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# ABSTRACT

## Through the Looking Glass: Transparency about Others' Luck and Effort Enhances Redistribution*

How do we persuade people to part with money they feel they have rightly earned? We conducted a dyadic experiment (N=1,986) where luck determined which of the players' performance counted toward winning the game. Despite luck playing a large part, we found strong evidence of justified deservingness among the winners. The better they performed in the task, the less they redistributed to their nonwinning partner. However, in treatments where performance was transparent, winners significantly increased redistribution to nonwinners who performed similarly well. We find that transparency can effectively alter redistributive preferences even when people feel fully deserving of their income.

**JEL Classification:** C9, D9

**Keywords:** luck, efforts, survivalship bias, redistribution, inequality, deservingness

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It is empirically well established that support for redistribution declines as the perceived deservingness of incomes increases (see, e.g., Benabou and Tirole, 2006; Aarøe and Petersen, 2014). However, the key driving mechanisms behind why many people struggle to part with money they feel they have fully earned remain imperfectly understood. One explanation comes from theories on deservingness heuristics and preferences for meritocratic fairness, which predict that people rely on intuitive rules of thumb to determine worthiness of support (e.g., Gilens, 1999; Larsen, 2008). This research shows that the source of income matters to the formation of perceived deservingness. One of the main findings from laboratory experiments is that people tend to feel more deserving—and, in turn, redistribute less—of their earned incomes than incomes that had been generated by pure luck, such as lottery wins (Alesina and Angeletos, 2005; Balafoutas et al., 2013; Lefgren et al., 2016; Gee et al., 2017). However, empirical evidence and casual observations suggest that not all inherently random sources of income are perceived as random in the real world. Inheritance is one example. A recent study by the Tax Foundation shows a continuous decline in public support for inheritance taxes in many countries around the world (Cole, 2015), including the most meritocratic countries such as Denmark and Finland (Weinberg et al., 2021). Given that inheritance is randomly assigned at birth, this declining support for inheritance taxes appears to be at odds with theories on deservingness heuristics and meritocratic fairness.

What explains why luck is often underappreciated as a major cause of economic success and inequality? The nature of luck—for example, winning the "birth lottery" (Chetty et al., 2014) or starting a business in the right place at the right time—is not always transparently salient to most people. By making the random nature of these incomes more salient, recent experiments have shown that by reducing feelings of entitlement, we can, to an extent, nudge people to redistribute more of their windfall to the less fortunate (Akbaş et al., 2019; Lekfuangfu et al., 2022).

Survivorship bias can also explain why many people underappreciate the role of luck in determining their current and future incomes (see, e.g., Brown et al., 1992). Although initial luck—whether in resources or opportunity—almost always determines people's selection into a path where efforts have a higher probability of guaranteeing survivorship and future economic successes (e.g., Chetty et al., 2014), survivorship bias suggests that the survivors tend to learn only selected information about their own successes and not failures. Consequently, when explaining their economic successes, they are likely to underestimate the impact of nonsalient but necessary factors of the selection process, such as luck (Denrell and Liu, 2012; Lifchits et al., 2021), while simultaneously overweighting the more salient features of their characteristics, such as efforts, when explaining their economic successes. Unlike lottery wins or inheritance, the existing solution of reminding people about the random nature of the origins of their incomes is unlikely to be effective for sources of incomes perceived to have been 'clearly earned' through hard work.

In the face of such survivorship bias, how do we encourage people to become more empathetic of others' unluckiness in the selection process? Can we effectively reduce people's feelings of deservingness of their incomes and, consequently, improve redistribution of earned incomes to the less fortunate? These are difficult but important questions that are relatively unexplored scientifically in the literature. To reduce people's feelings of deservingness over their earned incomes, we argue that the solution must tackle the root cause of survivorship bias, namely the logical error that luck in the selection process plays a significantly smaller role than subsequent efforts in determining economic successes. In the present paper, we propose a novel and unique solution to this problem: **reminding people of others' deservingness**.

More specifically, our study is one of the first to investigate whether making nonwinners' success rates at the end of a series of tasks transparent to winners can increase the winners' willingness to redistribute their earned incomes. In other

words, can we effectively reduce people's feelings of deservingness of their earned incomes if they know that people who randomly drop out are equally, if not more, competent as they are at completing the task?

Using a large, random sample of Americans ($N$=1,986), following a pre-registered experimental design and analysis plan, we found strong evidence that winners who are randomly assigned to receive information about their partners' performance are more willing to share a substantial fraction of their earned incomes when their partner also performs equally well, if not better, than they do in either a quiz or a guessing-game task. The effect size of others' performance on an individual's willingness to share earned incomes is large: A 1 percentage point increase in the partner's performance increases the winner's willingness to redistribute their earned incomes by approximately 3 percentage points. As expected, we found little evidence of a positive relationship between winners' willingness to share their earned incomes and their partner's performance in treatments where the partner's performance is not transparent to winners. Consistent with a reduction in survivorship bias, we found that our results are driven by an increase in winners' feelings of others' deservingness through the recognition that their high-performing opponent could have just as easily been selected to win. Finally, we found that a more equitable redistribution decision in the quiz/guessing-game task, in part due to the transparency of partners' performance in the transparency treatments, results in nonwinners showing a greater willingness to cooperate in a subsequent public-goods game.

The rest of this paper is organized as follows. Section I discusses the background literature. Section II describes the experimental design and procedures. Section III presents and discusses the empirical strategy and results, and Section IV concludes.

## I. Background literature

Despite recent evidence that income redistribution makes donors significantly happier (Dwyer and Dunn, 2022), empirical evidence and casual observations suggest that people often find it difficult to give away money they themselves feel fully deserving of (e.g., Freeman, 1986; Glazer, 1998). One of the main findings in the deservingness literature is that people tend to judge whether to help others in need based on their perceived deservingness. For instance, Gilens (1999) argues that opposition to the current welfare system by many Americans is rooted in their (inaccurate) beliefs that it mainly benefits Black and lazy people, who are, in their eyes, undeserving.

What determines why we feel someone is more deserving than others? A key criteria of deservingness in welfare theory is the degree of control individuals have over their situation (Oorschot, 2000). Individuals who are not in control of their neediness—those who experience bad luck—are more deserving of income redistribution because they are not to blame for their situation. The degree of control the poor have over their situations can be shaped by geographical differences in institutional factors such as job growth and opportunities, wage-setting mechanisms, and mobility (Larsen, 2008). In social democratic regimes, these factors generally restrict how much control individuals have over their outcomes. In contrast, liberal regimes with *laissez-faire* policies tend to give more control to individuals. This might be related to why Europeans more commonly recognize the importance of luck in life's outcomes (Alesina and Angeletos, 2005) than Americans, who tend to believe that rewards arise from hard work (McCall, 2013).

In relation to this, another key criteria of deservingness is whether an individual's outcome is due to their effort or luck. Numerous experimental studies have found significantly less support for *ex-post* redistribution when inequality is due to effort versus luck (Balafoutas et al., 2013; Durante et al., 2014) even when inequality is relatively high (Gee et al., 2017). Furthermore, both high and low

earners exhibit similar decreases in their demand for redistribution when effort is the reason for inequality (Lefgren et al., 2016). In other words, if the root of inequality is believed to result from heterogeneous efforts and not heterogeneous luck, the poor deserve to remain poor, while the rich deserve to remain rich. Such redistributive preferences appear to be driven not only by self-interest but also by a sense of deservingness for hard workers, regardless of one's position; see, e.g., Fong (2001). Nevertheless, when deciding on redistribution *ex-ante*, strong desires to restore equity can trump meritocratic deservingness. Ku and Salmon (2013) found that when people's status is based on merit rather than randomly assigned, the disadvantaged are less willing than the advantaged to accept a Pareto-improving outcome that disproportionately benefits the advantaged.

Despite evidence of a positive relationship between effort and perceived deservingness, a few empirical studies have shown that people generally overcome their initial feelings of undeservingness of windfall incomes over time. Using a large-scale longitudinal dataset of lottery winners, Gardner and Oswald (2007) found that they take, on average, two years to benefit from their lottery win psychologically. One reason could be that the salience of luck decreases over time, thus increasing one's own perceived deservingness. Lekfuangfu et al. (2022) tested how the salience of luck can affect the perceived deservingness of windfall incomes and found that redistribution of luck-based inheritance is higher than effort-based. Like effort-based earnings, which are redistributed more fairly when procedural justice is made more salient (Akbaş et al., 2019; Hansson et al., 2021), luck-based earnings are also redistributed more fairly when the luckiness of the income is made more salient.

Nevertheless, most successes in life are a combination of luck and effort. Disentangling the role of luck by making it more salient to individuals can be complex, because efforts tend to be more easily recognizable than the impact of chance. One of the main reasons why the role of luck is often underappreciated in

people's successes is survivorship bias, which is when we select only the 'survivors' (i.e., the successful) and base their successes on their attributes, without considering people with similar characteristics who either failed to perform or were unlucky not to be given the same opportunity (Lifchits et al., 2021). For example, Elton et al. (1996) tracked all funds that existed at the end of 1976 to estimate survivorship bias over different horizons. They found that previous studies had drawn erroneous conclusions about the characteristics of a high-performing fund. Those study samples were incomplete because the set of surviving funds are typically truncated by survivorship bias (Brown et al., 1992). When examining an unbiased sample of funds, Grinblatt and Titman (1989) and Connor and Korajczyk (1991) found that smaller funds, maximum capital gain, and growth funds perform worse than expected, contrary to previous findings.

Similarly, Denrell and Liu (2012) found that ability is a poor predictor of extreme performance when considering the effects of noise and self-reinforcing dynamics (i.e., the rich get richer). In addition, the implications of survivorship bias on people's perceptions of what constitutes success have also been demonstrated experimentally in Lifchits et al. (2021). They presented participants with true but selective information regarding whether the founders of successful companies were college dropouts or graduates and then asked them to bet on the success of a new firm. Participants who learned about college dropout founders were 55 percentage points more likely to bet on a new firm started by a college dropout than participants who learned about a graduate founder, despite acknowledging bias in the sample. Participants who learned about dropout founders drew biased conclusions and made riskier decisions due to their overestimation of salient information and lack of recognition that there are other, less salient factors that drive success.

Why are feelings of deservingness that arise from survivorship bias so prevalent among successful people? Babcock and Loewenstein (1997) found that a self-serving attribution bias in bargaining impasses can lead each party to interpret

the fairness of the negotiation situation differently, focusing on the aspects that are most advantageous to themselves. Likewise, survivorship bias allows survivors to reflect on their success story and focus on the elements that best serve their self-concept. Being able to reflect on one's hard work is more psychologically flattering than recognizing the stroke of luck that was most likely a critical part of one's success.

The seminal work of Tversky and Kahneman (1973) on availability heuristics also offers some potential psychological explanations for survivorship bias. In a series of experiments, they demonstrated that people can better recall information that is more readily available or easier to come to mind. Survivorship bias is likely to operate similarly. To explain their success, an individual will turn to the factors most readily available in their mind, for example the many hours of hard work invested in a project, and overlook the factors that require deeper consideration, for example being at the right place at the right time or being born into a family of privilege.

The scientific literature on tackling survivorship bias remains scarce, especially in contexts where success is due to a combination of luck and effort. Given that this bias causes people to overattribute efforts in the narrative of their path to financial success, it is unlikely that increasing the salience of luck will effectively lower their feelings of income entitlement, particularly when incomes are viewed as clearly earned. Instead, we propose a novel solution that addresses the erroneous belief that efforts play a larger role than luck in determining success. Instead of prompting participants to think about the factors that have led to their own success, we appeal to what causes survivorship bias in the first place, namely, the omission of nonsurvivors' performance. We do so by randomly showing winners information about how well the nonwinners performed in the same task even when they were not in the running to gain anything from their efforts (or luck). Like Elton et al. (1996), who reduced survivorship bias in mutual-fund studies by

examining all funds, even disappearing ones, over a given period, we tested the impact of highlighting the ability/efforts of individuals who could have been successful had they been luckier in the selection process.

We were also interested in the spillover effects that result from equitable or inequitable outcomes in the first partnered task. In other words, we tested whether transparency that is aimed at increasing others' deservingness can encourage reciprocity between participants in an otherwise unrelated subsequent social dilemma game. Previous studies in economics have found evidence of behavioral spillovers between strategic games. For example, Dreber et al. (2014) discovered that in a setting where participants play a repeated prisoner's dilemma game first, followed by the dictator game, cooperators tend to give less in the dictator game than defectors. Similarly, Cason et al. (2012) found that successful coordination in the median game has a positive spillover effect on coordination in the minimum game when the games are played sequentially. Following previous work in this area, we tested whether fair treatments in the redistribution task positively spill over into the later cooperation task.

## II. Experimental design and procedures

### A. Design

We conducted a pre-registered (https://osf.io/dg3mz/) online Prolific (www.prolific.co) experiment, programmed using the software o-Tree (Chen, Schonger and Wickens, 2016). In a dyadic game, the *hotseat game*, pairs worked through six rounds of either luck- or ability-based tasks, with either transparency or no transparency of the other player's outcomes. Subsequently, they had to make redistributive decisions on any earnings and played a public-goods game. Figure 1A in the appendix illustrates the flow of the experiment. In the following subsections, we describe the key elements of our experiment. Screenshots can be seen in Figure 2A in the appendix.

*The hotseat game*

In each round, both participants in the pair had to complete a task. However, whether they could proceed to the next round depended on whether one of them, the 'hotseat' player, completed the task successfully. If the hotseat player was unsuccessful, the round repeated, with a new task to be completed. At the end of each task, a summary was provided to each player in the pair.

Hotseat players were assigned as follows. Before the start of the first round, one of the participants in each pair was randomized into the position of the hotseat. If a hotseat player was successful in the round, they would be the hotseat player in the next round. If a hotseat player was instead unsuccessful in the round, the hotseat was re-randomized and the group could not proceed to the next round. This design captures how opportunities to perform are determined by luck; it also captures how individuals who are successful obtain further opportunities.

The game ended when a total of six[1] rounds of tasks were completed, with the hotseat player at the end declared the winner and awarded 100 points, the equivalent of $0.89.

*Redistribution*

After the winner was declared, they were given the opportunity to redistribute their points to the other player (the nonwinner). Meanwhile, the nonwinner was asked whether they would have redistributed any of the points had they won the game.

[1] Note that we deviated slightly from the experimental protocol outlined in the pre-registration. Although we had initially planned for groups to work through ten rounds of the task, we ultimately chose to limit the number of required successful rounds to six, as our initial piloting data showed sufficient variation in hotseat tenure and a significant reduction in the time participants spent on the study.

Nonwinners were subsequently informed of the redistributive decision of the winning player, but their own hypothetical redistributive decision was not shared. We also elicited participants' sense of deservingness via responses to eight questions (listed alongside their rotated factor loadings in Table 1A in the appendix).

*Treatments*

Our main treatments were based on a $2 \times 2$ factorial design where on one dimension, we varied whether the task was luck-based or ability-based. On the other dimension, we varied whether the other player's performance was transparent or nontransparent. The four main treatments were: luck, no transparency (LNT); luck, transparency (LT); ability, no transparency (ANT); and ability, transparency (AT). To minimize any differences in (perceptions of) effort, especially in the ability task, we conducted a fifth treatment, ability, transparency, redistribution (ATR). This was an extension of the AT treatment in which players were warned from the start that the winner would have the opportunity to redistribute their points to the nonwinner; see Figure 3A in the appendix for an illustration of the treatment groups. Players were randomly matched into pairs and randomized into one of these five treatments.

*Task treatments*

In the luck-based task, they were asked to predict the outcome of a fair coin toss. They were considered to be successful in the task if they guessed the correct outcome. In the ability-based task, they had to answer a trivia question. They were considered to be successful if they answered the question correctly. Players had 40 seconds to predict the coin toss outcome or respond to the trivia quiz question. Failure to respond resulted in a nonresponse, and participants with three consecutive nonresponses were automatically excluded.

*Transparency treatments*

Pairs assigned to the transparency treatments received additional information in the summary at the end of the task, as well as during the redistribution task. In particular, both partners in the hotseat and nonhotseat were able to observe the other person's success at each task (as well as their own). When redistributing, players were also shown the percentage of successful outcomes of the other player (as well as their own). Pairs assigned to the nontransparency treatments received no such information.

*Public-goods game*

Following the hotseat game and redistribution, participants then moved on to a public-goods game to measure their consequent levels of cooperation. Players were paired with the same partner as in the hotseat game and endowed with 50 points. Combined contributions were multiplied by a factor of 1.5, and the resulting product was redistributed between the two.

It is important to note that participants did not know that they would later proceed to play this public-goods game with the same player when making their redistributive choice immediately after the hotseat game. Winning participants therefore made their redistributive choices with no incentive to influence their partner's later behavior.

*Questionnaire*

At the end, participants completed a questionnaire, which included questions on the inclusion of the other in the self (ios), which measures their sense of closeness with the other player (Aron et al., 1992), altruism, sex, age, pre-tax household income, education level, ethnicity, employment status, state of residency, and marital status. Participants in the nontransparency conditions were asked to guess the success rate

of their opponents in the first task and were then shown the correct figure. They were then asked whether they would have redistributed the 100 points differently. In addition, participants in the ability condition were asked whether (and if so, to what extent) they cheated and whether they thought their partner cheated in the ability task (multiple rounds of general knowledge questions) by googling the answers within the time limit[2].

*Outcomes of interest*

The primary outcome variable is the winner's redistributive decision at the end of the first task. We also examined the extent to which participants felt deserving of their outcome. Finally, we looked at how participants' redistributive outcomes influenced their cooperation in the subsequent public-goods game.

Note that participants experienced two different forms of luck: The first was experienced by participants who predicted coin flips in the luck conditions (LNT and LT), and the second was experienced by all participants via the hotseat randomization. By incorporating these two features in the experimental design, we created conditions in which some won entirely due to luck and others due to a combination of ability and luck. This allowed us to differentiate our experiment from others that have explored purely luck or purely ability-based earnings, by simulating the conditions of real life in which earnings tend to be the result of a combination of effort/ability and luck.

## B. Data

[2] Cheating was theoretically possible only in the ability tasks (ANT, AT, ATR). However, we disabled copying and pasting from the screens in our program, therefore making it much more difficult for participants to cheat.

We invited 2,786 participants to join the study on Prolific from August through October 2022[3]. Participants gave informed consent and were paid a show-up fee of $1.75 and a bonus payment commensurate to the points earned at an exchange rate of 100 points = $0.89. We were unable to match 36 participants with another player at the time of joining the study. A further 750 participants dropped out due to one of the players in the pairs dropping out after having matched, either by timing out, failing attention checks, or choosing to quit. Participants who were abandoned before the end of the experiment still received the show-up fee. We therefore collected completed data of 2,000 participants in total. A further 14 participants (7 pairs) were excluded from the analysis due to a server issue that resulted in the winning player's wrong assignment and, therefore, an invalid redistribution decision. This resulted in the main analysis sample of 1,986 participants, with 363 in LNT, 347 in LT, 468 in ANT, 384 in AT, and 424 in ATR; see Figure 3A. LNT and LT contain an odd number of participants because one participant in each condition completed the study whose partner had dropped out only after the dyadic portion of the games had been completed. We kept only those players who had complete data. Descriptive summary statistics of the sample are in Table 2A in the appendix. This table also confirms successful randomization, as the sample was appropriately balanced on observable characteristics across all treatment conditions.[4]

## C. Empirical strategy

[3] See Peer et al. (2022) for a discussion on the overall quality of the Prolific sample, which is significantly superior compared to those obtained using the Amazon MTurk and CloudResearch.

[4] Data and the .do file to run the analysis can be downloaded from

To formally investigate the effect of transparency around the other player's performance on the winner's redistribution, we estimate the following general specification using the ordinary least squares estimator:

$$(1) \quad Redistribution_i = \alpha + \beta_1 Treat_i + \beta_2 Own\_perf_i + \beta_3(Treat_i \times Own\_perf_i) + \beta_4 Other\_perf_i + \beta_5(Treat_i \times Other\_perf_i) + X_i'\gamma + \varepsilon_i,$$

where $i$ = 1, …, $N$ of winners from each matched pair; $Redistribution_i$ is the amount redistributed by the winning player $i$, with values ranging from 0 to 100; $Treat_i$ is a vector of dummy variables representing different treatment assignments LT, ANT, AT, and ATR, where LNT is the reference group; $Own\_perf_i$ represents the proportion of correct answers (ability-based task) or correct predictions (luck-based task) managed by the winner $i$ (measured on a continuous, 0–1 scale); and $Other\_perf_i$ indicates the proportion of correct answers (ability-based task) or correct predictions (luck-based task) managed by the nonwinner in the pair. In this fully interacted model, $\beta_1$ denotes the main effect of the treatment assignment; $\beta_2$ is the main effect of the winner's own performance in the task; and $\beta_4$ is the main effect of the nonwinner's performance on the winner's redistributive preferences. The interaction terms $\beta_3$ and $\beta_5$ correspond to the moderating effect of one's own and other's performance on the winner's redistribution by treatment assignment. The hypotheses are that $\beta_2 < 0$ and $\beta_3 < 0$ in all treatments, that is, an increase in one's own performance reduces the winner's redistribution; $\beta_4 = 0$, that is, other's performance in the LNT treatment does not have a statistically significant relationship with the winner's redistribution; and $\beta_5 > 0$ for LT, AT, and ATR treatments, that is, an increase in other's performance increases the winner's redistribution.

The demographic covariates in vector $X_i'$ include the following information about the winner: proportion of rounds in the hotseat, number of sequences the team needed to complete the six-round task, age, age squared, female (vs. male) dummy, above (vs. below) $60,000 income dummy, college (vs. not college) educated, marital status dummies (cohabitating, divorced, married, separated, widowed vs. single never married), white (vs. nonwhite) dummy, altruism index, whether they have stated to have cheated (ability treatments only), and whether they suspect their opponent of having cheated (ability treatments only), and $\epsilon$ is the error term. We report heteroskedasticity-consistent standard errors (HC3) in parentheses in all regression tables (Long and Ervin, 2000; Young, 2019).

As described in section III, we modified Eq.1 to include measures of one's own and others' deservingness to test for the mechanisms that might be driving the results. To test for the spillover effect, we also estimated a specification in which cooperation in the public-goods game is a linear function of the winner's redistribution in the earlier task.

## III. Results

How does the source of earnings affect redistribution in our experiment? To make a first pass at this question, Figure 1 shows the average winner's redistribution after the first task, by treatments. While previous studies have suggested that the willingness to redistribute should be higher in the luck-based task (Balafoutas et al., 2013; Durante, Putterman and van der Weele, 2014; Gee, Migueis and Parsa, 2017), we found no evidence of such effects in a two-sample Kolmogorov–Smirnov test for equality of distribution function: LNT vs. ANT, $p$=0.999; LT vs. AT, $p$=0.996. Pooling the effort tasks and the luck tasks and comparing across transparency conditions, we found little evidence that these differences are statistically significantly different from each other (Wilcoxon rank-sum test: LNT vs. LT, $p$=0.087; ANT vs. AT, $p$=0.237), which suggests that transparency might

not have an overall effect at raising the average redistribution for the treated. However, we must treat the raw data patterns with caution because we still need to account for heterogeneity in how people performed in the task.

Figure 2 illustrates how performance—either by correctly predicting the coin tosses or correctly answering the general knowledge questions—in the task correlates with redistribution. Across all five treatments, including the luck treatments, the better the winners performed, the less they redistributed. Surprisingly, our results also suggest that even winners in the luck conditions who were luckier at predicting the outcome of coin flips felt more deserving of their winnings and therefore redistributed less of their income than those who were not as lucky at predicting coin flips. This implies that participants may have attributed their successes at predicting random events to their ability or personal luck, which they somehow felt they entirely deserved. This is in line with Lekfuangfu et al.'s (2022) finding that people feel they deserve their inheritance.

In contrast, we found that the relationship between the winner's redistribution and their partner's performance is not homogeneous across all treatment groups. The locally weighted scatterplot smoothing plots in Figure 3 show a positive slope between partner's performance and redistribution in the transparency conditions (LT, AT, and ATR), as opposed to a negative slope in the nontransparency conditions (LNT, ANT). Hence, these raw data numbers provide first evidence of how reducing survivorship bias through making information on the nonwinners' performance salient to the winners can improve redistribution.

To formally test our hypotheses, Eq.1's estimates are reported in Table 1. Estimates obtained from the most parsimonious specification, in which the treatment assignment dummies, the winner's performance, and the nonwinner's performance are the only independent variables, are displayed in the first column of Table 1. Here, we can see that only the coefficient on the ATR treatment is positive and statistically significantly different from zero; on average, winners in

the ATR treatment redistributed 6.4 percentage points of their winnings to the nonwinners more than those in the LNT treatment. Consistent with the hypothesis on deservingness, there is strong evidence that the better the winners performed, the less they redistributed. The effect is economically significant as well as statistically meaningful: A 1 percentage point increase in the winner's performance is correlated with a 2.4 percentage points decrease in the redistribution rate. Also consistent with the hypothesis on others' deservingness and survivorship bias, we found evidence of a positive and statistically significant relationship between the nonwinner's performance and the winner's redistribution. The absolute size of the estimated coefficient on the nonwinner's performance on the winner's redistribution is about half of the coefficient on the winner's performance: A 1 percentage point increase in the nonwinner's performance is associated with a 1.2 percentage points increase in the winner's redistribution, on average.

Given that information on the nonwinner's performance is available only to winners in the transparency treatments, we attempted to separate the average nonwinners' performance effect by including in Column 2 of Table 1 the interaction terms as specified in Eq.1. With the inclusion of the interaction terms, we continued to see the main effect of the winner's own performance, $\beta_2$, to be negative and statistically significantly different from zero: A 1 percentage point increase in the winner's own performance is associated with 2.7 percentage points decrease in redistribution. The statistically insignificant interaction coefficients between the winner's performance and other treatment dummies imply that the winner's feelings of deservingness increase with their own performance irrespective of the treatment conditions, that is, luck-based versus ability-based, and transparency versus nontransparency.

By contrast, the main effect of the nonwinner's performance on the winner's redistribution, $\beta_4$, while continuing to be negative in its sign, is now not statistically significantly different from zero. This is not surprising considering that $\beta_4$ now

captures the effect of the nonwinner's performance on the winner's redistribution in the LT treatment, which is the baseline treatment in the fully interacted model. In contrast, the coefficients on LT × Correct answers by partner and AT × Correct answers by partner are both positive and statistically significant at the 5% level; the estimated interaction coefficients for these two transparency treatments are 31.3 (*SE* = 15.9) and 31.3 (*SE* = 14.1), respectively. In other words, a 1 percentage point increase in the nonwinner's performance is associated with 3.1 percentage points increase in the winner's redistribution in the LT and AT treatments. By simply making the nonwinners' performance available to winners, we can effectively increase the winners' willingness to share a significant portion of their earned or windfall incomes with those who performed well but were less fortunate in the selection process. However, note that the interaction coefficient on ATR × Correct answers by partner, although positive and sizable, is not statistically significantly different from zero. We are not certain why. Because both players were told at the start that their performance could determine redistribution at the end, we suspect that the winners might have viewed a better performance by nonwinners to be strategic and, as a result, not entirely deserving.

Column 3 of Table 1 reports the full specification with all the control variables. The results in Column 2 continue to be statistically robust in the full specification. For winners in the LT and AT treatments, the relative sizes of the winner's and nonwinner's performance coefficients suggest that a 1% increase in the nonwinner's performance can completely offset the same increase in the winner's performance on the winner's unwillingness to redistribute. In other words, we found evidence that learning about the nonwinners' performances can completely offset the feeling of deservingness that arises from improving one's own performance. Also, as an illustration, Figures 4 and 5 plot the marginal effects of winners' and nonwinners' performance on redistribution by treatments.

Column 3's other results are also interesting and worth discussing further. For example, we found that a 1 percentage points increase in the proportion of total rounds spent in the hotseat is associated with 1.3 percentage points drop in the winner's redistribution. This coefficient, which is statistically significant at the 1% level, suggests that people's sense of deservingness increases with the amount of time they spend in the hotseat, even when the opportunity to be in the hotseat is purely randomized. This result is robust to controlling for the proportion of correct answers given by the winners. On average, women redistribute more of their winnings than men, and redistribution increases with the altruism index. Other control variables do not enter the redistribution equation in a statistically important manner. Note also here that Table 1's full specification results continue to be statistically robust even when adjusting the standard errors to address the multiple comparisons problem (Benjamini and Hochberg, 1995); see Table 3A in the appendix.

One question of interest is what explains why the average treatment effect of transparency on redistribution is not statistically significant, despite the evidence of a positive, partial correlation between winners' redistribution and nonwinners' performance in the transparency treatments. One possible explanation is that winners rewarded only those nonwinners who either performed extremely well and/or at least as well as them. We found evidence supporting this hypothesis (Tables 4A and 5A in the appendix). First, winners' redistribution is positive and statistically significant only when nonwinners' performance is in either of the top two quintiles (Table 4A), suggesting that winners rewarded nonwinners only when they performed extremely well in the LT and AT treatments. Second, winners' redistribution peaks when the relative gap between winner and nonwinner in a dyad equals zero in the AT and ATR treatments (Table 5A), suggesting that winners' redistribution increases with nonwinners' performance up to a point where both players' performances are equally matched. Because 60% of nonwinners performed

worse than the winners (see Table 6A in the appendix), this would explain why the average treatment effect of transparency on redistribution is not statistically significantly different from zero.

What about the nonwinners' redistributive preferences? To understand their insights after the task, we re-estimated Eq.1 on nonwinners (Table 2). Here, the dependent variable measures how much each nonwinner would have redistributed had they won. Like the winners, the better the nonwinners performed, the lower they would have (hypothetically) redistributed: A 1 percentage point increase in nonwinners' performance is associated with 2.5 percentage points reduction in the hypothetical redistribution. However, we found little statistical evidence that an increase in winner's performance increases nonwinner's willingness to redistribute. These results suggest that either the nonwinners may have underestimated the power of transparency on their willingness to redistribute if they had won, or that they felt disheartened at losing the game.

To better understand the mechanisms that drive these effects, we estimated regression equations where the dependent variables are rotated factors of the winners' subjective deservingness (Table 3).[5] Note that all dependent variables are standardized to have a mean of 0 and a standard deviation of 1, and the higher values represent agreeing more with the statement. We found strong evidence (Column 1) that an increase in winners' performance decreases their perception that "the opponent was skillful" in two out of three ability-based treatments (ANT and AT), while an increase in nonwinners' performance increases winners' perception that "the opponent was skillful" in the ability-based and transparency treatments (AT and ATR).

[5] These are shown graphically in Figures 4A–11A in the online appendix. Nonwinners' sense of deservingness is shown in Table 7A in the online appendix.

We found (Column 2, Table 3) that the better the winners in the ANT and AT treatments performed, the less they thought that "the winning was due to chance". In contrast, the better the nonwinners in the AT performed, the more the winners agreed with that "the winning was due to chance". Note also that the main effects of both AT and ATR treatment dummies are negative and statistically significant in this regression. Column 3's results reveal only one important predictor of "I completely deserve the win", which is the main effect of the winner's performance. In other words, the better the winners performed, the more they felt that "I completely deserve the win" irrespective of the treatment assignment. Finally, an increase in the winner's performance also increases the winner's perception that "I put in maximum effort/skills" into the task (Column 4, Table 3).[6]

Taken together, our results support the hypotheses that deservingness increases with one's own performance but decreases with others' performance when others' performance is salient. In short, transparency allows people to consider others' deservingness when evaluating one's own performance and reasons for success.

To test whether these measures of subjective deservingness are the main drivers of Table 1's results, we include them as additional independent variables in Eq.1's specification and report the results in Table 4. Here, we see that the winner's redistribution increases with the variable "the opponent was skillful" and decreases with "I completely deserved the win". Although both "the winning was due to chance" and "I put in maximum effort/skills" enter the redistribution regression with the correct signs, they are not statistically significantly different from zero. In any case, we found strong and consistent evidence that, other things held constant, winners redistribute less if they feel they completely deserve the win, and more if they feel their opponent is skillful. These two coefficients are also of similar size,

[6] We found a similar set of data for the nonwinners sample (Table 7A in the appendix).

and we cannot reject a null hypothesis that their absolute values have the same magnitude. More importantly, by including the winner's subjective deservingness variables, both one's own and others' performance variables are driven toward zero and are now no longer statistically significant. This implies that Table 1's results can be explained by considering how one's own and others' performances affect our sense of deservingness for ourself and others.

Finally, we tested how equitable redistribution in the first task affected cooperation in the second task (Table 5). The question was simple: Since transparency produces a more equitable outcome through increased winners' redistribution, will nonwinners who receive more redistribution reciprocate by acting more cooperatively in a subsequent social dilemma game?

We found (Table 5) that an increase in the winner's redistribution in the first task resulted in a significantly higher contribution by nonwinners in the public-goods task ($\beta = 0.204$; $SE = 0.025$). This may be due to a reciprocity effect, in which the first task's nonwinner appreciates being compensated for their contributions and is therefore more willing to cooperate in the task at hand. In addition, when losing players allocated more points to the other player in their hypothetical redistribution of task 1's earnings, they also contributed significantly more in the public-goods game ($\beta = 0.124$; $SE = 0.026$). This could be evidence of a desire to justify their higher hypothetical redistribution, which perhaps reflects the level of redistribution that they expected or hoped to receive from the actual winner. In setting the redistributive bar this high, nonwinners must demonstrate that they are considerate of other players' efforts, and are therefore more willing to pay in and cooperate in the public-goods game. More generally, we found evidence that transparency increases redistribution that, in turn, may increase cooperation between participants in an otherwise unrelated social dilemma situation. These results are consistent with previous studies of behavioral spillover effects between games (see, e.g., Cason et al., 2012; Dreber et al., 2014).

## IV. Conclusions

Once human minds are made up, they are difficult to change. One of these commonly held ideas is that we fully deserve incomes that we have worked hard to earn, which explains why many are unwilling to share their earned incomes with others. Such feelings of entitlement toward earned incomes are pervasive even in cases where luck plays a major role in success (Tonin and Vlassopoulos, 2017). To the best of our knowledge, no studies have systematically investigated how we can effectively reduce feelings of deservingness and, in turn, increase people's willingness to redistribute their hard-earned incomes. Doing so has implications not only for the overall egalitarianism of our society, but also for the net global happiness achievable through voluntary redistribution (Dwyer and Dunn, 2022).

In our paper, we have revealed what happens to people's redistributive preferences when we can directly address survivorship bias (Miller and Ross, 1975; Babcock and Loewenstein, 1997), which is one of the root causes of perceived income entitlement. By making nonwinners' performance available to winners in a game where only one randomly selected person can win, we can effectively induce winners to redistribute more of their earned incomes to nonwinners who performed equally well, if not better, than them. We have shown that this effect is driven largely by a moderated sense of one's own and other's deservingness because nonwinners' efforts and relative ill fortune are taken into consideration by winners when they evaluate how deserving nonwinners are of their earned income. Perhaps surprisingly, we also found that people rationalize luck-based wins (successfully predicting the outcomes of coin flips), which leads them to redistribute less of their windfall incomes. However, we found that even transparency of how lucky the nonwinners had been in their predictions of coin flips caused winners to start redistributing more of their windfall incomes to their nonwinning partners.

Our study also contributes to the literature on luck and efforts by modeling a scenario where one of two players is randomly given an opportunity to play to win (hotseat placement) before exercising their ability to achieve success that originates from their luck (correctly answering trivia questions). We found strong evidence of winners effectively discounting luck as a factor of their success: The better winners performed in the general knowledge quiz, the less they redistributed to the nonwinners. We have also shown that, other things being held constant, the longer the winners spend in the hotseat, the less they redistribute as well. Our results thus contribute to the literature on redistributive justice by providing new evidence that, when made salient, information on nonsurvivors' performances can effectively reduce survivors' entitlement even in scenarios where the line between luck and efforts is blurred.

There are several policy implications to our findings. The first is to create a curriculum based around success and luck in primary and secondary schools. Such a curriculum could include lessons that allow young students to gain early exposures to inequality, which has been empirically shown to affect people's redistribution (Sands, 2017), as well as teach students about survivorship bias and the long-term impacts of initial luck on lifetime success (see, e.g., Frank, 2016). Second, companies may wish to train managers to recognize and identify both the luck and efforts of all employees when evaluating their performances for promotions and bonuses. There may also be a call for more transparency (and normalization) around managers' and employees' successes and failures. Finally, building on the well-established literature on taxpayer nudges (Antinyan and Asatryan, 2019), there may be scope for tax offices around the world to use information about the deservingness of the less fortunate to increase tax compliance.

Our finding that winners significantly increase redistribution only to well-performing nonwinners also offers new empirical insights into the effect of

transparency on meritocracy. Given that 60% of our nonwinners performed worse than winners, meritocracy implies that transparency may lower rather than raise the average redistribution from winners to nonwinners—because less than half of the nonwinning population deserves redistribution (McCoy and Major, 2007). However, although our estimated average treatment effects of transparency on redistribution are not statistically significantly different from zero, they are far from negative. Hence, our results suggest that increased meritocracy does not necessarily reduce equality even when there are relatively more people who underperform in society.

Like all studies in social sciences, our study is not without limitations. One main concern is the external validity of our findings, as the sample in our experiment is not nationally representative. Therefore, it remains to be seen whether our findings can be replicated using samples that represent all echelons of society. In addition, while a set of trivia quiz questions is not the most reflective task of abilities that are associated with economic success, it is a first start in investigating the collision of luck and effort. Future research could examine other effort-based tasks that are more likely to incite a sense of ownership and deservingness for one's own successes. Finally, while our study makes a significant contribution to the literature by uncovering the survivorship bias that impacts our sense of deservingness and redistributive choices, our study did not allow us to disentangle whether this relationship is driven by increasing the salience of the existence of nonsurvivors, of their ability, or of the luck that has contributed to one's own survivorship. Further research would investigate which of these solutions more precisely explains the underlying mechanism of our findings.

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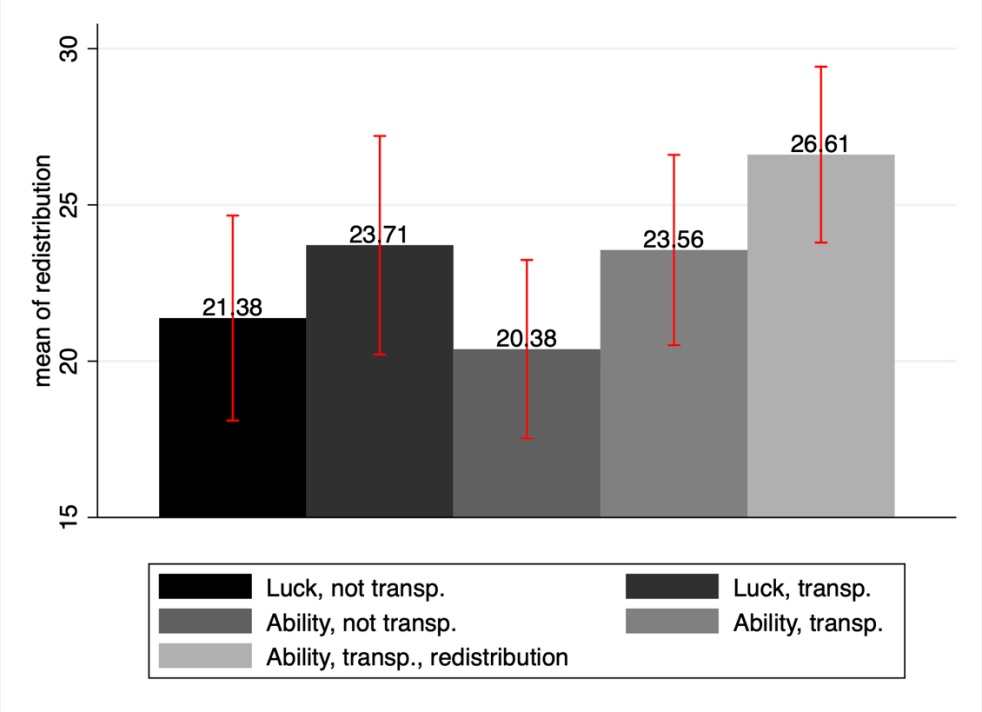
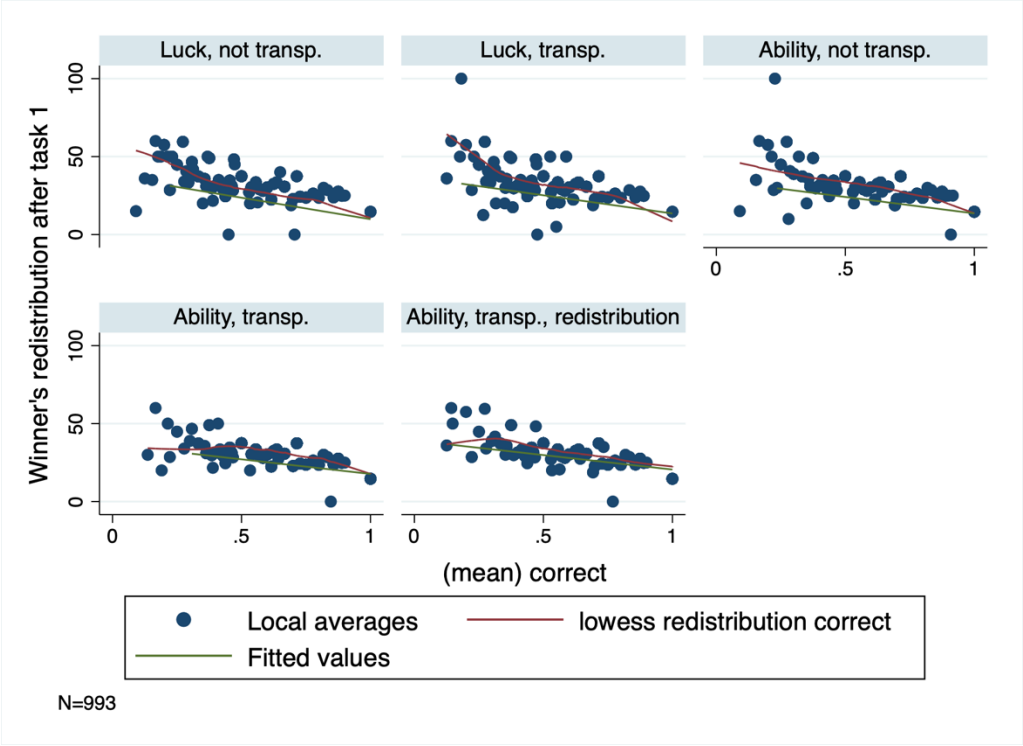
**Figure 1: Average winner's redistribution after the first task by treatment groups**



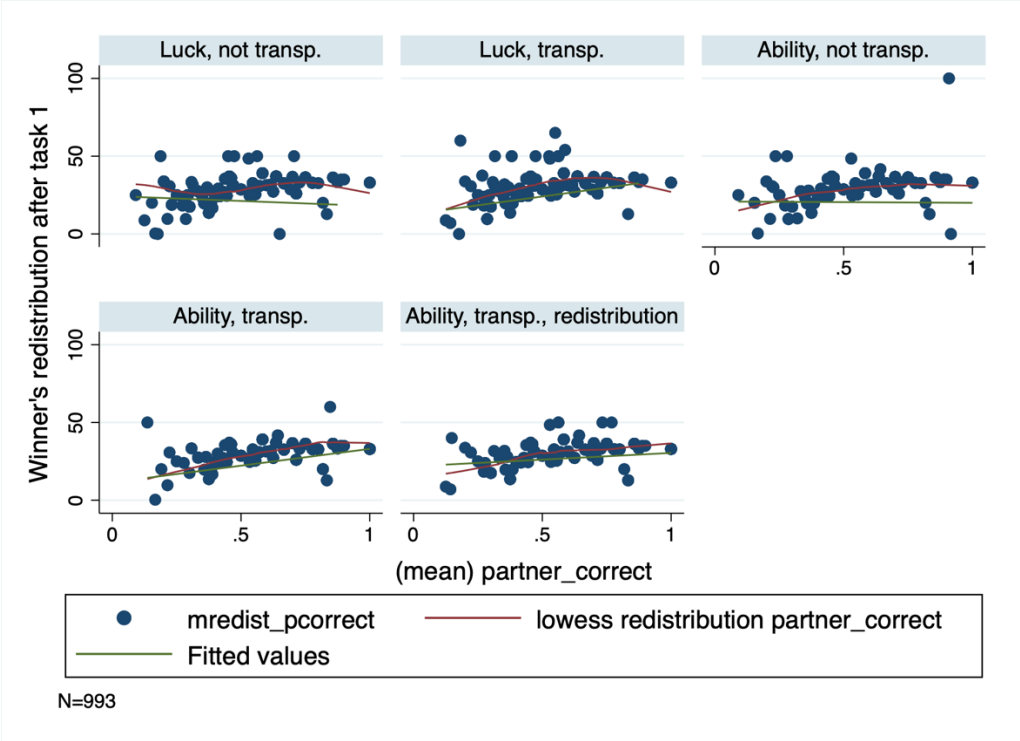

**Note:** 95% Confidence Intervals are reported.

**Figure 2: Relationship between winner's redistribution after task 1 and the proportion of correct answers given by self in the task by treatment groups**




**Note:** Locally weighted regression of the winner's redistribution after task 1 as a function of the proportion of correct answers given by self in the task. Each dot is a local average calculated for each proportion of correct answers given by self in the task.

**Figure 3: Relationship between winner's redistribution after task 1 and the proportion of correct answers given by partner in the task by treatment groups**




**Note:** Locally weighted regression of the winner's redistribution after task 1 as a function of the proportion of correct answers given by partner in the task. Each dot is a local average calculated for each proportion of correct answers given by partner in the task.

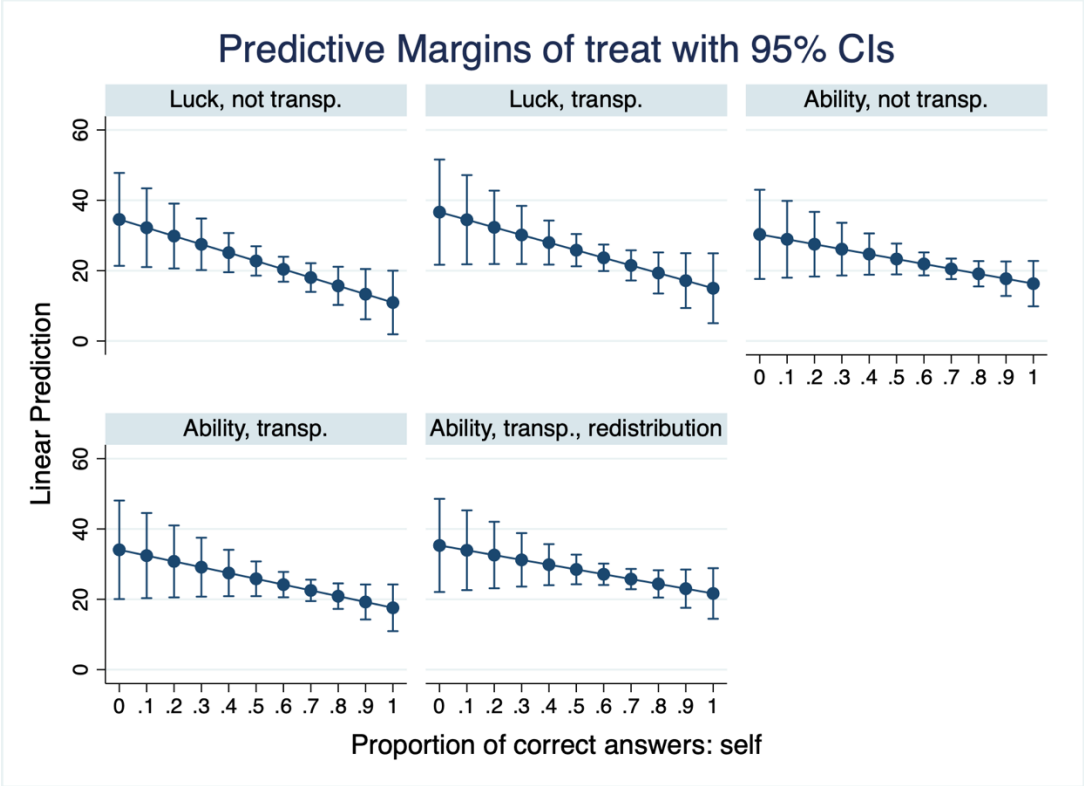
**Table 1: Winner's redistribution after task 1: OLS regressions**

| VARIABLES | (1) | (2) | (3) |
|---|---|---|---|
| **Treatments** | | | |
| Luck, transparent (LT) | 1.775 | -13.851 | -15.035 |
| | (2.407) | (11.106) | (11.341) |
| Ability, not transparent (ANT) | 0.037 | -6.203 | -11.357 |
| | (2.251) | (10.768) | (10.895) |
| Ability, transparent (AT) | 3.627 | -14.412 | -18.830 |
| | (2.295) | (11.257) | (11.761) |
| Ability, transparent, redistribution (ATR) | 6.377*** | -5.805 | -9.556 |
| | (2.204) | (10.678) | (10.708) |
| **Proportion of correct answers** | | | |
| Prop. of correct answers given by self | -23.648*** | -27.342*** | -23.621** |
| | (4.093) | (9.628) | (10.722) |
| LT × Correct answers by self | | 1.039 | 1.977 |
| | | (14.360) | (14.706) |
| ANT × Correct answers by self | | 5.795 | 9.607 |
| | | (12.676) | (12.965) |
| AT × Correct answers by self | | 3.462 | 7.137 |
| | | (12.961) | (13.504) |
| ATR × Correct answers by self | | 5.212 | 9.967 |
| | | (13.410) | (13.564) |
| Prop. of correct answers given by partner | 12.192*** | -5.100 | -9.618 |
| | (4.182) | (10.904) | (10.852) |
| LT × Correct answers by partner | | 31.365** | 32.091** |
| | | (15.952) | (16.313) |
| ANT × Correct answers by partner | | 7.690 | 13.327 |
| | | (13.558) | (13.500) |
| AT × Correct answers by partner | | 31.334** | 34.393** |
| | | (14.057) | (14.299) |
| ATR × Correct answers by partner | | 18.809 | 19.316 |
| | | (13.935) | (13.906) |
| **Control variables** | | | |
| Proportion of being in the 'hot seat' | | | -12.900*** |
| | | | (3.047) |
| Total number of rounds until complete the task | | | -0.155 |
| | | | (0.353) |
| Respondent's age | | | 0.741 |
| | | | (2.515) |

| | | | |
|---|---|---|---|
| Respondent's age squared | | | 0.085 |
| | | | (0.462) |
| Gender: Female | | | 2.897** |
| | | | (1.470) |
| Gender: Prefer not to answer | | | -7.168 |
| | | | (9.572) |
| Gender: Other | | | 8.167 |
| | | | (5.844) |
| Income: Earner $60k per annum or more | | | -0.797 |
| | | | (1.543) |
| Highest education: College degree | | | -1.095 |
| | | | (1.568) |
| Cohabitating/Domestic Partnership | | | 2.468 |
| | | | (2.447) |
| Divorced | | | 0.942 |
| | | | (3.511) |
| Married | | | -1.235 |
| | | | (1.803) |
| Separated | | | -2.839 |
| | | | (9.280) |
| Widowed | | | -3.709 |
| | | | (5.365) |
| White | | | -1.063 |
| | | | (1.601) |
| Did you cheat? (Yes, in some rounds) | | | 3.692 |
| | | | (3.657) |
| Do you think the other person cheated? (Yes, in some rounds) | | | -2.178 |
| | | | (3.042) |
| Altruism index | | | 0.325*** |
| | | | (0.082) |
| Constant | 29.376*** | 39.693*** | 37.084*** |
| | (3.360) | (8.293) | (11.975) |
| Observations | 993 | 993 | 993 |
| R-squared | 0.045 | 0.053 | 0.105 |

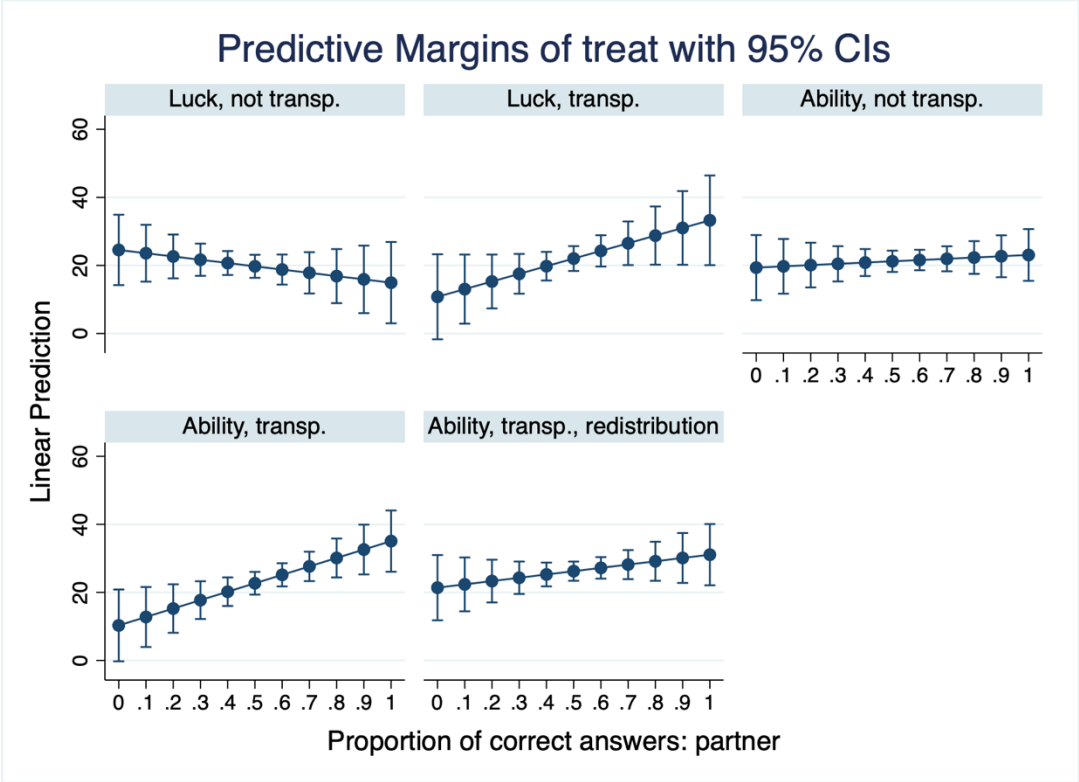
**Note:** **<5%; ***<1%. The sample consists only of the eventual winners in the task 1. Heteroskedasticity-consistent standard errors (HC3) are in parentheses.

**Figure 4: Plots of marginal effects of correct answers given by self in task 1 by treatment groups**




**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 1, Column 3's estimates.

**Figure 5: Plots of marginal effects of correct answers given by partner in task 1 by treatment groups**




**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 1, Column 3's estimates.

**Table 2: Nonwinners' attitudes towards redistribution**

| VARIABLES | Nonwinners' attitudes towards redistribution after the first task |
|---|---|
| **Treatments** | |
| Luck, transparent (LT) | 10.510 |
| | (11.973) |
| Ability, not transparent (ANT) | 3.643 |
| | (11.133) |
| Ability, transparent (AT) | -0.901 |
| | (11.658) |
| Ability, transparent, redistribution (ATR) | -6.797 |
| | (11.320) |
| **Proportion of correct answers** | |
| Prop. of correct answers given by self | -25.292** |
| | (11.256) |
| LT × Correct answers by self | -1.304 |
| | (16.474) |
| ANT × Correct answers by self | 12.484 |
| | (13.443) |
| AT × Correct answers by self | 15.984 |
| | (14.809) |
| ATR × Correct answers by self | 13.606 |
| | (14.499) |
| Prop. of correct answers given by partner | 12.155 |
| | (11.400) |
| LT × Correct answers by partner | -10.610 |
| | (15.059) |
| ANT × Correct answers by partner | -10.379 |
| | (13.537) |
| AT × Correct answers by partner | -6.878 |
| | (15.293) |
| ATR × Correct answers by partner | -1.292 |
| | (14.312) |
| **Control variables** | Yes |
| Observations | 993 |
| R-squared | 0.106 |

**Note:** **<5%; ***<1%. The sample consists only of non-winners in task 1. Heteroskedasticity-consistent standard errors (HC3) are in parentheses. Other control variables are as in Table 1.

**Table 3: Deservingness regressions: winners**

| VARIABLES | (1) F1: My opponent was skillful | (2) F2: My winning was due to chance | (3) F3: I completel y deserve the win | (4) F4: I put in maximum effort/skills |
|---|---|---|---|---|
| **Treatments** | | | | |
| Luck, transparent (LT) | -0.290 | -0.157 | 0.515 | 1.037** |
| | (0.416) | (0.363) | (0.417) | (0.503) |
| Ability, not transparent (ANT) | 0.766 | -0.157 | 0.392 | 0.406 |
| | (0.439) | (0.387) | (0.363) | (0.395) |
| Ability, transparent (AT) | 0.225 | -0.876** | 0.581 | 0.819** |
| | (0.434) | (0.407) | (0.392) | (0.411) |
| Ability, transparent, redistribution (ATR) | -0.339 | -0.813** | 0.507 | 0.442 |
| | (0.425) | (0.413) | (0.376) | (0.407) |
| **Proportion of correct answers** | | | | |
| Prop. of correct answers given by self | 0.223 | -0.338 | 1.143*** | 1.508*** |
| | (0.477) | (0.435) | (0.429) | (0.554) |
| LT × Correct answers by self | 0.279 | -0.029 | -0.237 | -0.912 |
| | (0.580) | (0.541) | (0.553) | (0.754) |
| ANT × Correct answers by self | -1.182** | -1.451*** | 0.002 | 0.515 |
| | (0.593) | (0.535) | (0.458) | (0.606) |
| AT × Correct answers by self | -1.196** | -1.193** | -0.427 | -0.016 |
| | (0.563) | (0.550) | (0.497) | (0.598) |
| ATR × Correct answers by self | -0.973 | -0.894 | -0.267 | 0.740 |
| | (0.593) | (0.565) | (0.467) | (0.601) |
| Prop. of correct answers given by partner | -0.133 | -0.328 | 0.218 | 0.254 |
| | (0.504) | (0.346) | (0.452) | (0.588) |
| LT × Correct answers by partner | 0.748 | 0.765 | -0.735 | -1.077 |
| | (0.639) | (0.492) | (0.606) | (0.774) |
| ANT × Correct answers by partner | 0.439 | 0.297 | -0.367 | -0.187 |
| | (0.630) | (0.476) | (0.499) | (0.647) |
| AT × Correct answers by partner | 2.013*** | 1.373*** | -0.349 | -0.305 |
| | (0.623) | (0.521) | (0.537) | (0.657) |
| ATR × Correct answers by partner | 2.634*** | 0.851 | -0.397 | -0.657 |
| | (0.602) | (0.481) | (0.515) | (0.649) |
| **Control variables** | Yes | Yes | Yes | Yes |
| Observations | 993 | 993 | 993 | 993 |
| R-squared | 0.202 | 0.367 | 0.115 | 0.285 |

**Note:** **<5%; ***<1%. The sample consists only of winners in task 1. Each dependent variable is a rotated factor component of deservingness, standardized to have a mean of 0 and a standard deviation of 1; See Table 1A for the rotated factor loadings. Heteroskedasticity-consistent standard errors (HC3) are in parentheses. Other control variables are as in Table 1.

**Table 4: Including deservingness as possible mechanisms in the redistribution regressions**

| VARIABLES | Winner's redistribution after the first task |
|---|---|
| **Standardized rotated factors of deservingness** | |
| F1: My opponent was skillful | 5.845*** |
| | (0.807) |
| F2: My winning was due to chance | 0.841 |
| | (0.869) |
| F3: I completely deserved the win | -4.438*** |
| | (1.163) |
| F4: I put in maximum efforts/skillful | -1.194 |
| | (0.991) |
| **Treatments** | |
| Luck, transparent (LT) | -9.685 |
| | (11.575) |
| Ability, not transparent (ANT) | -13.476 |
| | (10.612) |
| Ability, transparent (AT) | -15.855 |
| | (11.506) |
| Ability, transparent, redistribution (ATR) | -4.114 |
| | (10.605) |
| **Proportion of correct answers** | |
| Prop. of correct answers given by self | -17.768 |
| | (10.964) |
| LT × Correct answers by self | -1.765 |
| | (15.098) |
| ANT × Correct answers by self | 18.356 |
| | (12.715) |
| AT × Correct answers by self | 13.215 |
| | (13.296) |
| ATR × Correct answers by self | 16.102 |
| | (13.237) |
| Prop. of correct answers given by partner | -7.294 |
| | (10.977) |
| LT × Correct answers by partner | 22.530 |
| | (16.595) |
| ANT × Correct answers by partner | 8.659 |
| | (13.117) |

| | |
|---|---|
| AT × Correct answers by partner | 19.563 |
| | (14.284) |
| ATR × Correct answers by partner | 0.661 |
| | (13.833) |
| **Control variables** | Yes |
| Observations | 993 |
| R-squared | 0.166 |

**Note:** **<5%; ***<1%. The sample consists only of winners in task 1. Each deservingness variable is a rotated factor component of deservingness, standardized to have a mean of 0 and a standard deviation of 1. Heteroskedasticity-consistent standard errors (HC3) are in parentheses. Other control variables are as in Table 1

**Table 5: Evidence of spillover effect into the subsequent public goods game**

| VARIABLES | Non-winners: PGG contribution |
|---|---|
| Winner's redistribution in the first task | 0.204*** |
| | (0.025) |
| Non-winners' expectation of the winner's redistribution level | 0.124*** |
| | (0.026) |
| Proportion of being in the 'hot seat' | -1.388 |
| | (2.233) |
| Total number of rounds until complete the task | -0.113 |
| | (0.166) |
| Respondent's age | -1.894 |
| | (1.644) |
| Respondent's age squared | 0.344 |
| | (0.301) |
| Gender: Female | -4.960*** |
| | (1.054) |
| Gender: Prefer not to answer | -12.984*** |
| | (5.016) |
| Gender: Other | -0.182 |
| | (5.803) |
| Income: Earner $60k per annum or more | 2.669** |
| | (1.144) |
| Highest education: College degree | -1.061 |
| | (1.141) |
| Cohabitating/Domestic Partnership | 0.072 |
| | (1.814) |
| Divorced | 3.404 |
| | (2.360) |
| Married | -0.285 |
| | (1.459) |
| Separated | -0.615 |
| | (7.149) |
| Widowed | 0.130 |
| | (6.003) |
| White | 2.344** |
| | (1.132) |
| Altruism index | 0.263*** |
| | (0.062) |

| | |
|---|---|
| Constant | 16.405*** |
| | (3.123) |
| Observations | 992 |
| R-squared | 0.160 |

**Note:** **<5%; ***<1%. Heteroskedasticity-consistent standard errors (HC3) are in parentheses.

**Online Appendix**

**Figure 1A: Experimental flow**

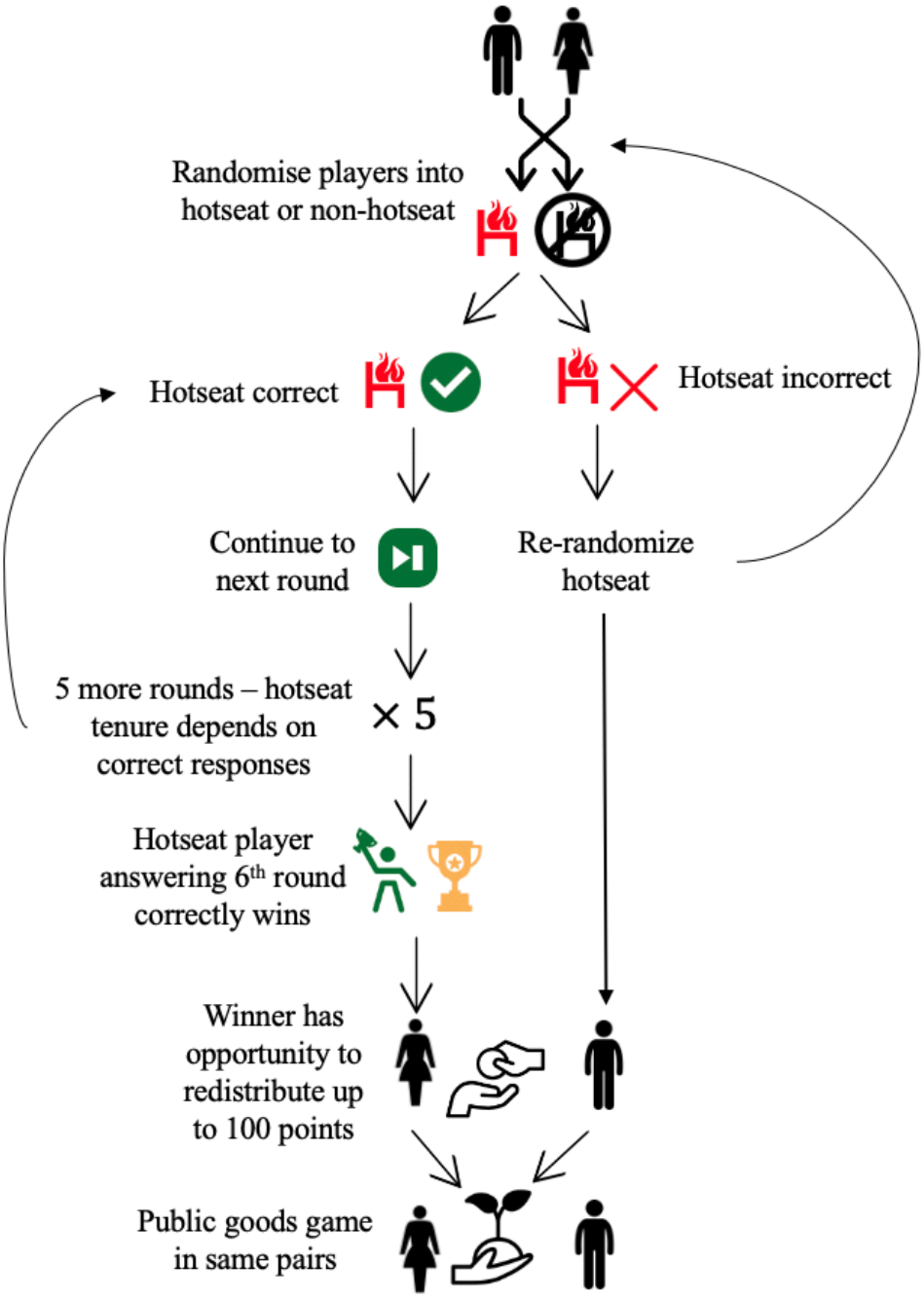


**Figure 2A: Screenshots of experiment**

## *Participant Information Sheet and Consent*

Study title: Partnered tasks

Investigators: Nattavudh Powdthavee, Warwick Business School
Juliane Wiese, Warwick Business School
Jonathan Yeo, Nanyang Technological University
Yohanes E. Riyanto, Nanyang Technological University
Welcome and thank you for participating! This purpose of this research is to examine your behavior in a partnered task. You will be asked to take part in two strategic games and answer some questions about your attitudes and opinions, as well as your demographics. The research is funded by research funds from the Warwick Business School and Nanyang Technological University.

Your participation is completely voluntary. You can withdraw at any time, and for any reason, simply by closing your browser. However, we are only able to pay you if you complete the survey. No identifiable data will be collected from you as part of this study. This means that once your responses have been submitted to the research team, it will not be possible to withdraw this data as your individual responses cannot be identified.

Data will be securely stored on secure server accessed via a password-protected computer and will be processed only for the purpose of scientific analysis. Access to the data will be restricted to the investigators listed above. Summaries may be presented at conferences and included in scientific publications. Data will be reviewed on completion of the research, in line with the University of Warwick data retention policy. Please refer to the University of Warwick Research Privacy Notice which is available here: https://warwick.ac.uk/services/idc/dataprotection/privacynotices/researchprivacynotice or by contacting the Information and Data Compliance Team at GDPR@warwick.ac.uk.

This study has been reviewed and given favourable opinion by the University of Warwick's Humanities and Social Sciences Research Ethics Committee (HSSREC).

If you require further information, please contact Juliane Wiese at juliane.wiese@warwick.ac.uk.

Who should I contact if I wish to make a complaint?
Any complaint should be addressed to the person below, who is a senior University of Warwick official entirely independent of this study:
Research & Impact Services
University House
University of Warwick
Coventry
CV4 8UW
Email: researchgovernance@warwick.ac.uk
Tel: 02476 575733
If you wish to raise a complaint on how we have handled your personal data, you can contact our Data Protection Officer who will investigate the matter: DPO@warwick.ac.uk.
If you are not satisfied with our response or believe we are processing your personal data in a way that is not lawful you can complain to the Information Commissioner's Office (ICO).

Thank you for taking the time to read this Participant Information Leaflet.
I have read the above and:

- I consent to take part in the study → [participants continue to Introduction]
- I do not wish to participate → [participants excluded from study]

## *Introduction*

Welcome to the study!

This entire session will take approximately 15 minutes to complete.

Throughout the session, you may be rewarded with up to 187.5 max points which will be used to determine your total payment. Points will be rounded to the nearest integer. On average, participants earn 112.5 points. 100 points correspond to $0.89. On top of this, you will receive a **completion fee of $1.75**. Thus, the **average total earnings rewarded for the entire experiment will be $2.76.**

In this study, you will have to complete several tasks which involve anonymous interactions with another randomly matched participant. You will receive detailed instructions at the beginning of each task.

Once all tasks have been completed, there will also be a short questionnaire to fill in.

We will calculate and transfer your payment only after the entire study has concluded.

**Please note: these tasks are best supported by the Google Chrome browser.**

Because this game involves real-time interactions with other players, **please join this game only if you can commit to completing it in one session.** The entire game is expected to take 15 minutes. If you idle for more than 5 minutes, you will be automatically excluded from the study.

### *Prolific ID*

Please enter your Prolific ID. Ensure that this is accurate or we will not be able to pay you!

## *Introduction for Task 1*

*Luck conditions (LT, LNT) in non-bracketed text below.*
*Ability conditions (AT, ANT, ATR) with variations in bracketed text.*
*Transparency conditions (LT, AT, ATR) with underlined text.*

In this task, you will have to participate in a 2-player game. You will be matched with a randomly assigned participant.

The aim of this 2-player game is to complete **6 rounds** of successful coin flip predictions [general knowledge and logic quiz questions].

In each round, each player will be given **40 seconds** to make a prediction of a fair coin toss: heads or tails [answer a multiple-choice quiz question]. A coin will then be flipped, and players will learn whether their own prediction matches the outcome, <u>as well as whether the other player's prediction matches the outcome</u>. [Players will learn whether they have answered correctly, <u>as well as whether the other player has answered correctly</u>]

Furthermore, in each round, there will be a single player in the **hotseat** .
The hotseat player's outcome will determine whether the group can move on to the subsequent round.

Who is in the hotseat will be determined as follows:

- At the start of the first round:
  - one of the two players (including you) will be randomly selected to be in the hotseat.
- In subsequent rounds:
  - If a hotseat player's prediction is **correct** (matches the outcome) [answer is **correct**], he/she remains in the hotseat and the **group proceeds to the next round.**
  - If a hotseat player's prediction is **incorrect** (does not match the outcome) [answer is **incorrect**], one of the two players (including you) will again be randomly selected to be in the hotseat. Thus, either you get to remain in the hotseat or the other player is selected to be in the hotseat. **The group does not proceed to the next round.**

**It is important to remember that the outcome of the player not in the hotseat will be irrelevant for whether the group can advance to the next round.**

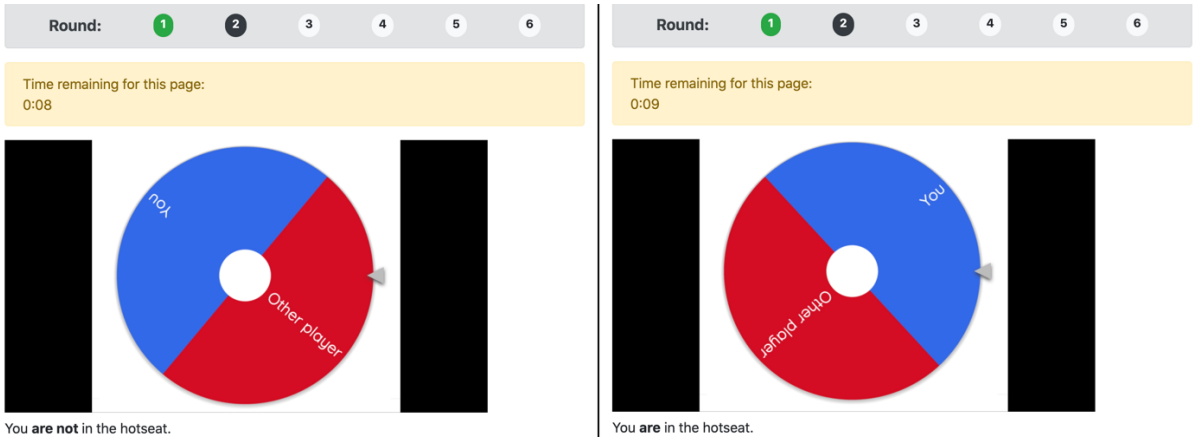
The player in the hotseat at the end of the 6th successful round will receive 100 points.

*[Ability, Transparent, Redistribution only:]* The winning player will have the opportunity to redistribute these winnings with the losing player, and each player will receive a summary of the other's performance.

## *Task 1 Screenshots – split screen images to show screens of a matched pair*

*Ability tasks example*


*Luck tasks example*



Please make a prediction of the coin flip:

---------

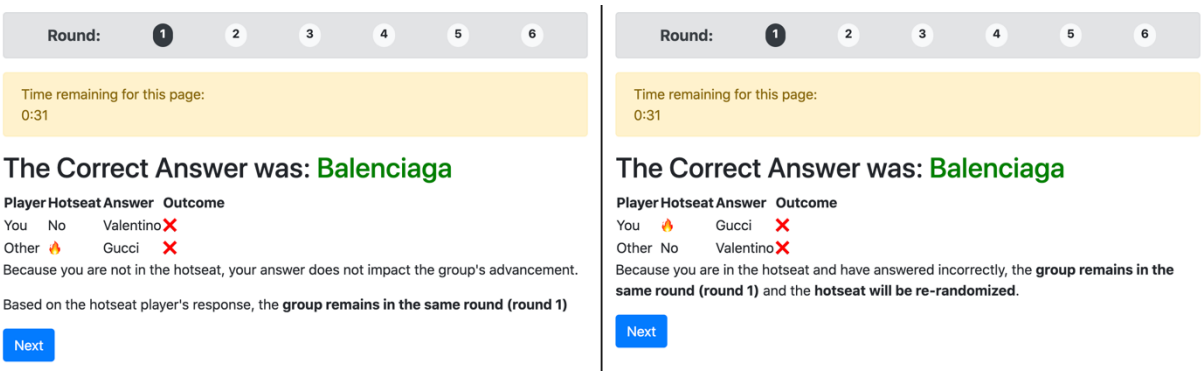
Next

Please make a prediction of the coin flip:

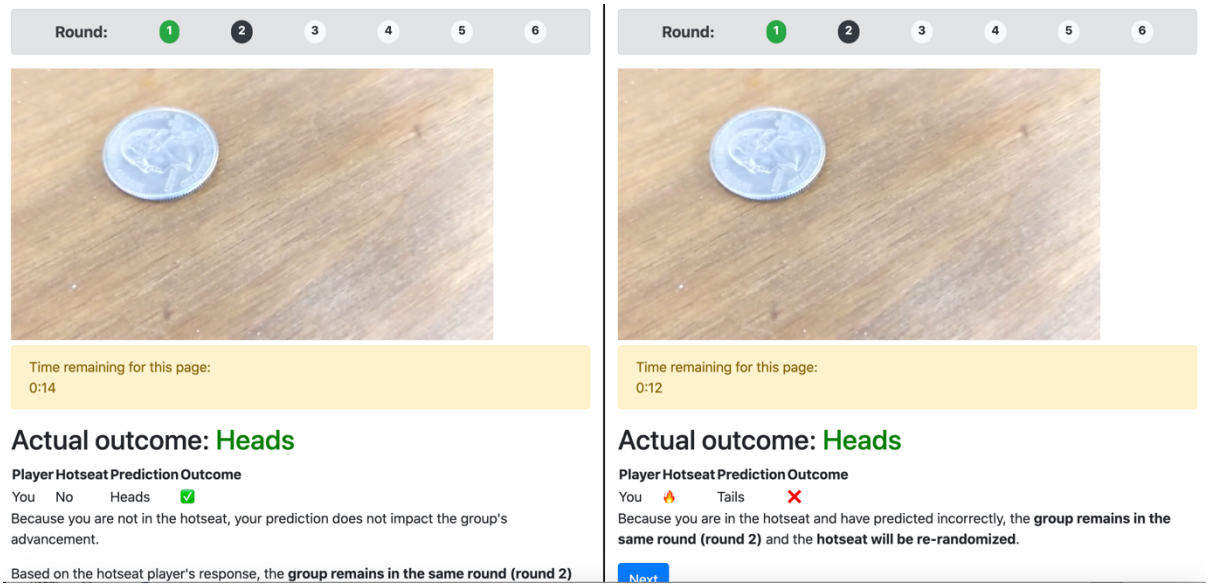
---------

Next

*Ability, transparent outcome example*

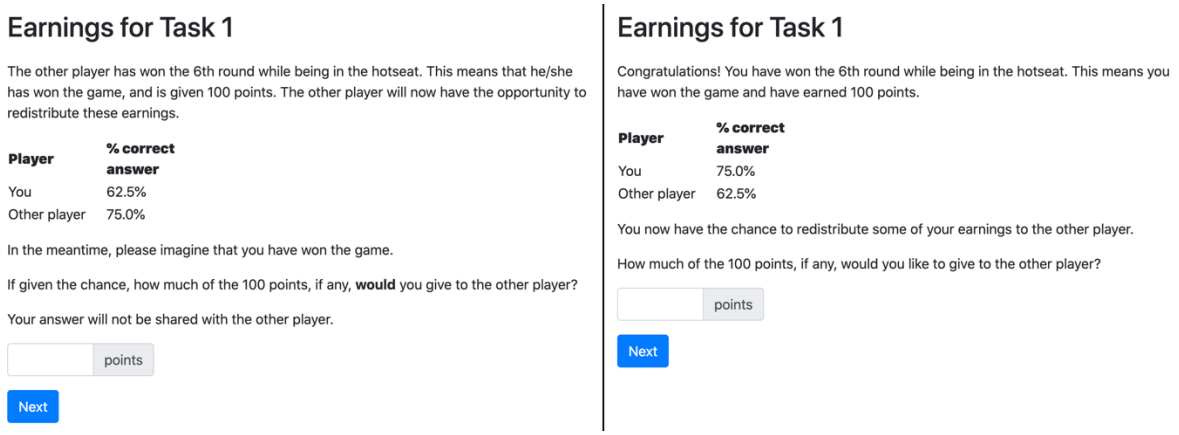



*Luck, Non-transparent outcome example*

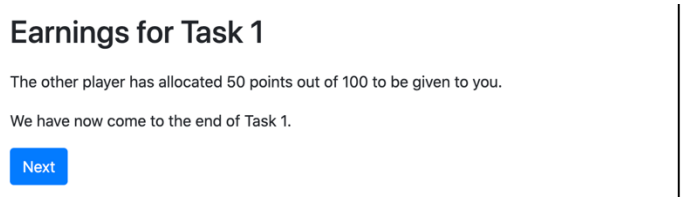


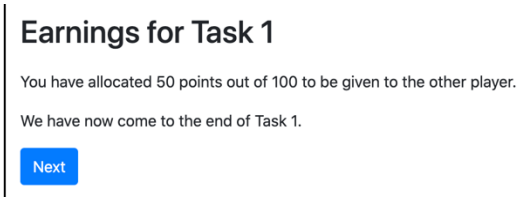

*Transparent earnings outcome example at the end of Task 1*

*Note:* Non-transparent conditions are identical with the omission of the "Other player" line in the table

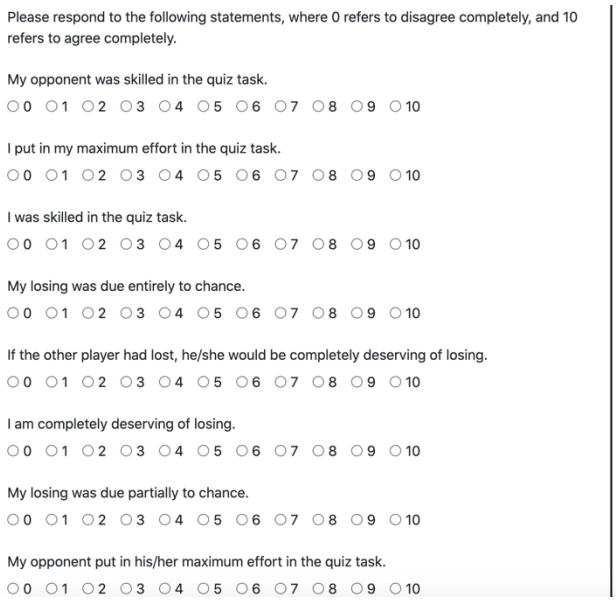


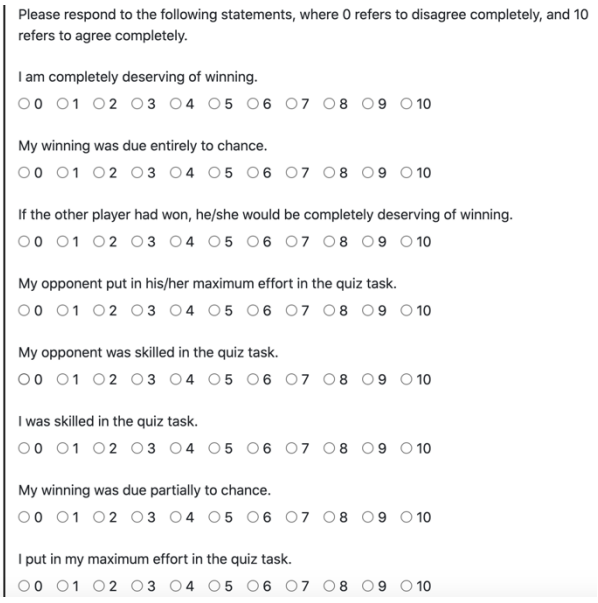

*Earnings summary*




*Deservingness questions (screenshot from Ability task)*



## *Task 2 Screenshots*

### Introduction for Task 2

You will now have to complete a second task with the **same player** as before.

In this task, each player will have an endowment of 50 points. Players can contribute as much of their endowment as they like to a group project which will benefit both players monetarily.

The total amount contributed by both players will be multiplied by 1.5, and this resulting product will be shared equally by you and the other player, regardless of your original contribution amounts.

You get to keep the remainder of your endowment which you choose not to contribute to the project, plus half of the multiplied joined product.

In other words, you will earn:

$$\underbrace{50 - \textit{own contribution}}_{\text{Personal income}} + \underbrace{\frac{1.5 \times (\textit{own contribution} + \textit{other's contribution})}{2}}_{\text{Share of group income}}$$

Personal income
(The number of points kept for yourself)

Share of group income
(Your half of total group income)

Next

# Contribution for Task 2

Remember, you and your player each receive an endowment of 50 points.

The total amount contributed by both players will be multiplied by 1.5, and this resulting product will be shared equally by you and the other player, regardless of your original contribution amounts. You will also keep any points that you don't contribute.

In other words, you will earn:
**(50 - Your contribution) + (Your contribution + Other's contribution)*1.5 / 2**

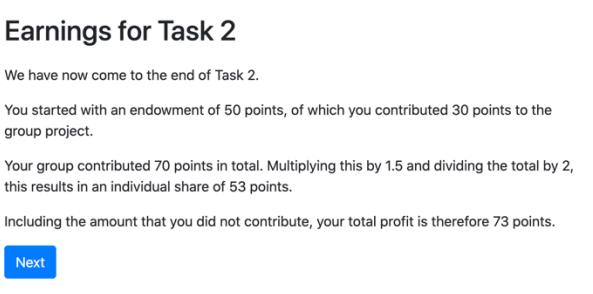
How much will you contribute to the group project?

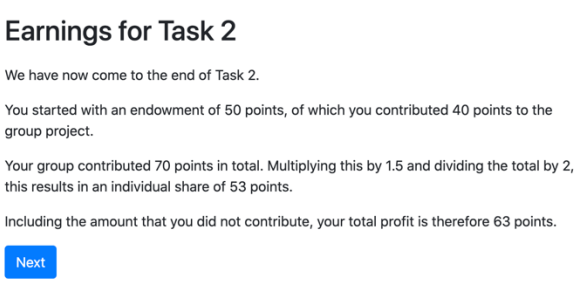
points

How much do you think the other player will contribute to the group project?

points

Next

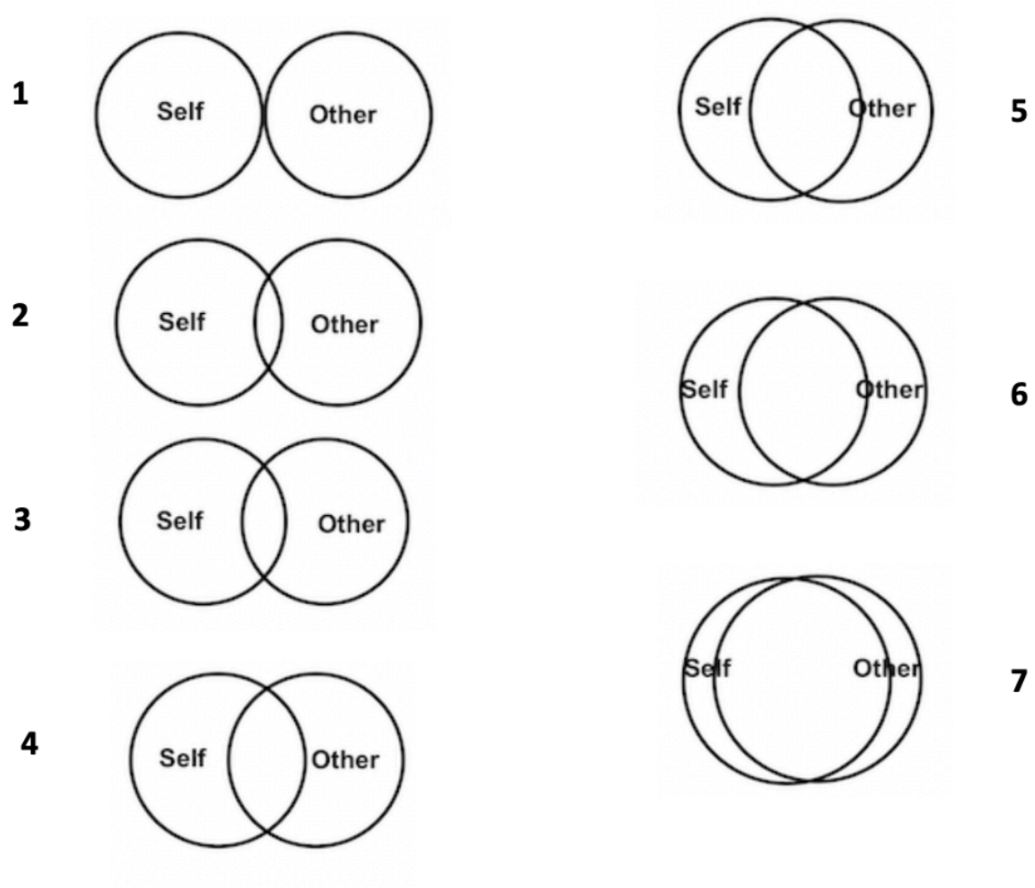


## *Post-task questionnaire*

*IOS (inclusion of other in self) question (Ar*on et al., 1992)

Which picture best describes your relationship with the other player?




*Altruism questions*

How often would you exhibit the following behaviours? (Never, once, more than once, often, very often) (Witt and Boleman, 2009).

1. I would give directions to someone I did not know.
2. I would make changes for someone I did not know.
3. I would give money to a charity.
4. I would donate clothes or goods to a charity.
5. I would help carry belongings of someone I did not know.
6. I would delay an elevator and hold the door for someone I did not know.
7. I would allow someone I did not know to go in front of me in line.
8. I would point out a clerk's error in undercharging me for an item.
9. I would let a neighbour I did not know well borrow an item of value to me.
10. I would help a classmate who I did not know well with a homework assignment when my knowledge was greater than his or hers.
11. I would voluntarily look after a neighbour's pet or children without being paid.
12. I would offer to help a handicapped or elderly person across the street.
13. I would offer my seat on a train or bus to someone who was standing.
14. I would help an acquaintance move houses.

*Performance estimate question [Non-transparent conditions only]*
In task 1, your outcomes were correct ___% of the time. If you have to guess, the other player had what percentage (%) of correct outcomes?

In Task 1, the other player's outcomes were correct ___% of the time, while yours were correct ___% of the time. Knowing how the other player performed, if you could redistribute your Task 1 earnings of 100 points again, how would you redistribute them?

*Cheating questions [Ability conditions only]*
To what extent do you believe the other player cheated in Task 1 (quiz questions)?

- Not at all
- In some rounds
- In most rounds
- In every round

To what extent did you cheat in Task 1 (you won't be penalised)?

- Not at all
- In some rounds
- In most rounds
- In every round

*Demographic questions [randomised order]*
Gender:

- Female
- Male
- Prefer not to say
- Other

Age:

- Under 20
- 20-29
- 30-39
- 40-49
- 50-59
- 60+

Please indicate the category that includes your entire household income in the previous year before taxes:

- <$10,000
- $10,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $59,999
- $60,000 - $69,999
- $70,000 - $79,999
- $80,000 - $89,999
- $90,000 - $99,999
- $100,000 - $149,999
- $150,000 or more
- Prefer not to say

Education:

- No schooling completed
- Nursery school to 8th grade
- Some high school, no diploma
- High school diploma or equivalent
- Some college, no degree
- College degree

- Master's degree
- PhD or higher

Ethnicity:
- White
- Black or African American
- Native American or American Indian
- Hispanic or Latino
- Asian
- Pacific Islander
- Multiracial
- Other

Employment status. Are you currently:
- Employed for wages
- Self-employed
- Out of work and looking for work
- Out of work but not currently looking for work
- Homemaker

State: [List of US states]

Marital status:
- Single, never married
- Cohabitating/domestic partnership
- Married
- Widowed
- Divorced
- Separated

What do you think this experiment is about?

***Final page example***

# Summary and Completion code

Throughout the tasks, you have earned 123 points. At a rate of 100 points per $0.89, this means you have earned $1.09.

In addition to your earnings, you will receive a completion payment of $1.75.

In sum, your total payment is $2.84.

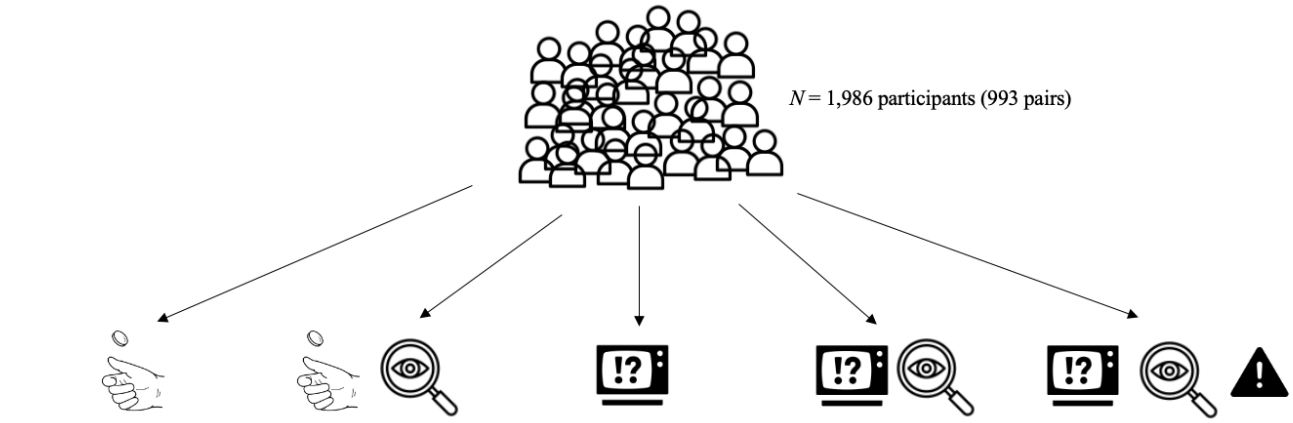
Please return to Prolific and enter the following completion code: 115C5840. **If you do not do this, we cannot pay you.**

**Table 1A: Rotated factor loadings on the deservingness variables**

| Deservingness variables | Factor 1 | Factor 2 | Factor 3 | Factor 4 | Uniqueness |
|---|---|---|---|---|---|
| **"I am completely deserving of winning // I am completely deserving of losing"** | 0.0960 | -0.2314 | 0.6245 | 0.0621 | 0.5433 |
| **"If the other player had won, he/she would be completely deserving of winning // If the other player had lost, he/she would be deserving of losing"** | 0.0515 | -0.0336 | 0.6303 | 0.1079 | 0.5870 |
| **"My opponent was skilled in the quiz [prediction] task"** | 0.6376 | -0.1428 | 0.0796 | 0.1286 | 0.5296 |
| **"I was skilled in the quiz [prediction] task"** | 0.1072 | -0.1837 | 0.2334 | 0.4643 | 0.6847 |
| **"My opponent puts in his/her maximum effort in the quiz [prediction] task"** | 0.7402 | 0.0671 | 0.0402 | 0.0070 | 0.4365 |
| **"I put in my maximum effort in the skills [prediction] task"** | 0.3826 | -0.0582 | 0.1709 | 0.3060 | 0.6532 |
| **"My winning was due entirely to chance // My losing was due entirely to chance"** | -0.0431 | 0.6799 | -0.1675 | -0.1414 | 0.4875 |
| **"My winning was due partially to chance // My losing was due partially to chance"** | 0.0218 | 0.6394 | -0.0468 | 0.0365 | 0.5866 |

**Note:** Responses to the eight deservingness questions range from 0 = completely disagree, 10 = completely agree. Effort/luck variation in brackets. Left-hand side phrases are for winners, and right-hand side phrases are for losers. Number of observations = 1,986. Retained factors = 4. Uniqueness represents the variance that is unique to the variable and not shared with other variables.

**Figure 3A: Treatment groups**




**Luck, no transparency**
*N* = 363
Participants predict the outcome of a fair automated coin toss. They see only their own result.

**Luck, transparency**
*N* = 347
Participants predict the outcome of a fair automated coin toss. They see their own result and the result of their partner.

**Ability, no transparency**
*N* = 468
Participants respond to general knowledge questions. They see only their own result.

**Ability, transparency**
*N* = 384
Participants respond to general knowledge questions. They see their own result and the result of their partner.

**Ability, transparency, redistribution**
*N* = 424
Participants respond to general knowledge questions. They see their own result and the result of their partner. They are warned at the beginning of the game that the winner will have the opportunity to redistribute with the loser.

## Table 2A: Descriptive statistics and balance checks

| | LNT (1) | LT (2) | ANT (3) | AT (4) | ATR (5) |
|---|---|---|---|---|---|
| Age: under 20 | 0.044 | 0.029 | 0.028 | 0.031 | 0.024 |
| Age: 20-29 | 0.347 | 0.363 | 0.348 | 0.370 | 0.363 |
| Age: 30-39 | 0.344 | 0.268 | 0.325 | 0.312 | 0.297 |
| Age: 40-49 | 0.121 | 0.141 | 0.143 | 0.143 | 0.160 |
| Age: 50-59 | 0.096 | 0.107 | 0.107 | 0.076 | 0.094 |
| Age: 60+ | 0.047 | 0.092 | 0.049 | 0.068 | 0.061 |
| Female | 0.477 | 0.496 | 0.434 | 0.503 | 0.542 |
| High income | 0.499 | 0.493 | 0.498 | 0.492 | 0.493 |
| College | 0.620 | 0.625 | 0.590 | 0.633 | 0.620 |
| White | 0.656 | 0.654 | 0.677 | 0.664 | 0.675 |
| Single, never married | 0.490 | 0.467 | 0.524 | 0.453 | 0.481 |
| Altruism index | 32.653 | 33.311 | 33.370 | 33.701 | 33.991 |
| **N** | **363** | **347** | **468** | **384** | **424** |

| P-values | (1) vs. (2) | (1) vs. (3) | (1) vs. (4) | (1) vs. (5) | (2) vs. (3) | (2) vs. (4) | (2) vs. (5) | (3) vs. (4) | (3) vs. (5) | (4) vs. (5) |
|---|---|---|---|---|---|---|---|---|---|---|
| Age: under 20 | 0.280 | 0.205 | 0.357 | 0.109 | 0.929 | 0.848 | 0.650 | 0.765 | 0.694 | 0.504 |
| Age: 20-29 | 0.656 | 0.972 | 0.519 | 0.639 | 0.662 | 0.852 | 0.998 | 0.515 | 0.642 | 0.846 |
| Age: 30-39 | 0.028 | 0.553 | 0.355 | 0.157 | 0.081 | 0.187 | 0.372 | 0.702 | 0.374 | 0.637 |
| Age: 40-49 | 0.431 | 0.357 | 0.376 | 0.117 | 0.937 | 0.938 | 0.461 | 0.998 | 0.474 | 0.499 |
| Age: 50-59 | 0.653 | 0.623 | 0.308 | 0.921 | 0.992 | 0.143 | 0.572 | 0.117 | 0.537 | 0.340 |
| Age: 60+ | 0.017 | 0.877 | 0.221 | 0.373 | 0.015 | 0.221 | 0.106 | 0.247 | 0.426 | 0.712 |
| Female | 0.611 | 0.219 | 0.478 | 0.065 | 0.080 | 0.852 | 0.196 | 0.045 | 0.001 | 0.258 |
| High income | 0.877 | 0.983 | 0.861 | 0.874 | 0.886 | 0.987 | 0.997 | 0.869 | 0.883 | 0.983 |
| College | 0.880 | 0.380 | 0.714 | 0.990 | 0.304 | 0.835 | 0.885 | 0.200 | 0.352 | 0.714 |
| White | 0.967 | 0.511 | 0.809 | 0.576 | 0.488 | 0.779 | 0.552 | 0.682 | 0.928 | 0.752 |
| Single, never married | 0.532 | 0.344 | 0.309 | 0.797 | 0.110 | 0.710 | 0.693 | 0.041 | 0.207 | 0.426 |
| Altruism index | 0.347 | 0.287 | 0.137 | 0.046 | 0.930 | 0.576 | 0.306 | 0.620 | 0.328 | 0.663 |

**Note:** The balance check shows appropriate balance on observable characteristics. We observe slight imbalance in age-groups and gender when comparing LNT & LT, ANT & AT, and ANT & AT. Nevertheless, we control for these characteristics in the full specification of our models to address any potential impact on the results.

**Table 3A: Adjusted p-values accounting for multiple comparisons of our main specification (Table 1, column 3).**

| | Unadjusted p-values | Adjusted significance threshold | Adjusted significance |
|---|---|---|---|
| LT × Correct answers by partner | 0.049** | 0.05 | Yes, $p < 0.05$ |
| ANT × Correct answers by partner | 0.324 | 0.1 | No |
| AT × Correct answers by partner | 0.016** | 0.025 | Yes, $p < 0.05$ |
| ATR × Correct answers by partner | 0.165 | 0.075 | No |

**Note:** **<5%; ***<1%.

In order to account for the multiple comparison problem, we perform the Benjamini-Hochberg procedure to control the false discovery rate, which is proven to have gains in power when compared to controlling the family-wise error rate (Benjamini and Hochberg, 1995). We set the false discovery rate to 0.1 and find that the two terms that are significant prior to correcting for multiple comparisons (LT × Correct answers by partner & AT × Correct answers by partner) continue to be significant at the 5% level.

**Table 4A:Non-linearity of partner's performance and redistribution**

| VARIABLES | (1) Redistribution (LNT) | (2) Redistribution (LT) | (3) Redistribution (ANT) | (4) Redistribution (AT) | (5) Redistribution (ATR) |
|---|---|---|---|---|---|
| Percentage correct | -21.383 | -23.328 | -9.432 | -9.999 | -12.956 |
| | (15.789) | (17.108) | (12.196) | (15.562) | (13.070) |
| Partner correct: 2nd quartile | -0.268 | 3.034 | 2.202 | 4.621 | -0.804 |
| | (5.022) | (5.160) | (4.088) | (4.510) | (4.491) |
| Partner correct: 3rd quartile | 2.483 | 6.854 | 4.440 | 12.695*** | -2.560 |
| | (4.707) | (5.420) | (4.576) | (4.740) | (4.035) |
| Partner correct: 4th quartile | 0.230 | 14.212** | -2.462 | 10.367 | 0.848 |
| | (7.072) | (7.181) | (4.824) | (6.101) | (5.797) |
| Constant | 28.860 | 27.885 | 22.189 | 17.980 | 35.077** |
| | (21.969) | (22.019) | (19.595) | (23.165) | (17.014) |
| Demographic controls | YES | YES | YES | YES | YES |
| Observations | 182 | 173 | 234 | 192 | 212 |
| R-squared | 0.161 | 0.156 | 0.169 | 0.200 | 0.143 |

**Note:** **<5%; ***<1%. The sample consists only of the eventual winners in task 1. Heteroskedasticity-consistent standard errors (HC3) are in parentheses. Demographic controls include hotseat tenure, total number of rounds completed, age, age squared, gender, income dummy, university dummy, marital status, white dummy, cheating indicators, and altruism index.

We created dummy variables for each quartile of partner's performance by treatment group. Each column above demonstrates the relationship between each quartile of the partner's performance (calculated by treatment group) and the winner's redistribution choice. We find that for groups in LT and AT, winners whose partners scored in the fourth and third quartiles, respectively, redistribute significantly more than winners whose partners scored in the bottom quartile ($\beta$=14.212 *S.E.*=7.181 for LT; $\beta$=12.695 *S.E.*=4.740 for AT). This demonstrates

that our findings in Table 1 are driven primarily by winners in the transparency conditions whose partners performed in the higher quartiles of their cohort's performance.

**Table 5A: Winner's redistribution after task 1: OLS regressions with performance gap**

| VARIABLES | (1) |
|---|---|
| **Treatments** | |
| Luck, transparent (LT) | -1.825 |
| | (3.068) |
| Ability, not transparent (ANT) | -0.509 |
| | (2.828) |
| Ability, transparent (AT) | -0.393 |
| | (2.783) |
| Ability, transparent, redistribution (ATR) | 3.055 |
| | (2.715) |
| **Performance gap** | |
| Negative performance gap (winner – non-winner) | 4.629 |
| | (4.262) |
| Zero performance gap | -6.790 |
| | (4.423) |
| LT × Negative performance gap | 6.669 |
| | (6.093) |
| LT × Zero performance gap | 12.049 |
| | (6.793) |
| ANT × Negative performance gap | -4.287 |
| | (5.491) |
| ANT × Zero performance gap | 10.728 |
| | (6.076) |
| AT × Negative performance gap | 5.516 |
| | (6.070) |
| AT × Zero performance gap | 13.845** |
| | (6.172) |
| ATR × Negative performance gap | -0.076 |
| | (5.726) |
| ATR × Zero performance gap | 17.550*** |
| | (5.710) |
| **Control variables** | |
| Proportion of being in the 'hot seat' | -13.893*** |
| | (2.990) |
| Total number of rounds until complete the task | 0.146 |
| | (0.247) |

| | |
|---|---|
| Respondent's age | 1.595 |
| | (2.487) |
| Respondent's age squared | -0.062 |
| | (0.452) |
| Gender: Female | 2.439 |
| | (1.464) |
| Gender: Prefer not to answer | -7.447 |
| | (9.012) |
| Gender: Other | 7.426 |
| | (5.937) |
| Income: Earner $60k per annum or more | -1.196 |
| | (1.512) |
| Highest education: College degree | -1.060 |
| | (1.566) |
| Cohabitating/Domestic Partnership | 2.599 |
| | (2.449) |
| Divorced | 1.519 |
| | (3.510) |
| Married | -1.232 |
| | (1.791) |
| Separated | -1.202 |
| | (9.160) |
| Widowed | -3.735 |
| | (5.674) |
| White | -0.879 |
| | (1.572) |
| Did you cheat? (Yes, in some rounds) | 2.341 |
| | (3.582) |
| Do you think the other person cheated? (Yes, in some rounds) | -1.531 |
| | (3.054) |
| Altruism index | 0.312*** |
| | (0.083) |
| Constant | 15.372*** |
| | (5.662) |
| Observations | 993 |
| R-squared | 0.110 |

**Note:** **<5%; ***<1%. The sample consists only of the eventual winners in the task 1. Heteroskedasticity-consistent standard errors (HC3) are in parentheses.

Table 5A presents the original model from Table 1, replacing the continuous "partner correct" variable with a categorical performance gap variable, which captures whether the difference in the winner's and non-winner's performance is positive, negative, or zero. This models shows that the optimal context for maximizing redistribution is when players perform equally well. In other words, redistribution rises as the non-winner's performance improves, and peaks when the gap between winners' and non-winners' performance is at zero.

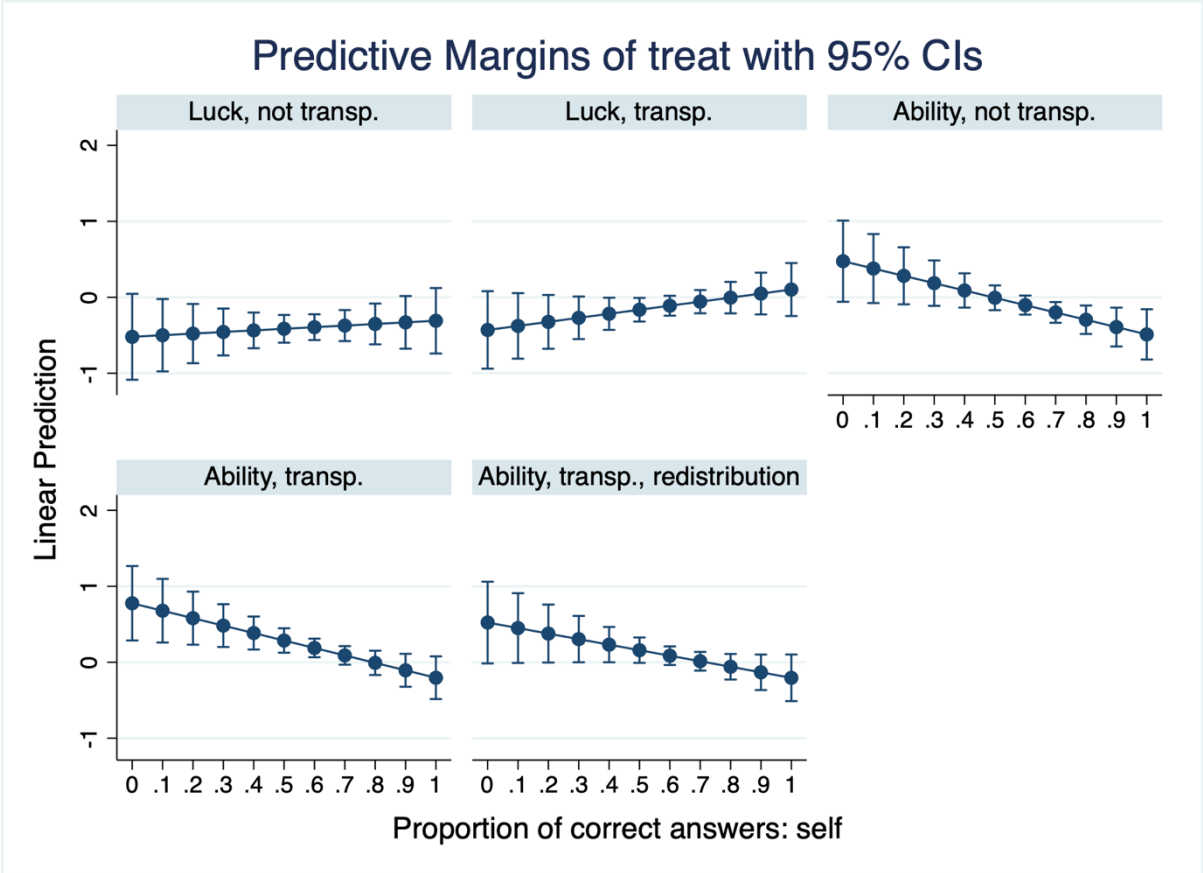
**Table 6A: Descriptive statistics of players' relative performances**

| Performance gap (winner – non-winner) | LNT (1) | LT (2) | ANT (3) | AT (4) | ATR (5) |
|---|---|---|---|---|---|
| Negative (%) | 26.4 | 27.8 | 23.5 | 20.3 | 21.2 |
| | *(26.4)* | *(27.8)* | *(23.5)* | *(20.3)* | *(21.2)* |
| Zero (%) | 14.8 | 14.5 | 19.2 | 16.7 | 16.5 |
| | *(41.2)* | *(42.2)* | *(42.7)* | *(37.0)* | *(37.7)* |
| Positive (%) | 58.8 | 57.8 | 57.3 | 63.0 | 62.3 |
| | *(100)* | *(100)* | *(100)* | *(100)* | *(100)* |
| **N** | **182** | **173** | **234** | **192** | **212** |

**Note:** The sample consists only of the eventual winners in task 1. Cumulative percentages by treatment group are in parentheses.

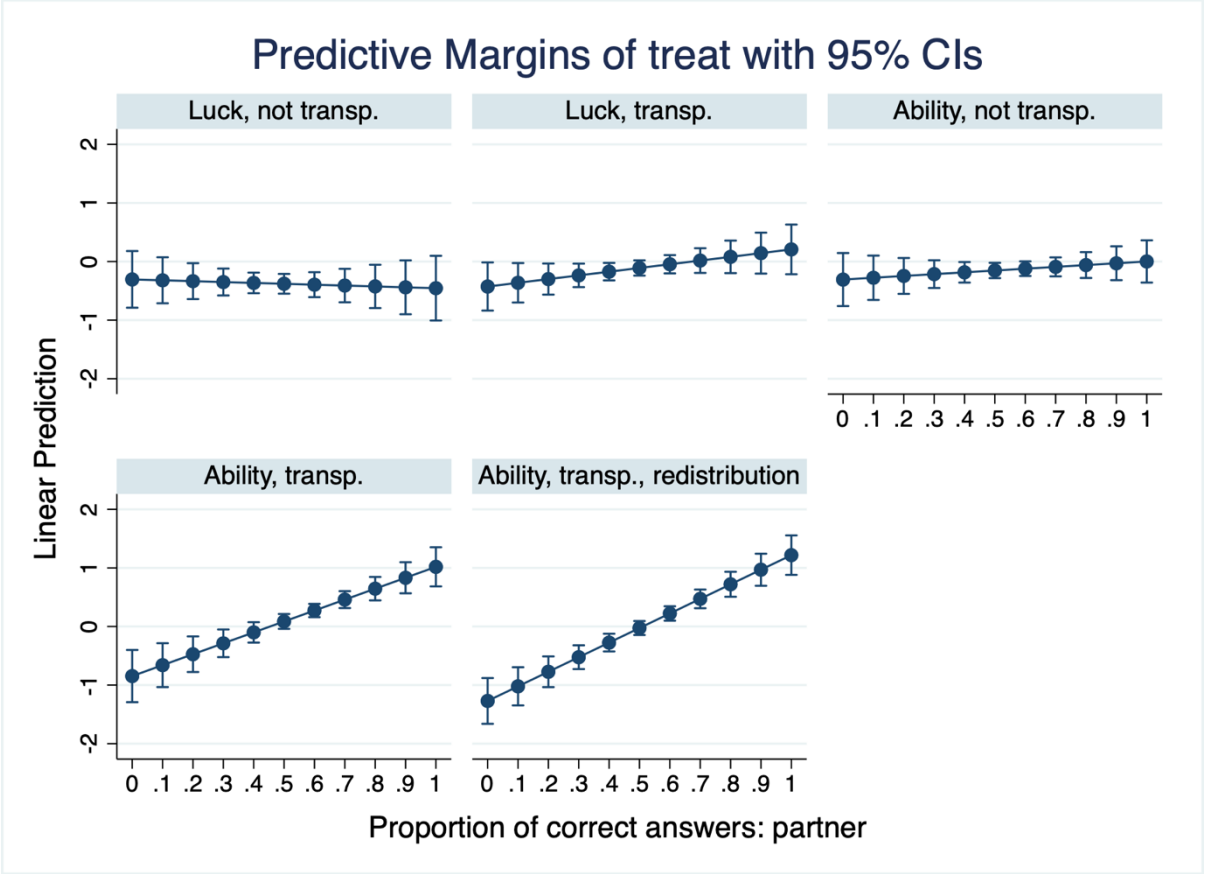
These descriptive statistics help us to understand why we do not find an average treatment effect in our main models. They show that show that around 40% of non-winners performed just as well or better than the winners, whereas around 60% of non-winners performed worse. This is true across all treatment groups. The main results in Table 1 show that receiving information about a well-performing non-winner increases redistribution in both luck and ability tasks; however, this effect does not hold when non-winners perform poorly. The variability of non-winners' performance resulted in a heterogeneity of redistribution decisions, even in transparent groups, ultimately cancelling out the redistributive effects and resulting in the null result that we find. In sum, this shows that transparency alone is not enough to increase redistributive preferences.

**Figure 4A: Marginal effects of correct answers given by self on 'F1: My opponent was skillful'**



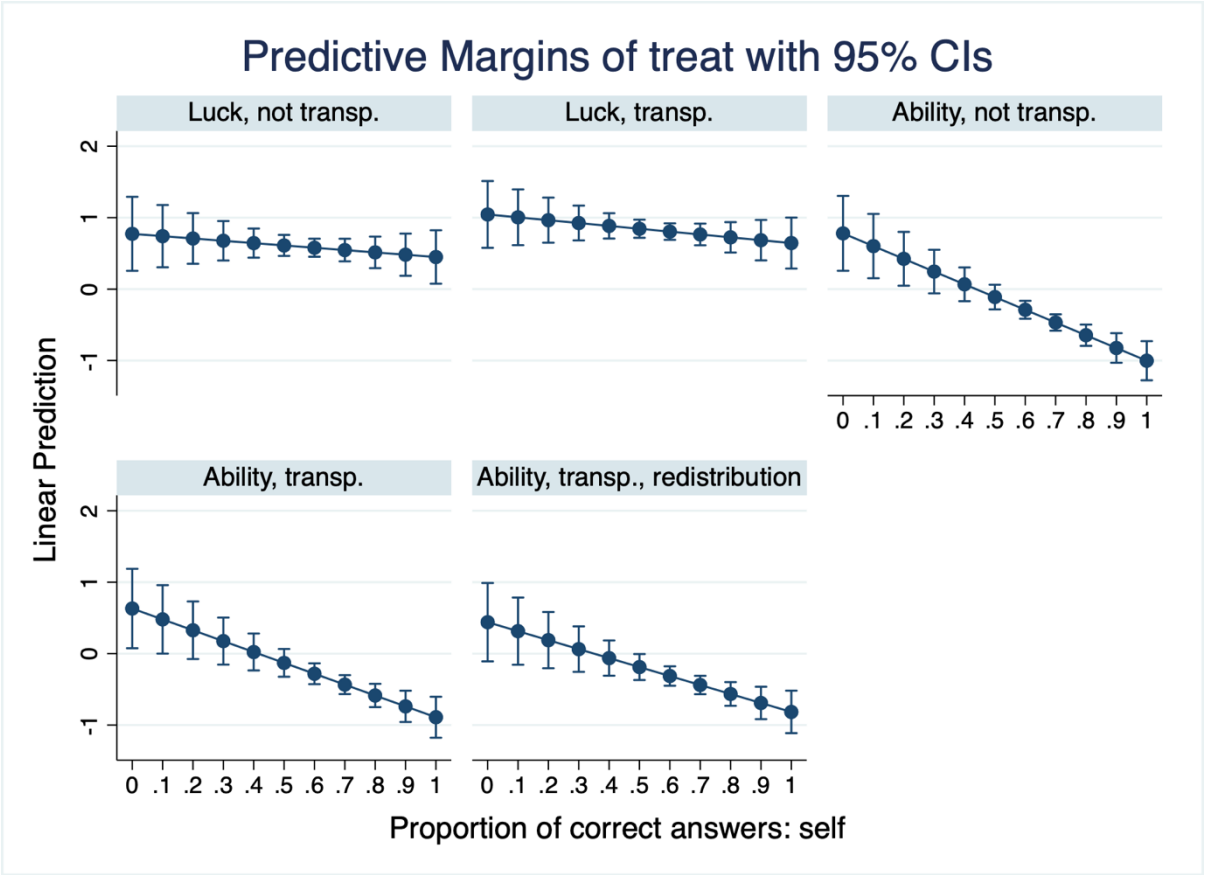

**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 4's estimates.

**Figure 5A: Marginal effects of correct answers given by partner on 'F1: My opponent was skillful'**



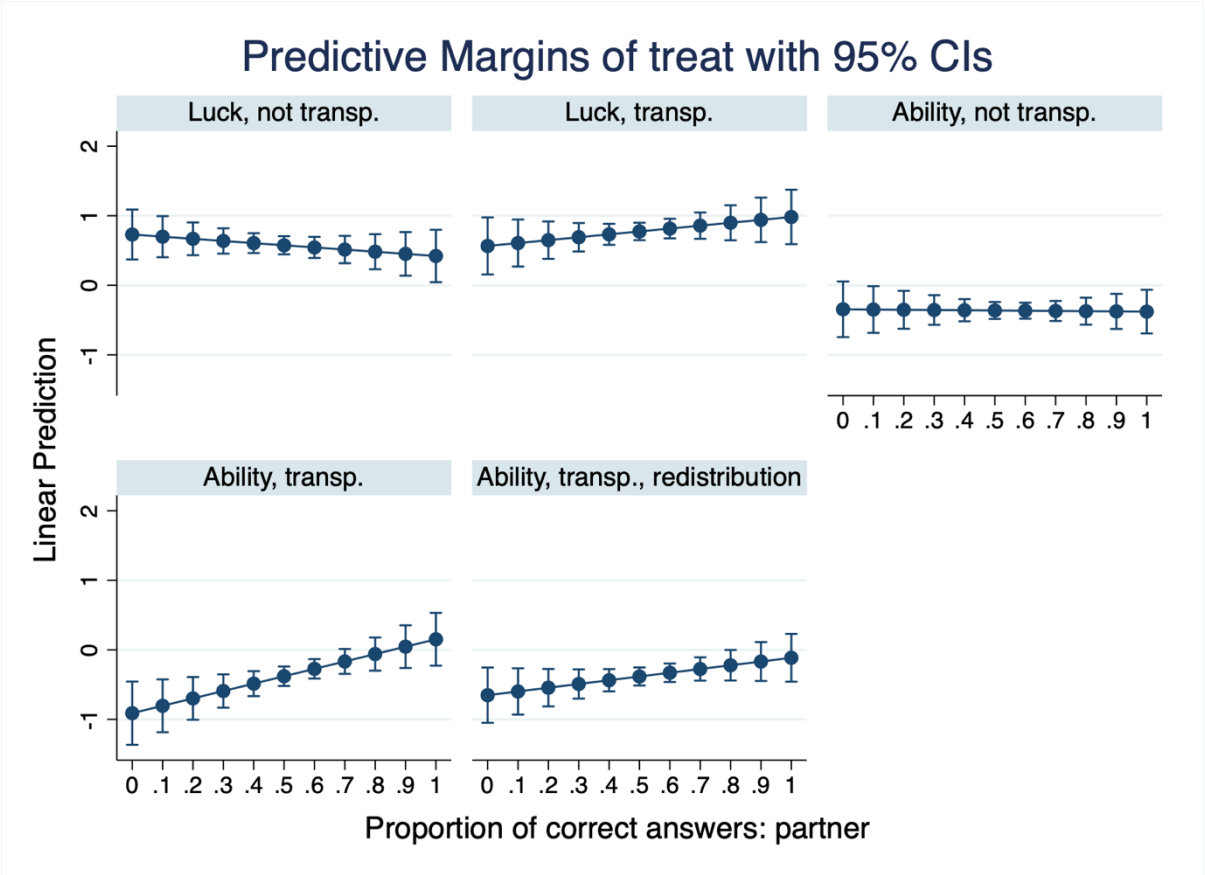

**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 4's estimates.

**Figure 6A: Marginal effects of correct answers given by self on 'F2: My winning was due to chance'**



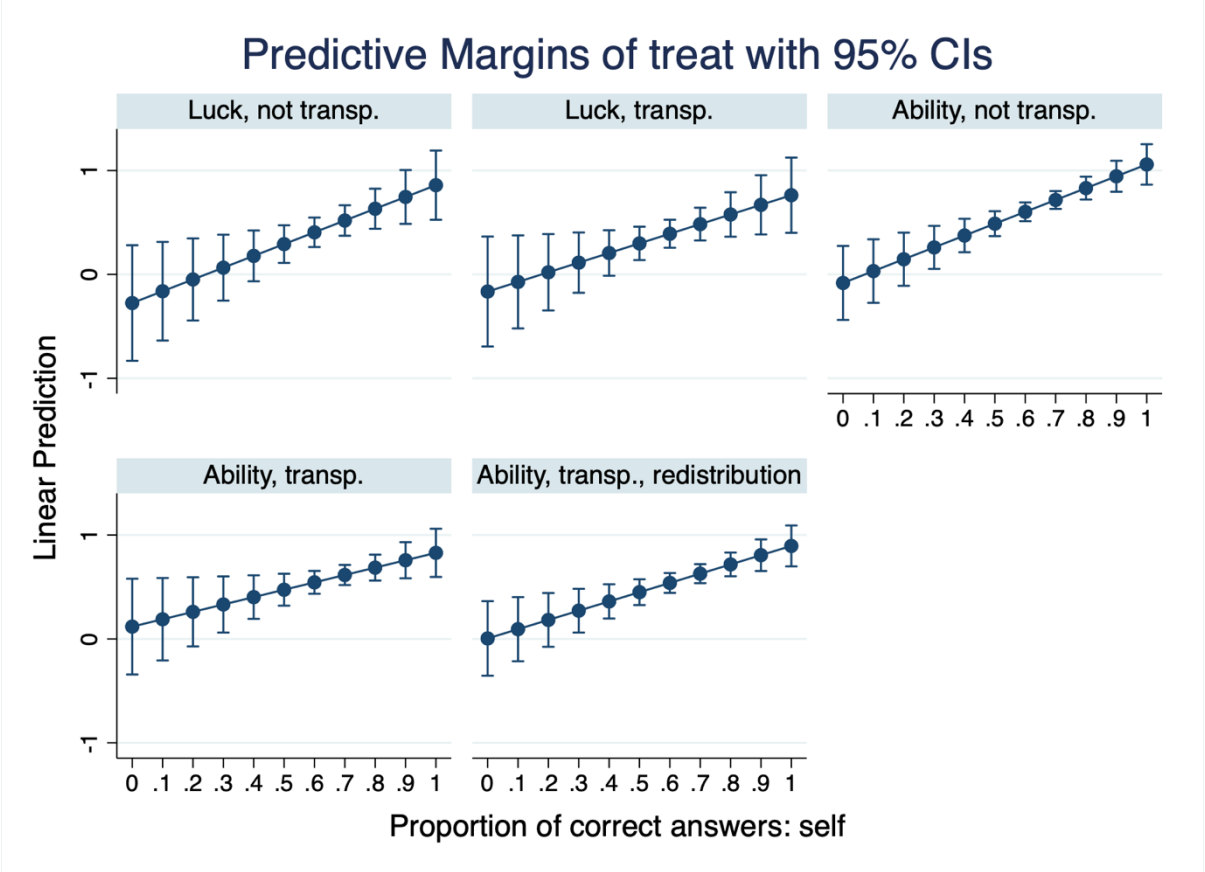

**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 4's estimates.

**Figure 7A: Marginal effects of correct answers given by partner on 'F2: My winning was due to chance'**



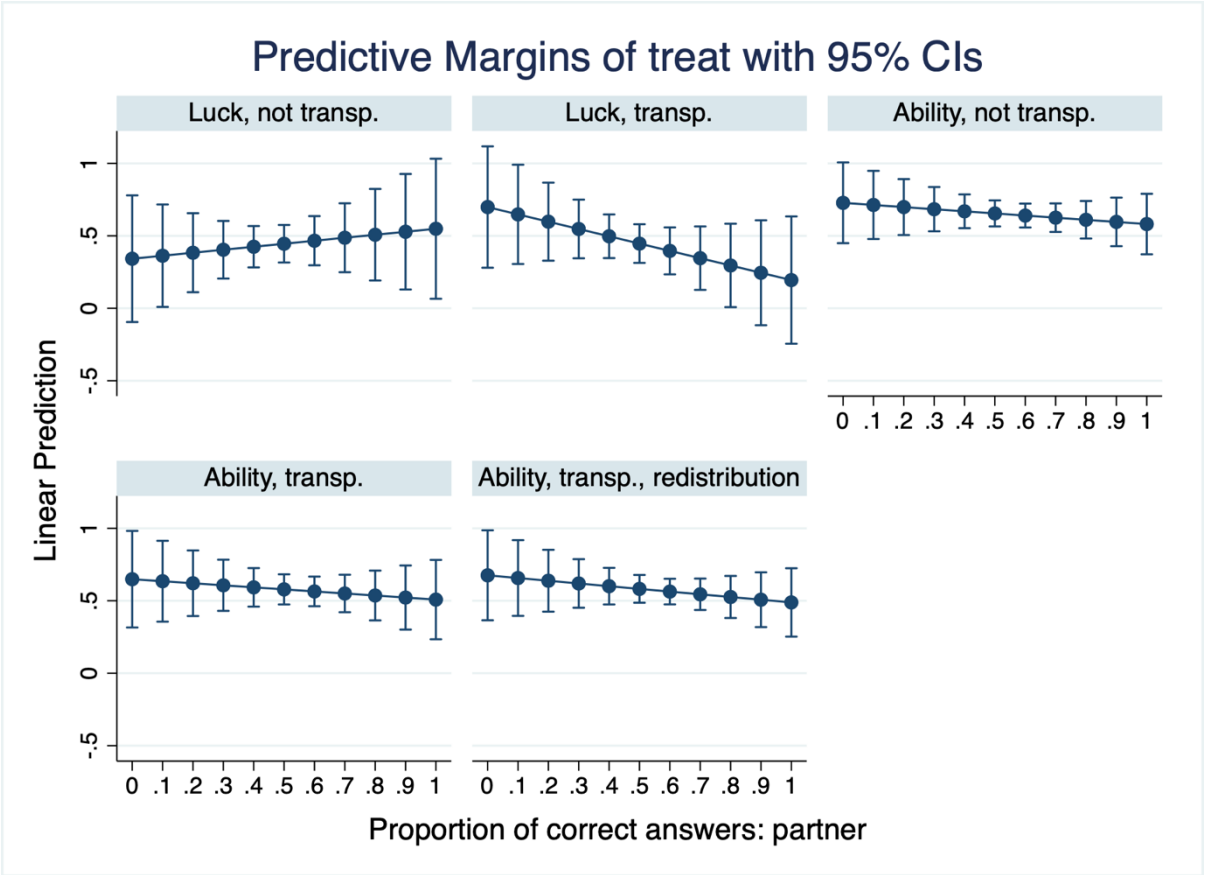

**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 4's estimates.

**Figure 8A: Marginal effects of correct answers given by self on 'F3: I completely deserved the win'**



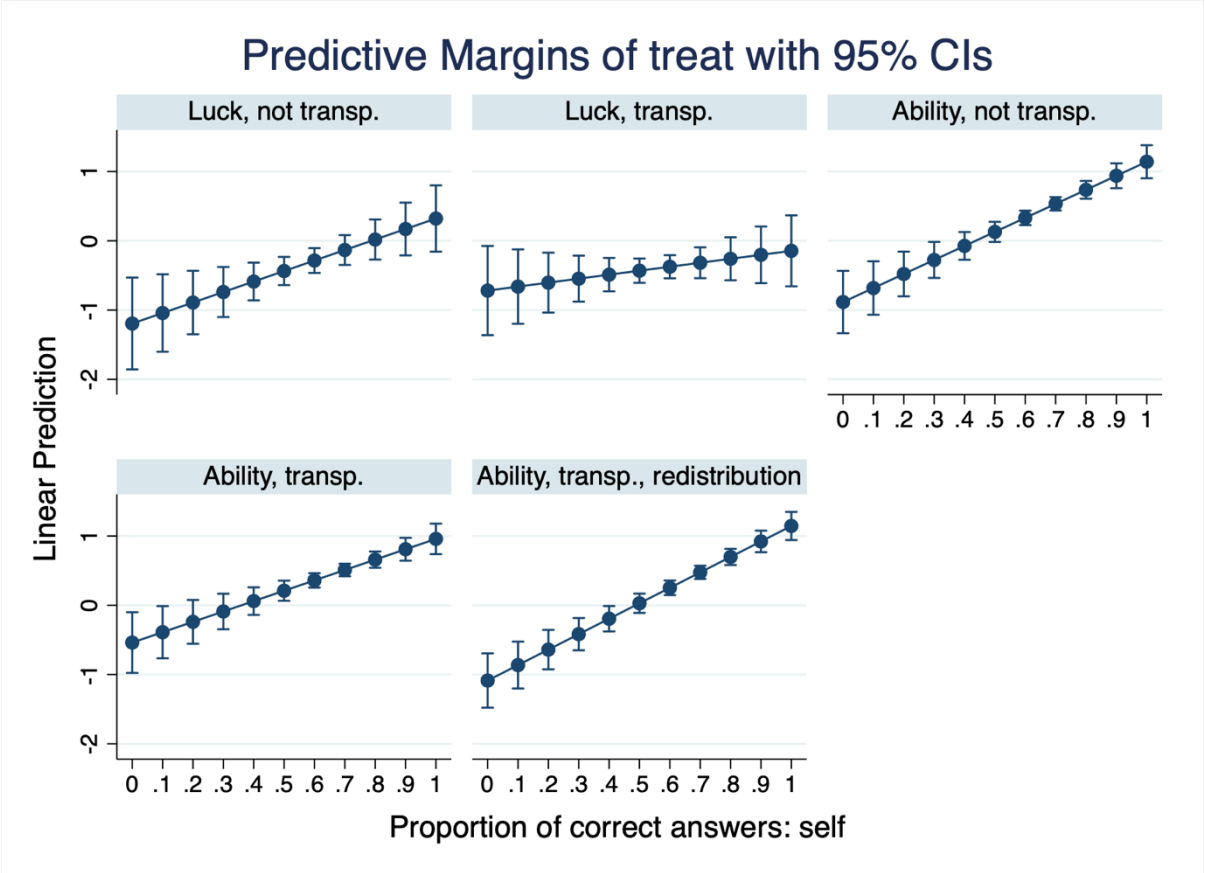

**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 4's estimates.

**Figure 9A: Marginal effects of correct answers given by partner on 'F3: I completely deserved the win'**



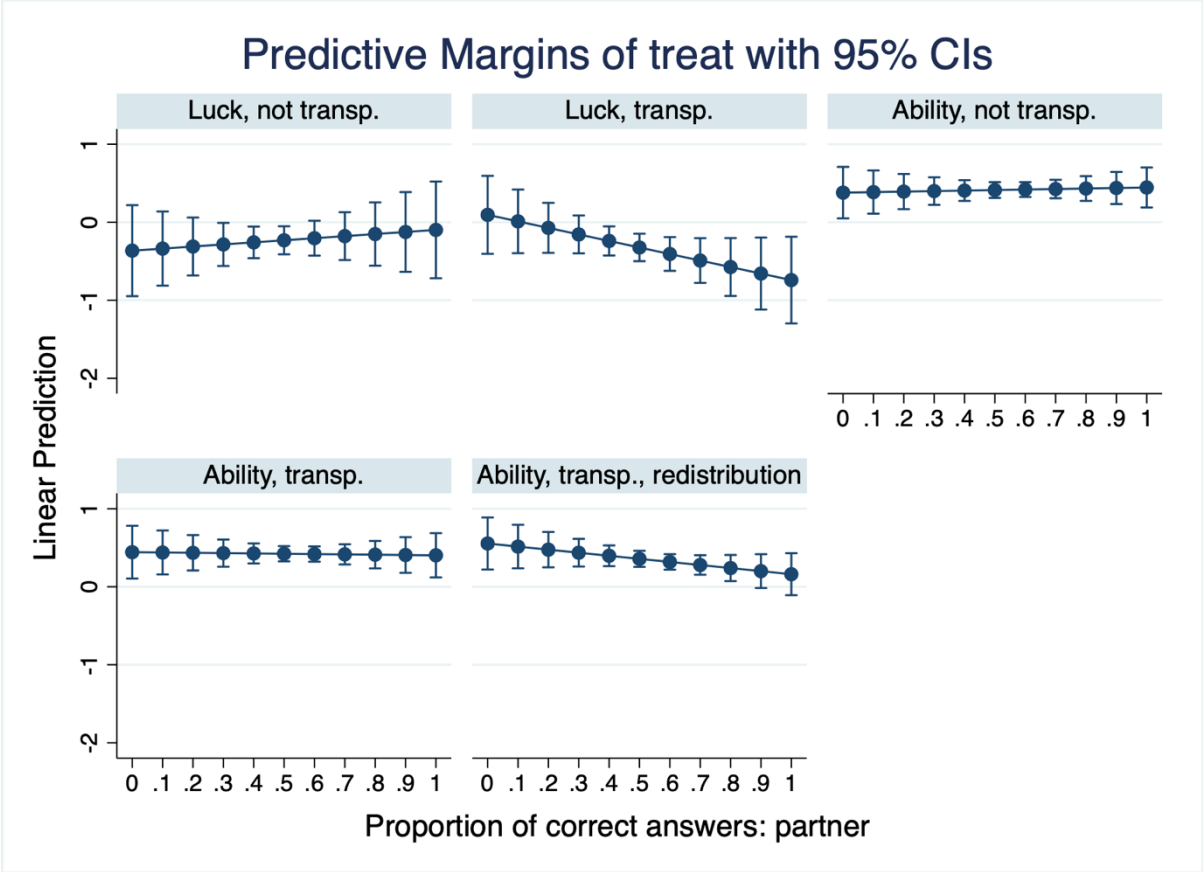

**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 4's estimates.

**Figure 10A: Marginal effects of correct answers given by self on 'F4: I put in maximum efforts/skillful'**




**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 4's estimates.

**Figure 11A: Marginal effects of correct answers given by partner on 'F4: I put in maximum efforts/skillful'**




**Note:** 95% confidence intervals are reported. The marginal effects are based on Table 4's estimates.

**Table 7A: Deservingness regressions: nonwinners sample**

| VARIABLES | (1) F1: My opponent was skillful | (2) F2: My winning was due to chance | (3) F3: I completely deserve the loss | (4) F4: I put in maximum effort/skills |
|---|---|---|---|---|
| **Treatments** | | | | |
| Luck, transparent (LT) | -0.649 | -0.250 | -0.091 | -0.025 |
| | (0.506) | (0.383) | (0.472) | (0.559) |
| Ability, not transparent (ANT) | -0.830 | -1.278*** | 0.721 | -0.365 |
| | (0.502) | (0.435) | (0.402) | (0.507) |
| Ability, transparent (AT) | -0.710 | -1.254*** | 0.690 | -0.047 |
| | (0.495) | (0.439) | (0.435) | (0.531) |
| Ability, transparent, redistribution (ATR) | -0.747 | -1.260*** | 0.551 | 0.132 |
| | (0.501) | (0.444) | (0.447) | (0.507) |
| **Proportion of correct answers** | | | | |
| Prop. of correct answers given by self | -0.403 | 0.191 | -0.595 | 0.606 |
| | (0.562) | (0.399) | (0.477) | (0.598) |
| LT × Correct answers by self | 0.489 | 0.348 | 1.093 | 1.064 |
| | (0.841) | (0.541) | (0.694) | (0.846) |
| ANT × Correct answers by self | 0.384 | 0.637 | -0.285 | 1.336** |
| | (0.647) | (0.497) | (0.551) | (0.675) |
| AT × Correct answers by self | 0.082 | 0.895 | -0.441 | 1.893*** |
| | (0.691) | (0.540) | (0.592) | (0.718) |
| ATR × Correct answers by self | -0.286 | 1.320** | -0.318 | 1.838*** |
| | (0.692) | (0.512) | (0.620) | (0.696) |
| Prop. of correct answers given by partner | -0.615 | -0.400 | 0.559 | -0.148 |
| | (0.582) | (0.454) | (0.450) | (0.571) |
| LT × Correct answers by partner | 1.086 | 0.324 | -0.724 | -0.818 |
| | (0.692) | (0.522) | (0.649) | (0.761) |
| ANT × Correct answers by partner | 2.159*** | 0.047 | -0.167 | 0.462 |
| | (0.651) | (0.549) | (0.526) | (0.619) |

| | | | | |
|---|---|---|---|---|
| AT × Correct answers by partner | 2.230*** | -0.431 | -0.068 | -0.697 |
| | (0.662) | (0.557) | (0.534) | (0.600) |
| ATR × Correct answers by partner | 2.490*** | -0.838 | 0.213 | -0.918 |
| | (0.673) | (0.572) | (0.597) | (0.617) |
| **Control variables** | Yes | Yes | Yes | Yes |
| Observations | 993 | 993 | 993 | 993 |
| R-squared | 0.198 | 0.331 | 0.118 | 0.234 |

**Note:** **<5%; ***<1%. The sample consists only of losers in task 1. Each dependent variable is a rotated factor component of deservingness, standardized to have a mean of 0 and a standard deviation of 1; See Table 1A for the rotated factor loadings. Heteroskedasticity-consistent standard errors (HC3) are in parentheses. Other control variables are as in Table 1.

**References**

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