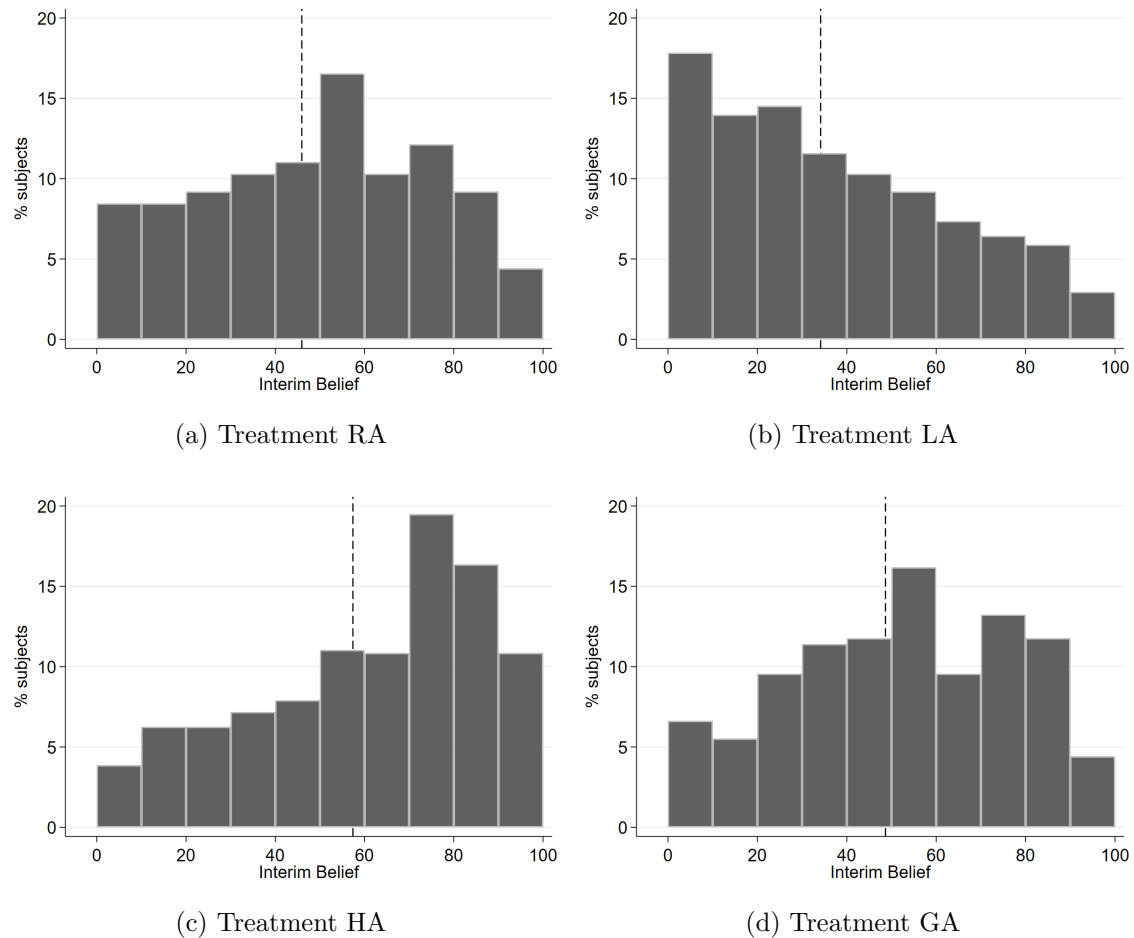


Figure 5: Distributions of group members' interim beliefs

represents the mean interim belief.

The histograms in Figure 5 suggest that group members respond to the mechanism used to appoint the DM, as stated in Hypothesis 1. In treatment RA, the DM is randomly assigned and the members' beliefs are approximately centered on 50%, with a mean of 45.94% (panel a). In contrast, the distribution of interim beliefs is highly skewed to the right in treatment LA with a mean of 34.15% (panel b), and to the left in treatment HA with a mean of 57.40% (panel c). Pairwise comparisons using the Kolmogorov-Smirnov test reveal that the distributional differences are statistically significant (RA vs. LA: $p\text{-value} < 0.001$; RA vs. HA: $p\text{-value} < 0.001$). Similarly, pairwise Wilcoxon signed-rank tests reject the null hypotheses that the average interim belief is equal between treatments RA and LA ($p\text{-value} < 0.001$), and also between treatments RA and HA ($p\text{-value} < 0.001$).

When the DM is appointed based on the preferences of the group in treatment GA (panel d), the distribution of interim beliefs shifts slightly to the right relative to that in treatment RA, and the average interim belief increases to 48.65% which is lower than that in treatment HA. The distribution of interim beliefs in treatment GA is statistically significantly different from that in treatment HA but not from that in treatment RA (Kolmogorov-Smirnov tests: (i) HA vs. GA: p-value < 0.001; (ii) RA vs. GA: p-value = 0.664). Nonetheless, Wilcoxon signed-rank tests reveal that the average interim belief in treatment GA is statistically significantly higher than that in treatment RA (p-value = 0.006) and lower than that in treatment HA (p-value < 0.001).

Table 3 presents OLS estimates for the regressions of interim beliefs against treatment variables, controlling for Game 1, order effects (in columns 1 and 3), and individual fixed effects (in columns 2 and 4). In all the specifications, treatment RA is the comparison group. The last row presents the results of a Wald test of equality between treatments HA and GA. The coefficient estimates in columns (1) and (2) support our conclusions from the non-parametric analysis. We also find that the members' beliefs are on average lower in Game 1 treatments than in Game 0 treatments. This difference is statistically significant in both columns (1) and (2) (p-value = 0.005 in both columns).

In columns (3) and (4), we control for the subjects' own decision as a DM. A subject who chooses to exert high effort when placed in the position of the DM under a specific appointment mechanism is also more likely, as a group member, to expect the DM to choose high effort under the same appointment mechanism. This effect is statistically significant (p-value < 0.001 in both columns).²⁴ The treatment effects remain similar in both direction and magnitude after controlling for the consensus effect, although the estimates for treatment GA are now statistically insignificant in column (3) and marginally statistically significant in column (4) (p-values = 0.159 and 0.093, respectively).

We summarize our results in support for Hypothesis 1 as follows:

²⁴Consistent with this finding, the consensus effect is also present in members' beliefs about the DM's behavior in the dictator game. Specifically, in additional regression analyses of members' beliefs about the DM's dictator game behavior, we find a statistically significant positive relationship between the members' own giving behavior and their beliefs about the DM's giving behavior in the dictator game. Details of these additional analyses are available from the authors upon request.

Table 3: Regression of members' interim belief

Variables	Dependent variable: Interim belief			
	(1)	(2)	(3)	(4)
Treatment LA	-13.268*** (1.417)	-13.268*** (1.416)	-12.237*** (1.403)	-12.646*** (1.372)
Treatment HA	9.982*** (1.311)	9.982*** (1.309)	8.950*** (1.263)	9.359*** (1.246)
Treatment GA	2.717** (1.355)	2.717** (1.353)	1.857 (1.273)	2.198* (1.271)
Chooses high effort as DM			23.382*** (1.848)	14.109*** (1.588)
% endowment invested in RG	-0.104** (0.043)		-0.073** (0.036)	
Game 1	-2.952*** (1.041)	-2.952*** (1.040)	-1.405 (0.955)	-2.018** (0.964)
Constant	59.182*** (3.916)	48.890*** (1.237)	48.117*** (3.534)	44.066*** (1.243)
Order Effects	Y	N	Y	N
Individual FE	N	Y	N	Y
Observations	1,632	1,632	1,632	1,632
R-squared	0.137	0.251	0.286	0.305
<u>Test of HA = GA</u>				
test statistic	5.604	5.610	5.738	5.860
p-value	0.000	0.000	0.000	0.000

Robust standard errors clustered at the subject level in parentheses. For all regressions, treatment RA is the reference treatment.

RG: Risk Game.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Result 1: Group members respond to the appointment mechanism in their interim beliefs. The interim beliefs are the lowest in treatment LA and the highest in treatment HA. The interim beliefs in treatment RA are lower than those in treatment GA.

4.3 Analysis of posterior beliefs

4.3.1 Overview

Figure 6 presents the members' posterior beliefs given the interim beliefs at the subject-round level, conditional on observing a good outcome (panel a) and a bad outcome (panel b), respectively. The size of each bubble is proportional to the frequency of each pair of interim/posterior beliefs. The solid line represents the posterior beliefs derived under Bayes' rule given the members' interim beliefs, while the 45° line represents posterior beliefs that are equal to interim beliefs.

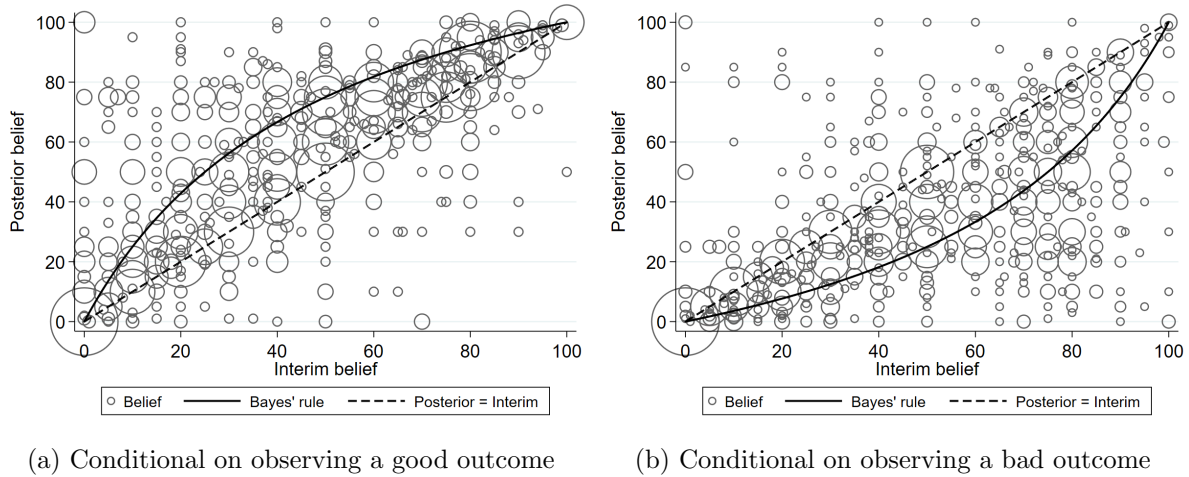


Figure 6: Scatter plot of posterior versus interim beliefs

The bubble plots in Figure 6 reveal that, on average, the members' posterior beliefs move in the same direction as predicted by Bayes' rule. However, group members significantly deviate from the Bayesian benchmark when revising their beliefs. A large proportion of members under-update their beliefs relative to a Bayesian, by having posterior beliefs between the solid and dashed lines in the figure. A relatively modest proportion of members over-update their beliefs, where their beliefs are above (below) that predicted by Bayes' rule when a good (bad) outcome is observed. Moreover, the bubble plots reveal that a non-trivial proportion of members either: (i) do not tend to revise their beliefs at all (i.e., the bubbles are on the dashed line); or (ii) revise their beliefs in a direction opposite to the observed signal (i.e., the bubbles are below (above) the dashed line when a good (bad) outcome is observed). We discuss these two types of updating behavior in

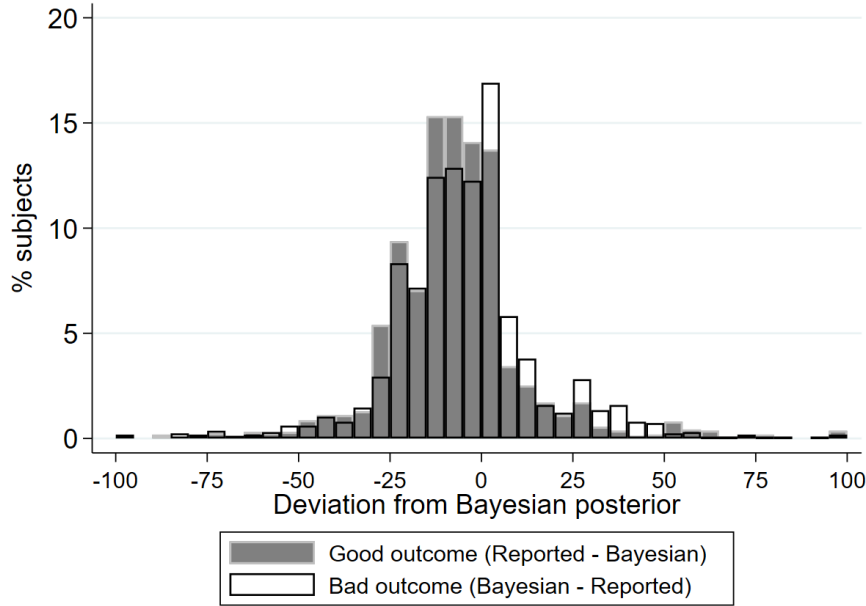


Figure 7: Deviations of reported posterior beliefs from Bayesian benchmark

Section 4.3.3.

Figure 7 presents the distribution of deviations of the members' reported posterior beliefs from the Bayesian benchmark as calculated from their stated interim beliefs. Note that if a member under-updates his/her belief in response to a good (bad) outcome, then s/he arrives at a posterior that is lower (higher) than the Bayesian benchmark. The figure is plotted such that, for any observed outcome, a negative deviation from the Bayesian benchmark represents the case of under-updating relative to the Bayesian benchmark and a positive deviation represents the case of over-updating.

Overall, Figure 7 suggests that, relative to the Bayesian benchmark, members under-update their beliefs upon observing the DM's outcomes. The average deviations are statistically significantly different from zero for both good and bad outcomes (Wilcoxon signed-rank tests: p -values < 0.001). Importantly, there is an asymmetry in the way members treat good and bad outcomes. Members appear to update their beliefs less when a good outcome is observed as compared to when a bad outcome is observed. The difference in the distributions of deviations is statistically significant (Kolmogorov-Smirnov test: p -value < 0.001).

Figure 8 presents a scatter plot of the deviations of members' reported posterior

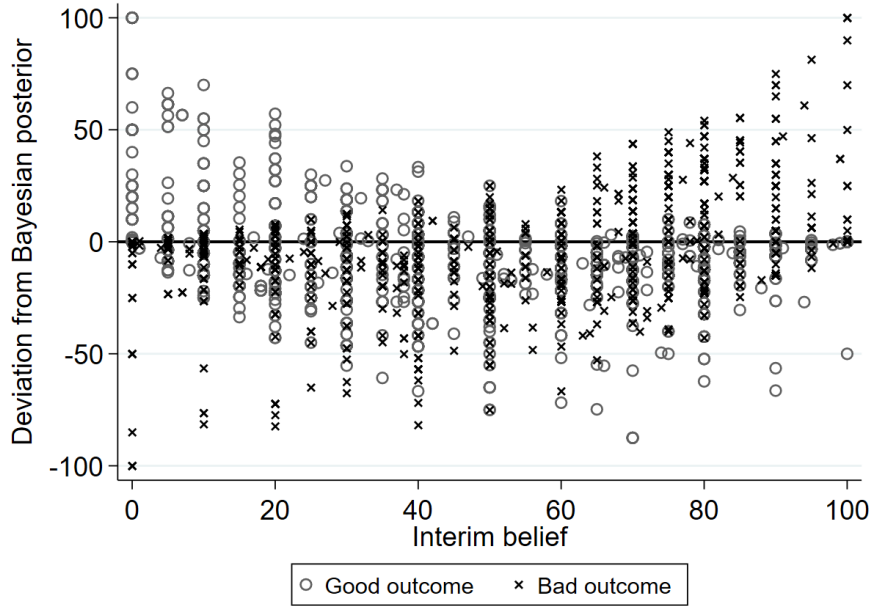


Figure 8: Deviations from Bayesian benchmark against interim beliefs

beliefs from the Bayesian benchmark against their interim beliefs when they observe a good outcome (represented by circles) and a bad outcome (represented by crosses), respectively. The solid line represents points where, given an interim belief, the posterior belief equals to that predicted by Bayes' rule. The graph shows that there is a correlation between the extent of deviation from the Bayesian benchmark and the members' interim beliefs, and that the direction of this relationship depends on whether a good outcome or a bad outcome is observed. For instance, when members observe a good outcome, they tend to over update their beliefs relative to the Bayesian benchmark when their interim beliefs are low, but under update their beliefs when their interim beliefs are high. On the contrary, when members observe a bad outcome, they tend to under (over) update their beliefs relative to the Bayesian benchmark when their interim beliefs are low (high). Hence, we observe evidence of base-rate neglect where, relative to the Bayesian benchmark, members tend to over respond to the DM's outcome when the outcome contradicts their initial expectations of the DM.

We next present in Section 4.3.2 the estimation strategy we use to analyze the updating behavior in greater detail.

4.3.2 Estimation strategy for posterior beliefs

As an empirical test of Hypothesis 2, we augment the belief updating equation in (5) in the following way to determine the channels through which deviations from the Bayesian benchmark occur:

$$\text{logit}(\hat{\phi}_i^\Psi | Q) = \delta \text{logit}(\hat{\mu}_i^\Psi) + \gamma_G I(Q = Q_H) \cdot \text{logit}(p) + \gamma_B I(Q = Q_L) \cdot \text{logit}(1 - p) + \varepsilon_i, \quad (6)$$

where ε_i captures non-systematic errors. This specification allows us to determine the weights members place on their interim beliefs and the signals they receive.²⁵ Note that $\delta = \gamma_G = \gamma_B = 1$ equates (6) to (5). This is the case where there is no bias in belief updating. Formally, Hypothesis 2 states that $\delta = \gamma_G = \gamma_B = 1$ both (i) at the pooled level and (ii) for each appointment mechanism.

Any deviation in the parameters from 1 is interpreted as non-Bayesian updating behavior. Specifically, δ captures the weight that a group member places on his/her interim belief in the updating process, γ_G captures the extent to which a member responds to a signal of good outcome from the DM, and γ_B captures the extent to which a member responds to a signal of bad outcome from the DM. We use Figures 9 and 10 to explain these parameters in more detail.

Figure 9 shows the implications of different values of δ on the relationship between the member's posterior and interim beliefs, conditional on observing a good outcome and holding γ_G constant (at 1).²⁶ Note that δ corresponds to the slope of the linear regression. If $\delta < 1$, then the member suffers from *base-rate neglect* in that s/he places too little weight on his/her interim belief. To see this, consider a member whose interim belief μ_A is less than 0.5. This corresponds to $\text{logit}(\mu_A) < 0$ in Figure 9. Hence, the member believes that the DM is more likely to have chosen low effort. When Q_H is observed, the signal contradicts with the interim belief. However, s/he arrives at a posterior belief that is greater than that of a Bayesian (i.e., point A' instead of point A). In other words, the member neglects his/her interim belief and over-updates in response to receiving a signal

²⁵See, e.g., Grether (1980), Möbius et al. (2014), Ambuehl and Li (2018), Buser et al. (2018), and Coutts (2018) for similar estimation approaches.

²⁶A similar analysis can be done for the case where a bad outcome is observed.

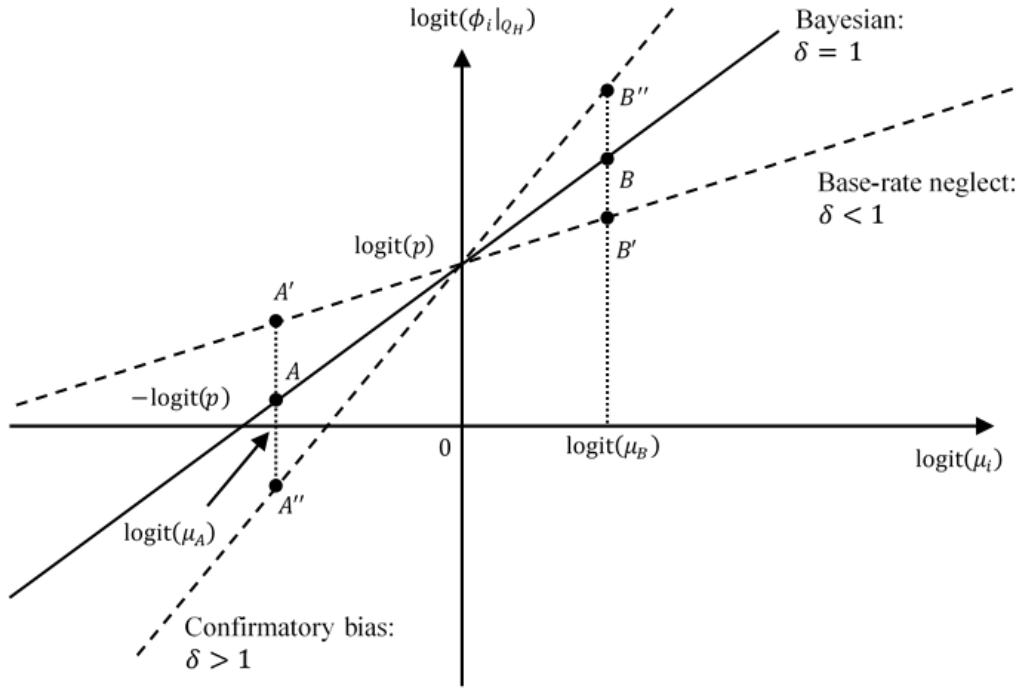


Figure 9: Interpretation of δ given Q_H observed and $\gamma_G = 1$

that contradicts with what s/he initially believes to be true.²⁷

Conversely, $\delta > 1$ implies that the member suffers from *confirmatory bias* in that s/he places too much weight on his/her interim belief. To see this, consider a member whose interim belief μ_B is greater than 0.5, i.e., $\text{logit}(\mu_B) > 0$ in Figure 9. When Q_H is observed, the signal confirms the interim belief. However, his/her posterior belief is at point B'' instead of point B . Hence, the member over-updates relative to a Bayesian when s/he receives a signal that confirms what s/he initially believes to be true.²⁸

Figure 10 shows the implications of different values of γ_G on the relationship between the member's posterior and interim beliefs.²⁹ Note that γ_G and γ_B correspond to the intercepts of the regression conditional on the signal received by the member. If $\gamma_G > 1$,

²⁷Now consider a member whose interim belief μ_B is greater than 0.5. After observing Q_H , a signal that confirms this belief, suppose that his/her posterior belief is at B' . This implies that a member who suffers from base-rate neglect does not update as much as a Bayesian would when s/he receives a signal that confirms his/her interim belief.

²⁸Alternatively, consider a member whose interim belief μ_A is less than 0.5. After observing Q_H , a signal that contradicts with this belief, suppose that his/her posterior belief is at A'' . This implies that a member who suffers from confirmatory bias does not update as much as a Bayesian would when s/he receives information that contradicts with his/her interim belief.

²⁹A similar analysis can be done for the case where a bad outcome is observed.

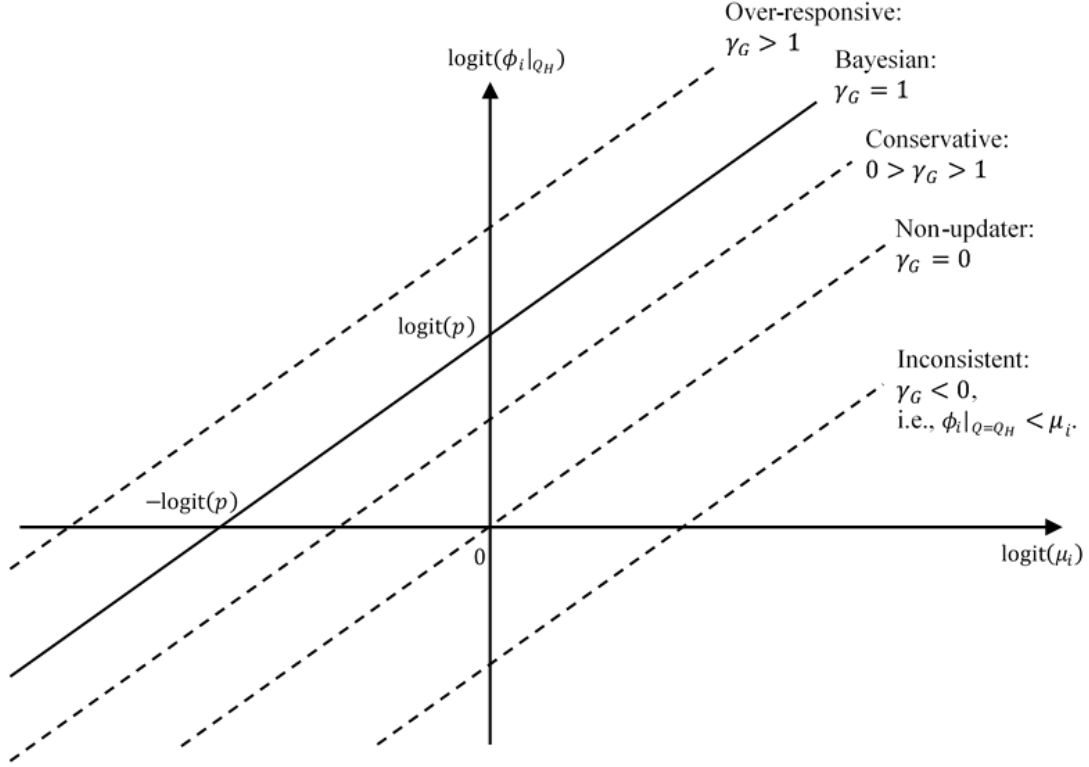


Figure 10: Interpretation of γ_G given Q_H observed and $\delta = 1$

then the member is, on average, over-responsive to good signals relative to a Bayesian, and tends to arrive at a posterior that is higher than that of a Bayesian. Specifically, the biased member attributes good outcomes more to the DM's decision as compared to an unbiased Bayesian member. On the other hand, if $\gamma_G < 1$, then the member is conservative in his/her response to good signals, and tends to arrive at a posterior that is lower than that of a Bayesian on average. In this case, the biased member attributes good outcomes more to luck as compared to an unbiased Bayesian member. Figure 10 also shows what happens when $\gamma_G = 0$ or $\gamma_G < 0$, which correspond to a non-updater and an inconsistent updater, respectively.

Finally, we can also capture asymmetric updating of beliefs, i.e., asymmetric attribution of outcomes to the DM's decision (effort choice) and luck. If $\gamma_G > \gamma_B$, then the member is more likely to attribute a good outcome to the DM's decision and a bad outcome to luck. Conversely, if $\gamma_G < \gamma_B$, then the member is more likely to attribute a bad outcome to the DM's decision and a good outcome to luck.

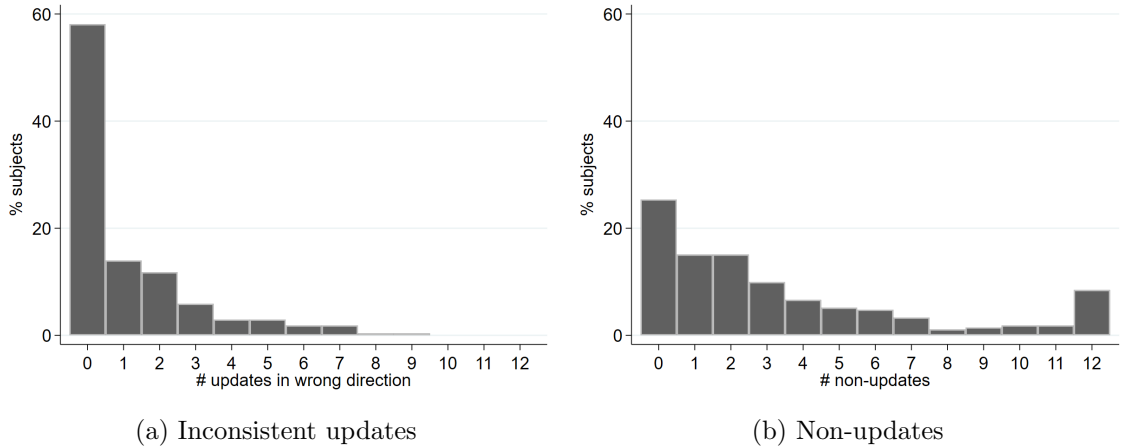


Figure 11: Distribution of inconsistent and non-updates by subjects

4.3.3 *Inconsistent and non-updaters*

Figure 11 presents the distribution of subjects based on the number of inconsistent updates and non-updates throughout the experiment. A belief update is classified as *inconsistent* if the posterior belief is in the opposite direction to that predicted by Bayes’ rule. A belief update is classified as a *non-update* if the posterior belief is equal to the interim belief.

The histograms reveal that a non-trivial proportion of subjects update their beliefs inconsistently or not at all. This may be because some subjects fail to understand the experiment or fully engage with it (despite the detailed instructions and practice round provided to them). The inclusion of these observations in the analysis may result in biased or incorrect conclusions, particularly if these subjects are reporting beliefs that do not genuinely reflect their true posterior beliefs. Hence, for the remainder of the analysis, we exclude a subject if 25% or more of his/her posterior beliefs are inconsistent (44 out of 272 subjects in total) or if s/he reports a posterior belief equal to the interim belief across all six rounds of the experiment (23 subjects in total). These two groups jointly constitute 24.6% of the sample. Note that these numbers are largely in line with what is found in the literature (see, e.g., Möbius et al., 2014; Barron, 2018; Coutts, 2018).³⁰

³⁰We present the analyses including these subjects in Appendix D.2. While the inclusion of these subjects leads to an attenuation of the coefficient estimates of γ_G and γ_B , the results remain the same qualitatively. In addition, our results are robust to using different thresholds for excluding inconsistent updaters. Details of the analyses using different thresholds are available upon request.

Table 4: Regression of members' posterior beliefs

Variables	Dependent variable: Logit(posterior)				
	(1) Pooled	(2) RA	(3) LA	(4) HA	(5) GA
δ : Logit(interim belief)	0.695*** (0.039)	0.764*** (0.071)	0.692*** (0.054)	0.703*** (0.058)	0.529*** (0.135)
γ_G : Good outcome \times logit(p)	0.751*** (0.051)	0.744*** (0.089)	0.622*** (0.079)	0.847* (0.081)	0.798** (0.098)
γ_B : Bad outcome \times logit($1 - p$)	0.966 (0.067)	0.932 (0.092)	1.058 (0.117)	0.946 (0.072)	0.876 (0.114)
Observations	2,460	410	820	820	410
R-squared	0.608	0.686	0.651	0.583	0.421
<u>Test of $\gamma_G = \gamma_B$</u>					
test statistic	-3.190	-1.588	-3.065	-1.081	-0.512
p-value	0.002	0.114	0.002	0.281	0.609

Robust standard errors clustered at the subject level in parentheses. This analysis excludes subjects classified as inconsistent or non-updaters.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Null hypothesis is coefficient = 1.

4.3.4 Estimating deviations from Bayes' rule

We now estimate equation (6) using ordinary least squares (OLS) to analyze the biases that members suffer from when updating their beliefs.³¹ Table 4 presents the regression results both at the pooled level (column 1) and at the treatment level (columns 2 to 5).³² As a test of Hypothesis 2, our primary interest is to examine whether the coefficients are different from 1. Hence, asterisks are used in the table to indicate whether a coefficient is statistically significantly different from 1.

Column (1) shows that group members are biased in their belief-updating process. The estimate for δ suggests that they suffer from base-rate neglect on average (test of $\delta = 1$: p -value < 0.001).³³ The estimate for γ_G suggests that after controlling for

³¹One concern with estimating (6) using OLS is that the estimates are biased if there are measurement errors in the subjects' reported beliefs. For example, subjects could make mistakes or are imprecise when reporting their beliefs. For robustness, we also consider an alternative specification where the appointment mechanisms are used as instruments for the logit of members' interim beliefs for the analysis at the pooled level. This instrumental-variable (IV) approach requires that the appointment mechanisms are a strong predictor of the members' interim beliefs (as we show in Section 4.2) and do not have a separate direct effect on their posterior beliefs. We find that the IV estimates lead to similar conclusions. Details of the results from the IV regression analysis can be found in Appendix D.3.

³²Note that the logit function is only defined for beliefs in (0,100). Instead of excluding observations of subjects who state 0 or 100 as their interim or posterior belief about the DM, we take the logit of 0.01 or 99.99 as an approximation.

³³An alternative and less restrictive specification to (6) is to allow δ to depend on the signal received

the weight members place on their interim beliefs, members are conservative in their responses to good outcomes. That is, they attribute good outcomes to luck more than a Bayesian would and this effect is statistically significant (test of $\gamma_G = 1$: p-value < 0.001). However, there is no statistically significant evidence that members respond to bad outcomes differently from a Bayesian (test of $\gamma_B = 1$: p-value = 0.608). Hence, relative to the Bayesian benchmark, group members give too little credit for the DM's success but the right amount of blame for the DM's failure.

The last two rows of Table 4 present the results of a Wald test of equality between γ_G and γ_B , giving us a test of the presence of an asymmetric attribution bias. Overall, members update their beliefs about the DM asymmetrically (i.e., $\gamma_G < \gamma_B$). They tend to attribute good outcomes more to luck and, relatively, bad outcomes more to the DM's decision. This effect is statistically significant (p-value = 0.002).

In summary, we fail to find support for Hypothesis 2(i). Members are not Bayesian when updating their beliefs after observing their DM's outcomes.

Result 2: On average, group members suffer from base-rate neglect in their updating behavior. Moreover, their treatment of good and bad outcomes are statistically significantly different. Compared to the Bayesian benchmark, members attribute good outcomes more to luck, but their average response to bad outcomes is not different from Bayesian.

The biases and asymmetry in updating behavior that we find at the aggregate level may not necessarily hold at the individual level. We consider heterogeneity in updating behavior using finite mixture model analyses in Appendix D.4. The results show that although members consistently suffer from base-rate neglect, for most updates this is at a modest level. Moreover, the majority of belief updates in the sample is characterized by under-responsiveness to the DM's outcomes and an asymmetric attribution of the DM's outcomes to his/her decision and luck.

In addition to showing evidence of individual heterogeneity, it is useful to explore the factors which may contribute to it. We consider subjects' effort choices (which are

by the members. However, estimating δ_G and δ_B separately, we find that both of these parameter estimates are statistically significantly different from 1 (p-values < 0.001 for both), but the difference between them is not statistically significant (p-value = 0.692).

Table 5: Regression of members' posterior beliefs based on effort choice as DMs

Variables	Dependent variable: Logit(posterior)	
	(1)	(2)
	Chose low effort	Chose high effort
δ : Logit(interim belief)	0.710*** (0.048)	0.603*** (0.073)
γ_G : Good outcome \times logit(p)	0.698*** (0.059)	0.957 (0.110)
γ_B : Bad outcome \times logit($1 - p$)	0.923 (0.080)	0.951 (0.100)
Observations	1,646	814
R-squared	0.626	0.553
<u>Test of $\gamma_G = \gamma_B$</u>		
t-statistic	-2.568	0.049
p-value	0.011	0.961

Robust standard errors clustered at the subject level in parentheses. This analysis excludes subjects classified as inconsistent or non-updaters.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Null hypothesis is coefficient = 1.

determined by their types). The results in Section 4.2 reveal that subjects' effort choices affect their interim beliefs. Since we already control for interim beliefs in equation (6), our aim here is to test whether there is another channel through which subjects' effort choices as DMs affect their posterior beliefs, specifically through the updating behavior.

Table 5 reports separate parameter estimates of (6) based on whether the subjects have chosen low effort (column 1) or high effort (column 2) as DMs within a given round in the investment task. The estimates of δ and γ_B are not statistically significantly different between columns (1) and (2) (p-values = 0.222 and 0.818, respectively). However, the estimate of γ_G is statistically significantly different between the two columns (p-value = 0.035). While the estimate for γ_G is statistically significantly less than 1 in column (1) (p-value < 0.001), it is not different from 1 in column (2) (p-value = 0.697). Hence, regardless of their effort choices as DMs in a given round of the task, subjects suffer from base-rate neglect ($\delta < 1$) and are no different from a Bayesian in their response to bad outcomes ($\gamma_B = 1$) on average. However, in a given round of the investment task, those

individuals who choose low effort as DMs are more likely to attribute good outcomes to luck when they make decisions as group members.

We conclude our analysis of posterior beliefs by analyzing the members' updating behavior across the different appointment mechanisms (Hypothesis 2(ii)). The coefficient estimates in columns (2)-(5) of Table 4 reveal that biases similar to the ones observed at the pooled level exist at the treatment level. Under each appointment mechanism, members consistently suffer from base-rate neglect, attribute good outcomes more to luck, and treat bad outcomes no differently from a Bayesian. The asymmetry observed in the attribution of outcomes is statistically significant in treatment LA only (Wald tests of $\gamma_G = \gamma_B$: p-values = 0.002, 0.114, 0.281 and 0.609, respectively, for treatments LA, RA, HA and GA). The lack of statistical significance in the other treatments may be because there are fewer observations for the analyses at the treatment level.

In summary, we do not find support for Hypothesis 2(ii).

Result 3: On average, group members are biased in their updating behavior under all the appointment mechanisms. Moreover, they exhibit similar biases regardless of the way the DM has been appointed.

Comparing the magnitudes of the biases across the appointment mechanisms, we fail to reject the null hypotheses that the estimates for δ , γ_G , and γ_B are jointly equal to one another (Wald tests of $\delta^{RA} = \delta^{LA} = \delta^{HA} = \delta^{GA}$: p-value = 0.395; $\gamma_G^{RA} = \gamma_G^{LA} = \gamma_G^{HA} = \gamma_G^{GA}$: p-value = 0.110; and $\gamma_B^{RA} = \gamma_B^{LA} = \gamma_B^{HA} = \gamma_B^{GA}$: p-value = 0.686). However, we observe that the estimate for γ_G is the lowest and the estimate for γ_B is the highest in treatment LA. Pairwise comparisons reveal that only the differences between the estimates of γ_G in treatments LA and HA, and the estimates of γ_G in treatments LA and GA are statistically significant (p-values = 0.020 and 0.099, respectively). This suggests that when the most selfish individual is appointed to be the DM, group members are less likely to believe that good outcomes result from a choice of high effort.

5 Conclusion

In many environments, the determinants of outcomes are not observable. What beliefs do individuals hold in such circumstances about the role of luck versus decisions in the determination of others' outcomes? Do these beliefs depend on the outcome, i.e., whether the outcome is good or bad? These are the questions we address in this paper.

In a group setting, we find that how the DM is appointed is important in that it affects the expectations (unconditional beliefs) group members have about the type of their DM. We also find that after observing the outcome of the DM's investment decision, members suffer from biases in their belief-updating process. Relative to the Bayesian benchmark, members suffer from base-rate neglect in that in the belief updating process, they place too little weight on their beliefs prior to the realization of the outcome. After controlling for base-rate neglect, members attribute good outcomes more to luck as compared to a Bayesian. In their response to bad outcomes, they are no different from a Bayesian. The asymmetry we observe in the way good and bad outcomes are treated suggests that the credit DMs receive for good outcomes is less than the blame they get for bad outcomes. Importantly, we find that the biases in updating behavior tend to be driven by those subjects who choose low effort as DMs.

Determining the systematic biases that individuals may have in the way they process new information and update their beliefs about the decisions of others is critical in a wide range of economic and social interactions. One general implication of our study is that the biases we identify may affect the generosity of DMs in environments where social preferences matter. For example, they may act less generously if they know that they will not receive sufficient credit for good outcomes. The biases may also affect DMs' willingness to take risk. For instance, if business or political leaders are aware that they are given relatively more blame for their failures than credit for their successes, then this may perpetuate a culture of failure avoidance. Such a 'fear of failure' culture may reduce their incentives to exert costly effort or their tolerance towards risk.

Our study is a first step in identifying the biases which may exist in the evaluation of others' decisions. We specifically focused on a context where altruistic preferences play a

key role in decision-making. More work needs to be done to understand whether different types of biases exist in other contexts and what the implications of these biases are.

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[FOR ONLINE PUBLICATION]

A Experimental instructions

Overview of Experiment

Thank you for agreeing to take part in this study which is funded by the Australian Research Council. Please read the following instructions carefully. A clear understanding of the instructions will help you make better decisions and increase your earnings from the experiment.

You will participate in two experiments today: Experiment 1 and Experiment 2. You will receive detailed instructions for each experiment before you participate in them. Note that your decisions in Experiment 2 will not change the earnings that you receive from Experiment 1. You will be informed of the outcomes of both experiments at the end of today's session.

You will be paid for the decisions you make in either Experiment 1 or Experiment 2. This implies that you should carefully consider all of the decisions you make in both experiments as they may determine your earnings. Whether you will be paid for Experiment 1 or Experiment 2 will be randomly determined at the end of the session. Your final payment today will also include a \$10 participation fee.

During the experiments, we will be using Experimental Currency Units (ECU). At the end of the session, we will convert the amount you earn into Australian Dollars (AUD) using the following conversion rate: 10 ECU = 1 AUD.

At the end of Experiment 2, you will be asked to fill out a brief questionnaire asking you some general questions. All of the decisions you make in today's session will remain anonymous.

Please do not talk to one another during the experiment. If you have any questions, please raise your hand and we will come over to answer your questions privately.

Experiment 1

You will participate in Experiment 1 in groups of two. The computer will randomly match you with one other person in the room. You will never learn the identity of your partner.

Each of you is given an endowment of 300 ECU, and you are asked to divide this amount between yourself and the person you are matched with.

At the end of today's session, if this experiment is picked for payment, then you will be paid either according to your decision or according to the decision made by your randomly matched partner. The computer will randomly determine whose allocation decision will be implemented.

Example. Suppose you choose to divide your endowment by keeping 200 ECU for yourself and giving 100 ECU to your matched partner. Your matched partner decides to keep 130 ECU and give 170 ECU to you. If, at the end of the experiment, the computer randomly determines that it is the allocation of your matched partner that gets implemented, then your payment will be 170 ECU and your matched partner's payment will be 130 ECU.

Are there any questions? If not, we will proceed with Experiment 1.

Experiment 2

Experiment 2 consists of six identical rounds. At the end of the experiment, if you are paid for Experiment 2, then the computer will randomly pick one of the six rounds for payment.

You will participate in each round in groups of three. At the beginning of each round, the computer will randomly match you with two other people in this room with whom you have not been matched before. You will never learn the identity of your partners. Each round consists of three stages.

Stage 1: Appointment of a group leader.

In this stage, one group member will be assigned to be the leader of the group. There will be four possible methods to determine who is assigned the role of the leader. At the beginning of each round, the computer will reveal which method will be used to determine the leader for that round.

Method 1: One group member will be randomly assigned by the computer to be the leader. Hence, each group member has an equal chance of being assigned the role of the leader.

Method 2: The group member who transferred the lowest amount to his/her matched partner in Experiment 1 will be assigned to be the leader (ties will be broken randomly).

Method 3: The group member who transferred the highest amount to his/her matched partner in Experiment 1 will be assigned to be the leader (ties will be broken randomly).

Method 4: Each individual within the group will be asked to indicate whether you prefer your leader to be someone who has transferred the highest or the lowest amount to his/her matched partner in Experiment 1. The computer will then randomly pick one of the decisions of the group members to implement. If your decision is implemented, then one of your other two group members will be appointed to be the leader based on your preference. Hence, you will not be appointed to be the leader if your decision is implemented.

Example 1. Suppose the leader is appointed using Method 4. In Experiment 1, Player 1 chose to transfer 100 ECU to his/her matched partner, and Player 2 chose to transfer 160 ECU to his/her matched partner. Player 3 indicates that his/her preferred leader is someone who has transferred the lowest amount to his/her matched partner in Experiment 1. If the computer randomly determines that Player 3's decision will be implemented, then Player 1 will be assigned the role of the leader.

You will only need to indicate your preferred leader for Method 4 once, at the beginning of Experiment 2. The same decision will be used whenever Method 4 is being used to determine the appointment of the group leader.

Stage 2: Investment decision by the group leader.

The leader will be given an endowment of 300 ECU. S/he will be asked to choose between two investment options that will affect the payoffs of all group members. Each investment can either fail or succeed. The two investment options have different chances of success/failure. They also have different costs to the leader.

Specifically, the two investments are:

Investment X: This investment costs 250 ECU to the leader. It will succeed with a 75% chance, and fail with a 25% chance.

Investment Y: This investment costs 50 ECU to the leader. It will succeed with a 25% chance, and fail with a 75% chance.

The payoffs to the leader and each group member in this stage of Experiment 2 are calculated as follows:

1. Payoff to leader = 300 ECU – Cost of investment + Returns on investment
2. Payoff to each group member = Returns on investment

Note that the amount that you receive from each investment may be different in each round, and this may affect the final payoffs to the leader and each group member. However, you will always receive a higher payoff if the investment succeeds, and a lower payoff if it fails. Please pay attention to these numbers on the screen in each round.

Figure A.1 shows an example where the returns of each investment options are 200 ECU if the investment succeeds, and 0 ECU if the investment fails, i.e., as shown by the numbers in **red**.

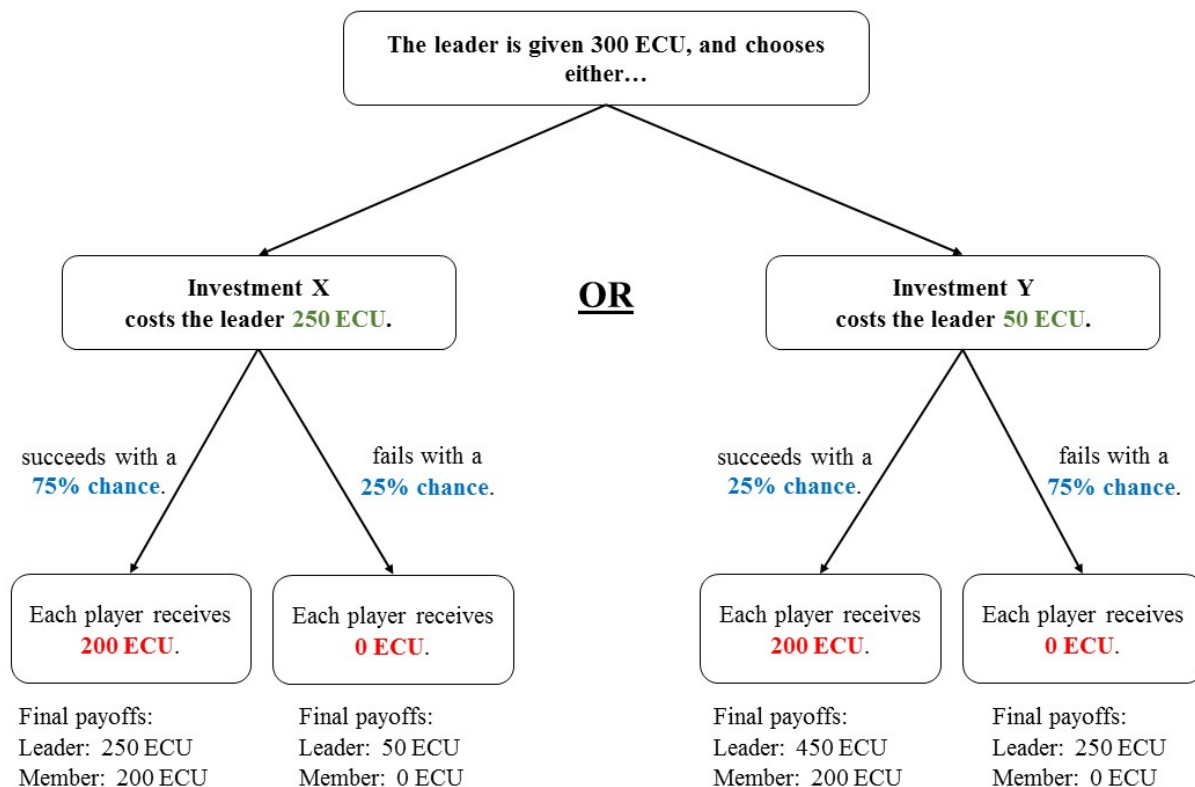


Figure A.1: Investment Options (Example of a Round)

Example 2. Suppose in the round depicted in Figure A.1, the leader chooses Investment X for the group. Then, the investment costs the leader 250 ECU, and will succeed with a 75% chance and fail with a 25% chance. At the end of the experiment, if the investment succeeds, then each group member will receive 200 ECU, and the leader will receive $(300 - 250 + 200) = 250$ ECU for this stage of Experiment 2.

Example 3. Suppose in the round depicted in Figure A.1, the leader chooses Investment Y for the group. Then, the investment costs the leader 50 ECU, and will succeed with a 25% chance and fail with a 75% chance. At the end of the experiment, if the investment fails, then each group member will receive 0 ECU, and the leader will receive $(300 - 50 + 0) = 250$ ECU for this stage of Experiment 2.

You will be informed whether you have been assigned the role of the leader at the end of the experiment. Hence, you will be asked to make an investment decision in Stage 2 of each round assuming that you have been assigned the role of the leader. Your decision will be implemented if you have been assigned the role of the leader for that round.

At the end of the experiment, all group members will learn how much they have received from the chosen investment, but they will not learn the investment decision of the leader.

Stage 3: Beliefs of the other group members.

After you have made your investment decision, you will be asked to predict which investment your leader has chosen, assuming that someone else in your group has been assigned the role of the leader.

Specifically, we would like to know how likely it is in your opinion that the leader has chosen Investment X. *Suppose there were 100 people in the position the leader is in now. How many of them do you think would choose Investment X?*

You will need to choose a number between 0 and 100. A higher number means that you think the leader is more likely to have chosen Investment X.

The specific questions you will be asked are listed below.

Question 1

Suppose there were 100 people in the position the leader is in now. How many of them do you think would choose Investment X?

In Question 2, you are given additional information. You are asked to evaluate the same question with this additional information. Specifically, you should consider whether your guess of the leader's decision will be different, given that you know the outcome of the investment chosen by your leader.

Question 2

Suppose you are informed that the investment chosen by your leader has succeeded, and you have therefore received the high payoff.

Now consider whether your guess will be higher than, lower than, or the same as the one you stated in Question 1. That is, suppose there were 100 people in the position the leader is in now. Given an outcome of high payoff, how many of them do you think have chosen Investment X?

Suppose you are informed that the investment chosen by your leader has failed, and you have therefore received the low payoff.

Now consider whether your guess will be higher than, lower than, or the same as the one you stated in Question 1. That is, suppose there were 100 people in the position the leader is in now. Given an outcome of low payoff, how many of them do you think have chosen Investment X?

The computer will randomly select one of these two questions and you will be paid for your response to this question. If Question 2 is chosen for payment, then you will be paid for your answer to the scenario that corresponds to the actual outcome of the investment chosen by your leader.

The section below describes how your payoff in Stage 3 will be determined. This procedure has been used in many other studies. We explain the procedure in detail, but what is most important is that this payoff structure is designed such that it is in your best interest to report your true belief about your leader's decision.

Your payment for the question randomly chosen by the computer is determined as follows. You will receive 10 ECU with some chance. Your chance of receiving 10 ECU depends on your answer and the leader's decision. The closer your guess is to the actual decision made by your leader, the higher is your chance of receiving the fixed payment of 10 ECU.

Specifically, your chance of receiving 10 ECU is determined by the following formula:

$$\text{Chance of receiving 10 ECU} = \left[1 - \left(\frac{x - \text{your guess}}{100} \right)^2 \right] \times 100.$$

x takes the value of 100 if your leader chose Investment X, and x takes the value of 0 if your leader chose Investment Y.

To illustrate, suppose your leader has chosen Investment X. This means that $x = 100$ in the formula above, and your chance of receiving 10 ECU will be higher if your guess is higher. If you state 100 as your guess that the leader has chosen Investment X, then your chance of receiving 10 ECU will be $[1 - (\frac{100-100}{100})^2] \times 100 = 100$. On the other hand, suppose your leader has chosen Investment Y instead, while your guess remains at 100. This means that $x = 0$ in the formula above, and your chance of receiving 10 ECU will be $[1 - (\frac{0-100}{100})^2] \times 100 = 0$.

Here is another example:

Example 4. Suppose you guess 70 as the chance that your leader has chosen Investment X for the group. At the end of the experiment, the computer reveals that your leader has chosen Investment X for the group. Hence, your chance of receiving 10 ECU will be $[1 - (\frac{100-70}{100})^2] \times 100 = 91$.

To determine whether you receive 10 ECU, the computer will randomly draw a number between 0 and 100 (including decimal points). If the number drawn by the computer is less than or equal to your chance of receiving 10 ECU as determined by the formula above, then you will receive 10 ECU. Otherwise, you will receive 0 ECU. Hence, in Example 4 above, if the number randomly drawn by the computer is less than or equal to 91, then you will receive 10 ECU. Otherwise, you will receive 0 ECU.

Payment for Experiment 2:

At the end of the experiment, if you are paid for Experiment 2, then the computer will randomly select one of the six rounds for payment. For the randomly chosen round:

1. If you are assigned the role of the leader, then you will be paid according to your investment decision in Stage 2 only.
2. If you are not assigned the role of the leader, then you will be paid according to your leader's investment decision in Stage 2, plus your decisions in Stage 3. The computer will randomly select one of the two questions in Stage 3, and you will be paid for your response to this question.

Summary

1. You will participate in six identical rounds in Experiment 2. At the beginning of each round, the computer will randomly match you to a new group with two other people. Each round consists of three stages.
2. In Stage 1, one group member will be assigned to be the leader of the group. There are four possible methods to determine who is assigned the role of the leader. You will be informed which method will be used to determine the leader at the beginning of each round.

In Method 1, the computer will randomly assign one group member to be the leader.

In Method 2, the group member who transferred the lowest amount to his/her matched partner in Experiment 1 will be assigned to be the leader.

In Method 3, the group member who transferred the highest amount to his/her matched partner in Experiment 1 will be assigned to be the leader.

In Method 4, you will be asked to indicate whether you prefer your leader to be someone who has transferred the highest or the lowest amount to his/her matched partner in Experiment 1. The computer will pick one of the decisions of the group members to implement. If your decision is implemented, then one of your other two group members will be appointed to be the leader based on your preference. Hence, you will not be appointed to be the leader if your decision is implemented.

You will be asked to indicate your preferred leader for Method 4 once, at the beginning of Experiment 2. The computer will use the same decision whenever Method 4 is being used to determine the leader.

3. In Stage 2, you will be asked to make an investment decision, assuming that you have been assigned the role of the leader. The leader will be given an endowment of 300 ECU, and s/he will be asked to choose between two investment options that will affect the payoffs of all group members. Your decision will be implemented for your group only if you have been assigned the role of the leader for that round.
4. Investment X and Investment Y may be different in each round. In each round, the amount that you receive from each investment may be different, but you will always receive a higher payoff if the investment succeeds, and a lower payoff if it fails. The investment options will be shown on your computer screens.

5. In Stage 3, you will be asked to predict which investment your leader has chosen, assuming that you have not been assigned the role of the leader. You will be asked two questions.

In Question 1, you will be asked to predict how likely it is in your opinion that the leader has chosen Investment X. You will need to choose a number between 0 and 100. A higher number means that you think the leader is more likely to have chosen Investment X.

In Question 2, you are given additional information. Specifically, you will be asked the same question under two different scenarios: (i) suppose you are told that the investment has succeeded; and (ii) suppose you are told that the investment has failed. You should consider whether your guess of the leader's decision will be higher than, lower than, or the same as the one you stated in Question 1, given that you know the outcome of the investment chosen by your leader.

6. The payoff structure used to determine your payment in Stage 3 is designed such that it is in your best interest to report your true beliefs about your leader's decision.
7. At the end of the experiment, the computer will randomly select one of the six rounds for payment. For the randomly chosen round, if you are assigned the role of the leader, then you will be paid according to your decision in Stage 2. If you are not assigned the role of the leader, then you will be paid according to your leader's decision in Stage 2, as well as your decisions in Stage 3. The computer will randomly select one of the two questions in Stage 3 for payment.

If you have any questions, please raise your hand and an experimenter will come to you to answer your questions privately. Otherwise, please wait patiently for the experimenter to launch the practice questions on your computer screens. The purpose of these practice questions is to make sure that you understand the experiment. If you have any questions at any time, please raise your hand and an experimenter will come over to answer your questions privately.

Once everyone has completed the practice questions, we will proceed with one practice round for Experiment 2. The purpose of the practice round is to allow you to familiarize yourself with the decision screens. Your decisions in the practice round will not affect your payments for today's experiment. We will proceed with Experiment 2 once everyone has completed the practice round.

Practice Questions (Experiment 2)

1. I will be paid for the decisions in both experiments today. True/False [Ans: False]
2. We will participate in six identical rounds in Experiment 2. If we are paid for Experiment 2, then we will be paid for our decisions in one of the six rounds. True/False. [Ans: True]
3. We will participate in each round of Experiment 2 in groups of three. One group member will be assigned the role of the leader. True/False [Ans: True]
4. In Experiment 1, Player 1 chose to transfer 160 ECU to his/her matched partner, Player 2 chose to transfer 115 ECU to his/her matched partner, and Player 3 chose to transfer 160 ECU to his/her matched partner.

Suppose the leader is appointed using Method 2. Which of the following is correct?
[Ans: (b)]

- (a) Player 1 will be assigned the role of the leader.
 - (b) Player 2 will be assigned the role of the leader.
 - (c) Player 3 will be assigned the role of the leader.
 - (d) Both Player 1 and Player 3 have an equal chance of being assigned the role of the leader.
5. In the above example, suppose the leader is appointed using Method 3. Which of the following is correct? [Ans: (d)]
 - (a) Player 1 will be assigned the role of the leader.
 - (b) Player 2 will be assigned the role of the leader.
 - (c) Player 3 will be assigned the role of the leader.
 - (d) Both Player 1 and Player 3 have an equal chance of being assigned the role of the leader.
 6. Suppose the leader is appointed using Method 4. Suppose also that your preference for leadership appointment is randomly chosen by the computer to be implemented. Which of the following is correct? [Ans: (b)]
 - (a) Depending on what I indicate as my preference of the appointed leader, I have a chance of being assigned the role of the leader.
 - (b) Regardless of what I indicate as my preference of the appointed leader, I will definitely not be assigned the role of the leader.

7. Suppose the leader is appointed using Method 4. In Experiment 1, Player 1 chose to transfer 200 ECU to his/her matched partner, and Player 2 chose to transfer 85 ECU to his/her matched partner. Player 3 indicates that his/her preferred leader is someone who has transferred the highest amount to his/her matched partner in Experiment 1.

Suppose Player 3's decision is randomly chosen by the computer to be implemented. Which of the following is correct? [Ans: (a)]

- (a) Player 1 will be assigned the role of the leader.
- (b) Player 2 will be assigned the role of the leader.
- (c) Both Player 1 and Player 2 have an equal chance of being assigned the role of the leader.

8. Which of the following is correct? [Ans: (b)]

- (a) The other group members will be informed of the investment chosen by the leader, but not the amount they have received from the investment.
- (b) The other group members will be informed of the amount they have received from the investment chosen by the leader, but not the investment chosen by him/her.
- (c) The other group members will be informed of the investment chosen by the leader, and the amount they have received from the investment.

9. Consider the investment options depicted in the figure below.

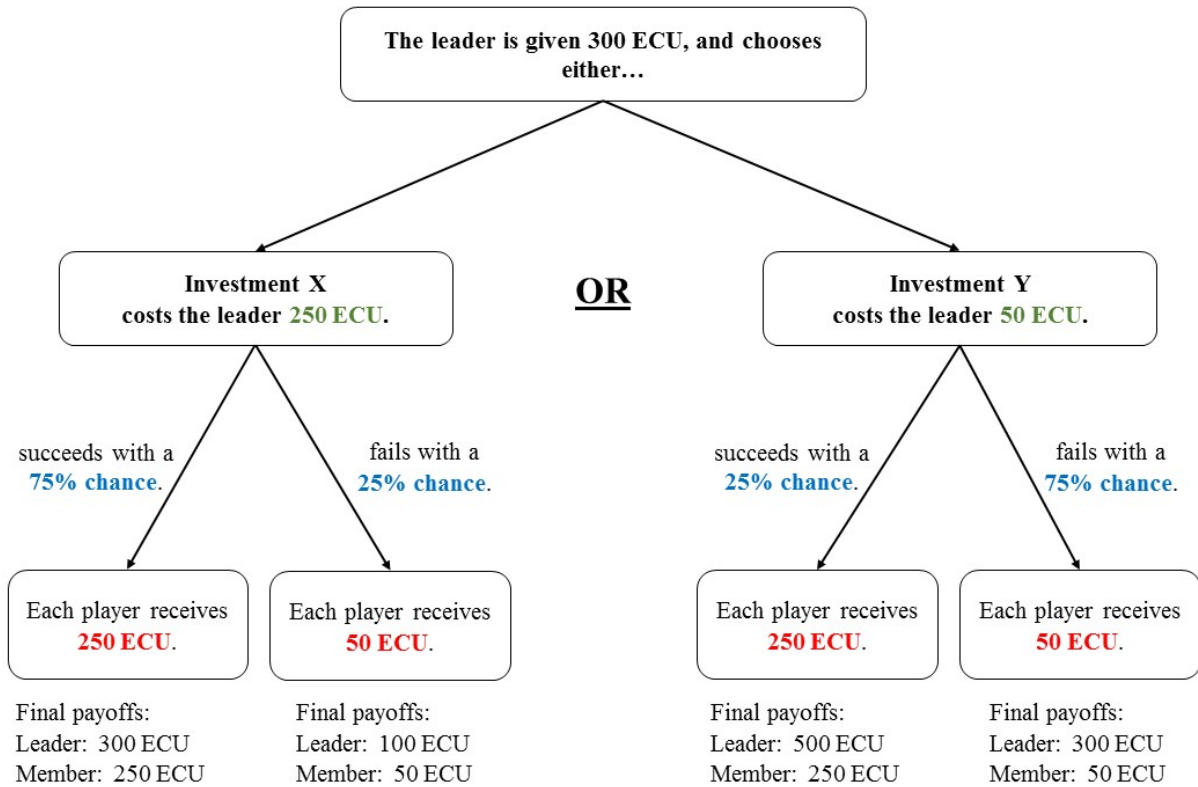


Figure A.2: Investment Options (Practice Question)

Suppose the leader chooses Investment X.

- (a) At the end of the experiment, the computer randomly determines that the investment succeeds.

If you are not the leader, how many ECU will you receive from Stage 2 of Experiment 2? [Ans: 250 ECU]

- (b) At the end of the experiment, the computer randomly determines that the investment fails.

If you are the leader, how many ECU will you receive from Stage 2 of Experiment 2? [Ans: 100 ECU]

10. Which of the following is true? [Ans: (c)]
- (a) I will be paid for my decision in Stage 3 of Experiment 2 regardless of whether I have been assigned the role of the leader or not.
 - (b) I will be paid for my decision in Stage 3 of Experiment 2 only if I have been assigned the role of the leader.
 - (c) I will be paid for my decision in Stage 3 of Experiment 2 only if I have not been assigned the role of the leader.
11. In Stage 3, I will be asked two questions. If I am paid for Stage 3 of Experiment 2, then I will be paid according to my answers to both questions. True/False [Ans: False]
12. Suppose you strongly believe that the leader of your group has chosen Investment Y. Which of the following statement is true? [Ans: (b)]
- (a) It is in my best interest to choose a higher number as my guess of “how likely is my leader to have chosen Investment X”.
 - (b) It is in my best interest to choose a lower number as my guess of “how likely is my leader to have chosen Investment X”.
 - (c) It is in my best interest to choose 50 as my guess of “how likely is my leader to have chosen Investment X”.

B Screenshots for belief elicitation task

Experiment 2: Round 1 Stage 3

In this round, the group member who transferred the lowest amount to their matched partner in Experiment 1 has been assigned to be the leader.

Suppose you have not been assigned the role of the leader for your group.

The investment chosen by your leader can lead to two possible outcomes:

- (i) the investment succeeds and each group member receives the high payoff; or
- (ii) the investment fails and each group member receives the low payoff.

For each question, please choose a whole number between 0 and 100 to indicate how likely you think your leader has chosen Investment X.

Question 1

Suppose there were 100 people in the position the leader is in now. How many of them do you think would choose Investment X?

(a) Interim Belief

Experiment 2: Round 1 Stage 3

We will now give you additional information about the outcome of the investment chosen by your leader. You are asked to evaluate the same question as Question 1 with this additional information.

Your response to Question 1 is given below.

Question 1

Suppose there were 100 people in the position the leader is in now. How many of them do you think would choose Investment X? 50

In Question 2, you are asked to consider whether your guess of the leader's decision will be different, given that you know the outcome of the investment chosen by your leader.

Question 2

Suppose you are informed that the investment chosen by your leader has succeeded, and you have therefore received the high payoff (200 ECU).

Now consider whether your guess will be higher than, lower than, or the same as the one you stated in Question 1.

That is, suppose there were 100 people in the position the leader is in now. Given an outcome of high payoff, how many of them do you think have chosen Investment X?

Suppose you are informed that the investment chosen by your leader has failed, and you have therefore received the low payoff (0 ECU).

Now consider whether your guess will be higher than, lower than, or the same as the one you stated in Question 1.

That is, suppose there were 100 people in the position the leader is in now. Given an outcome of low payoff, how many of them do you think have chosen Investment X?

(b) Posterior Beliefs

Figure B.1: Decision screens – Elicitation of beliefs

C Derivation of Hypothesis 1

There are $N > 2$ members in a group, and each member i has type $\beta_i \in [0, 1]$ drawn from a distribution $F(\beta)$ with density $f(\beta)$. $F(\beta)$ is common knowledge. A member of the group is appointed to be the DM under one of four possible appointment mechanisms, $\Psi \in \{RA, LA, HA, GA\}$.

We are interested in how members form interim beliefs about their DM's type under each appointment mechanism, given that they have been informed that someone else in the group is the DM. For a given appointment mechanism Ψ , we denote as μ_i^Ψ member i 's interim belief that the appointed DM is of type $\beta \geq \beta^*$.

Random appointment (RA). Each member has an equal chance of being appointed as the DM. This implies that

$$\mu_i^{RA} = \Pr(\beta \geq \beta^*) = 1 - F(\beta^*). \quad (\text{C.1})$$

Appointment of lowest type (LA). The member with the lowest β is appointed to be the DM. Consider member i of type β_i who is informed that someone else in the group has been appointed to be the DM under this mechanism. Hence, s/he knows that the DM has type $\beta \leq \beta_i$ as otherwise s/he would have been appointed to be the DM. Denote the minimum of the remaining $N - 1$ members' types as β^{\min} .

Given this, there are two possible cases. First, if $\beta_i < \beta^*$, then it must be that $\mu_i^{LA} = 0$ since the DM has type $\beta \leq \beta_i < \beta^*$. Second, if $\beta_i \geq \beta^*$, then the probability that the appointed DM is of type $\beta \geq \beta^*$ is given by

$$\begin{aligned} \Pr(\beta^{\min} \geq \beta^* | \beta^{\min} < \beta_i) &= 1 - \Pr(\beta^{\min} < \beta^* | \beta^{\min} < \beta_i) \\ &= 1 - \frac{\Pr(\beta^{\min} < \beta^*)}{\Pr(\beta^{\min} < \beta_i)} \quad (\text{since } \beta^* \leq \beta_i) \\ &= 1 - \frac{1 - [1 - F(\beta^*)]^{N-1}}{1 - [1 - F(\beta_i)]^{N-1}}. \end{aligned}$$

Hence, for member i ,

$$\mu_i^{LA} = \begin{cases} 0 & \text{if } \beta_i < \beta^*, \\ \frac{[1 - F(\beta^*)]^{N-1} - [1 - F(\beta_i)]^{N-1}}{1 - [1 - F(\beta_i)]^{N-1}} & \text{if } \beta_i \geq \beta^*. \end{cases} \quad (\text{C.2})$$

Clearly $\mu_i^{LA} \leq \mu_i^{RA}$ for $\beta_i < \beta^*$, which holds as an equality if $\beta^* = 1$. For $\beta_i \geq \beta^*$, $\mu_i^{LA} - \mu_i^{RA} = \frac{[1 - F(\beta^*)]^{N-1} - [1 - F(\beta_i)]^{N-1} - [1 - F(\beta^*)]\{1 - [1 - F(\beta_i)]^{N-1}\}}{1 - [1 - F(\beta_i)]^{N-1}}$. The denominator is ≥ 0 . The numerator can be simplified to give $[1 - F(\beta^*)]^{N-1} - [1 - F(\beta^*)] - F(\beta^*)[1 - F(\beta_i)]^{N-1}$, which is ≤ 0 since $[1 - F(\beta^*)]^{N-1} \leq [1 - F(\beta^*)]$. Hence, $\mu_i^{LA} \leq \mu_i^{RA}$ for $\beta_i \geq \beta^*$.

Appointment of highest type (HA). The individual with the highest β is appointed to be the DM. Consider member i of type β_i who is informed that someone else in the group has been appointed to be the DM under this mechanism. Hence, s/he knows that the DM has type $\beta \geq \beta_i$ as otherwise s/he would have been appointed as the DM. Denote the maximum of the remaining $N - 1$ members' types as β^{\max} .

Given this, there are two possible cases. First, if $\beta_i \geq \beta^*$, then it must be that $\mu_i^{HA} = 1$ since the DM is of type $\beta \geq \beta_i \geq \beta^*$. Second, if $\beta_i < \beta^*$, then the probability that the appointed DM is of type $\beta \geq \beta^*$ is given by

$$\begin{aligned} \Pr(\beta^{\max} \geq \beta^* | \beta^{\max} \geq \beta_i) &= \frac{\Pr(\beta^{\max} \geq \beta^*)}{\Pr(\beta^{\max} \geq \beta_i)} \quad (\text{since } \beta^* > \beta_i) \\ &= \frac{1 - F(\beta^*)^{N-1}}{1 - F(\beta_i)^{N-1}}. \end{aligned}$$

Hence, for member i ,

$$\mu_i^{HA} = \begin{cases} 1 & \text{if } \beta_i \geq \beta^*, \\ \frac{1 - F(\beta^*)^{N-1}}{1 - F(\beta_i)^{N-1}} & \text{if } \beta_i < \beta^*. \end{cases} \quad (\text{C.3})$$

Clearly $\mu_i^{HA} \geq \mu_i^{RA}$ for $\beta_i \geq \beta^*$, which holds as an equality if $\beta^* = 0$. For $\beta_i < \beta^*$, $\mu_i^{HA} - \mu_i^{RA} \geq 0$ since $1 - F(\beta^*)^{N-1} \geq 1 - F(\beta_i)^{N-1}$ and $1 - F(\beta_i)^{N-1} \leq 1$. Hence, $\mu_i^{HA} \geq \mu_i^{RA}$ for $\beta_i < \beta^*$ also.

Group appointment (GA). All members indicate how they would like their DM to be appointed. Specifically, they may choose to appoint as DM: (i) the lowest-type member; (ii) the highest-type member; or (iii) a randomly picked member. One of the group members' appointment decisions is randomly chosen to be implemented and the DM is appointed from the remaining group members based on this individual's preference.

It is trivial to see that all members will prefer to have the highest type appointed as the DM regardless of their own type. Intuitively, this is because it increases the chance that the appointed DM is of type $\beta \geq \beta^*$ and chooses a high effort level, leading to higher expected payoffs for the members.

Consider member i of type β_i who is informed that someone else in the group has been appointed to be the DM. There are two possible cases. First, if member i 's appointment decision is implemented, then the probability that the DM is of type $\beta \geq \beta^*$ depends on the probability that at least one of the other $N - 1$ group members is of type $\geq \beta^*$. This is given by $1 - F(\beta^*)^{N-1}$. Second, if member i 's appointment decision is not implemented, then s/he knows that the DM is of type $\beta \geq \beta_i$ as otherwise s/he would have been appointed to be the DM. Specifically, the DM's type is given by the maximum of the remaining $N - 2$ members' types (excluding member i and the member whose decision is implemented). The derivation of the probability that the DM is of type $\beta \geq \beta^*$ under

this scenario is similar to that of mechanism HA with $N - 2$ other members.

We next evaluate member i 's posterior belief that his/her appointment decision has been implemented, given the information that someone else in the group has been appointed to be the DM. Using Bayes' rule, this is given by $\frac{\frac{1}{N} \cdot (1)}{\frac{1}{N} \cdot (1) + \frac{N-1}{N} [1 - F(\beta_i)^{N-2}]} = \frac{1}{1 + (N-1)[1 - F(\beta_i)^{N-2}]}$. The numerator is the product of the prior probability that member i 's appointment decision is implemented ($\frac{1}{N}$) and the probability that s/he is not assigned to be the DM conditional on having his/her decision implemented. Conditional on member i 's decision being implemented, s/he does not become the DM with certainty. The denominator is the probability that member i is not appointed to be the DM. The first term is the same as the numerator. The second term is the product of the prior probability that member i 's appointment decision is not implemented ($\frac{N-1}{N}$) and the probability that s/he is not assigned to be the DM conditional on not having his/her appointment decision implemented. Conditional on member i 's appointment decision not being implemented, the probability that member i is not appointed to be the DM is $1 - F(\beta_i)^{N-2}$. This is the probability that at least someone else in the group (other than both member i and the member whose decision is implemented) has type $\beta \geq \beta_i$ and is therefore appointed to be the DM.

Putting all these together, we have for member i ,

$$\mu_i^{GA} = A \times [1 - F(\beta^*)^{N-1}] + (1 - A) \times \begin{cases} 1 & \text{if } \beta_i \geq \beta^*, \\ \frac{1 - F(\beta^*)^{N-2}}{1 - F(\beta_i)^{N-2}} & \text{if } \beta_i < \beta^*, \end{cases} \quad (\text{C.4})$$

where $A \equiv \frac{1}{1 + (N-1)[1 - F(\beta_i)^{N-2}]}$.

We would like to show that $\mu_i^{GA} \geq \mu_i^{RA}$. Note that μ_i^{GA} is a convex combination of two terms since $A \leq 1$. For both $\beta_i \geq \beta^*$ and $\beta_i < \beta^*$, these two terms are $\geq \mu_i^{RA}$ for $\beta^* > 0$. For $\beta^* = 0$, $\mu_i^{GA} = \mu_i^{RA}$.

Next, we would like to show that $\mu_i^{GA} \leq \mu_i^{HA}$. Again, since μ_i^{GA} is a convex combination of two terms, it is sufficient to show that these two terms are $\leq \mu_i^{HA}$. This is clearly the case for $\beta_i \geq \beta^*$. For $\beta_i < \beta^*$, we need to show that $\frac{1 - F(\beta^*)^{N-2}}{1 - F(\beta_i)^{N-2}} \leq \frac{1 - F(\beta^*)^{N-1}}{1 - F(\beta_i)^{N-1}}$. This is equivalent to showing that

$$\frac{[1 - F(\beta^*)^{N-1}][1 - F(\beta_i)^{N-2}] - [1 - F(\beta^*)^{N-2}][1 - F(\beta_i)^{N-1}]}{[1 - F(\beta_i)^{N-2}][1 - F(\beta_i)^{N-1}]} \geq 0.$$

The denominator is ≥ 0 . Let $x \equiv F(\beta^*)$ and $y \equiv F(\beta_i)$ with $x > y$ since $\beta^* > \beta_i$. Then, the numerator becomes $(1 - x^{N-1})(1 - y^{N-2}) - (1 - x^{N-2})(1 - y^{N-1})$. Simplifying gives us

$$x^{N-2} - x^{N-1} + y^{N-1} - y^{N-2} + x^{N-1}y^{N-2} - x^{N-2}y^{N-1} \quad (\text{C.5})$$

Hence, for the numerator to be ≥ 0 , we need to show the following:

Claim: $x^{N-2} - x^{N-1} + y^{N-1} - y^{N-2} + x^{N-1}y^{N-2} - x^{N-2}y^{N-1} \geq 0$ for $x, y \in [0, 1]$, $x > y$, and $N > 2$.

Proof. The proof is by induction. Let $x \equiv \alpha y$, $\alpha > 1$. Then, (C.5) becomes

$$(\alpha y)^{N-2} - (\alpha y)^{N-1} + y^{N-1} - y^{N-2} + \alpha^{N-1}y^{2N-3} - \alpha^{N-2}y^{2N-3} \quad (\text{C.6})$$

Consider first $N = 3$. (C.6) becomes

$$\alpha y - (\alpha y)^2 + y^2 - y + \alpha^2 y^3 - \alpha y^3 = y(\alpha - 1)(1 - y)(1 - \alpha y)$$

which is ≥ 0 since $y \in [0, 1]$, $\alpha y = x \in [0, 1]$, and $\alpha > 1$. Now suppose (C.6) ≥ 0 for some $N = k$. Rearranging (C.6), we have

$$\alpha^{k-2}y^{k-2}(1 - \alpha y) + \alpha^{k-2}y^{2k-3}(\alpha - 1) \geq y^{k-2} - y^{k-1}. \quad (\text{C.7})$$

Next consider $N = k + 1$. (C.6) becomes $\alpha^{k-1}y^{k-1}(1 - \alpha y) + y^k - y^{k-1} + \alpha^{k-1}y^{2k-1}(\alpha - 1)$, which is equal to

$$y [\alpha^{k-1}y^{k-2}(1 - \alpha y) + \alpha^{k-1}y^{2k-2}(\alpha - 1) + y^{k-1} - y^{k-2}]. \quad (\text{C.8})$$

We want to show that this expression is ≥ 0 given that (C.7) holds. Since $y \geq 0$, this is equivalent to showing the terms inside the brackets are ≥ 0 , or

$$\alpha^{k-1}y^{k-2}(1 - \alpha y) + \alpha^{k-1}y^{2k-2}(\alpha - 1) \geq y^{k-2} - y^{k-1}. \quad (\text{C.9})$$

Note that the RHS of (C.9) is the same as the RHS of (C.7) and is ≥ 0 . To conclude the proof, we show that the LHS of (C.9) is \geq the LHS of (C.7). Note that this is equivalent to showing

$$(\alpha - 1)(1 - \alpha y)\alpha^{k-2}(y^{k-2} - y^{(k-2)+(k-1)}) \geq 0,$$

which holds because $\alpha > 1$, $\alpha y \in [0, 1]$, and $y^{k-2} \geq y^{(k-2)+(k-1)}$. Hence, (C.8) is ≥ 0 if (C.7) holds. ■

D Additional analysis

D.1 Analysis with Game 1 treatments only

This section presents the analyses with the Game 1 treatments only. We show that Results 1, 2, and 3 hold with the exclusion of the Game 0 treatments.

Table D.1 presents marginal-effects estimates from a probit model for the relationship between the subjects' decisions as DMs in the investment task and their dictator game behavior. The estimates in the table reveal that a DM who transfers 1% more of their endowment to their matched partner in the dictator game is 1.3% more likely to choose e_H in the investment task on average, and this effect is statistically significant (p-value < 0.001). Hence, we conclude that the dictator game is a good proxy for an individual's type β_i even when we consider only the Game 1 treatments.

Table D.1: Regression of DM's effort choice (Game 1)

Variables	Dependent variable: =1 if DM chooses e_H (1)
% endowment transferred in DG	0.004*** (0.001)
% endowment invested in RG	-0.001 (0.001)
Treatment LA	-0.045 (0.028)
Treatment HA	0.048 (0.030)
Treatment GA	0.039 (0.028)
Order Effects	Y
Observations	1,088

Marginal effects of probit model reported. Robust standard errors in parentheses. Standard errors are clustered at the subject level. DG: Dictator Game; RG: Risk Game.
*** p<0.01, ** p<0.05, * p<0.10.

Table D.2 presents OLS estimates for the regressions of interim beliefs against the treatment variables. Similar to the main analysis in the paper, we control for order effects in columns (1) and (3) and individual fixed effects in columns (2) and (4). Treatment RA is the comparison group in all the specifications. Overall, the coefficient estimates reveal that Result 1 is robust to the exclusion of the Game 0 treatments. In particular, group

members respond to the appointment mechanism in their interim beliefs in the Game 1 treatments.

Table D.2: Regression of members' interim belief (Game 1)

Variables	Dependent variable: Interim belief			
	(1)	(2)	(3)	(4)
Treatment LA	-13.074*** (1.484)	-13.074*** (1.482)	-11.989*** (1.465)	-12.375*** (1.425)
Treatment HA	9.787*** (1.397)	9.787*** (1.396)	8.702*** (1.352)	9.088*** (1.332)
Treatment GA	2.717** (1.355)	2.717** (1.354)	1.813 (1.277)	2.135* (1.265)
Chooses high effort as DM			24.584*** (1.960)	15.832*** (1.984)
% endowment invested in RG	-0.086* (0.045)		-0.055 (0.039)	
Constant	55.525*** (3.990)	45.938*** (0.812)	45.400*** (3.636)	41.572*** (0.890)
Order Effects	Y	N	Y	N
Individual FE	N	Y	N	Y
Observations	1,088	1,088	1,088	1,088
R-squared	0.111	0.233	0.278	0.306
<u>Test of HA = GA</u>				
test statistic	5.202	5.209	5.341	5.461
p-value	0.000	0.000	0.000	0.000

Robust standard errors clustered at the subject level in parentheses. For all regressions, treatment RA is the reference treatment.

RG: Risk Game.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table D.3 presents the results from the OLS estimation of equation (6). Similar to the main analysis in the paper, we drop the inconsistent and non-updaters in the analysis. We find that Result 2 is also robust to the exclusion of the Game 0 treatments. Within the Game 1 treatments, members suffer from base-rate neglect relative to a Bayesian (test of $\delta = 1$: p -value < 0.001), attribute good outcomes more to luck than a Bayesian would (test of $\gamma_G = 1$: p -value < 0.001), and treat bad outcomes like a Bayesian (test of $\gamma_B = 1$: p -value = 0.492). Consequently, they tend to attribute good (bad) outcomes more to the DM's luck (decision), i.e., $\gamma_G < \gamma_B$ (p -value = 0.012).

Moreover, similar to Result 3, we find that members exhibit similar biases in their updating behavior across all the appointment mechanisms even with the exclusion of the Game 0 treatments. However, we find that the asymmetry in the attribution of outcomes is now marginally statistically insignificant in treatment LA (p-value = 0.103) while it is statistically significant in treatment HA (p-value = 0.028).

Table D.3: Regression of members' posterior beliefs (Game 1)

Variables	Dependent variable: Logit(posterior)				
	(1)	(2)	(3)	(4)	(5)
	Pooled	RA	LA	HA	GA
δ : Logit(interim belief)	0.733*** (0.049)	0.764*** (0.071)	0.793*** (0.057)	0.771** (0.093)	0.529*** (0.135)
γ_G : Good outcome \times logit(p)	0.742*** (0.060)	0.744*** (0.089)	0.728*** (0.078)	0.752*** (0.094)	0.798** (0.098)
γ_B : Bad outcome \times logit($1 - p$)	0.948 (0.076)	0.932 (0.092)	0.937 (0.119)	0.994 (0.090)	0.876 (0.114)
Observations	1,640	410	410	410	410
R-squared	0.636	0.686	0.741	0.613	0.421
<u>Test of $\gamma_G = \gamma_B$</u>					
test statistic	-2.522	-1.588	-1.637	-2.218	-0.512
p-value	0.012	0.114	0.103	0.028	0.609

Robust standard errors clustered at the subject level in parentheses.

This analysis includes only the Game 1 treatments but includes subjects classified as inconsistent or non-updaters.

*** p<0.01, ** p<0.05, * p<0.10. Null hypothesis is coefficient = 1.

D.2 Analysis of posterior beliefs with the entire sample (including inconsistent and non-updaters)

Table D.4 presents results from the OLS estimation of equation (6) with the inclusion of inconsistent and non-updaters. Overall, we find that the inclusion of these subjects leads to an attenuation of the estimates for γ_G and γ_B . Consequently, at the pooled level (column 1), the estimates now reveal that members tend to attribute bad outcomes more to luck than a Bayesian would (test of $\gamma_B = 1$: p-value = 0.026). This bias is also present at the treatment level, although it is statistically significant in treatments RA, HA, and GA (p-values = 0.056, 0.087, and 0.011, respectively), but not in treatment LA (p-value = 0.379).

Despite the attenuation in the estimates for γ_G and γ_B , we still find statistically significant evidence that members attribute good and bad outcomes asymmetrically. In particular, even with the inclusion of inconsistent and non-updaters, the estimates in Table D.4 suggest that members tend to attribute good outcomes more to luck and bad outcomes more to the DM's decision (i.e., $\gamma_G < \gamma_B$). This effect is statistically

Table D.4: Regression of members' posterior beliefs (entire sample)

Variables	Dependent variable: Logit(posterior)				
	(1)	(2)	(3)	(4)	(5)
	Pooled	RA	LA	HA	GA
δ : Logit(interim belief)	0.701*** (0.039)	0.737*** (0.068)	0.709*** (0.047)	0.716*** (0.060)	0.539*** (0.106)
γ_G : Good outcome \times logit(p)	0.530*** (0.064)	0.548*** (0.086)	0.358*** (0.094)	0.618*** (0.083)	0.662*** (0.093)
γ_B : Bad outcome \times logit($1 - p$)	0.848** (0.068)	0.830* (0.089)	0.903 (0.110)	0.867* (0.078)	0.742** (0.100)
Observations	3,264	544	1,088	1,088	544
R-squared	0.550	0.620	0.606	0.488	0.382
<u>Test of $\gamma_G = \gamma_B$</u>					
test statistic	-3.218	-2.132	-3.376	-2.060	-0.550
p-value	0.001	0.034	0.001	0.040	0.583

Robust standard errors clustered at the subject level in parentheses. This analysis includes all subjects.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Null hypothesis is coefficient = 1.

significantly at the pooled level (p-value = 0.001) and in treatments RA, LA, and HA (p-values = 0.034, 0.001, and 0.040, respectively).

D.3 IV regression of posterior beliefs

Table D.5 presents the results from the IV estimation of equation (6). We use the appointment mechanisms as instruments for the logit of members' interim beliefs.³⁴ The conclusions from the IV estimates are similar to those obtained from the OLS estimates in column (1) of Table 4. Specifically, we find that members suffer from base-rate neglect relative to a Bayesian (test of $\delta = 1$: p-value < 0.001). Moreover, members attribute good outcomes more to luck than a Bayesian would (test of $\gamma_G = 1$: p-value < 0.001), but they are no different from a Bayesian in their treatment of bad outcomes (test of $\gamma_B = 1$: p-value = 0.267). Consequently, we find that members tend to attribute good outcomes more to luck and bad outcomes more to the DM's decision, and this effect is statistically significant (test of $\gamma_G = \gamma_B$: p-value = 0.042).

D.4 Heterogeneity in updating behavior

As revealed in Figure 6, members appear to be heterogeneous in their updating behavior. To explore this further, Table D.6 presents the results from both a 2-component (column 1) and 3-component (column 2) finite mixture model analysis of members' updating

³⁴Results from our first-stage regression suggest that the appointment mechanisms are relevant instruments (F-statistic = 35.23).

Table D.5: IV regression of members' posterior beliefs

	Dependent variable: Logit(posterior)
	(1)
Variables	Pooled
δ : Logit(interim belief)	0.792*** (0.046)
γ_G : Good outcome \times logit(p)	0.787*** (0.056)
γ_B : Bad outcome \times logit($1 - p$)	0.929 (0.064)
Observations	2,460
<u>Test of $\gamma_G = \gamma_B$</u>	
test statistic	-2.030
p-value	0.042

Robust standard errors clustered at the subject level in parentheses. This analysis excludes subjects classified as inconsistent or non-updaters.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Null hypothesis is coefficient = 1.

behavior at the pooled level.³⁵

In both models considered in Table D.6, component 1 constitute the majority of updates in the sample (88.9% of the updates in the 2-component and 65.9% of the updates in the 3-component model). This component is characterized by a low level of base-rate neglect and under-responsiveness to both good and bad outcomes. Within this group of belief updates, in relative terms, members attribute good outcomes more to luck and bad outcomes more to the DM's decision, although this difference is statistically significant in the 2-component model (p-value = 0.001) but not in the 3-component model (p-value = 0.190).

The estimates in the table reveal that belief updates in the remaining sample suffer from a higher level of base-rate neglect. Moreover, the under-responsiveness to outcomes is no longer present within this group of updates. Instead, group members are over-responsive to outcomes in component 2 (11.1% of the updates in the 2-component and 4.8% of the updates in the 3-component model). In addition, a third sub-group is identified in the 3-component model (constituting 29.4% of the sample) where members

³⁵We also consider a 4-component model which does not change our main conclusions and does not provide further insight.

respond to outcomes like a Bayesian.

Overall, our finite mixture model analysis suggests that there is heterogeneity in members' updating behavior. Although members consistently suffer from base-rate neglect, for most updates this is at a modest level. Moreover, the majority of belief updates in the sample is characterized by under-responsiveness to the DM's outcomes and an asymmetric attribution of the DM's outcomes to his/her decision and luck.

Table D.6: Finite mixture model for updating behavior

	Dependent variable: Logit(posterior)	
	2-Component Model	3-Component Model
	(1)	(2)
Component 1		
δ : Logit(interim belief)	0.936*** (0.011)	0.972*** (0.005)
γ_G : Good outcome \times logit(p)	0.535*** (0.031)	0.431*** (0.031)
γ_B : Bad outcome \times logit($1 - p$)	0.668*** (0.044)	0.477*** (0.040)
<u>Test of $\gamma_G = \gamma_B$</u>		
t-statistic	-3.47	-1.31
p-value	0.001	0.190
Component 2		
δ : Logit(interim belief)	0.148*** (0.086)	-0.109*** (0.137)
γ_G : Good outcome \times logit(p)	1.936** (0.405)	3.566*** (0.807)
γ_B : Bad outcome \times logit($1 - p$)	1.945** (0.407)	2.942*** (0.642)
<u>Test of $\gamma_G = \gamma_B$</u>		
t-statistic	-0.02	0.70
p-value	0.984	0.485
Component 3		
δ : Logit(interim belief)		0.302*** (0.031)
γ_G : Good outcome \times logit(p)		1.029 (0.054)
γ_B : Bad outcome \times logit($1 - p$)		1.103 (0.067)
<u>Test of $\gamma_G = \gamma_B$</u>		
t-statistic		-0.89
p-value		0.372
Latent Class Marginal Probabilities		
μ_1	0.889 (0.020)	0.659 (0.028)
μ_2	0.111 (0.020)	0.048 (0.009)
μ_3		0.294 (0.027)
Model Fit		
Log likelihood	-3317.86	-3028.11
AIC	6653.720	6084.223
BIC	6705.991	6165.534

Robust standard errors clustered at the subject level in parentheses. This analysis excludes subjects classified as inconsistent or non-updaters.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Null hypothesis is coefficient = 1.