Fair Shares and Selective Attention

Dianna Amasino
Davide Pace
Joël van der Weele

1 University of Amsterdam
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Tinbergen Institute Rotterdam
Burg. Oudlaan 50
3062 PA Rotterdam
The Netherlands
Tel.: +31(0)10 408 8900
Fair Shares and Selective Attention*

Dianna R. Amasino¹, Davide D. Pace¹,², and Joël van der Weele¹,²

¹University of Amsterdam, Amsterdam School of Economics
²Tinbergen Institute

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Abstract

Attitudes towards fairness and redistribution differ along socio-economic lines, resulting in political conflict. To understand the formation of such views and find levers to affect them, we study the role of attention. In a large online experiment, we investigate how subjects allocate their visual attention to the contributions of merit and luck in the generation of a surplus and how they decide on its division. We find that subjects who randomly obtained an advantaged position pay less attention to information about true merit and retain more of the surplus. Both the attentional and behavioral patterns persist, although with smaller effect sizes, when dictators subsequently divide money between pairs of advantaged and disadvantaged subjects in the role of a benevolent judge. Moreover, attention has a substantial causal effect: forcing subjects to look for one second more at merit information relative to overall outcomes reduces the effect of having an advantaged position on allocations by about 25%. These findings open a new window on socio-economic cleavages in attitudes towards redistribution, and suggest that attention-based policy interventions may be effective in reducing polarized views on inequality.

Keywords: Redistribution, self-serving bias, attention, mouse-tracking, fairness.

JEL codes: C91, D83, D87.

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1 Introduction

Elites often find ways to justify their economic advantage. Across countries, higher incomes correlate with stronger condemnation of “blue collar crimes” like benefit fraud and weaker condemnation of “white collar crimes” like tax evasion (Ostling, 2009). Affluent Americans are more likely than average Americans to believe that inequalities result from hard-work and intelligence rather than from luck (Suhay et al., 2020), and less likely to redistribute income than the general population (Cohn et al., 2019). The effect of economic privilege is causal: the accidental allocation of land-titles can lead to more pro-market views (Di Tella et al., 2007), and the random allocation of an economic advantage to laboratory subjects causes them to redistribute less to unfortunate peers (Konow, 2000; Deffains et al., 2016). In contrast, random shocks that worsen people’s economic situations, like sickness and disability, increase the moral appeal of equality (Hvidberg et al., 2020).

These diverging views about the origin of economic success may result in political conflict and fuel the rise of populism. For instance, Sandel (2020) maintains that resentment at the bottom of the income distribution derives from systemic advantages for elites dressed up as meritocracy. Gethin et al. (2021) empirically demonstrate a decades-long migration of low-income voters in Western countries towards right-wing populist parties and hypothesize that the embrace of education-based meritocracy by the left is responsible for the shift. Despite the importance of views on merit and redistribution to contemporary political debates, there is little empirical evidence about the formation of these views and the development of differences along socio-economic lines.

To address this gap, we investigate the role of attention as a driver of attitudes towards merit and redistribution. Attention matters since it is the filter through which people understand their environment, and may depend on an individual’s background. For instance, citizens of different socio-economic status may pay attention to news media that provide different narratives about the nature and origin of inequality. In this paper, we ask how socio-economic status shapes attention to the role of merit and luck, and how such attention affects concerns for fairness and redistribution. The answers to these questions can provide policy levers to combat bias and polarization in attitudes towards meritocracy and economic success, and help understand the competition for attention by activists and politicians.

Before describing our main investigation, we motivate our research question with survey evidence on the relation between socio-economic status and attention. In an online survey ($N = 767$), we asked respondents from different income groups to read one of two articles titled...
“Luck looms larger in success than most of us think” and “Why high earners work longer hours”. We expected that people with high socio-economic status are more reluctant to learn about the role of luck, and hence less likely to attend to the “luck-article”, as it may raise doubts about the merits of their relatively higher income. Indeed, Figure 1 shows that only 35.7% of high income participants chose to look at the luck article, compared to 56.4% of the low income participants ($\chi^2=32.09, p<0.001$). Higher income also has a strong, negative correlation with positive attitudes towards redistribution (Kendall rank correlation $\tau = -0.306, p<0.001$).

Figure 1: Choice to learn about the role of luck by income level.

Choice of article split by income level, with Low Income defined as $<\text{£10,000}$, and High Income as $>\text{£70,000}$. The Y-axis shows the percentage of participants choosing the article titled “Luck looms larger in success than most of us think” instead of the one titled: “Why high earners work longer hours”. The error bars represent 95% confidence intervals.

These results suggest an interplay between economic status, attention to merit and luck, and attitudes towards redistribution. We rigorously investigate the causal links between these variables in our main study, consisting of a series of large online experiments ($N=1500$). In a design inspired by Konow (2000), participants first produce a surplus by providing correct responses in a series of real effort tasks. In two “Status” treatments, we create “Advantaged” and “Disadvantaged” subjects by explicitly randomizing half of the subjects to a higher pay rate per correct response. Subsequently, a subset of the subjects assume the role of “dictator” and divide the surplus generated by two participants, one with Advantaged status and one

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1Details about the implementation and outcomes of the survey are in Appendix A.1.
with Disadvantaged status, in a sequence of allocation tasks. In the “Involved” condition, the dictator is one of the participants who generated the surplus, while in the subsequent “Impartial” condition, the dictator divides the surplus generated by two other participants.

Crucially, before making an allocation decision, dictators can uncover two sources of information. First, “outcome” information shows the total contribution of each participant to the surplus, combining merit (correct answers) and luck (the randomly determined pay rate) mirroring the results of a flawed meritocratic system. Second, “merit” information shows the number of correct answers of both participants, thus providing a measure of performance net of the aleatory pay rate. We measure the visual attention to these two sources with the tool MouselabWEB, tracking how each subject moves their mouse over the screen to uncover different types of information (Willemsen and Johnson, 2019). orthogonal to the Status treatments, we implement three “Focus” treatments. In the “Free Focus” treatment, participants face no restrictions on their attention. In contrast, the “Merit Focus” and “Outcome Focus” treatments impose restrictions on the time that can be spent looking at different types of information, encouraging participants to pay more attention to the focus information (merit or outcome).

Our results show strong evidence of self-serving bias: compared to Disadvantaged dictators, Advantaged ones keep a larger share of the pie in the Involved condition, where the dictator’s own income is at stake, and allocate more to other Advantaged recipients in the Impartial trials, where allocation involves two other participants and dictator’s own income is not at stake. Thus, economic status changes what subjects consider as a fair allocation, replicating results from Konow (2000).

We then turn to our main interest: the role of attention. First, we find evidence for selective attention: compared to Disadvantaged dictators, Advantaged ones pay relatively more attention to outcome information, which incorporates the random differences in pay rate that favor the Advantaged participants. By contrast, Disadvantaged dictators pay more attention to merit information, which is based on performance only. This pattern arises over multiple trials in the Involved decisions and persists in subsequent Impartial decisions.

Second, and perhaps most importantly, we find that attention plays a causal role in redistribution decisions: The Outcome Focus treatment, which encourages people to look longer at contributions that include the luck component, increases the share of the pie going to Advantaged recipients compared to the Merit Focus treatment. This effect of attention is particularly pronounced among Advantaged dictators. The effect of attention is substantial: making dictators look one second longer at merit versus outcome information (that is, redirecting, about a quarter
of average dwell time), reduces the impact of having an advantaged position on allocations by more than 25% when dictators own income is at stake.

Relative to previous literature on redistributive attitudes, which we survey in more detail below, our focus on attention allows us to study the cognitive underpinnings of polarization and self-serving bias. We show that attention plays a causal role in redistribution and fairness decisions, and attention-based interventions are effective as a lever to influence such decisions. This opens a new window on socio-economic cleavages in attitudes towards meritocracy and redistribution, and provides a starting point for interventions to reduce bias, not just in redistributive decisions, but also in other domains where discrimination of disadvantaged groups plays a role.

2 Literature Review

Our research relates to a large behavioral literature on the role of merit in redistribution. A number of laboratory experiments vary the source of inequality and consistently show that this impacts perceptions of fairness. In particular, participants in experiments are more willing to redress inequalities based on luck rather than merit (Krawczyk 2010; Cappelen et al. 2013; Durante et al. 2014; Lefgren et al. 2016; Cappelen et al. 2017; Bortolotti et al. 2017; Buser et al. 2020). Almás et al. (2020) have shown that this tendency is robust across countries, even if there are differences in the overall tendency to redistribute. Piff et al. (2020) show that priming people with situational rather than dispositional attributions for poverty causes an increase in egalitarianism. We add to these insights by showing that allocations depend on the attention paid to information about the role of merit and luck. In particular, we show that pushing people to focus attention on the merit dimension affects their allocation of an economic surplus.

Perceptions of fairness also depend on socio-economic status. In particular, the seminal paper by Konow (2000), which is the inspiration for our design, identifies a self-serving bias exhibited by players with a randomly-assigned advantage who give more to themselves and also to other advantaged players, even when their own income is not at stake. Rodriguez-Lara and Moreno-Garrido (2012) and Deffains et al. (2016) use similar designs and replicate these main results. Espinosa et al. (2020) show that the bias is robust to ex-post information provision highlighting the role of luck in the formation of inequality. Several papers, cited in the introductory paragraph, demonstrate self-serving bias outside the laboratory; other forms of self-serving bias have been found in a wide range of domains (Bénabou and Tirole 2016). While this literature demonstrates the importance and self-serving nature of fairness views, it has treated the formation of such beliefs largely as a black box. This limits the possibility to
address such biases with policy instruments. Our paper opens the box by focusing on the role of attention to merit and luck and its relation to the economic status of participants.

With our focus on attention, we contribute to a small but growing literature on the role of attention in economic decisions. Theoretically, Gossner et al. (2020) shows that manipulating attention can change subjects’ decisions even if the manipulation does not change neither preferences nor the information available. More generally, there is an increasing interest in theory about the role of “process” variables in decision-making, such as decision time (Fudenberg et al., 2018). While previous research that we review below examines attention to payoff options in allocation decisions, our study examines attention to the inputs generating the surplus to be allocated, going one step earlier in the decision process.

Empirically, several recent papers use attention-tracking techniques to investigate pro-social decisions. Fiedler et al. (2013) show correlations between eye movements and social preferences in social allocation problems. These correlations are replicated in mouselabWEB by Bieleke et al. (2020). Further, participants adjust their gaze to appear prosocial or take others payoffs more into account in strategic settings where their payoffs depend on others’ decisions (Fischbacher et al., 2020). Jiang et al. (2016) investigate eye-movements and decisions in three-person distribution experiments, and show that the eye-movements correspond to the use of particular choice rules, like the minimization of envy. Replicating and extending Pärnamets et al. (2015), Ghaffari and Fiedler (2018) manipulate attention to payoffs in a social allocation problem by interrupting the decision-making process after subjects look at a certain option for a pre-determined amount of time. This exogenous variation can explain about 11% of the variation in visual attention and about 1% of changes in choice. Fiedler and Hillenbrand (2020) show that loss framing increases attention to one’s own payoff and reduces altruistic choice compared to gain framing only when the subject is in an advantaged position with a higher starting allocation. Attention has also been measured to understand in-group allocation biases, showing that attention to group information enhances biases (Rahal et al., 2020; Fischbacher et al., 2021).

These empirical studies, and all attention-tracing studies in this domain that we are aware of, measure attention to the payoffs in an economic game. By contrast, we study attention to the determinants of economic production and show how this affects distributive decisions. Thus, our analysis helps in understanding the role of attention in self-serving biases and fairness views. In this way, our paper relates closely to Waldfogel et al. (2021), one of the few studies on attention towards economic inequality. They show that political ideology affects whether people detect inequalities in everyday situations, whereas we focus on the determinants of inequality.
3 Design

In this paper we report the results of two experiments. Experiment 1 aims a) to replicate previous findings on the relationship between economic status and attitudes toward redistribution and b) to establish a causal relationship between economic status and attention. Experiment 2 allows us c) to investigate the causal relationship between attention and attitudes towards redistribution. Experiment 1 generated the data for the Free Focus treatments, while Experiment 2 generated the data for the Constrained Focus treatments outlined in Table 1.

Each experiment happened over 2 days: on Day 1, participants completed real effort tasks to generate a surplus, and on Day 2, participants in the role of dictators divided the surplus. Figure 2 displays the timeline shared by the two experiments. For Experiment 1, we recruited 200 dictators and 300 recipients from Prolific.co. The data was collected between the 13th and 19th of July, 2020. For Experiment 2, we recruited 400 dictators and 600 recipients from Prolific.co. The data was collected between the 23rd and 30th of November, 2020. Across both experiments, we paid a completion fee of £2.85 for Day 1 and £6.15 for Day 2 plus an average bonus of around £3 per participant.

3.1 Day 1: Surplus Generation

On Day 1, participants completed 8 real effort tasks. There were 4 different types of tasks: moving sliders to a predetermined position, logic questions, counting the number of zeros in a table, and solving Raven’s matrices. Each type of task was repeated twice. In every task, each correct answer earned a monetary reward. When completing the tasks, participants did not know the exact monetary reward they would receive. However, they knew that they would randomly be assigned a high or low pay rate per correct answer, the amount of both pay rates, and that they would learn which pay-rate applied to them at a later stage. The high pay rate was always 3 times the low pay rate, but pay rates were calibrated (based on pilot data) according to task type to result in an average surplus of £3.5 per task.

Similarly, participants were aware that the assignment to a high or low pay rate would apply to all of their tasks. We checked participants understanding of the randomness and persistence of the pay-rates with two comprehension questions. Participants were also informed that they would be paired with other participants and their earnings would go into a single common account but did not know how this would be divided.

\footnote{We recruited more recipients than dictators because in the Impartial trials the dictators split the amount generated by two recipients}
3.2 Day 2: Surplus Division

After the Day 1 surplus generation was complete, we split participants into dictator and recipient roles. Only the dictators were invited to Day 2, which started one day after Day 1. Day 2 was divided into 3 parts. In part 1, dictators split earnings between themselves and recipients, termed “Involved” allocations. In part 2, they split earnings between pairs of recipients, termed “Impartial” allocations. In part 3, they answered questions about their strategies, beliefs, and perceptions of norms.

At the beginning of Day 2, dictators learned their pay-rate per correct answer. We call participants who received the high pay rate “Advantaged” and those with the low pay rate “Disadvantaged,” and we refer to this difference as the “Privilege Status” or “Status” treatment. Participants then received instructions for the Involved allocation task. The joint earnings of a pair in a task were merged into a common account, and the dictator chose how to allocate this common account between themselves and the paired recipient. Over 20 trials, the dictators were matched with different recipients, with one of the 8 tasks underlying the common account in each of the trials. All recipients were assigned the opposite pay rate of the dictator, thus
implementing inequality in the pair. During each trial, dictators received information about how the common account was generated (detailed in the next section) and made their allocation decisions.

In the next part of Day 2, dictators made Impartial allocation decisions for two recipients. Just as in the Involved allocations, the Impartial allocations always included one Advantaged and one Disadvantaged recipient. Over 20 trials, dictators chose how the divide the common account produced by pairs of different recipients. Participants always completed the Involved trials before the Impartial trials in order to test whether self-serving biases developed in Involved decisions persisted into Impartial decisions, as in Konow (2000).

Decisions were incentivized by implementing one of each dictator’s 40 decisions. The average surplus per pair of participants in each task was £6.99 in Experiment 1 and £7.10 in Experiment 2. These amounts are approximately 1.4 times the minimum hourly wage on Prolific, so the allocation decisions had reasonably high stakes. If the decision came from the Involved allocations, the dictator received a bonus payment equal to the amount they kept for themselves, and the recipient received the amount allocated to them. If the decision came from the Impartial allocations, the dictator received £1 and each of the two recipients received what the dictator allocated them.

3.3 Attention and Focus Treatments

Before every decision, the dictators could look at information about the way the money in the common account was generated, as illustrated in Figure 3. First, dictators could see the amount of money in the common account and the type of task that produced it. All 8 tasks were used approximately equally across the 40 trials. Dictators could spend as much time as they wanted on this screen. Next, dictators had 6 seconds during which they could reveal information about the number of correct questions each participant answered in the task - merit information - and the monetary contribution of each member of the pair to the account - outcome information. This information was divided in four boxes labelled with participant and information type. All boxes were initially closed, but participants could open a box by hovering over it with their mouse cursor. Only one box could be opened at any time: when the cursor moved away, the box closed again. This was implemented with MouselabWEB which also allowed us to easily record the number of times each box was open and the amount of time the dictators spent on each decision.

3We pre-assigned which type of trial (involved or impartial) would be relevant for payment, and which recipients would get the bonus to ensure that all dictators and recipients were paid a bonus based on a single allocation decision. Recipients could appear in multiple different dictators’ allocation decisions.
box (Willemsen and Johnson, 2019). When the time limit was reached, the page automatically updated to the allocation screen where participants decided how to split the money using a slider.

**Figure 3: Information sequence**

The image shows the sequence of information during allocation decisions. First, participants saw the amount in the common account and the task that generated the surplus. Next, they had 6 seconds to reveal merit and outcome information by hovering over the boxes with their cursor: The closed green boxes indicate the type of information, and opened boxes are grey with the values inside. Finally, participants made allocation decisions.

We implemented three “Focus” treatments that varied the time different types of information could be accessed. In Experiment 1, which implemented the “Free Focus” treatment, there was no limit on the number of times a box could be reopened or for how long it could be opened within the overall 6 s time limit. In Experiment 2, with our exogenous attention manipulation, there were additional constraints on the time participants could see the information, building on prior work that has manipulated attention with a slightly different approach (Pachur et al., 2018; Pärnamets et al., 2015; Ghaffari and Fiedler, 2018). These restrictions were designed to shift dwell times on the different types of information, without making any information unavailable and preventing implementation of any particular decision criterion. In the discussion section, we show evidence that this strategy was successful.

In every trial, two of the four boxes could be opened for no more than 400 ms each. The other two boxes could be opened for no more than 1600 ms each. Boxes could still be opened

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4These time restrictions allow for a maximum of 4 s spent on box information which was chosen to closely
multiple times within the 6 s time limit, each time counting against the individual box time limit. Participants with these constraints were informed that some boxes might close permanently before the 6 s was over, but they were not informed which boxes would close.

In the “Merit Focus” treatment, 14 of the 20 decisions restricted outcome information to 400 ms and merit information to 1600 ms. This made outcome relatively hard to access and merit relatively easier, pushing participants to look more at merit. In the remaining six trials, the 400 ms restrictions were placed either on merit information (2 decisions), Advantaged member information (2 decisions), or Disadvantaged member information (2 decisions)\textsuperscript{5}. In contrast, in the “Outcome Focus” treatment, 14 trials restricted merit information to 400 ms and outcome information to 1600 ms, pushing participants to reveal outcome information more. Again, the remaining 6 trials split the 400 ms restrictions evenly between the other information dimensions. Across Involved and Impartial trials and Focus treatments, the order in which the trials with different restrictions appeared were randomized at the individual level.

The Focus treatment assignment was orthogonal to the Advantaged/Disadvantaged status, giving a $3 \times 2$ design with 6 between-subject treatments and 100 dictators in each treatment cell. Table 1 gives a summary overview of all our treatments.

Table 1: Overview of treatments and number of dictators

<table>
<thead>
<tr>
<th>Privilege Status</th>
<th>Attention</th>
<th>Free Focus</th>
<th>Merit Focus</th>
<th>Outcome Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td>Advantaged</td>
<td>100</td>
<td>100</td>
<td>100</td>
<td></td>
</tr>
<tr>
<td>Disadvantaged</td>
<td>100</td>
<td>100</td>
<td>100</td>
<td></td>
</tr>
</tbody>
</table>

Overview of the treatments in our $3 \times 2$ design. The data for the Free Focus treatments comes from Experiment 1. The data from the Merit and Outcome Focus treatments come from Experiment 2. The numbers in the cells indicate the number of dictators per treatment.

3.4 Surveys

After both experiments, we asked dictators a series of questions about their strategy, their perceptions of various fairness criteria, and their demographics. We asked participants an open-ended question about how they chose to make their allocations. We also asked them to rate

\textsuperscript{5}These remaining trials were implemented to obfuscate the attention manipulation: to limit experimenter demand through inferences that one type of information was more important and about the research goals more generally. This strategy seems to have worked: in a final questionnaire, no participant indicated that the box timing was a purpose of the experiment, and 66% of participants did not detect a systematic difference in box closing times (25.5% guessed at least partially correctly and 8.5% guessed wrongly).
the moral permissibility of dividing according to egalitarian (equal split), meritocratic (effort-based), and libertarian (maintain differences due to effort and luck) criteria, as well as the social norms related to these criteria using the method in [Krupka and Weber (2013)]. Next, we asked them how they thought others would rate these different criteria, overall, and depending on the other’s Dis(Advantaged) status. Participants could earn a bonus of £1 for correctly predicting others' answers. We also asked for gender, country, political leaning, education, and income level. In Experiment 2, we additionally elicited incentivized beliefs about some aspects of other participants’ performance using the same [Krupka and Weber (2013)] method and £1 bonus for correct prediction as for social norms.

4 Hypotheses

Our overall aim is to characterize the role of attention in redistributive decisions and self-serving bias, induced by our Privilege Status treatment. To do so, we identify three causal relationships, depicted in Figure 4 which drive our research questions and hypotheses. We preregistered these hypotheses on Aspredicted.org in two separate files, one for each experiment, which are included in Appendix B.

Figure 4: Framework for the Experimental Design and Hypotheses.

The first relationship concerns the impact of status on attention. Following a literature on motivated reasoning (e.g. [Kunda 1990] [Bénabou and Tirole 2016]), we expect that dictators in the Involved conditions need a justification for transferring a larger amount to themselves. Selective attention is employed in the search for such justifications. Independently of their performance in the tasks, Advantaged dictators benefit more from looking at and dividing according
to outcome information that incorporates their random advantage in pay-rate. In contrast, Dis-
advantaged dictators may find more justifications in ignoring the luck component in outcome 
and focusing on effort-based merit information. Thus, motivated or selective attention leads to 
the following hypothesis.

**Hypothesis 1** (Status and Attention). *In the Involved condition, Advantaged dictators spend 
relatively less time on correct answer information and more time on monetary contribution 
information than Disadvantaged dictators.*

The second relationship involves status and behavior. To understand whether self-serving 
biases affect fairness decisions, we try to replicate the effects documented by Konow (2000) and 
follow-up studies Rodriguez-Lara and Moreno-Garrido (2012).

**Hypothesis 2** (Status and Allocations). *In the Involved condition, Advantaged dictators give 
less money to the recipients, and more money to themselves, than Disadvantaged dictators.*

The third and main insight relates to the causal role of attention on behavior, which we 
address using our attention manipulations in Experiment 2. Since a large literature has shown 
that merit matters in fairness decisions, we expect that increasing the focus on merit relative to 
outcome will lead to a reduction in giving to Advantaged participants.

**Hypothesis 3** (Attention and Allocations). *In the Involved condition, increased attention to 
merit in the Merit Focus condition leads to a reduction in giving to Advantaged recipients com-
pared to the Outcome Focus manipulation.*

Our Constrained Focus treatments will allow us to estimate the importance of attention as 
a mediating variable in the development of bias, by separating the causal attentional effect from 
other drivers.

Finally, we investigate how much the effects persist in Impartial allocations, where dictators 
decide between two recipients, and hence their self-interest is not at stake. This is a measure of 
how much subjects internalized the fairness criteria they formed during the Involved stage.

**Hypothesis 4** (Persistence). *The patterns in Hypothesis 1, 2 and 3 continue to hold in the 
Impartial trials.*

Following our preregistration, we test all our hypotheses with rank-sum tests, based on 
the average of individual decisions over all rounds, thus eliminating concerns of dependence 
of observations. In addition, we use linear regressions controlling for subject characteristics, 
clustering standard errors by individual.
Attention measures. We measure attention as the dwell time on the two different types of information: merit and outcome information. Dwell time is the focus of most of the literature on visual attention. As a measure of selective attention, we use the difference between these two dwell times, which we will shorthand with “ΔAttention”, i.e.

\[ \Delta \text{Attention} := \text{Dwell time on merit information} - \text{Dwell time on outcome information}, \]

where each variable is measured in seconds. To calculate the dwell time on merit (outcome) information, we simply sum up the dwell time on the merit (outcome) for both contributors to the surplus, as the comparison is necessary to make an informed comparison.

In keeping with the literature, we disregard dwell times when a box is opened for less than 200 ms, as this is considered too short to process any information (Willemsen and Johnson 2019; Pachur et al. 2018). In our main specifications, we will not control for the total dwell time of individuals, which is an endogenous regressor that could bias the estimated effect sizes. In any case, in Appendix A.3 we show that our main regression results are robust to the inclusion of this control. All our statistical tests are two-sided, even though our preregistered hypotheses are directional and therefore would have justified a one-sided test.

5 Results

We first characterize overall behavior in both experiments to evaluate the comparability of the experiments and the engagement of the participants with the merit and outcome information. In the Session 1 production phase, participants exhibited similar performance across Experiment 1 and Experiment 2. On average, participants achieved 13 correct answers per task in Experiment 1 and 13.5 in Experiment 2, suggesting that participants put effort in completing the tasks in both experiments.

Table 2 summarizes the means of the most important outcome variables.

We analyze these data in detail below, but a number of additional observations are noteworthy. First, Impartial allocation decisions did not differ drastically from Involved decisions; the share of the surplus given to Advantaged members averaged over both dictator types was 56% for Involved allocations and 54% for Impartial allocations in Experiment 1 and 55% for Involved allocations and 54% for Impartial allocations in Experiment 1 and 55% for Involved allocations and 54%.

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6 Each treatment should have 2000 observations, but fewer than 1.5% of observations were not recorded, leading to the varying number of observations. Because the study was conducted online, it is not clear whether these observations were dropped due to an issue with our online database or with participants’ computers. However, given the number of non-recordings is low and spread across treatments and participants, it is unlikely to affect our results.
Table 2: Summary Statistics

**Panel A: Involved Trials**

<table>
<thead>
<tr>
<th>Allocation</th>
<th>Free Focus</th>
<th>Merit Focus</th>
<th>Outcome Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td>% given to Adv.</td>
<td>61.5%</td>
<td>50.4%</td>
<td>59.1%</td>
</tr>
<tr>
<td>% given to self</td>
<td>61.5%</td>
<td>49.6%</td>
<td>59.1%</td>
</tr>
<tr>
<td>Attention</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Merit Info (s)</td>
<td>1.68</td>
<td>1.81</td>
<td>1.41</td>
</tr>
<tr>
<td>Outcome Info (s)</td>
<td>2.12</td>
<td>1.90</td>
<td>0.91</td>
</tr>
<tr>
<td>Δ Attention (s)</td>
<td>-0.44</td>
<td>-0.093</td>
<td>0.50</td>
</tr>
</tbody>
</table>

**Panel B: Impartial Trials**

<table>
<thead>
<tr>
<th>Allocation</th>
<th>Free Focus</th>
<th>Merit Focus</th>
<th>Outcome Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td>% given to Adv.</td>
<td>56.3%</td>
<td>52.0%</td>
<td>54.4%</td>
</tr>
<tr>
<td>Attention</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Merit Info (s)</td>
<td>1.96</td>
<td>2.08</td>
<td>1.52</td>
</tr>
<tr>
<td>Outcome Info (s)</td>
<td>1.90</td>
<td>1.57</td>
<td>0.79</td>
</tr>
<tr>
<td>Δ Attention (s)</td>
<td>0.07</td>
<td>0.51</td>
<td>0.73</td>
</tr>
</tbody>
</table>

for Impartial allocations in Experiment 2. Fewer than 5% of dictators kept the entire surplus for themselves, in accordance with previous findings that dictator games with production have very few completely selfish choices.\[Cappelen et al., 2010\] [Rodriguez-Lara and Moreno-Garrido, 2012].

Second, participants engaged with the provided merit and outcome information before making their allocations. In the Free Focus treatment, they spent on average 3.8 seconds of the available 6 seconds revealing information in both Involved and Impartial decisions. Furthermore, pooling across Involved and Impartial decisions, information-seeking was equally distributed between information about correct answers (merit) and monetary contribution (outcome).\[^7\] In Experiment 2, where certain types of information were restricted, participants spent on average...
2.3 seconds revealing information in the Involved decisions and 2.1 seconds in the Impartial decisions, also approximately evenly distributed among merit and outcome information pooling across decision types. This is a relatively large reduction in the time spent revealing information compared to endogenous attention in Experiment 1, likely due to the time limits. Nevertheless, even under these restrictions, participants still engaged with the information during both Involved and Impartial decisions.

Third, Table 2 shows that the attention manipulation in Experiment 2 actually shifted attention (see also Figure 5 below). Overall, participants in the Merit Focus treatment spent 60% of the time looking at merit information, whereas those in the Outcome Focus treatment spent only 43% of the time looking at merit information. Both Focus treatments shifted attention away from the endogenous baseline of 47% of time spent on merit information. The difference in the distributions of relative dwell times between Merit and Outcome Focus is statistically significant ($p < 0.001$, rank-sum test). We find similar effects of our manipulation on attention in both Involved and Impartial trials.

### 5.1 Status and Attention

**Involved Trials.** To test Hypothesis 1, we investigate whether Privilege Status influences the way in which participants engaged with merit and outcome information in the Involved allocation decisions. The Free Focus treatment offers the best test for this influence because this treatment imposed no restrictions on attention that could interfere with subjects’ endogenous attention patterns. Figure 5 shows an overview of $\Delta$ Attention, the difference between dwell time on merit and outcome information, in each treatment in the Involved trials (left panel) and Impartial trials (right panel). Positive values of $\Delta$ Attention indicate that participants looked longer at merit information, whereas negative values indicate that they looked longer at outcome information. In the Involved trials, Advantaged dictators spent longer on outcome information than Disadvantaged dictators in the Free Focus treatment, resulting in a more negative $\Delta$ Attention. A two-sided Wilcoxon rank-sum test of average individual dwell times over the 20 trials confirms that distribution of attention is different for the two groups in the Free Focus treatments ($p = 0.011$). Furthermore, the left panel of Figure 5 makes clear that in the Free Focus treatment Involved trials, differences arose gradually over time.$^8$ The gradual increase of the difference in attention happens as participants spend less time looking at information over the course of multiple trials ($p < 0.001$, t-test). These two trends taken together suggest that, in later trials, participants have a better idea of which information is most important for them and they focus their attention on it.

---

$^8$The gradual increase of the difference in attention happens as participants spend less time looking at information over the course of multiple trials ($p < 0.001$, t-test). These two trends taken together suggest that, in later trials, participants have a better idea of which information is most important for them and they focus their attention on it.
less consistent in the Merit and Outcome Focus treatments where our attention manipulation dominated the effect of status. The left panel of Figure 5 shows a Status difference in the Outcome Focus treatment (rank-sum test $p = 0.011$) but not in the Merit Focus treatment (rank-sum test $p = 0.45$).

Figure 5: The Dynamics of Attention by Treatment.

(a) Involved Trials  
(b) Impartial Trials

The dynamics of attention by trial number and treatment, in both Involved decisions (left panel) and Impartial decisions (right panel). $\Delta$ Attention is the difference in dwell time on merit and outcome information. The data are displayed with LOESS smoothing and shaded 95% confidence bands.

To further investigate these results, Panel A of Table 3 shows the result of regression analyses with standard errors clustered at the individual level and controlling for subject characteristics. Column (1) shows that, aggregated over all treatments, the Advantaged dictators spent 150 ms more on outcome information, a difference that is statistically significant at the 5% level. Column (2) shows that this pattern is larger at about 310 ms in the Free Focus treatment but also less precisely estimated, because of the dynamic effects over trials (see Figure 5, left panel). Column (3) includes a control for trial number and an interaction with trial number and treatment, confirming that Advantaged dictators paid relatively less attention to merit over time. Column (4) and (5) investigate the effect in the Constrained Focus treatments, showing a statistically significant difference in the Outcome, but not the Merit Focus treatment.

Impartial Trials. During the Impartial trials, the dictator did not derive any private benefit from the allocations, so any treatment differences capture the internalization of self-serving fairness considerations that developed during the Involved trials. The right panel of Figure 5 shows that the effect of Status on attention persists in the Free Focus treatment. The average difference is slightly larger than in the Involved case, stable over time, and statistically significant.
Table 3: Differences in Attention.

<table>
<thead>
<tr>
<th></th>
<th>Panel A: Involved Trials</th>
<th></th>
<th>Panel B: Impartial Trials</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) All data</td>
<td>(2) Free Focus</td>
<td>(3) All data</td>
</tr>
<tr>
<td></td>
<td>∆ Attention</td>
<td>Δ Attention</td>
<td>∆ Attention</td>
</tr>
<tr>
<td>Advantaged</td>
<td>-0.15** (0.074)</td>
<td>-0.31* (0.17)</td>
<td>0.069 (0.18)</td>
</tr>
<tr>
<td>Trial number</td>
<td>0.021* (0.0099)</td>
<td></td>
<td>0.0036 (0.0083)</td>
</tr>
<tr>
<td>Advantaged * Trial number</td>
<td>-0.036** (0.013)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
| Observations     | 11930 | 3988 | 3988 | 3972 | 3970 | 11923 | 3984 | 3984 | 3975 | 3964 |}

All models are linear regressions with the dependent variable the difference in dwell time between merit and outcome information. Standard errors clustered by participant in parentheses. *p < 0.10, **p < 0.01, ***p < 0.001. List of controls: task type (4 categories), age, gender (man, woman, other), political affiliation (5 categories), education (6 categories), income (7 categories), continent (4 categories).

as indicated by a rank-sum test ($p = 0.044$). The regressions (Table 3, Panel B) show a significant effect across treatments (Column 1), and in the Outcome Focus treatment (Column 5), but not in the Free Focus or Merit Focus treatments (Columns 2 and 4, respectively). In addition, unlike in the Involved trials, there is no significant time trend in the Free Focus treatment.

Finally, comparing the two panels of Figure 5 shows that both Advantaged and Disadvantaged participants spent relatively more time on merit information in the Impartial trials compared to the Involved trials. While we did not hypothesize this pattern, the shift is present in both Experiment 1 and Experiment 2. This recurring pattern suggests that merit information was considered relatively more important for the Impartial decisions.

**Result 1.** In line with Hypothesis 1, dictator status has a causal effect on attention for Involved decisions. In the Free Focus treatment, with no limits on attention, Advantaged dictators spent a smaller fraction of the time looking at merit information compared to Disadvantaged dictators, an effect that develops over multiple trials. This difference in attention by status persists in the
Impartial treatments, although it is less precisely estimated.

5.2 Status and Allocations

We now examine Hypothesis 2, which relates to the share of the surplus given to the Advantaged member of the pair. Figure 6 displays the allocation dynamics split by Status and Focus treatments. The left panel shows behavior in the Involved trials. In the Free Focus condition, Advantaged dictators gave a larger share of the surplus to the Advantaged members of the pair, i.e. to themselves. A rank-sum test of the average share each dictator gave to the Advantaged recipients across rounds confirms that the allocation distributions of the two groups are significantly different ($p < 0.001$). This pattern is also pronounced in the Merit and Outcome Focus treatments (rank-sum test $p < 0.001$ for both).

Table 4 shows the results of regression analyses with standard errors clustered at the individual level and controlling for subject characteristics. Column (1) shows that, aggregated over all treatments, dictators give 10 percentage points more of the surplus to the Advantaged member ($p < 0.001$). Columns (3-5) split this effect by Focus treatment. The effect is highly statistically significant in all treatments, but the size fluctuates: it is lowest in the Merit Focus treatment and highest in the Outcome Focus treatment, a result we explore in more detail below.

Figure 6: The Dynamics of Allocation Decisions by Treatment.

(a) Involved Trials

(b) Impartial Trials

Share of the surplus given to the Advantaged member split by Status and Focus treatment, shown by round. The data are displayed with LOESS smoothing and shaded 95% confidence bands.

Giving to Self. The fact that Advantaged dictators allocate more to Advantaged members than Disadvantaged dictators do is consistent with dictators simply keeping most of the surplus.
However, the impact of being Advantaged is also apparent in comparing the share dictators kept for themselves, with Advantaged dictators keeping 61.5% compared to Disadvantaged dictators keeping slightly less than 50%. This result is highly significant on both a rank-sum test ($p < 0.001$) as well as in a regression with controls (Table 4 - Column 2), and replicates prior work on behavioral allocation biases whereby the participants randomly assigned a higher pay rate keep more for themselves [Konow 2000, Rodriguez-Lara and Moreno-Garrido 2012, Deffains et al., 2016]. In fact, the two ways of looking at the division are almost equivalent, because as Table 2 shows, the Disadvantaged dictators are very close to splitting the surplus 50-50.

Table 4: Differences in Share Given to Advantaged

<table>
<thead>
<tr>
<th></th>
<th>All data % to Adv.</th>
<th>All data % Kept</th>
<th>Free focus % to Adv.</th>
<th>Mer. Focus % to Adv.</th>
<th>Out. Focus % to Adv.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Advantaged</strong></td>
<td>10.0***</td>
<td>10.4***</td>
<td>9.05***</td>
<td>7.97***</td>
<td>12.9***</td>
</tr>
<tr>
<td></td>
<td>(1.00)</td>
<td>(1.02)</td>
<td>(1.61)</td>
<td>(1.66)</td>
<td>(1.78)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>11930</td>
<td>11930</td>
<td>3988</td>
<td>3972</td>
<td>3970</td>
</tr>
</tbody>
</table>

**Panel B: Impartial Trials**

<table>
<thead>
<tr>
<th></th>
<th>All data % to Adv.</th>
<th>Free focus % to Adv.</th>
<th>Mer. Focus % to Adv.</th>
<th>Out. Focus % to Adv.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Advantaged</strong></td>
<td>3.44***</td>
<td>3.94**</td>
<td>1.27</td>
<td>4.80***</td>
</tr>
<tr>
<td></td>
<td>(0.70)</td>
<td>(1.28)</td>
<td>(1.17)</td>
<td>(1.15)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>11923</td>
<td>3984</td>
<td>3975</td>
<td>3964</td>
</tr>
</tbody>
</table>

All models are linear regressions. Dependent variable: the percentage of the pie allocated to the Advantaged member of the pair in Columns 1, 3, 4, and 5, the percentage of the pie kept by the dictator in Column (2). Standard errors clustered by participant in parentheses. $^+ p < 0.10$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$. List of controls: Share of correct answers coming from the advantaged member over the total number of correct answers of the pair, task type (4 categories), age, gender (man, woman, other), political affiliation (5 categories), education (6 categories), income (7 categories), continent (4 categories).

**Impartial Allocations.** The right panel of Figure 6 shows that allocation differences persist into the Impartial trials, with the Advantaged dictators still acting more favourably towards Advantaged members of the pair. Again focusing on the Free Focus treatment, a rank-sum test confirms differences between the two treatment groups ($p = 0.002$). Combining all three Focus treatments, Column (1) of Panel B of Table 4 shows that Advantaged dictators gave 3.4 percentage points more of the surplus to the Advantaged member after controlling for individual characteristics, a result that is statistically significant. Columns (4) and (5) show that these results are statistically robust in the Outcome Focus, but not the Merit Focus treatment. This last result provides another indication that attention patterns affect self-serving bias. While over-
all statistically significant, these differences in Impartial allocations are quantitatively smaller than in the Involved trials, accounting for less than half of the status bias. This smaller share results from eliminating the role of self-interest, and the remaining difference can be attributed to internalized differences in the fairness views applied by the two types of dictators.

**Result 2.** *In line with Hypothesis 2, dictator status affects allocations. Advantaged dictators gave a larger share of the common account to themselves than Disadvantaged dictators gave to Advantaged recipients or themselves. These differences in allocations persist for Impartial choices, although the effect is less than half the size.*

### 5.3 Attention and Allocations

We now come to our main question, as captured by Hypothesis 3, namely whether attention influences dictator allocations. Having already established that Status influences patterns of information-gathering, Figure 7 shows correlational evidence relating these divergences in attention to allocation behavior. The lines represent a linear fit to the data from all treatments, while the colored dots plot the averages of the different Focus treatments. When we look at Involved decisions (left panel), we see that individual differences in the dwell time on merit vs. outcome information affected the share allocated to Advantaged members, regardless of Status or Focus treatment. Pooling data from all the Involved trials the correlation coefficient is $-0.15$ ($p < 0.001$).

These correlations could be due to different preferences or conceptions of fairness across participants. Thus, we turn to our Constrained Focus treatments that manipulated attention and allow us to quantify the causal role of attention. The treatment averages (represented by colored dots in Figure 7) show the extent to which they shifted attention as well as allocations. The shifts suggest that selective attention indeed contributes to the development of self-serving biases, particularly for Advantaged dictators, where the slope of the relationship is steeper. Participants gave 53.6% of the surplus to the Advantaged members of the pair in the Merit Focus treatment compared to 56.3% in the Outcome Focus treatment, a significant difference in allocation distributions (rank-sum test $p = 0.028$).

Table 5 reports linear regressions in the Constrained Focus treatments, with controls for round and demographic characteristics. Using the Merit Focus treatment as a baseline, Column (1) shows that the Outcome Focus treatment increases the money given to the Advantaged recipients, an effect that is statistically significant at the 5% level. With a size of 3 percentage points, or about 30% of the effect of Status found in Table 4, this effect of attention is also
Figure 7: The Relation Between Attention and Allocation.

(a) Involved Trials  
(b) Impartial Trials

Illustration of the influence of attention on allocations, split by advantaged status. The lines include data from both Free Focus and Constrained Focus treatments and are linearly smoothed with shaded 95% confidence bands. The mean attention and allocations are split by information condition.

Economically large. The left panel of Figure 6 suggests that the difference between the Focus treatments is due in large part to the Advantaged dictators. Therefore, we split the effect in Column (1) in separate regressions for the Disadvantaged (Column 2) and Advantaged dictators (Column 3). This shows that the effect of Outcome Focus on allocations is indeed only found among Advantaged dictators. The difference between the two coefficients is statistically significant at the 10% level ($p = 0.054$, Wald test).

Quantifying the impact of dwell time. To quantify the causal impact of differences in dwell times, as opposed to the effect of our Constrained Focus treatments, we perform an instrumental

---

9 A potential confound is that participants in the Constrained Focus treatment put more effort in looking at the information that is restricted in the majority of rounds. However, this asymmetric effort is unlikely to appear in our setting: only a minority of subjects was able to identify after the experiment which type of information was restricted, see Footnote 12. More importantly, any asymmetric effort would only make the Merit and Outcome Focus more similar to each other, reducing the strength of our attention manipulation.

10 This asymmetric finding is in line with field evidence from Di Tella et al. (2007), who show that an exogenous increase in property increases pro-market beliefs, but a failure to obtain such property rights does not depress them.
Table 5: The Effect of Attention on Allocation.

<table>
<thead>
<tr>
<th>Panel A: Involved Trials, Constrained Focus only</th>
<th>(1) All data % to Adv.</th>
<th>(2) Disadvantaged % to Adv.</th>
<th>(3) Advantaged % to Adv.</th>
<th>(4) All Data % to Adv.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Outcome Focus</td>
<td>2.93* (1.36)</td>
<td>0.26 (1.94)</td>
<td>4.96*** (1.47)</td>
<td></td>
</tr>
<tr>
<td>Δ Attention</td>
<td>-2.63* (1.21)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>7942</td>
<td>3970</td>
<td>3972</td>
<td>400</td>
</tr>
<tr>
<td>F-statistic - first stage</td>
<td>552</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Impartial Trials, Constrained Focus only</th>
<th>(1) All data % to Adv.</th>
<th>(2) Disadvantaged % to Adv.</th>
<th>(3) Advantaged % to Adv.</th>
<th>(4) All Data % to Adv.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Outcome Focus</td>
<td>0.82 (0.84)</td>
<td>-0.73 (1.11)</td>
<td>1.88 (1.16)</td>
<td></td>
</tr>
<tr>
<td>Δ Attention</td>
<td>-0.77 (0.78)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>7939</td>
<td>3974</td>
<td>3965</td>
<td>400</td>
</tr>
<tr>
<td>F-statistic - first stage</td>
<td>244</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The dependent variable is the percentage of the pie allocated to the Advantaged member of the pair. Standard errors clustered by participant in parentheses. \(^{+}p < 0.10\), \(^{*}p < 0.05\), \(^{**}p < 0.01\), \(^{***}p < 0.001\). Column 1-3 are linear regressions. Fourth column instrumental variable conducted by 2sls: endogenous regressors difference in dwell time between merit and outcome information, instruments attention restriction implemented in a round (4 categories). List of controls columns 1-3: Share of correct answers coming from the advantaged member over the total number of correct answers of the pair, task type (4 categories), age, gender (man, woman, other), political affiliation (5 categories), education (6 categories), income (7 categories), continent (4 categories). Column 4 has the same controls with the exception of task type.

In this way, we can estimate the effect of increasing Δ Attention by one second in every round on the average allocation. In the first stage, we instrument attention with the Focus treatment to which the subject is assigned. The F-statistic of our first stage is above 550, indicating a strong instrument and a minimal expected bias in the estimates.

---

11 Pooling the data at the individual level is necessary because the instrument - the Focus treatment - varies between but not within subjects. As such, in the second stage, a participants’ predicted Δ Attention is the same in every round.

12 The exclusion restriction is that attention constraints only affect allocations via dwell time. This is in line with standard models of attention like drift diffusion models, which focus on dwell time as the exclusive variable. We can also exclude that our restrictions have a demand effect: in our post-experiment questionnaire, no subjects mentioned time restrictions as the purpose of the research. Furthermore, only 25.5% of subjects guessed correctly at least one of the boxes that were restricted (20% for both boxes), with 8.5% guessing wrongly and 66% not reporting any specific box restrictions. Furthermore, the time limit on at least one box is binding in 90.5% of the Involved trials, indicating that our IV estimate is informative about most of our observations. Furthermore, the monotonicity assumption is satisfied in our setting because would-be defiers have no way to alter the time restrictions on a box in a given round.
In the second stage regression, we use this exogenously-induced variation in attention to explain dictator allocations. Column (4) of Table 5 shows that increasing $\Delta$ Attention by one second leads to a 2.6 percentage point decrease in allocations to Advantaged members. To put this in context, note that one can increase $\Delta$ Attention by one second by shifting 500 ms of attention from outcome to merit information, a shift that is similar in magnitude to the one produced by the Outcome Focus treatment. Hence, in our setting, one second is a reasonable unit of attention that does not involve extrapolation of our treatment effects. Relative to total attention, reallocating 500 ms implies a shift in attention equivalent to 23% of the average dwell time in the Constrained Focus treatment. The reallocation produces an equally large behavioral response reducing the effect of Status on allocations found in Table 4 by more than 25%.

**Impartial Allocations.** The right panel of Figure 7 shows that patterns are similar in the Impartial allocations, although with less pronounced differences between Advantaged and Disadvantaged dictators, and a lower correlation between attention and allocations: Pooling data from all the Impartial trials, the correlation coefficient is $-0.13$ ($p < 0.001$). When it comes to causal evidence of attention, we find no statistically significant effect of the attention manipulation. Participants in the Merit Focus treatment, gave 53.5% of the surplus to the Advantaged members of the pair compared with 54.2% in the Outcome Focus treatment ($p = 0.33$, rank-sum test).

Panel B of Table 5 performs the same analyses for the Impartial allocations as we did for Involved allocations. It shows a similar null result in a regression with demographic controls. Moreover, there is no statistically significant evidence that the influence of Focus treatment on allocations differs between Advantaged and Disadvantaged dictators ($p = 0.10$, Wald test). These results do not show clear evidence a causal effect of attention in the Impartial condition although we cannot rule out that there is some effect among the Advantaged dictators, which we don’t have the power to detect.

**Result 3.** In line with Hypothesis 3, the Outcome Focus information condition increases the share of the surplus given to the Advantaged participants compared to Merit Focus, particularly for Advantaged dictators. We find that a one second increase in relative dwell time on merit decreases can explain about 25% of the impact of Status on allocations. Effects are smaller in the Impartial trials and not statistically significant.

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13 The similarity is reflected in the fact that the absolute value of the coefficients in columns (1) and (4) of Table 7 are close to each other.
6 Discussion

In this section, we discuss a number of additional aspects of our data: additional measures of attention and information avoidance, the consistency of behavior with fairness criteria, psychological mechanisms of self-serving bias, and the mediating effect of attention on self-serving bias.

6.1 Information Avoidance

For several reasons, participants may not attend to information at all: they may decide independently of merit or outcome and thus have no use for the information, or they might want to avoid information in order not to face psychological conflicts from taking the most money for themselves [Dana et al. 2007; Grossman and Van der Weele 2017].

Table 6: Avoidance of Merit and Outcome Information

<table>
<thead>
<tr>
<th>Panel A: Involved Trials</th>
<th>Free Focus</th>
<th>Merit Focus</th>
<th>Outcome Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Merit avoidance</td>
<td>4.2%</td>
<td>4.9%</td>
<td>1.3%</td>
</tr>
<tr>
<td>Outcome avoidance</td>
<td>5.5%</td>
<td>3.7%</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Impartial Trials</th>
<th>Free Focus</th>
<th>Merit Focus</th>
<th>Outcome Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Merit avoidance</td>
<td>5.4%</td>
<td>6.1%</td>
<td>2.6%</td>
</tr>
<tr>
<td>Outcome avoidance</td>
<td>14.0%</td>
<td>15.8%</td>
<td>13.9%</td>
</tr>
</tbody>
</table>

To see whether information avoidance is common, Table 6 Panel A shows the fraction of subjects in the Involved trials who open some information, but avoid one type of information (merit or outcome) entirely. The amount of information avoidance is low, with less than 10% of subjects on aggregate avoiding either type of information. Disadvantaged dictators were slightly more likely to avoid both types of information (merit: rank-sum test $p = 0.043$; outcome: rank-sum test $p = 0.044$). One explanation for the low information avoidance is that the presence of both types of information signaled relevance to the decision to the subjects, who felt compelled to look at it.

As in all our analyses, this analysis uses filtered data where dwell times of below 200 ms are excluded, which may overestimate avoidance. We exclude trials in which participants did not reveal any information because they are rare and difficult to interpret: it is unclear whether participants were distracted or truly chose not to reveal information on those trials. Such full-avoidance trials account for approximately 1.2% of Involved trials and 1.8% of Impartial trials.
In the Impartial trials, Panel B of Table 6, there is more information avoidance overall, particularly for outcome information. On these trials, avoidance of outcome information hovers around 15% compared to around 5% for merit information. This accords with our observation of longer dwell time on merit information in the Impartial decisions. Furthermore, here we also see Disadvantaged participants avoiding outcome information more frequently than Advantaged participants (rank-sum test $p = 0.030$), but not merit information (rank-sum test $p = 0.31$). Participants are unlikely to avoid such information out of guilt in the Impartial trials, and instead may not find outcome information as interesting or relevant for Impartial decisions.

6.2 Other Attention Measures

Dwell time is not the only measure of attention found to matter in choice. Other important measures in process-tracing include the instances of looking at information (i.e. the number of times each box is opened) and the last information examined (Willemsen and Johnson [2019]). We test whether these measures also matter. The number of times each box is opened is similar in concept to dwell time, with the average of each measure per subject highly correlated at 0.88, $p < 0.0001$ for both Involved and Impartial trials. Therefore, it is unsurprising that similar dynamics and a similar relationship between attention and allocations can be found for this measure, illustrated in Appendix A.3, shown in Appendix Figure 8 and Figure 9.

Last fixations are another measure shown to influence choice, as in Ghaffari and Fiedler (2018) who use the last-fixated information as their main attention manipulation. In our data, we find a large difference in this measure between treatments, with the Constrained Focus treatments biasing participants to look at the less-restricted focus information last. We also find differences across Status in the Free and Outcome Focus treatments, with the Disadvantaged participants more often looking at merit information last (Appendix A.3, Figure 10).

Despite the large differences in last fixations, they have a limited effect on allocations in the Constrained Focus treatments. We only find a significant relationship between last-fixated information (merit vs. outcome) and allocations in the Free Focus Involved trials where looking last at outcome information increases the allocation to Advantaged participants (Wilcoxon rank-sum test, $p = 0.032$; Merit Focus $p = 0.85$; Outcome Focus $p = 0.48$). This is similar to the findings of Ghaffari and Fiedler (2018) who show that last fixations affect allocation decisions only in self-directed or autonomous attention conditions. They interpret this as an indication of the primacy of preferences rather than salience or other exogenous factors in information search.
6.3 Fairness Types

A complementary way to study the behavior of dictators is to evaluate their consistency with different fairness criteria. We consider three such criteria that are routinely used to analyse decisions in the experimental literature on fairness (e.g. Konow 2000, Cappelen et al. 2007, Bortolotti et al. 2017). The Egalitarian criterion requires splitting the surplus in equal parts among participants. The Meritocratic criterion requires splitting the surplus proportionally to the ratio of correct answers of the two participants in the real effort task. Finally, the Libertarian criterion requires splitting the surplus proportionally to the ratio of monetary contributions of each participant in the pair.

We are mostly interested in consistency with these criteria in the Impartial trials, where self-interest is removed and fairness becomes the most salient consideration. We consider an allocation to be consistent with a fairness criterion if the distance between the chosen allocation and the prescription implied by the criterion is less than 5% of the total surplus size. For example, we consider any allocation for which a member of the pair receives between 45% and 55% of the surplus to be consistent with the Egalitarian criterion. Using these definitions, 78% of the allocations are consistent with at least one fairness criterion and 66% of the allocations are consistent with only one criterion. Overall, 20% of the choices are Egalitarian, 35% are Meritocratic, and 23% are Libertarian.

The three panels in Table 7 show the results of linear probability models with each of the three fairness criteria as the dependent variable, using data from the Impartial treatment only. Column (1) shows the impact of Status, using data from all Focus treatments. Column (2) looks at the Constrained Focus treatments, quantifying the impact of the Outcome Focus compared to the Merit Focus treatment, the omitted category. Column (3) adds the interaction between the Advantaged and Outcome Focus treatments.

Panel A shows that Advantaged dictators are about 6 percentage points less likely to conform to an Egalitarian criterion. Column (3) displays some weak evidence that this effect is smaller in the Outcome Focus, a somewhat counter-intuitive result. Turning to the Meritocratic criterion in Panel B, there is some weak evidence that Advantaged dictators are less Meritocratic (Column 1). Interestingly, this effect is much stronger and statistically significant at the 1% level in the Outcome Focus treatment. Finally, Advantaged Dictators are 10 percentage points more likely to be Libertarian (Panel C - Column 1). The Libertarian criterion favours Advantaged

---

\[15\] In some rounds different criteria require similar allocations. For example, this happens if the participants answered the same number of questions correctly in a task. In that case, both the egalitarian and the meritocratic criteria require an equal split.
Table 7: Probability of Making a Choice Consistent with a Fairness Criterion

**Panel A: Egalitarian criterion**

<table>
<thead>
<tr>
<th></th>
<th>(1) All Data</th>
<th>(2) Constrained Focus</th>
<th>(3) Constrained Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Egalitarian</td>
<td>Egalitarian</td>
<td>Egalitarian</td>
</tr>
<tr>
<td>Advantaged</td>
<td>-0.063***</td>
<td>-0.098**</td>
<td>(0.019)</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.032)</td>
<td></td>
</tr>
<tr>
<td>Outcome Focus</td>
<td>0.013</td>
<td>-0.030</td>
<td>(0.024)</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.035)</td>
<td></td>
</tr>
<tr>
<td>Adv. * Out. Focus</td>
<td>0.088+</td>
<td></td>
<td>(0.047)</td>
</tr>
<tr>
<td>Observations</td>
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<td>7939</td>
<td>7939</td>
</tr>
</tbody>
</table>

**Panel B: Meritocratic criterion**

<table>
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<tr>
<th></th>
<th>(1) All Data</th>
<th>(2) Constrained Focus</th>
<th>(3) Constrained Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Meritocratic</td>
<td>Meritocratic</td>
<td>Meritocratic</td>
</tr>
<tr>
<td>Advantaged</td>
<td>-0.037+</td>
<td>0.037</td>
<td>(0.021)</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.036)</td>
<td></td>
</tr>
<tr>
<td>Outcome Focus</td>
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<td>0.043</td>
<td>(0.025)</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.035)</td>
<td></td>
</tr>
<tr>
<td>Adv. * Out. Focus</td>
<td>-0.14**</td>
<td></td>
<td>(0.049)</td>
</tr>
<tr>
<td>Observations</td>
<td>11923</td>
<td>7939</td>
<td>7939</td>
</tr>
</tbody>
</table>

**Panel C: Libertarian criterion**

<table>
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<th>(1) All Data</th>
<th>(2) Constrained Focus</th>
<th>(3) Constrained Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Libertarian</td>
<td>Libertarian</td>
<td>Libertarian</td>
</tr>
<tr>
<td>Advantaged</td>
<td>0.10***</td>
<td>0.038</td>
<td>(0.020)</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.031)</td>
<td></td>
</tr>
<tr>
<td>Outcome Focus</td>
<td>0.038</td>
<td>-0.022</td>
<td>(0.024)</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.027)</td>
<td></td>
</tr>
<tr>
<td>Adv. * Out. Focus</td>
<td>0.12*</td>
<td></td>
<td>(0.047)</td>
</tr>
<tr>
<td>Observations</td>
<td>11923</td>
<td>7939</td>
<td>7939</td>
</tr>
</tbody>
</table>

Linear probability models for consistency with a fairness norm in the Impartial treatment. The dependent variable is a dummy equal 1 if the allocation is consistent with the fairness criterion and zero otherwise. An allocation is considered consistent with a fairness criterion if the distance between the allocation and criterion prescription is less than 5% of the total surplus size. Standard errors clustered by participant in parentheses. + $p < 0.10$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$. Data from the Impartial trials only. List of controls: task type (4 categories), age, gender (man, woman, other), political affiliation (5 categories), education (6 categories), income (7 categories), continent (4 categories).
dictators the most, and Advantaged dictators are 3 times more likely than Disadvantaged to act consistently with it. Again, this effect appears to be most pronounced in the Outcome Focus treatment.

Overall, these results support the idea that dictators internalize self-serving fairness criteria during the Involved treatment and act upon them in the Impartial treatment, where Advantaged dictators conform less to Egalitarian and more to Libertarian fairness criteria. Moreover, driving dictators to pay more attention to Outcome information strengthens these effects for two of the criteria: it causes Advantaged Dictators to be less Meritocratic and more Libertarian.

6.4 Attention and the Psychological Mechanisms of Self-serving Bias

What does our analysis of visual attention tell us about the underlying cognitive mechanisms of self-serving bias? We distinguish two plausible accounts. The first is that attention is a cognitive tool by which people develop the self-serving bias. In this view, the attention treatments shift behavior because they shift the weight on what is viewed as an appropriate or fair division. One test of this account is the degree to which attention shifts beliefs about what is fair. As demonstrated in the previous subsection, subjects do change their behavior in line with different fairness criteria. To obtain an independent measure of their fairness views, we asked participants in both experiments about their endorsement of the three fairness norms we outlined in the previous subsection. We asked for both (“moral appropriateness”) and their expectations of other’s endorsement of the same norms (“social appropriateness”), using the method by Krupka and Weber (2013) in the latter case. However, we do not find any clear pattern for the effect of attention on these fairness norms, leaving the evidence for this channel somewhat ambiguous.

Another way through which attention may produce bias is via the formation of motivated beliefs. In particular, Advantaged participants may convince themselves that their superior contribution in production was driven by merit rather than luck. To measure this, we asked subjects in Experiment 2 to report the amount of rounds in the Involved trials the recipient answered more questions correctly than they did. Because of the way we created the rounds, on average, the true answer was that recipients outperformed them in 50% of the trials. Again, we find no statistically significant difference when we compare beliefs between the Merit and the Outcome Focus treatments (rank-sum test $p = 0.57$).

A second possible mechanism is that visual attention is a way to implement bias that has been developed through other cognitive pathways. In this view, the attention treatments shift behavior because the 400 ms attention restriction limits subjects’ ability to implement their
preferred (and possibly biased) fairness norm. We can test this account directly using our attention manipulations. Recall that in every attention treatment, the attention restrictions on one type of information were implemented only in 14 out of the 20 rounds and randomly allocated to other dimensions in the remaining six rounds. Thus, in a given Focus treatment, participants should have the same fairness criteria but different restrictions on implementation. If these restrictions affected the implementation of fairness norms, we should see a difference in allocations between the Merit trials and Outcome trials. However, the type of trial does not have a statistically or quantitatively meaningful impact on behavior. Furthermore, the effect of the Outcome Focus treatment on allocation does not go down once we control for trial type.

In summary, our behavioral data suggest that attention shifts fairness criteria, but we cannot pin down the precise cognitive mechanism. The data also provide further clues that the full picture is likely to be complex. Comparing the first panels of Figures 5 and 6, we observe that while the behavioral biases exists from round 1 onward, the attentional bias develops over time, a finding that speaks against both accounts elaborated above.

6.5 Attention as a Mediator of Self-Serving Bias

Above, we established that Status affects attention, and attention affects allocations. However, how much of the total effect of Status on self-serving allocation biases can be explained by selective attention? To answer this question, we perform a causal mediation analysis based on Imai et al. (2010). The mediation analysis divides the total average treatment effect (ATE) into the average direct effect of status (ADE) and the complier average causal effect mediated by attention (CACME). The causal identification of CACME uses the Constrained Focus treatments as an instrument, and it is informative about those subjects in the Constrained Focus treatments who were impacted by the imposed restrictions. As discussed in Imai et al. (2013) the identification of the causal mediation relies on the same assumptions as any IV estimations. Our two experiments taken together are the first example in economics of the Parallel Encouragement Design proposed by Imai et al. (2013).

Table 8 presents the results of this exercise. Column (1) focuses on the share of the pie given to Advantaged recipients in the Involved trials. It shows that only 3.4% of the effect of Status passes via attention. In absolute terms, the ability to manipulate attention allows Advantaged

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16 For instance, in the Outcome Focus treatment, 14 rounds would restrict merit information, 2 rounds outcome information, 2 rounds “own” contribution/merit, and 2 rounds “other” contribution/merit.

17 Table 13 displays the regression results for this analysis.

18 As noted in Footnote 12, these assumptions are likely to be satisfied in our setting.
Table 8: Causal Mediation Analysis

<table>
<thead>
<tr>
<th></th>
<th>(1) Involved % to Adv.</th>
<th>(2) Involved % kept</th>
<th>(3) Impartial % to Adv.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Complier average causal effect mediated by ∆Attention (CACME)</td>
<td>0.35 (0.18)</td>
<td>0.27 (0.18)</td>
<td>0.14 (0.17)</td>
</tr>
<tr>
<td>Average direct effect of status (ADE)</td>
<td>9.69 (0.97)</td>
<td>10.14 (1.03)</td>
<td>3.31 (0.68)</td>
</tr>
<tr>
<td>Average treatment effect of status (ATE)</td>
<td>10.03 (1.00)</td>
<td>10.42 (1.02)</td>
<td>3.44 (0.70)</td>
</tr>
<tr>
<td>CACME/ATE</td>
<td>0.034</td>
<td>0.026</td>
<td>0.040</td>
</tr>
</tbody>
</table>

The estimates of the ATE come from Column (1) and (2) of Table 4. The estimate of the ADE comes from a two stage least squares procedure: first endogenous regressors difference in dwell time between merit and outcome information is instrumented with the experimental attention restrictions (4 categories). The point estimate for the CACME is the difference between the ATE and ADE, the standard error is computed following the procedure described in Imai et al. (2010) paragraph 4.1 and using the exact variance formula from Goodman (1960).

dictators to keep 0.35% more of the pie for themselves. This effect is small but significantly larger than zero with 90% confidence ($p = 0.055$, Sobel test). Column (2) repeats the analysis for the share that dictators keep for themselves. Once again the mediating effect of attention is small (0.27% of the pie or 2.6% of the total effect) but this time not statistically different from zero at conventional levels ($p = 0.11$, Sobel Test). Column (3) of Table 8 shows the same exercise for Impartial allocations. In this case, we find that attention mediates 4.0% of the effect of Status, which not significantly different from zero ($p = 0.42$, Sobel Test).

In summary, the mediating effect of attention is small, mainly because the effect of Status on attention is not very large. As we demonstrated above, this coincides with a sizable causal effect of attention on allocations, which can be exploited by policy makers.

7 Conclusion

In this paper, we investigate the role of attention in fairness and redistribution decisions. We show that economic advantage causes selective attention, as it changes how long subjects look at merit-based information. Furthermore, we highlight the causal impact of attention on behavior. Some of these effects persist, albeit in somewhat weaker form, in situations where people have to make decisions between two other individuals and their own income is not at stake. These findings help explain the emergence of the self-serving views of inequality and fairness. People dedicate more time to information that is consistent with the allocation criterion that gives them the most money. This biased attention, in turn, increases the amount of money people allocate to themselves or other advantaged individuals.
Extrapolating beyond the laboratory, selective attention may explain why groups have different views on the nature and desirability of inequality, and provide insights for a current debate about the role of meritocracy in Western society. Elites may favor policies promoting open markets and low redistribution, while looking away from the institutionalized advantages that allow them to reap disproportionate benefits of such policies (Sandel, 2020). Our data also suggest that economic elites are likely to demand more disposition-based accounts of inequality, whereas disadvantaged groups are more likely to consume accounts highlighting their barriers to success. Future research could explicitly study the media consumption of those groups, and test whether exposing people to different types of information helps to reduce polarization in beliefs outside the laboratory. The results could be relevant for other domains where a subgroup of society enjoys institutionalized advantages, whether they are based on income, race or gender.

The results show that the effect of attention on decisions is substantial, and can reduce self-serving bias by a meaningful amount. This provides a promising starting point for the design of interventions and policies based on visual attention, such as online information campaigns or educational campaigns to combat bias. It also suggests that political advertising about the sources of inequality on social media or elsewhere can affect attitudes for redistribution. More research is needed to determine the ecological validity of these claims.

At the same time, the results of our mediation analysis show that there are important drivers of bias that are not captured by our measure of attention. There may be several reasons for this. First, attention in our experiment is measured in milliseconds and redistribution amounts in single or low double digits. It is plausible that more sustained exposure and higher amounts will lead to larger behavioral effects. The data from our Involved trials provides a clue to this, by showing that selective attention increases over trials. Second, other cognitive and affective factors may play a role. Given the importance of redistribution and polarized ideas of fairness, it is important that future research disentangle these factors, and improve the understanding of allocation decisions and self-serving bias.
References


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Rahal, Rima-Maria, Susann Fiedler, and Carsten KW De Dreu, “Prosocial preferences condition decision effort and ingroup biased generosity in intergroup decision-making,” *Scientific reports*, 2020, 10 (1), 1–11.


Appendices

A Additional results

A.1 Introductory survey methods and additional results

We recruited in total 767 participants from Prolific.co. Because we were interested in how income impacts information-seeking, we separately recruited two groups of about 380 participants at the extremes of income distributions. To do this, we used Prolific filters to recruit participants with personal incomes below 10,000 pounds and household incomes below 50,000 pounds for our low income group (N = 383). For our high income group, we filtered participants to have personal incomes above 70,000 pounds (N = 384). We also restricted our sample to exclude students and to include only participants currently living in the US and UK with an approval rate on Prolific of at least 98%.  

In the survey, we first asked participants demographic questions including age, gender, political leaning, current personal income, student status, educational attainment, and car ownership (brand and year). We confirmed participants’ student status and personal income by checking whether their answers in our survey matched the prolific filters. If there was a mismatch, participants were informed that they were ineligible for the study and excluded from the survey.

Our main task asked participants to choose between two educational news articles to read. One option focused on success due to merit (“Why high earners work longer hours”) and the other focused on the role of luck in success (“Luck looms larger in success than most of us think”). In order to incentivize the choice of articles, participants knew that they could earn a 1 pound bonus by answering comprehension questions about the article correctly on the first try, and they had to answer the questions correctly to proceed. Therefore, they knew that they would have to actually read the article.

After choosing an article and correctly answering comprehension questions, participants were asked about their news consumption behavior, more detailed questions about their source of income, household income and household size, and wealth outside of income, as well as their attitudes toward redistribution. We asked four questions about attitudes toward redistribution that come from the World Values Survey (Haerpfer et al., 2020) and the International Social

19 Despite our filtering, a few low income participants reported current household incomes over 50,000 pounds (30/383) and only 8 with incomes over 70,000 pounds. There were even fewer high income participants (8/343) reporting household incomes below 70,000 pounds despite reporting personal incomes of over 70,000 pounds and none who reported household incomes below 50,000 pounds.
Survey Programme (ISSP 2018). These included the share of taxes that high earners should pay, the relative role of luck vs. hard work in success, whether incomes should be more equalized, and governmental vs. individual responsibility. To create an index of redistribution attitudes, we normalized all questions to a range from 1-10 and averaged them.

To check the robustness of our results, we used regressions controlling for demographic variables including age and gender shown in Table 9. Using a linear probability model to predict article choice, we confirm that high personal income relates to a lower likelihood of choosing the luck article both for binary high compared to low income and a more continuous income measure. A linear regression of our redistribution index again confirms that higher household incomes (binary and continuous) are related to endorsing less redistribution while controlling for demographics.

Table 9: Survey results.

<table>
<thead>
<tr>
<th></th>
<th>Article Choice (High)</th>
<th>Article Choice (10,000s of pounds)</th>
<th>Redistribution Attitude (1)</th>
<th>Redistribution Attitude (10)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Personal Income (High)</td>
<td>-0.22*** (0.038)</td>
<td>-0.0020*** (0.00035)</td>
<td>-1.40*** (0.14)</td>
<td>-0.013*** (0.0013)</td>
</tr>
<tr>
<td>Age</td>
<td>0.0045** (0.016)</td>
<td>0.0046** (0.016)</td>
<td>-0.026*** (0.0061)</td>
<td>-0.025*** (0.0061)</td>
</tr>
<tr>
<td>Gender (Woman)</td>
<td>0.033 (0.038)</td>
<td>0.036 (0.038)</td>
<td>0.20 (0.14)</td>
<td>0.21 (0.14)</td>
</tr>
<tr>
<td>Gender (Other)</td>
<td>0.12 (0.12)</td>
<td>0.13 (0.55)</td>
<td>1.79*** (0.48)</td>
<td>1.85*** (0.48)</td>
</tr>
<tr>
<td>Gender (Prefer not to answer)</td>
<td>-0.014 (0.36)</td>
<td>-0.0021 (0.36)</td>
<td>2.70* (1.30)</td>
<td>2.76* (1.30)</td>
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<tr>
<td>Observations</td>
<td>767</td>
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<td>767</td>
</tr>
</tbody>
</table>

Regressions controlling for demographics including age and gender. Columns (1),(2) show the results of linear probability models with robust standard errors and with dependent variable: article choice where 0 = merit article and 1 = luck article. Columns (3),(4) show the results of linear regressions with dependent variable: attitudes toward redistribution. This is a composite index of four redistribution questions where 1 = low redistribution and 10 = high redistribution. Two measures of personal income are shown, Columns (1) and (3) use binary high or low income, whereas Columns (2) and (4) use a more continuous measure of income in 10,000s of pounds for the high-income participants. Standard errors are shown in parentheses. $^+ p < 0.10$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$. 39
## A.2 Effect of Status on Attention controlling for total dwell time

Table 10: Differences in Attention.

<table>
<thead>
<tr>
<th>Panel A: Involved Trials</th>
<th>(1) All data</th>
<th>(2) Free focus</th>
<th>(3) Free focus</th>
<th>(4) Mer. Focus</th>
<th>(5) Out. Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td>∆ Attention</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Advantaged</td>
<td>-0.13(^+)</td>
<td>-0.29(^+)</td>
<td>0.057</td>
<td>-0.017</td>
<td>-0.088(^*)</td>
</tr>
<tr>
<td></td>
<td>(0.072)</td>
<td>(0.16)</td>
<td>(0.18)</td>
<td>(0.055)</td>
<td>(0.041)</td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.014</td>
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<td></td>
<td></td>
</tr>
<tr>
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<td>(0.011)</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Advantaged * Round</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-0.033(^*)</td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td></td>
<td></td>
<td></td>
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<td>Observations</td>
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<table>
<thead>
<tr>
<th>Panel B: Impartial Trials</th>
<th>(1) All data</th>
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<th>(3) Free focus</th>
<th>(4) Mer. Focus</th>
<th>(5) Out. Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td>∆ Attention</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Advantaged</td>
<td>-0.21(^*)</td>
<td>-0.37</td>
<td>-0.42</td>
<td>-0.081</td>
<td>-0.20(^**)</td>
</tr>
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<td></td>
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<td>(0.25)</td>
<td>(0.26)</td>
<td>(0.084)</td>
<td>(0.062)</td>
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<td></td>
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<td></td>
<td>(0.0084)</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Advantaged * Round</td>
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<td></td>
<td>0.0050</td>
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<td></td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
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<td>3984</td>
<td>3975</td>
<td>3964</td>
</tr>
</tbody>
</table>

All models are linear regressions with the dependent variable the difference in dwell time between merit and outcome information. Standard errors clustered by participant in parenthesis. \(^+ p < 0.10, ^* p < 0.05, ^** p < 0.01, ^*** p < 0.001\). List of controls: total dwell time, task type (4 categories), age, gender (man, woman, other), political affiliation (5 categories), education (6 categories), income (7 categories), continent (4 categories).
A.3 Other measures of Attention

Figure 8: The dynamics of attention (box opens) by treatment

(a) Involved Trials

(b) Impartial Trials

The dynamics of attention (here measured as the number of times merit-outcome information boxes were opened) by round and treatment, in both Involved decisions (left panel) and Impartial decisions (right panel). The data are displayed with LOESS smoothing and shaded 95% confidence bands.
Illustration of the influence of attention (number of times merit - outcome information boxes were opened) on allocations, split by Status. The lines include data from both Free Focus and Constrained Focus treatments and are linearly smoothed with shaded 95% confidence bands. The mean attention and allocations are split by information condition.

The dynamics of attention (here measured as the proportion of final fixations on merit information) by round and treatment, in both Involved decisions (left panel) and Impartial decisions (right panel). The data are displayed with LOESS smoothing and shaded 95% confidence bands.
Table 11: Differences in Attention - Number of Boxes

<table>
<thead>
<tr>
<th></th>
<th>(1) All data</th>
<th>(2) Free focus</th>
<th>(3) Free focus</th>
<th>(4) Mer. Focus</th>
<th>(5) Out. Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
</tr>
<tr>
<td>Advantaged</td>
<td>0.15</td>
<td>0.17</td>
<td>-0.11</td>
<td>-0.024</td>
<td>0.26*</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.21)</td>
<td>(0.25)</td>
<td>(0.15)</td>
<td>(0.12)</td>
</tr>
<tr>
<td>Round</td>
<td>-0.030**</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.012)</td>
</tr>
<tr>
<td>Advantaged * Round</td>
<td>0.026+</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.015)</td>
</tr>
<tr>
<td>Observations</td>
<td>11930</td>
<td>3988</td>
<td>3988</td>
<td>3972</td>
<td>3970</td>
</tr>
</tbody>
</table>

Panel B: Impartial Trials

<table>
<thead>
<tr>
<th></th>
<th>(1) All data</th>
<th>(2) Free focus</th>
<th>(3) Free focus</th>
<th>(4) Mer. Focus</th>
<th>(5) Out. Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
</tr>
<tr>
<td>Advantaged</td>
<td>0.28*</td>
<td>0.24</td>
<td>0.34</td>
<td>0.082</td>
<td>0.46**</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
<td>(0.29)</td>
<td>(0.32)</td>
<td>(0.17)</td>
<td>(0.18)</td>
</tr>
<tr>
<td>Round</td>
<td>-0.013</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.0095)</td>
</tr>
<tr>
<td>Advantaged * Round</td>
<td>-0.0096</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.014)</td>
</tr>
<tr>
<td>Observations</td>
<td>11923</td>
<td>3984</td>
<td>3984</td>
<td>3975</td>
<td>3964</td>
</tr>
</tbody>
</table>

All models are linear regressions with the dependent variable the difference in number of boxes open containing merit and outcome information. Standard errors clustered by participant in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. List of controls: task type (4 categories), age, gender (man, woman, other), political affiliation (5 categories), education (6 categories), income (7 categories), continent (4 categories).
Table 12: Differences in Attention - Last Fixation.

<table>
<thead>
<tr>
<th>Panel A: Involved Trials</th>
<th>(1) All data</th>
<th>(2) Free focus</th>
<th>(3) Free focus</th>
<th>(4) Mer. Focus</th>
<th>(5) Out. Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
</tr>
<tr>
<td>Advantaged</td>
<td>0.038&lt;sup&gt;+&lt;/sup&gt;</td>
<td>0.031</td>
<td>0.0040</td>
<td>0.0084</td>
<td>0.084&lt;sup&gt;+&lt;/sup&gt;</td>
</tr>
<tr>
<td>(0.021)</td>
<td>(0.032)</td>
<td>(0.043)</td>
<td>(0.033)</td>
<td>(0.033)</td>
<td></td>
</tr>
<tr>
<td>Round</td>
<td>-0.0013</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.0020)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Advantaged * Round</td>
<td>0.0026</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.0029)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>11791</td>
<td>3953</td>
<td>3953</td>
<td>3909</td>
<td>3929</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Impartial Trials</th>
<th>(1) All data</th>
<th>(2) Free focus</th>
<th>(3) Free focus</th>
<th>(4) Mer. Focus</th>
<th>(5) Out. Focus</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
<td>Δ Boxes</td>
</tr>
<tr>
<td>Advantaged</td>
<td>0.041&lt;sup&gt;+&lt;/sup&gt;</td>
<td>0.031</td>
<td>0.0041</td>
<td>0.015</td>
<td>0.11&lt;sup&gt;**&lt;/sup&gt;</td>
</tr>
<tr>
<td>(0.023)</td>
<td>(0.032)</td>
<td>(0.043)</td>
<td>(0.036)</td>
<td>(0.040)</td>
<td></td>
</tr>
<tr>
<td>Round</td>
<td>-0.0010</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.0020)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Advantaged * Round</td>
<td>0.0025</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.0028)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>11729</td>
<td>3949</td>
<td>3949</td>
<td>3887</td>
<td>3893</td>
</tr>
</tbody>
</table>

All models are linear regressions with the dependent variable equal to 1 if the last fixation in on Outcome information. Standard errors clustered by participant in parentheses. + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. List of controls: task type (4 categories), age, gender (man, woman, other), political affiliation (5 categories), education (6 categories), income (7 categories), continent (4 categories).
Figure 11: The relationship between attention (final fixation) and allocations

(a) Involved Trials

Illustration of the influence of attention (final fixation on merit vs. outcome information) on allocations, split by Status and Focus treatment. 95% confidence intervals are plotted.

(b) Impartial Trials
A.4 Within subjects variation of attention restrictions

Table 13: Percentage given to the advantaged conditional of the trial and focus type

<table>
<thead>
<tr>
<th>Involved Trials</th>
<th>(1) % given Adv.</th>
<th>(2) % given Adv.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Advantaged</td>
<td>10.4*** [7.89,13.0]</td>
<td>10.4*** [7.89,13.0]</td>
</tr>
<tr>
<td>Outcome Focus</td>
<td>2.80* [0.34,5.27]</td>
<td>2.91* [0.37,5.44]</td>
</tr>
<tr>
<td>Outcome Trials</td>
<td>0.17 [-0.67,1.01]</td>
<td></td>
</tr>
<tr>
<td>Self Trials</td>
<td>0.97+ [-0.036,1.99]</td>
<td></td>
</tr>
<tr>
<td>Other Trials</td>
<td>-0.062 [-1.07,0.94]</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>7942</td>
<td>7942</td>
</tr>
</tbody>
</table>

The dependent variable is the percentage of the pie allocated to the Advantaged member of the pair. Standard errors clustered by participant. 95% confidence interval in brackets. + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. All models are linear regressions. The second columns controls for the round level attention manipulation. Baseline: Merit Trials where we encouraged participants to look at the merit information. In the Outcome Trials participants were encouraged to look at the Outcome information; in the Self Trials they were encouraged to look at the information about their-own monetary contribution and correct answers; in the Other Trials they were encouraged to look at the information about the other player’s monetary contribution and correct answers. List of controls: Share of correct answers coming from the advantaged member over the total number of correct answers of the pair, task type (4 categories), age, gender (man, woman, other), political affiliation (5 categories), education (6 categories), income (7 categories), continent (4 categories).
B Preregistration
1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What’s the main question being asked or hypothesis being tested in this study?
We study the origin of self-serving biases in monetary allocation problems. If people are randomly placed in a (dis)advantaged position, how does this affect their attention to meritocratic information, the ethical criteria for making decisions, and the subsequent allocation choices? Detailed hypotheses are specified in point 5).

3) Describe the key dependent variable(s) specifying how they will be measured.
In Part 1 of the experiment, subjects first produce a surplus together with a matched partner on several tasks. We create variation in contribution to the surplus by randomly giving one of the partners a higher piece rate than the other. In Part 2 of the experiment, some subjects are given information on the performance on the tasks as well as the total contribution, and make allocation decisions in the role of dictator. We use Mouselab to track the way subjects explore information about task performance.

Per every decision of the dictator we record:
- the split in the total surplus between dictator and recipient.
- dwelling time (mousetracked) on each of the following information 1) the dictator & recipient contribution to the pie in monetary terms, 2) the number of answers in the task the dictator & recipient got correct.

4) How many and which conditions will participants be assigned to?
Subjects are assigned to be “receivers” and “dictators”. Both groups take part in a series of performance tasks to determine the surplus. We are mostly interested in the dictators.

All dictators are assigned to one of two treatments:
Advantaged: receives a high piece rate per correct answer in the task.
Disadvantaged: receives a low piece rate per correct answer in the task.

Each dictator participates (in this order) in an
Involved condition: 20 allocations between themselves and another randomly matched participant
Benevolent condition: 20 allocations between two other participants.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
Hypothesis 1 (Behavior): In the involved condition, advantaged dictators give less money to the receivers than disadvantaged dictators.

We test this hypothesis with a non-parametric rank sum test. We will perform regressions to control for subject characteristics with standard errors clustered for each participant.

Hypothesis 2 (Attention): In the involved condition, advantaged dictators spend relatively less time on correct answer information and more time on monetary contribution information than disadvantaged dictators.
Across dictator groups, we investigate total time looking at information as well the proportion of time spent looking at correct answers, using a non-parametric rank sum test. We will also perform regressions with standard errors clustered for each participant.

Hypothesis 3 (Persistence): The effects documented in 1) and 2) persist in the benevolent condition.
The tests are the same as for Hypothesis 1 and 2, but now in the benevolent condition. We will also compare the effects in both conditions using a difference in difference approach.

Hypothesis 4 (Role of attention): Attention patterns drive giving decisions.
For correlational evidence, we use regressions to investigate how sensitive the treatment effect (Hypothesis 1) is to controlling for total and relative looking time. For a causal inference, we use an instrumental variable analysis to exploit variation generated by the (randomly varied) orientation of patterns on the
6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
Following standard Mouselab protocols, we will exclude information that was revealed for less than 200 ms.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
We will recruit 200 dictators from the online platform Prolific. These are divided 50-50 between the advantaged and disadvantaged condition. We recruit the corresponding number of recipients.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
We will conduct a number of secondary analyses:
- We will compare by treatment the fairness criteria people list in the questionnaire as being most socially appropriate.
- We compare by treatment the fairness “types” based on Cappelen et al. (2007), and correlate these types with attentional patterns.
- Correlate attention, behavior and political preferences elicited in the final questionnaire.
In addition, we will explore additional measures of attention, and their explanatory power for giving decisions. We will conduct robustness analysis on the revelation threshold in point 6).
1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What’s the main question being asked or hypothesis being tested in this study?
We study the origin of self-serving biases in monetary allocation problems. If people are randomly placed in a (dis)advantaged position, how does this affect their attention to meritocratic information, the ethical criteria for making decisions, and the subsequent allocation choices? In a previous version of the experiment, we showed that advantaged dictators pay less attention to information that reveals pure merit (actual task performance). In this experiment we ask how randomly induced variations in attention affect decision making.

3) Describe the key dependent variable(s) specifying how they will be measured.
In Part 1 of the experiment, subjects first produce a surplus together with a matched partner on several tasks. We create variation in contribution to the surplus by randomly giving one of the partners a higher piece rate than the other. In Part 2 of the experiment, some subjects are given information on the performance on the tasks as well as the total contribution, and make allocation decisions in the role of dictator. We manipulate how long different kinds of information are available to people.

Per every decision of the dictator we record:
- the split in the total surplus between dictator and recipient.
- dwelling time (mouse tracked) on each of the following information 1) the dictator & recipient contribution to the pie in monetary terms, 2) the number of answers in the task the dictator & recipient got correct.

4) How many and which conditions will participants be assigned to?
Subjects are assigned to be “receivers” and “dictators”. Both groups take part in a series of performance tasks to determine the surplus. We are mostly interested in the dictators.

All dictators are assigned to one of two treatments:
Advantaged: receives a high piece rate per correct answer in the task.
Disadvantaged: receives a low piece rate per correct answer in the task.

We cross-randomize these treatments with another dimension:
Merit focus: in a majority of trials, the information about task performance (merit) is available longer.
Output focus: in a majority of trials, information about total contribution to surplus is available longer.

Each dictator participates (in this order) in an
Involved condition: 20 allocations between themselves and another randomly matched participant
Benevolent condition: 20 allocations between two other participants

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
We test two main hypotheses for both the involved and the benevolent dictators:
1) Dictators in the “Merit Focus” treatment will give more to disadvantaged recipients.
We will test this in a regression with data for all trials and a dummy for all trials with Merit Focus, as well as controls for subject and trial characteristics.

2) Compared to a situation with freely chosen attention, making dictators look longer at “inconvenient” information (i.e. “Merit focus” for advantaged dictators, “Output focus” for disadvantaged dictators) will reduce the relative bias of advantaged dictators towards the advantaged recipients.
We combine the data from this experiment with a previous experiment in which dictators could freely choose what to look at. We will use regressions to evaluate the “difference in difference”.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
Following standard Mouselab protocols, we will exclude information that was revealed for less than 200 ms.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
We will recruit 400 dictators from the Prolific platform. Dictators will be evenly split between the 4 between subject conditions (i.e. 100 in each cell). We recruit a corresponding number of receivers.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
We will investigate whether the impact of merit/output information on giving differs between advantaged and disadvantaged dictators.
We will correlate giving and attention with several additional elicitations in the questionnaire on perceptions of fairness.
C Instructions

The instructions for the dictators in the experiment are shown below, together with the comprehension questions. The instructions were presented in several decks of slides. Participants could move across slides clicking on two buttons on the sides of the screen. Comprehension questions were presented on a separate page. Participants could move back to the instruction from the page with the comprehension questions.

C.1 Day 1

Welcome!
This is a study conducted by researchers at the University of Amsterdam.

All the tasks in this study are not mobile-compatible.
You can only participate with a desktop or laptop.

This study consists of two sessions:
- Session 1 lasts about 34 minutes
- Session 2 lasts about 55 minutes

The total reward for completing the two sessions is £9:
- £2.85 for session 1
- £6.15 for session 2

You need to complete both sessions for your submission to be approved and paid.
You must complete Session 2 between Tuesday 24th November at 1.00 pm (CET) and Wednesday 25th November at 10.00 pm (CET).
We will send you a reminder on Prolific when Session 2 will be open.
On top of the base earnings, in this study you can earn a **bonus.**

The amount of the bonus may depend on:

- Your **performance**
- Your **decisions** or the decisions of another participant
- **Luck**

The Ethics Committee Economics and Business (EBEC) of the University of Amsterdam has approved our study (EC 20200214090248). You can contact our Ethics Committee writing to secbs-abs@uva.nl.

To receive the approval we committed **not** to use misleading or untruthful instructions.

---

Please answer the questions on the next webpage.
Comprehension questions:

1. Your bonus might depend on the decisions taken by another participant. T/F [correct: T]

2. You can complete this study using a mobile device. T/F [F]

3. According to the ethical protocol under which we run this study, all the instructions you read must be truthful and not misleading. T/F [T]

4. You need to complete both sessions of this study for your submission to be approved. T/F [T]
Instructions for Session 1

In this session, you will do 8 tasks.
In each task you will have to answer several questions.

Within each task, every correct answer gives the same monetary reward.
However, different tasks give different monetary rewards per correct answer.

There are two possible reward levels, high and low.

We will split participants into two groups:
50% in the High Reward Group 50% in the Low Reward Group

What is the difference?
Participants in the High Reward Group will always receive the High Reward per correct answer.
Participants in the Low Reward Group will always receive the Low Reward per correct answer.
Luck decides if you are in the High or Low Reward Group.

The computer determines your group randomly. You have 50% chance to be in either group.

In each task, you are matched with a different participant.

After each task, the reward that both you and the other participant earned flows into a common account.

At a later stage, we will explain how the money in this account is paid out to the members of the pair.

If the computer assigns you to the High Reward Group, then you will be in the High Reward Group for the whole study.

Similarly, if the computer assigns you to the Low Reward Group, then you will be in the Low Reward Group for the whole study.

You will know if you are in the High or Low Reward Group only at a later stage.

Please answer the questions on the next webpage.
1. In this study you have to complete BLANK tasks. [8]

2. There are 3 groups of participants. T/F [F]

3. Luck determines if you are in the High Reward Group or in the Low Reward Group. T/F [T]

4. In some tasks, you will be in the High Reward Group, in others you will be in the Low Reward Group. T/F [T]

**Tasks 1 and 2**
You need to move a slider to a predetermined value. 
You have **2 minutes** to move as many sliders as you can.

- **Incorrect slider**
  - Set the slider to 234
- **Correct slider**
  - Set the slider to 324

![Slider](image1)

You get a reward per each slider you set to the correct value.
- **High Reward:** £0.30
- **Low Reward:** £0.10

**Tasks 3 and 4**
You need to answer a quiz. 
You have **4 minutes** to answer as many questions as you can.

![Quiz](image2)

You get a reward per each correct answer.
- **High Reward:** £0.90
- **Low Reward:** £0.30

**Tasks 5 and 6**
You need to count zeros in a table. 
You have **3 minutes** to count the zeros in as many tables as you can.

![Table](image3)

You get a reward each time you correctly state the number of zeros.
- **High Reward:** £0.30
- **Low Reward:** £0.10

**Tasks 7 and 8**
You need to find the missing tile in a figure. 
You have **3 minutes** to complete as many figures as you can.

![Figure](image4)

You get a reward each time you select the correct tile.
- **High Reward:** £0.60
- **Low Reward:** £0.20
The slides with the task instruction appeared before the relevant pair of tasks. To continue to the task, the participants had to correctly input the two possible pay-rates for the task.
C.2 Day 2

Comprehension questions:

1. I confirm that I am using a laptop or desktop. Y/N [Yes]

2. Your performance on the tasks in Session 1 carries over into Session 2. T/F [True]

3. We commit to providing entirely accurate and truthful information in all aspects of this study. T/F [True]
Welcome!
This is a study conducted by researchers at the University of Amsterdam.

Universiteit van Amsterdam

All the tasks in this study are not mobile-compatible.
You can only participate with a desk- or laptop, preferably with a mouse instead of a mousepad.

The Ethics Committee Economics and Business (EBEC) of the University of Amsterdam has approved our study. You can contact our Ethics Committee writing to secbs-abs@uva.nl.

To receive the approval we committed not to use misleading or untruthful instructions.

Universiteit van Amsterdam

In Session 1, you completed a series of tasks online and you earned a reward for each correct answer.

Moving Sliders Task
Counting Zeros Task
Quiz Questions Task
Missing Tiles Task

Welcome to Session 2 of this study.
This session consists of three parts that will last for approximately 55 minutes.
The second row shows the instructions for Advantaged participants, whereas Disadvantaged participants saw HIGH and LOW switched across slides. Disadvantaged participants were instructed that they were assigned the LOW, and the other participants the HIGH, reward per correct answer.

Comprehension questions:

1. Which reward condition were you and the other participants you are matched with assigned to? MULTIPLE CHOICE [Advantaged: You: High reward, Others: Low reward; Disadvantaged: You: Low reward, Others: High reward;]

2. What determines the common account on each round? MULTIPLE CHOICE [The combined amount you and the other participant earned on a single task from Session 1]

3. If Part 1 determines the bonus, how will you be paid? MULTIPLE CHOICE [The amount you gave yourself on a randomly selected round from Part 1]

4. If Part 1 determines the bonus, how will the other participant you are matched with be
In every round, the earnings from both participants on one of the tasks are combined into a common account.

Your earnings + The other's earnings = The Common Account

Your task in Part 1 is to divide these common accounts between yourself and the other participants. Your decision on one of the rounds might determine the bonus that you and the other participant receive at the end.

Your final bonus is based on one randomly selected decision in Part 1 or Part 2. If your bonus comes from Part 1, one of the rounds will be randomly selected by the computer.

You and the other participant will earn the bonus determined by your division of the common account in that round.

You will be paid the money you allocated for yourself, and the other participant will receive the money you allocated to them. Consider each decision carefully, as any of them could be selected for payment.

paid? MULTIPLE CHOICE [The amount you gave them on a randomly selected round from Part 1]
Before you decide on the division of the common account, we will show you some additional information.

On the same page, you will also see the total earnings in the common account that you will divide, as in the example below:

<table>
<thead>
<tr>
<th>Task: Counting Zeros</th>
<th>Common account: £10</th>
</tr>
</thead>
<tbody>
<tr>
<td>![Counting Zeros Image]</td>
<td>![Common Account Image]</td>
</tr>
</tbody>
</table>

Next, you have the chance to learn how both participants contributed to the common account. There are two kinds of information:

- **Correct answers**: the number of correct answers you and the other participant each got on the task.

- **Monetary contribution**: the earnings you and the other participant each contributed on the task. This depends on the correct answers and the reward (high or low):
  \[
  \text{correct answers} \times \text{reward per correct answer}
  \]
Two examples of different information orientations. We used all 8 possible orientations of participant and contribution information between subjects, evenly divided across subjects accounting for Advantaged status and Focus treatment. Each subject only saw one orientation to allow them to develop information-seeking patterns.
The information boxes are available for 6 seconds.

Within this time limit, you can decide which and how many boxes to open. Boxes can be opened more than once.

At times, the program might close some boxes. If this happens, you can't open those boxes again in that round.

In the first practice round, you can familiarize yourself with the layout of the information boxes for as long as you want.

In summary:
- In 20 rounds you will divide a common account earned by yourself and another participant.
- Each participants' contribution depends on the correct answers and the reward per correct answer on a single task.
- Before the division, you can inform yourself about correct answers and monetary contributions by hovering your cursor over information boxes.
- Any round could be chosen for payment at the end of the session.

You will have the opportunity to practice with these boxes on the next page.

The information boxes are filled with a placeholder number.

In the actual rounds, the information will be based on your performance in Session 1.

This practice will not count toward your bonus.

In the second practice round, you will familiarize yourself with the timing of the information boxes.

They will be presented for 6 seconds, like in the actual rounds.

In the first shown slide, the last paragraph "At times, the program might close some boxes" was only included in the constrained Focus treatments and left out in the Free focus treatment.
Comprehension questions:

1. On the information screen, what does "correct answers" refer to? MULTIPLE CHOICE [The number of answers you and the other participant each got correct on that task]

2. On the information screen, what does "monetary contribution" refer to? MULTIPLE CHOICE [The earnings (correct answers X reward rate) you and the other participant each contributed to the common account on that task]

Comprehension questions:

1. Which reward condition were Player High and Player Low assigned to? MULTIPLE CHOICE [Player High: High reward, Player Low: Low reward]

2. If Part 2 determines the bonus, how will you be paid? MULTIPLE CHOICE [A set 1 pound bonus]

3. If Part 2 determines the bonus, how will Player High and Player Low be paid? MULTIPLE CHOICE [The amount you gave to each of them on a randomly selected round from Part 2]
In row 2, the right slide switched the information about Players High and Low for Disadvantaged participants so Player Low was described first. The last slide showing the orientation of information varied based on the participant’s information orientation. Here, the orientation matched that of Involved trials such that for Advantaged players, Player High’s information was in the same row or column as Self information, and Player Low’s information was in the same row or column as Self information for Disadvantaged players.
You will complete 20 rounds of divisions.

For every round, you will make the decision for a different pair of players.

As a reminder:

Your bonus will be based on randomly selected decision from either Part 1 or Part 2.

If the computer selects one of your decisions from Part 2,

Player High and Player Low from a randomly selected round will be paid according to your decision.

You will receive a fixed bonus of £1.

Consider each decision carefully as any of them could be randomly selected for payment towards other participants at the end.
Part 3
In Part 3 we will ask you several questions.
At the end of the session, the computer will select one question from Part 3 at random.
In addition to your bonus from Part 1 or Part 2, you can win a bonus depending on your answer to this question.
We will give you more precise instructions about the bonus as you proceed through Part 3

Part 3.1
In the questions below, please give us your best estimate.
You will earn £1 if you are within 5% of the correct answer.

Elicitation questions

1. We selected a random task from Session 1 of the experiment and compared the task performance of 100 members of the HIGH group with the task performance of 100 members of the LOW group. The monetary earnings each person contributed is measured as the number of correct answers in the task times the reward rate. Remember that the reward rate per correct answer was higher in the HIGH group than in the LOW group.

   In how many of these 100 comparisons do you think that the member of the LOW group produced a larger monetary contribution than members of the HIGH group?

2. In Part 1, you were matched with 20 different participants and saw information on both your task performance and the task performance of the matched participants.
In how many of these 20 rounds did the participant you were matched with answered more question correctly than you did? 20

\[20\]

\[20\] We asked these two questions only in the Constrained Focus treatments.
Elicitation questions How did you decide how to split the common account? [OPEN QUESTION]

According to your moral values, how would you judge the following ways of splitting the common account?[21]

1. Giving to each participant the monetary contribution he/she produced in Session 1
2. Giving an equal amount to each participant
3. Splitting the account considering only the number of correct answers of each participant in Session 1
4. Keeping all for oneself

[Possible answers: Very morally inappropriate, Somewhat morally inappropriate, Somewhat morally appropriate, Very morally appropriate]

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[21] The order of the norms questions is randomized at the individual level and it is consistent across the different elicitation screen. That is if a participant sees the questions in the order meritocratic, libertarian, egalitarian in the screen about the moral norms, this order is preserved in the following screens as well.
Part 3.3

**Socially appropriate behavior** refers to an action that is “correct”, “fair”, or “ethical” according to most participants.

You will now have to judge whether the behavior described in some statements is **socially appropriate**.

Those statements are the same you read in the previous webpage.

**Bonus**

If the computer selects one question from Part 3.3 for payment,

- We will check how participants that split the common account judged the social appropriateness of the behavior described in the question.
- You will win an additional £1 if your judgment coincides with the most common judgment.

For example, you will see a question like this:

1) How do you judge the following behavior?

“I split the common account giving to each participant the monetary contribution he/she produced in Session 1.”

- Very socially inappropriate
- Somewhat socially inappropriate
- Somewhat socially appropriate
- Very socially appropriate

You will win a £1 bonus if you pick the answer that is selected with the highest frequency by the other participants.

Please answer the questions on the next webpage.
Comprehension questions

1. For socially appropriate we mean an action that: MULTIPLE CHOICE [Cost people will find "correct", "fair", or "ethical"]

2. If a question from Part 3.2 is selected for payment, you earn a bonus of £BLANK if you: MULTIPLE CHOICE [pick the answer that is selected with the highest frequency by the other participants that divided the common account.]

Elicitation questions:
Are the following ways of splitting the common account socially appropriate? Remember to select the answer you think is most common.

1. Giving to each participant the monetary contribution he/she produced in Session 1

2. Giving an equal amount to each participant

3. Splitting the account considering only the number of correct answers of each participant in Session 1

[Possible answers: Very socially inappropriate, Somewhat socially inappropriate, Somewhat socially appropriate, Very socially appropriate]
Part 3.4

For the next questions you will have to guess how a group of participants judged some behavior.

The groups that you will have to consider are:

- participants that a) received a **low reward** per each correct answer in Session 1 and b) that split the common account in Session 2
- participants that a) received a **high reward** per each correct answer in Session 1 and b) that split the common account in Session 2

**Bonus**

As before, if the computer selects one question from Part 3.4 for payment:

- We will check which is the most common judgment among the group specified by the question.
- You will win an additional £1 if guessed what is the most common answer in that group.

For example, you will see a question like this:

Consider the group of participants that a) received a low reward per correct answer in Session 1 and b) split the common account in Session 2

1) How do you think most of participants in this group judged the statement below?

- I split the common account giving to each participant the monetary contribution he/she produced in Session 1.

  - [ ] Very socially inappropriate
  - [ ] Somewhat socially inappropriate
  - [ ] Somewhat socially appropriate
  - [ ] Very socially appropriate

You will win a £1 if you guess the most common judgment among the group described in the question.

Please answer the questions on the next webpage.
Comprehension questions In Part 3.3 you will have to guess the way most participants in some groups judged a statement. Among the groups below, tick all the ones you will have to consider.

- A group composed of participants that a) received a low reward per correct answer in Session 1 and b) split the common account in Session 2 [Correct]
- A group composed of participants that a) received a high reward per correct answer in Session 1 and b) split the common account in Session 2 [Correct]
- A group composed of participants that a) received a low reward per correct answer in Session 1 and b) did not split the common account in Session 2
- A group composed of participants that a) received a high reward per correct answer in Session 1 and b) did not split the common account in Session 2

Elicitation questions

Consider the group of participants that a) received a HIGH REWARD per correct answer in Session 1 and b) split the common account in Session 2

How do you think most of participants in this group judged the following ways of splitting the common account?

1. Giving to each participant the monetary contribution he/she produced in Session 1
2. Giving an equal amount to each participant
3. Splitting the account considering only the number of correct answers of each participant in Session 1

Now, consider the group of participants that a) received a LOW REWARD per correct answer in Session 1 and b) split the common account in Session 2

How do you think most of participants in this group judged the following ways of splitting the common account?

1. Giving to each participant the monetary contribution he/she produced in Session 1
2. Giving an equal amount to each participant
3. Splitting the account considering only the number of correct answers of each participant in Session 1