

Amasino, Dianna; Pace, Davide; van der Weele, Joël J.

Working Paper

Fair shares and selective attention

Tinbergen Institute Discussion Paper, No. TI 2021-066/I

Provided in Cooperation with:

Tinbergen Institute, Amsterdam and Rotterdam

Suggested Citation: Amasino, Dianna; Pace, Davide; van der Weele, Joël J. (2021) : Fair shares and selective attention, Tinbergen Institute Discussion Paper, No. TI 2021-066/I, Tinbergen Institute, Amsterdam and Rotterdam

This Version is available at:

<https://hdl.handle.net/10419/248750>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

TI 2021-066/I
Tinbergen Institute Discussion Paper

Fair Shares and Selective Attention

Revision November 2021

Dianna R. Amasino¹

Davide D. Pace²

Joël van der Weele²

¹ University of Amsterdam

² University of Amsterdam, Tinbergen Institute

Tinbergen Institute is the graduate school and research institute in economics of Erasmus University Rotterdam, the University of Amsterdam and Vrije Universiteit Amsterdam.

Contact: discussionpapers@tinbergen.nl

More TI discussion papers can be downloaded at <https://www.tinbergen.nl>

Tinbergen Institute has two locations:

Tinbergen Institute Amsterdam
Gustav Mahlerplein 117
1082 MS Amsterdam
The Netherlands
Tel.: +31(0)20 598 4580

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. Pace^{1,2}, and Joël van der Weele^{1,2}

¹University of Amsterdam, Amsterdam School of Economics

²Tinbergen Institute

November 19, 2021

[Link to the latest version](#)

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 40%. The evidence is consistent with a habit formation effect of attention in fairness decisions. 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.

*We thank Vinska Johan Talita and Antonia Kurz for excellent research assistance, Jan Hausfeld, Kosuke Imai, Margarita Leib, Caroline Liqui Lung, Joep Sonnemans, Ivan Soraperra, Jan Stoop, and Egon Tripodi as well as seminar participants at the University of Amsterdam and the University of Pittsburgh, the Virtual Process Tracing conference, and the BBU flash talks workshop for very useful comments. Joël van der Weele gratefully acknowledges funding by the NWO in the context of VIDI grant 452-17-004.

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).

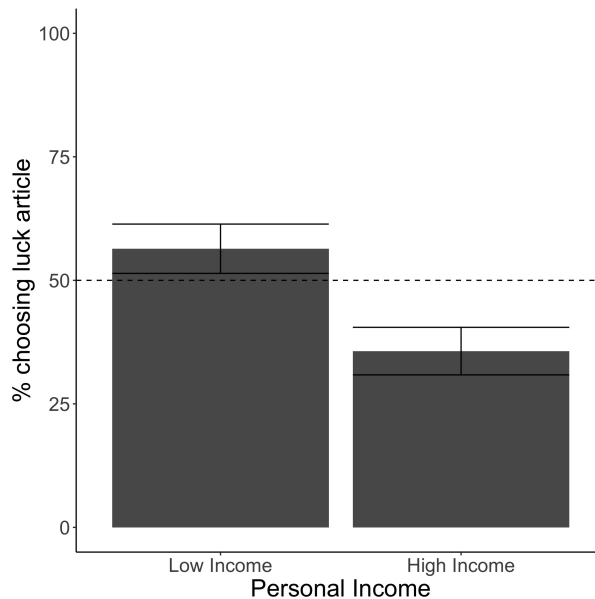
These diverging views about the origin of economic success may result in political conflict and fuel the rise of populism. For instance, Sandel (2020) maintains that resentment at the bottom of the income distribution derives from systemic advantages for elites dressed up as meritocracy. Gethin et al. (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 $\tau = -0.306$, $p < 0.001$).¹

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 $< \pounds 10,000$, and High Income as $> \pounds 70,000$. 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 with

¹Details about the implementation and outcomes of the survey are in Appendix A.1.

Disadvantaged status, in a sequence of allocation tasks. In the “Involved” condition, the dictator is one of the participants who generated the surplus. In the subsequent “Impartial” trials, the dictator divides the surplus generated by two other participants.

Before dictators make their allocations, we measure their visual attention to the sources of the surplus. 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), and 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).

We focus on visual attention or “dwell time” because it is key locus of competition for attention, and because salient contextual elements that affect gaze patterns have been shown to affect choice (Krajbich, 2019; Orquin and Mueller Loose, 2013; Bordalo et al., 2021). To understand the (causal) role of dwell time in dictator’s decisions, 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, enabling participants to pay more attention to the merit or outcome.

The 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. They also allocate more to other Advantaged recipients in the Impartial trials where dictator’s own income is not at stake, replicating results from Konow (2000). This result indicates that the experience of economic advantage changes allocation behavior beyond narrow self-interest.

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. We can rule out experimenter demand effects or processing errors as psychological mechanisms behind these results. Instead, the evidence suggests that attention causes subjects to form decision making habits that carry over to the impartial trials.

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 several strands of literature. First, we contribute to a behavioral literature on the role of merit in redistribution. A number of laboratory experiments shows that participants 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). Almås 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 attention to merit and luck is endogenous, and has a causal effect on the allocation of an economic surplus.

Second, we contribute to an understanding of well-documented self-serving biases in redistribution. In particular, the seminal paper by Konow (2000) 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 highlight-

ing 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. Our paper opens the box by focusing on the role of attention, opening new channels for policy interventions.

Third, our focus on attention contributes to a fast-growing literature on the role of attention in economic decisions. In particular, our approach relates to multiple attentional mechanisms that interact during choice (surveyed in Engelmann et al., 2021; Fisher, 2021). First, goals and preferences can direct “top-down” attention to the more highly-valued options during choice. We expand this literature to look at redistributive decisions, showing how economically advantaged decision makers look at information that is more “convenient”. Second, attention can also be captured in a “bottom-up” manner, where the “salience” of contextual elements affects attention and decisions. This approach has been modeled to explain various deviations of economic rationality (Shimojo et al., 2003; Bordalo et al., 2012, 2021). In the choice literature, top-down attention is understood to drive a large part of choice, but bottom-up salience and random fluctuations in attention have also been found to matter, especially for choices where participants are near indifference (Milosavljevic et al., 2012; Smith and Krajbich, 2018). For these more difficult choices, relative dwell time on options or attributes can impact choice (Krajbich et al., 2012; Konovalov and Krajbich, 2016; Fisher, 2021; Pärnamets et al., 2015; Mullett and Stewart, 2016; Smith and Krajbich, 2019). Finally, a newer area of research suggests that attentional history or habits can drive future attention, but its role in complex choice tasks has just started to be explored (Theeuwes, 2019; Jiang and Sisk, 2019; Gwinn et al., 2019). Our study contributes to this literature, by showing that manipulating dwell time affects monetary allocations in self-other and other-other decisions.

Finally, we relate to a small but growing literature on the role of attention in 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). Ghaffari and Fiedler (2018) look at the causal, bottom-up effect of attention. Replicating and extending Pärnamets et al. (2015), they 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. Other results have shown correlations of attention with loss-framing Fiedler and Hillenbrand (2020) and in-group bias (Rahal et al., 2020; Fischbacher et al., 2021) in social dilemmas.

Our approach differs from the empirical studies cited above, and all attention-tracing studies in this domain that we are aware of. Instead of measuring attention to the payoffs in an economic game, we study attention to the *determinants* of economic production and show how this affects distributive decisions. Thus, it is one of the first papers to link attentional processes with the reasoning behind fairness judgments, elucidating the origins of (self-serving) fairness views. The most closely related paper to this endeavor is 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

The study consists of two orthogonal treatment dimensions, leading to a 3×2 design, with 100 decision-makers (dictators) in each cell, as outlined in Table 1. The data were gathered in two experiments. Experiment 1 generated the data for the Free Focus treatment. It aims to a) replicate previous findings on the relationship between economic status and attitudes toward redistribution and b) establish a causal relationship between economic status and attention. Experiment 2 generated data for the Merit and Outcome Focus treatments, and allows us to c) investigate the causal relationship between attention and attitudes towards redistribution.

Table 1: Overview of treatments and number of dictators

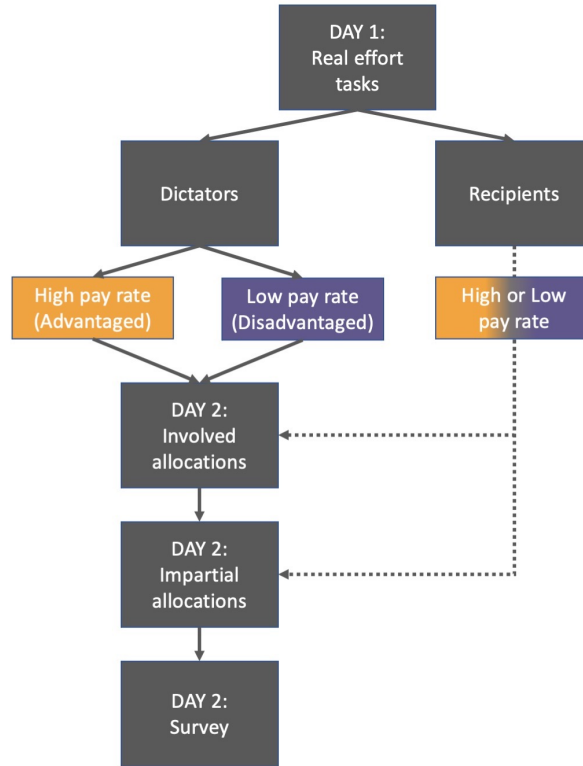
Privilege Status	Attention		
	Free Focus	Merit Focus	Outcome Focus
Advantaged	100	100	100
Disadvantaged	100	100	100

Overview of the treatments in our 3×2 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.

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.² 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.

Figure 2: Timeline for Day 1 and 2 for Both Experiments.



3.1 Day 1: Surplus Generation

On Day 1, participants completed 8 sets of real effort tasks. In each task set, participants had a limited time period to complete as many tasks as possible. 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. The 8 task sets were evenly split among the different task types. In every task set, each correct answer earned a monetary reward. When completing the task sets, 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

²We recruited more recipients than dictators because in the Impartial trials the dictators split the amount generated by two recipients.

stage. The high pay rate was always 3 times the low pay rate, but pay rates were calibrated (based on pilot data ³) according to task type to result in an average surplus of £3.5 per task set.

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, which they had to get correct to continue with the study. 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.

We informed participants about the two possible pay-rates and about the existence of the common account to provide incentives for exerting effort and, at the same time, be transparent at all stages of the study. Transparency is especially important towards the recipients as they would not continue to Day 2. Since all participants were given the same information and were not informed of their pay rate at this stage, the information should not affect participants differentially.

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 task sets 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. This allows us to investigate attention to merit and luck

³The pilot included 50 dictators with only allocation behavior (no attention data) and was collected February, 2020.

information in situations where luck differentially affected members of the pair, mimicking the uneven distribution of luck in real life. Trials were created such that dictators outperformed recipients on 50% to make sure that the effects of pay rate and relative performance were not confounded. 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 to 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). This order was chosen deliberately: putting the Involved trials first gives subjects experience of their economic status. This mirrors situations outside the laboratory where people have a lifetime of experience in their economic roles. The status in the Involved condition thus functions as an experimental treatment to investigate the bleed-over of fairness rules and attentional habits into impartial decisions.

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.⁴

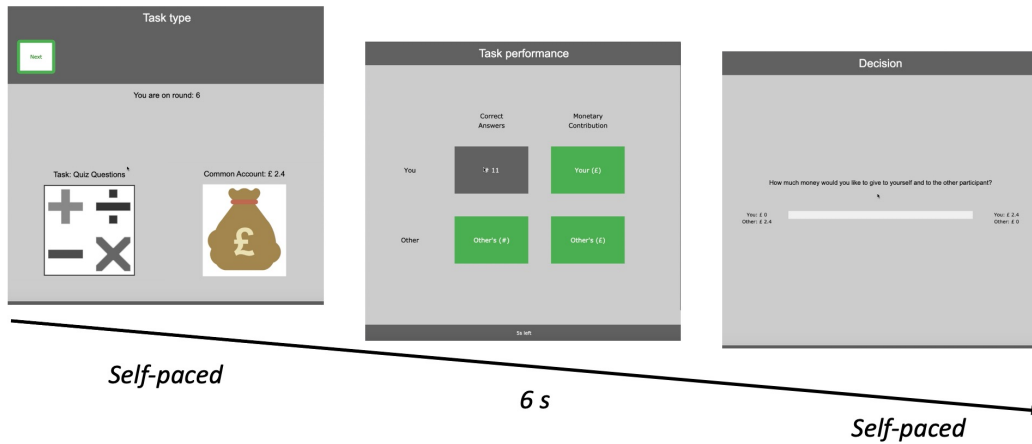
3.3 Attention Measurement

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 task sets 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

⁴We 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.

monetary contribution of each member of the pair to the account - outcome information. Merit and outcome information were chosen as they correspond directly to meritocratic and libertarian fairness criteria, respectively, which are relevant for dictator decision-making (Cappelen et al., 2007; Rodriguez-Lara and Moreno-Garrido, 2012).⁵ 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 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 type 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.

⁵In particular, outcome information is reflective of information typically available outside of the lab as it incorporates both merit and luck. Merit information isolates the role of merit and separates it from luck. This information is typically not easily available in real life, but can sometimes be obtained with some effort.

3.4 Focus Treatments

We implemented three “Focus” treatments that varied the time different types of information could be accessed. In 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. The 6 s time limit was chosen to control for the overall information-gathering period across participants so that differences in attention would be meaningfully comparable. Furthermore, the limit pushes participants to prioritize gathering information that they find relevant and meaningful which also reduces the obligation to reveal or explore all information. Finally, the time limit introduces in the experiment the tight attentional constraints that permeate real life (Gabaix, 2018). The 6 seconds limit is in line with prior research that uses limits as low as 3 s for decisions with 2 pieces of information to understand the impact of attention on choice, doubled to 6 s for 4 pieces of information (Ghaffari and Fiedler, 2018).

The Constrained Focus treatments limited the time participants could see particular information, building on prior work manipulating attention (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. The total maximum of 4 s spent on box information was chosen to closely match the average time spent on information from the Free Focus experiment, which was 3.8 s. The 400 ms constraint was chosen because information can still be processed and remembered for later use at this timing, whereas timings of 200 ms or lower may be actually restrictive for recognition (DiCarlo et al., 2012; Potter, 1976). Prior attention manipulations have used minimum dwell times of 250 ms and 300 ms (Armel et al., 2008; Pärnamets et al., 2015; Pachur et al., 2018; Fisher, 2021).

Participants are not required to look at any information: they can choose the sequence and which information to reveal, some information is simply available for a longer time if participants choose to reveal it for longer. Boxes could still be opened 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.

Demand effects and trial-by-trial restrictions. Experimenter demand effects may arise when certain information is made more salient or more readily available, as participants may infer that this information is more “important”. To obfuscate the nature of the restrictions and counter such effects, we implemented our main treatment in 14 of the 20 trials in each condition. In the remaining six trials, restrictions were placed on orthogonal box dimensions.⁶ Across Involved and Impartial trials and Focus treatments, the order in which the trials with different restrictions appeared were randomized at the individual level.

Our obfuscation strategy was successful, as only a small minority of subjects could identify the box restrictions they faced during the experiment (see Section 6.2). In addition, the contrast between the within-subject trial-by-trial changes in restrictions and the sustained between-subject treatment changes allows us to better understand the mechanisms of the attention manipulation (see Section 6.3).

3.5 Surveys

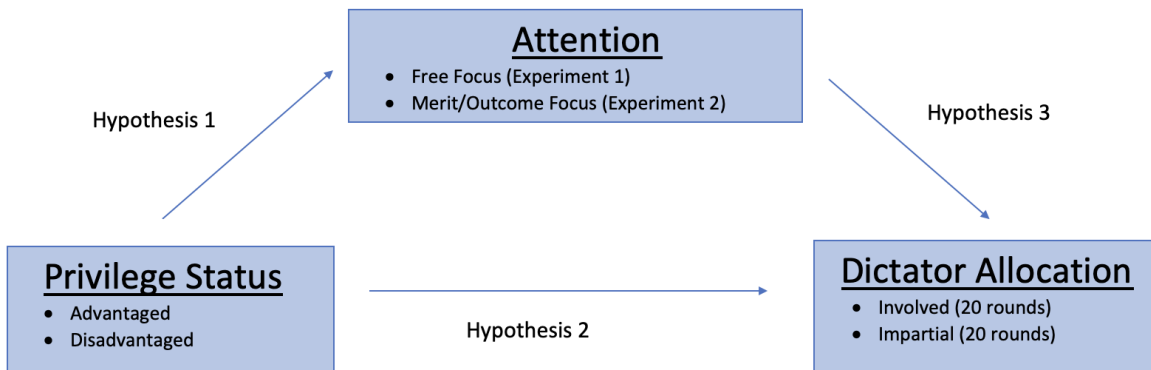
After both experiments, we asked dictators a series of questions about their strategy, their perceptions of various fairness criteria, and their demographics. We asked participants an open-ended question about how they chose to make their allocations. We also asked them to rate the moral appropriateness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

⁶For instance, in the “Merit Focus” treatment, 14 of the 20 decisions restricted outcome information to 400 ms and merit information to 1600 ms. This enabled participants to look longer 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). In contrast, in the “Outcome Focus” treatment, 14 trials restricted merit information to 400 ms, while the remaining 6 trials split the 400 ms restrictions evenly between the other information dimensions.

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, Disadvantaged dictators may find more justifications in ignoring the luck component in outcome and focusing on merit information, which is purely effort-based. This 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 hypothesis relates to the causal role of attention on behavior, which we address using our attention manipulations in Experiment 2. We expect that increasing the dwell time on merit relative to outcome will lead to a reduction in giving to Advantaged participants. This hypothesis depends on a large body of literature, reviewed in Section 2 showing that merit is an important criterion in redistribution, and that exogenous changes in salience or dwell time can affect choice.

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 compared to the Outcome Focus manipulation.*

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 or attentional habits 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 “ Δ Attention”, 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 fully process information (Willemsen and Johnson, 2019; Pachur et al., 2018; DiCarlo et al., 2012). 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 set in Experiment 1 and 13.5 in Experiment 2, suggesting that participants put effort in completing the tasks in both experiments.

Table 2: Summary Statistics

Panel A: Involved Trials							
		Free Focus		Merit Focus		Outcome Focus	
		Adv.	Dis.	Adv.	Dis.	Adv.	Dis.
<i>Allocation</i>	% given to Adv.	61.5%	50.4%	59.1%	48.3%	64.1%	48.4%
	% given to self	61.5%	49.6%	59.1%	51.7%	64.1%	51.6%
<i>Attention</i>	Merit Info (s)	1.68	1.81	1.41	1.33	0.86	0.88
	Outcome Info (s)	2.12	1.90	0.91	0.85	1.56	1.40
	Δ Attention (s)	-0.44	-0.093	0.50	0.48	-0.70	-0.52
	Observations	1995	1993	1986	1986	1986	1984
Panel B: Impartial Trials							
		Free Focus		Merit Focus		Outcome Focus	
		Adv.	Dis.	Adv.	Dis.	Adv.	Dis.
<i>Allocation</i>	% given to Adv.	56.3%	52.0%	54.4%	52.5%	56.5%	52.1%
	<i>Attention</i>						
	Merit Info (s)	1.96	2.08	1.52	1.36	0.82	0.90
	Outcome Info (s)	1.90	1.57	0.79	0.68	1.30	1.12
	Δ Attention (s)	0.07	0.51	0.73	0.69	-0.48	-0.22
	Observations	1994	1990	1987	1988	1978	1986

Table 2 summarizes the means of the most important outcome variables.⁷ 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 2. Fewer than 5% of dictators kept the entire surplus for themselves, in accordance with previous findings that dictators respect earned income (Cappelen et al., 2010; Rodriguez-Lara and Moreno-Garrido, 2012; Cherry et al., 2002).

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).⁸

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

⁷Each 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.

⁸We collapse across self and other boxes to focus only on merit and outcome information because these are our variables of interest as described in our hypotheses. Furthermore, there is evidence that participants look at information in an attribute-wise manner, comparing merit for self and other or outcome for self and other. The Payne Index (the proportion of option-wise (self-other) transitions minus attribute-wise transitions (merit-outcome)) indicated the frequency of comparison types, with a Payne Index of 1 indicating only option-wise comparisons and a Payne Index of -1 indicating only attribute-wise comparisons. We find consistently negative Payne Indices across experiments and decision types: Free Focus Involved = -0.43; Constrained Focus Involved = -0.45; Free Focus Impartial = -0.49; Constrained Focus Impartial = -0.53, supporting a focus on attributes in the analyses.

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.

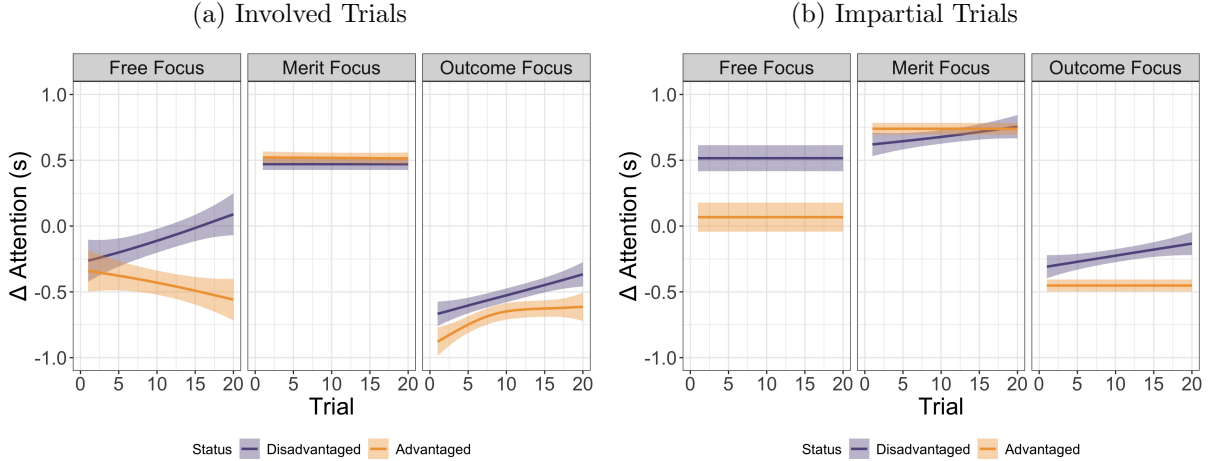
5.1 Status and Attention

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 Δ 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 Δ 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 Δ Attention. A two-sided Wilcoxon rank-sum test of average individual dwell times over the 20 trials confirms that the 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. This may indicate the formation of habits, an issue we discuss in Section 6.3.⁹ The impact of status is 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 $p = 0.45$).

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

⁹The 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.

Figure 5: The Dynamics of Attention by Treatment.



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

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 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*

Table 3: Differences in Attention.

Panel A: Involved Trials					
	(1) All data Δ Attention	(2) Free Focus Δ Attention	(3) Free Focus Δ Attention	(4) Mer. Focus Δ Attention	(5) Out. Focus Δ Attention
Advantaged	-0.15* (0.074)	-0.31 ⁺ (0.17)	0.069 (0.18)	0.043 (0.069)	-0.16** (0.059)
Trial number			0.021* (0.0099)		
Advantaged * Trial number			-0.036** (0.013)		
Observations	11930	3988	3988	3972	3970
Panel B: Impartial Trials					
	All data Δ Attention	Free Focus Δ Attention	Free Focus Δ Attention	Mer. Focus Δ Attention	Out. Focus Δ Attention
Advantaged	-0.21* (0.10)	-0.37 (0.25)	-0.43 (0.26)	0.047 (0.097)	-0.25** (0.092)
Trial number			0.0036 (0.0083)		
Advantaged * Trial number			0.0051 (0.012)		
Observations	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. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. 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).

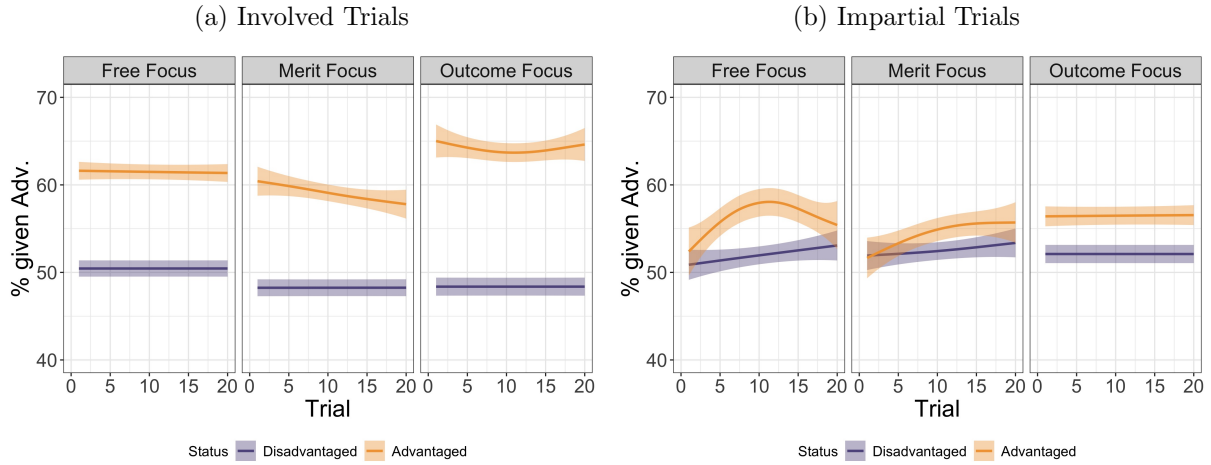
a smaller fraction of the time looking at merit information compared to Disadvantaged dictators, an effect that develops over multiple trials. This difference in attention by status persists in the Impartial treatments, although it is less precisely estimated.

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.



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.

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

Table 4: Differences in Share Given to Advantaged

Panel A: Involved Trials					
	(1)	(2)	(3)	(4)	(5)
	All data % to Adv.	All data % Kept	Free focus % to Adv.	Mer. Focus % to Adv.	Out. Focus % to Adv.
Advantaged	10.0*** (1.00)	10.4*** (1.02)	9.05*** (1.61)	7.97*** (1.66)	12.9*** (1.78)
Observations	11930	11930	3988	3972	3970
Panel B: Impartial Trials					
	All data % to Adv.		Free focus % to Adv.	Mer. Focus % to Adv.	Out. Focus % to Adv.
Advantaged	3.44*** (0.70)		3.94** (1.28)	1.27 (1.17)	4.80*** (1.15)
Observations	11923		3984	3975	3964

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).

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 overall 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 differences in the attention and 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 relates to 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 ($p < 0.001$).

Figure 7: The Relation Between Attention and Allocation.

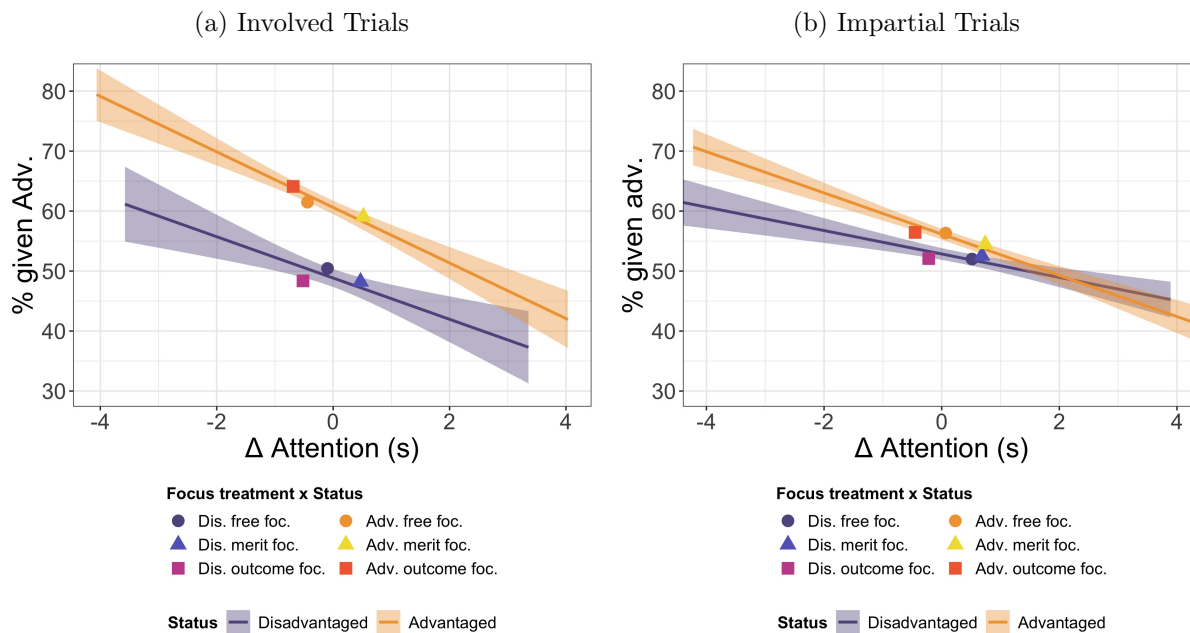


Illustration of the influence of attention on allocations, split by advantaged status. The lines include data from both Free Focus and Constrained Focus treatments and are linearly smoothed with shaded 95% confidence bands. The mean attention and allocations are split by information condition.

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 economically large.¹⁰ 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. These dictators reduce their allocation to the Disadvantaged by almost five percentage points in the Outcome Focus treatment, or 0.58 of a standard deviation - a substantial effect. The difference between the coefficients in Columns (2) and (3) is statistically significant at the 10% level ($p = 0.054$, Wald test).¹¹

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 variable analysis using a two stage least squares regression pooling the data at the subject level¹². 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.¹³

¹⁰A 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 13. 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.

¹¹This 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.

¹²Pooling 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.

¹³ 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 (Krajbich et al., 2010). We can also exclude that our restrictions have a demand effect - see Section 6.2.

Table 5: The Effect of Attention on Allocation.

Panel A: Involved Trials, Constrained Focus only						
	(1)	(2)	(3)	(4)	(5)	(6)
	All data % to Adv.	Disadvantaged % to Adv.	Advantaged % to Adv.	All data % to Adv.	Disadvantaged % to Adv.	Advantaged % to Adv.
Outcome Focus	2.93* (1.36)	0.26 (1.94)	4.96*** (1.47)			
Δ Attention				-2.63* (1.21)	-0.089 (1.91)	-4.08*** (1.16)
Observations	7942	3970	3972	400	200	200
F-statistic - first stage				552	182	407
Panel B: Impartial Trials, Constrained Focus only						
	(1)	(2)	(3)	(4)	(5)	(6)
	All data % to Adv.	Disadvantaged % to Adv.	Advantaged % to Adv.	All data % to Adv.	Disadvantaged % to Adv.	Advantaged % to Adv.
Outcome Focus	0.82 (0.84)	-0.73 (1.11)	1.88 (1.16)			
Δ Attention				-0.77 (0.78)	0.82 (1.25)	-1.54+ (0.91)
Observations	7939	3974	3965	400	200	200
F-statistic - first stage				244	84	159

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 the second stage regression, we use this exogenously-induced variation in attention to explain dictator allocations. Column (4) of Table 5 shows that increasing Δ 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 Δ 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.¹⁴ Relative to total attention, reallocating 500 ms implies a shift in attention equivalent to 23% of the average dwell

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 (Imbens and Angrist, 1994) is satisfied in our setting because would-be defiers have no way to alter the time restrictions on a box in a given round.

¹⁴The similarity is reflected in the fact that the absolute value of the coefficients in columns (1) and (4) of Table 5 are close to each other.

time in the Constrained Focus treatment.

To compute by how much a change in Δ Attention reduces the effect of Status on allocation, we repeat the IV analysis separately for the Advantaged and Disadvantaged dictators. Columns (5) and (6) of Table 5 report the results of this analysis. Increasing Δ Attention by one second cuts the share that Advantaged dictators give to Advantaged recipients by 4.1 percentage points ($p < 0.001$). By contrast, it cuts the share that Disadvantaged dictators give to Advantaged recipients only by 0.1 percentage points, a negligible and insignificant effect. Taking the difference between these two point estimates, we calculate that changing Δ Attention by one second reduces the gap between the allocation of Advantaged and Disadvantaged dictators by 4 percentage points ($p = 0.081$).

We conclude that reallocating 500 ms (or 23%) of dwell time from Outcome to Merit information reduces the effect of Status on Allocation found in Table 4 by 40%.

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. The results go in the same direction as for the Involved trials, but they are not statistically significant. Similarly, the effect does not differ significantly between Advantaged and Disadvantaged dictators ($p = 0.10$, Wald test). Column (6) of Table 5 shows the only result significant at the 10% level: increasing Δ attention by one second cuts the share that Advantaged dictators give to Advantaged recipients by 1.5 percentage points ($p = 0.093$). As for the involved trials, increasing Δ Attention has a small effect and not significant effect on the behavior of Disadvantaged dictators. If we combine the effects on the Advantaged and Disadvantage dictators we find that the effect of Status on Allocation goes down by 2.4 percentage points or 69% the effect of Status on Allocation found in Table 4 ($p = 0.055$).

Overall, these results do not show clear evidence a causal effect of attention in the Impartial condition, although there may be an effect among the Advantaged dictators, which we don't have the power to detect. In Section 6.1 we show stronger evidence for an effect of attention in

the impartial trials when we consider adherence to different fairness criteria.

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 by about 40% the impact of Status on allocations. Effects are smaller in the Impartial trials and not statistically significant.*

6 Discussion

In this section, we discuss the interpretation of our results. First, we show the impact of attention on adherence to fairness criteria. We then discuss and rule out potential confounds including experimenter demand effects and processing errors. Finally, we look at robustness to other attentional measures, and study information avoidance and the mediating effect of attention on self-serving bias.

6.1 Do the Treatments Affect Fairness Concerns?

Why do our treatments affect the behavior of dictators? One possibility is that they change how subjects perceive and internalize normative fairness criteria, as suggested in Konow (2000). For instance, Advantaged dictators may self-servingly downplay the normative attraction of an egalitarian split. Similarly, subjects who can look relatively longer at merit information, may be more likely to consider this information ethically relevant for their allocation.

To investigate this idea, we look at dictator adherence to three criteria that are often invoked in the fairness literature (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. The latter two criteria depend explicitly on information about the task performance of both participants in the pair.

Table 6 presents the results of linear probability models to test adherence to these norms. The outcome variable in the three panels is the consistency with the respective fairness criteria. 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. Defined in this way, 20% of the choices are Egalitarian, 35% are Meritocratic, and 23% are Libertarian.¹⁵ We only use data from the Impartial condition: Since self-interest is removed in these trials, the remaining differences between Advantaged and Disadvantaged dictators may reflect differences in fairness views.

Status and Fairness. The first column in each panel shows the effect of the Status treatment on adherence to difference fairness criteria. Advantaged Dictators are 6.3 percentage points less likely to adhere to the Egalitarian criterion (Panel A), 3.7 percentage points less likely to act in line with the Meritocratic criterion (Panel B) and 10 percentage points more likely to act in line with the Libertarian criterion (Panel C). The effect is both statistically and quantitatively significant for Egalitarian and Libertarian criteria.

The direction of these estimates support the idea that dictators adopt self-serving views of fairness. To further corroborate this finding, we turn to independent measures of fairness perceptions in the closing questionnaire. We asked participants in both experiments about their endorsement of the three fairness norms we outlined in the previous subsection. We asked for both personal evaluations (“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. We also asked dictators about their perceptions of relative performance vis-a-vis the receiver. For reasons of space, we leave a detailed examination of these variables to a companion paper (Amasino et al., 2021). There, we report that status changes norms and beliefs and that changes in norms and beliefs can explain about a quarter of the variation in dictator giving in Impartial trials. This suggests that changes in fairness views play a role, but also that other mechanisms must be at work.

Attention and Fairness. We now turn to the effect of our attention manipulations on the adherence to different fairness criteria. Column (2) of Table 6 quantifies the impact of the Outcome Focus compared to the Merit Focus treatment, the omitted category. It shows no statistically significant effect of the attention treatments in any of the panels, although the signs of the coefficients are in the expected direction. However, this may be due to a null-effect among disadvantaged dictators, as Table 5 shows that they are unresponsive to the treatment.

¹⁵For 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. 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 6: Probability of Making a Choice Consistent with a Fairness Criterion

Panel A: Egalitarian criterion			
	(1) All Data Egalitarian	(2) Constrained Focus Egalitarian	(3) Constrained Focus Egalitarian
Advantaged	-0.063*** (0.019)		-0.098** (0.032)
Outcome Focus		0.013 (0.024)	-0.030 (0.035)
Adv. * Out. Focus			0.088+ (0.047)
Observations	11923	7939	7939
Panel B: Meritocratic criterion			
	All Data Meritocratic	Constrained Focus Meritocratic	Constrained Focus Meritocratic
Advantaged	-0.037+ (0.021)		0.037 (0.036)
Outcome Focus		-0.027 (0.025)	0.043 (0.035)
Adv. * Out. Focus			-0.14** (0.049)
Observations	11923	7939	7939
Panel C: Libertarian criterion			
	All Data Libertarian	Constrained Focus Libertarian	Constrained Focus Libertarian
Advantaged	0.10*** (0.020)		0.038 (0.031)
Outcome Focus		0.038 (0.024)	-0.022 (0.027)
Adv. * Out. Focus			0.12* (0.047)
Observations	11923	7939	7939

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).

In Column (3), we therefore look at the interaction of the Status and Attention treatments. The estimates on the coefficients for the Advantaged dictators show that they are highly responsive to the attention manipulations: Outcome treatment makes them 12 percentage points less likely to act in line with Meritocratic, and 14 percentage points more likely to act in line with Libertarian criteria. Panel A shows that Advantaged dictators adhere slightly more to the Egalitarian criterion in the Outcome Focus treatment. This last result is counterintuitive, but because the Egalitarian criterion does not require information about the surplus, it is not clear what to expect of attentional nudges in this case.

In the questionnaire data, we find no clear patterns of the effect of the attention treatments on perceptions of norms and relative performance. This suggests that the effects of attention cannot be (fully) explained by their impact on the internalization of different fairness norms. An alternative explanation is that attention causes subjects in the Involved condition to create “habits” of behavior that are in line with particular (self-serving) fairness criteria (Theeuwes, 2019; Jiang and Sisk, 2019). This explains the patterns in Figure 5, where attention patterns in the Free Focus treatment develop over trials in the Involved condition, and persists in the impartial trials. In this interpretation, the attention manipulation strengthens or facilitates such attentional habits. In Section 6.3, we provide further evidence for this interpretation. Future research may disentangle the precise psychological pathways of attention.

Result 4. *When self-interest is removed, Advantaged dictators are more likely to act in line with a self-serving Libertarian fairness criterion. The Outcome Focus treatment causes a further shift towards this criterion. Independent measures of norms and beliefs cannot fully explain these shifts, indicating that other pathways like habit formation may be at work.*

6.2 Experimenter Demand Effects

During the design phase of the experiment, we worried that our attention manipulations might give subjects a feeling that some information was deemed more important, inducing experimenter-demand effects. To counter this and obfuscate the research goal, 6 of the twenty decision rounds featured attention manipulations that were orthogonal to that of the treatment, as described in Section 3.3. In addition, our questionnaire featured several questions about the perceived goal of the experiment and the perceived direction of the attention restrictions.

The final questionnaire clearly shows that demand effects are not an issue: on a free form question, *none* out of 400 dictators indicated that the box timing was a purpose of the experiment. Moreover, it appears our obfuscation strategy was successful: when asked explicitly

whether they perceived a difference in the timing closing of boxes, 60% of participants said they did not detect a systematic difference in box closing times. Overall, only 20% guessed the restrictions on both boxes correctly, a further 5.5% guessed one box correctly, and 8.5% guessed entirely wrongly.

To see if demand effects may have played a role, we test Hypothesis 3 using the same regressions as before, but restricting our sample to the 60% of participants who did not detect any difference in closing time. Table A.5 in the appendix provides the results of this analysis. We replicate our finding that attention changes allocation decisions. If anything the results are *stronger* in this sub-sample: shifting 500ms of dwell time from Outcome to Merit information reduces the effect of Status on allocation by 47% (Column 4). This further demonstrates that experimenter demand effects did not drive our results.

6.3 Dwell Time Restrictions and Processing Errors

Our attention treatments were designed to measure the impact of the length of time subjects engage with information, while preserving subjects' possibility to process each source of information. To this end, we dropped dwell time on boxes that were opened for less than 200 ms, as literature suggest this may be too short for subjects to fully process and use the information (DiCarlo et al., 2012). Moreover, the attention recognition literature suggests that recognition and memory consolidation for more complex scenes only takes up to 400 ms, and other processing studies have used dwell times of 250 ms or mouselab box times of 300 ms (Potter, 1976; Potter et al., 2014; Armel et al., 2008; Milosavljevic et al., 2012; Pärnamets et al., 2015; Pachur et al., 2018; Ghaffari and Fiedler, 2018; Fisher, 2021). Therefore, 400 ms is well-within the recognized time-window for processing a single piece of information.

In addition, there are several ways our data can identify potential processing errors. First, Disadvantaged dictators do not change their allocations with the different attention restrictions (see Tables 5 and 6). For instance, we do not see that the Outcome Focus treatment leads disadvantaged subjects to adhere less to Meritocratic and more to Libertarian and/or Egalitarian criteria. This result shows that subjects are able to choose the same information-based allocations under any type of restriction and speaks against the restrictions a having a mechanical effect on allocation.

Second, we can exploit within-subject variation in dwell time restrictions. Recall that in every attention treatment, the attention restrictions on one type of information were implemented only in 14 out of the 20 rounds. In the remaining six rounds, the restrictions were randomly allocated

to other dimensions (see Section 6.2). Thus, if the restrictions affected dictators' allocations through processing errors, we should see a difference between the 14 treatment-congruent trials with the 6 remaining trials. For instance, we should see that Advantaged subjects in the Outcome Focus treatment are more generous in the remaining 6 trials where merit information was less restricted.

Appendix Table A.6 shows the results of regressions that include trial-by-trial dummies of dwell time restrictions (Self, Other, Merit, Outcome), in addition to our main treatment dummy. We find that the type of trial has no statistically or quantitatively meaningful impact on behavior beyond our main treatment. Furthermore, the effect of the Outcome Focus treatment on allocation does not go down once we control for trial type.

These considerations show that the attention manipulation did not prevent subjects from making any particular allocation. Of course, dwell times may affect the ease with which subjects can incorporate information into decisions, but this is exactly the point of studying this variable in the first place. The results of the within-subject variation in restrictions also show that attention in any single trial does not have a strong influence on behavior. Rather, the results are consistent with a "habit-forming" effect of attention mentioned in Section 6.1, whereby a sustained and repeated dwell times on certain types of information affect fairness decisions.

6.4 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 7: Avoidance of Merit and Outcome Information

Panel A: Involved Trials						
	Free Focus		Merit Focus		Outcome Focus	
	Adv.	Dis.	Adv.	Dis.	Adv.	Dis.
Merit avoidance	4.2%	4.9%	1.3%	6.6%	2.7%	2.4%
Outcome avoidance	5.5%	3.7%	3.5%	5.5%	1.9%	5.7%
Panel B: Impartial Trials						
	Free Focus		Merit Focus		Outcome Focus	
	Adv.	Dis.	Adv.	Dis.	Adv.	Dis.
Merit avoidance	5.4%	6.1%	2.6%	7.0%	7.7%	2.3%
Outcome avoidance	14.0%	15.8%	13.9%	18.8%	11.1%	16.9%

To see whether information avoidance is common, Table 7, 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.¹⁶ 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.

In the Impartial trials, Panel B of Table 7, 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.

6.5 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; Rahal and Fiedler, 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 A.1 and Figure A.2.

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

¹⁶As 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.

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 A.3).

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. There 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.

6.6 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.¹⁷ 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 analysis. 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 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

¹⁷As noted in Footnote 13, these assumptions are likely to be satisfied in our setting.

Table 8: Causal Mediation Analysis

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

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) .

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 demonstrate the causal impact of dwell time 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. In particular, we show that in such settings, attentional shifts cause more libertarian allocations among Advantaged dictators. As underlying psychological mechanisms, we can rule out experimenter demand effects and processing errors, and find suggestive evidence that the formation of attentional habits affects allocations.

These findings help explain the emergence of the self-serving views of inequality and fairness in the experiment. 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 similarly (dis)advantaged individuals.

Extrapolating beyond the laboratory, selective attention may explain why groups have differ-

ent 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). 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.

Quantitatively, the effect of attention on decisions in the experiment is substantial, and reduces self-serving bias by a meaningful amount. This provides a promising base for further research on 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 the order of 7 pounds. It is plausible that more sustained exposure and/or higher amounts will lead to larger behavioral effects. 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

- Almås, Ingvild, Alexander W. Cappelen, and Bertil Tungodden**, “Cutthroat Capitalism versus Cuddly Socialism: Are Americans More Meritocratic and Efficiency-Seeking than Scandinavians?,” *Journal of Political Economy*, May 2020, *128* (5), 1753–1788.
- Amasino, Diana, Davide D. Pace, and Joël J. van der Weele**, “Self-serving Bias in Redistributive Decisions: The Role of Social Norms and Motivated Beliefs,” Technical Report 2021.
- Armel, K. Carrie, Aurelie Beaumel, and Antonio Rangel**, “Biasing simple choices by manipulating relative visual attention,” *Judgment and Decision making*, 2008, *3* (5), 396–403.
- Bieleke, Maik, David Dohmen, and Peter M. Gollwitzer**, “Effects of social value orientation (SVO) and decision mode on controlled information acquisition—A Mouselab perspective,” *Journal of Experimental Social Psychology*, 2020, *86*, 103896.
- Bordalo, Pedro, Nicola Gennaioli, and Andrei Shleifer**, “Salience in Experimental Tests of the Endowment Effect,” *American Economic Review*, May 2012, *102* (3), 47–52.
- , —, and —, “Salience,” Working Paper 29274, National Bureau of Economic Research September 2021.
- Bortolotti, Stefania, Ivan Soraperra, Matthias Sutter, and Claudia Zoller**, “Too Lucky to Be True - Fairness Views Under the Shadow of Cheating,” SSRN Scholarly Paper ID 3014734, Social Science Research Network, Rochester, NY July 2017.
- Buser, Thomas, Gianluca Grimalda, Louis Putterman, and Joël van der Weele**, “Overconfidence and gender gaps in redistributive preferences: Cross-Country experimental evidence,” *Journal of Economic Behavior & Organization*, October 2020, *178*, 267–286.
- Bénabou, Roland and Jean Tirole**, “Mindful economics: The production, consumption, and value of beliefs,” *Journal of Economic Perspectives*, 2016, *30* (3), 141–64.
- Cappelen, Alexander W., Astri Drange Hole, Erik Ø Sørensen, and Bertil Tungodden**, “The pluralism of fairness ideals: An experimental approach,” *American Economic Review*, 2007, *97* (3), 818–827.
- , **Erik Ø. Sørensen, and Bertil Tungodden**, “Responsibility for what? Fairness and individual responsibility,” *European Economic Review*, April 2010, *54* (3), 429–441.

- , **James Konow, Erik Ø Sørensen, and Bertil Tungodden**, “Just luck: An experimental study of risk-taking and fairness,” *American Economic Review*, 2013, *103* (4), 1398–1413.
- , **Karl O. Moene, Siv-Elisabeth Skjelbred, and Bertil Tungodden**, “The Merit Primacy Effect,” SSRN Scholarly Paper ID 2963504, Social Science Research Network, Rochester, NY April 2017.
- Cherry, Todd L, Peter Frykblom, and Jason F Shogren**, “Hardnose the dictator,” *American Economic Review*, 2002, *92* (4), 1218–1221.
- Cohn, Alain, Lasse J. Jessen, Marko Klasnja, and Paul Smeets**, “Why Do the Rich Oppose Redistribution? An Experiment with America’s Top 5%,” SSRN Scholarly Paper ID 3395213, Social Science Research Network, Rochester, NY May 2019.
- Dana, Jason, Roberto A. Weber, and Jason Xi Kuang**, “Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness,” *Economic Theory*, 2007, *33* (1), 67–80.
- Deffains, Bruno, Romain Espinosa, and Christian Thöni**, “Political self-serving bias and redistribution,” *Journal of Public Economics*, February 2016, *134*, 67–74.
- DiCarlo, James J., Davide Zoccolan, and Nicole C. Rust**, “How Does the Brain Solve Visual Object Recognition?,” *Neuron*, February 2012, *73* (3), 415–434.
- Durante, Ruben, Louis Putterman, and Joël van der Weele**, “Preferences for Redistribution and Perception of Fairness: An Experimental Study,” *Journal of the European Economic Association*, August 2014, *12* (4), 1059–1086.
- Engelmann, Jan, Alejandro Hirmas, and Joel J van der Weele**, “Top Down or Bottom Up? Disentangling the Channels of Attention in Risky Choice,” 2021.
- Espinosa, Romain, Bruno Deffains, and Christian Thöni**, “Debiasing preferences over redistribution: an experiment,” *Social Choice and Welfare*, December 2020, *55* (4), 823–843.
- Fiedler, Susann and Adrian Hillenbrand**, “Gain-loss framing in interdependent choice,” *Games and Economic Behavior*, 2020, *121*, 232–251.
- , **Andreas Glöckner, Andreas Nicklisch, and Stephan Dickert**, “Social Value Orientation and information search in social dilemmas: An eye-tracking analysis,” *Organizational Behavior and Human Decision Processes*, March 2013, *120* (2), 272–284.

- Fischbacher, Urs, David Grammling, and Jan Hausfeld**, “Redistribution beyond equality and status quo-heterogeneous societies in the lab,” Technical Report, Thurgauer Wirtschaftsinstitut, Universität Konstanz 2021.
- , **Jan Hausfeld, and Baiba Renerte**, “Strategic incentives undermine gaze as a signal of prosocial motives,” Technical Report, Thurgauer Wirtschaftsinstitut, Universität Konstanz 2020.
- Fisher, Geoffrey**, “Intertemporal choices are causally influenced by fluctuations in visual attention,” *Management Science*, 2021.
- Gabaix, Xavier**, “Behavioral Inattention,” 2018.
- Gethin, Amory, Clara Martínez-Toledano, and Thomas Piketty**, “Brahmin Left versus Merchant Right: Changing Political Cleavages in 21 Western Democracies, 1948-2020,” 2021.
- Ghaffari, Minou and Susann Fiedler**, “The Power of Attention: Using Eye Gaze to Predict Other-Regarding and Moral Choices,” *Psychological Science*, November 2018, *29* (11), 1878–1889.
- Goodman, Leo A.**, “On the exact variance of products,” *Journal of the American statistical association*, 1960, *55* (292), 708–713.
- Grossman, Zachary and Joël J Van der Weele**, “Self-image and willful ignorance in social decisions,” *Journal of the European Economic Association*, 2017, *15* (1), 173–217.
- Gwinn, Rachael, Andrew B Leber, and Ian Krajbich**, “The spillover effects of attentional learning on value-based choice,” *Cognition*, 2019, *182*, 294–306.
- Haerpfer, Christian, Ronald Inglehart, A. Moreno, Christian Welzel, K. Kizilova, J. Diez-Medrano, M. Lagos, P. Norris, E. Ponarin, and B. Puranen**, “World values survey: round seven–country-pooled datafile,” *Madrid, Spain & Vienna, Austria: JD Systems Institute & WVSA Secretariat*, 2020.
- Hvidberg, Kristoffer B., Claus Kreiner, and Stefanie Stantcheva**, “Social Position and Fairness Views,” Technical Report, National Bureau of Economic Research 2020.
- Imai, Kosuke, Dustin Tingley, and Teppei Yamamoto**, “Experimental designs for identifying causal mechanisms,” *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 2013, *176* (1), 5–51.

- , **Luke Keele, and Teppei Yamamoto**, “Identification, Inference and Sensitivity Analysis for Causal Mediation Effects,” *Statistical Science*, 2010, *25* (1), 51–71.
- Imbens, Guido W. and Joshua D. Angrist**, “Identification and Estimation of Local Average Treatment Effects,” *Econometrica*, 1994, *62* (2), 467–475.
- ISSP**, “2019 Social Inequality V, Final questionnaire,” 2018.
- Jiang, Yuhong V and Caitlin A Sisk**, “Habit-like attention,” *Current opinion in psychology*, 2019, *29*, 65–70.
- Konovalov, Arkady and Ian Krajbich**, “Gaze data reveal distinct choice processes underlying model-based and model-free reinforcement learning,” *Nature communications*, 2016, *7* (1), 1–11.
- Konow, James**, “Fair shares: Accountability and cognitive dissonance in allocation decisions,” *American economic review*, 2000, *90* (4), 1072–1091.
- Krajbich, Ian**, “Accounting for attention in sequential sampling models of decision making,” *Current opinion in psychology*, 2019, *29*, 6–11.
- , **Carrie Armel, and Antonio Rangel**, “Visual fixations and the computation and comparison of value in simple choice,” *Nature neuroscience*, 2010, *13* (10), 1292.
- , **Dingchao Lu, Colin Camerer, and Antonio Rangel**, “The attentional drift-diffusion model extends to simple purchasing decisions,” *Frontiers in psychology*, 2012, *3*, 193.
- Krawczyk, Michał**, “A glimpse through the veil of ignorance: Equality of opportunity and support for redistribution,” *Journal of Public Economics*, February 2010, *94* (1), 131–141.
- Krupka, Erin L. and Roberto A. Weber**, “Identifying Social Norms Using Coordination Games: Why Does Dictator Game Sharing Vary?,” *Journal of the European Economic Association*, June 2013, *11* (3), 495–524.
- Kunda, Ziva**, “The case for motivated reasoning.,” *Psychological bulletin*, 1990, *108* (3), 480.
- Lefgren, Lars J., David P. Sims, and Olga B. Stoddard**, “Effort, luck, and voting for redistribution,” *Journal of Public Economics*, November 2016, *143*, 89–97.

- Milosavljevic, Milica, Vidhya Navalpakkam, Christof Koch, and Antonio Rangel,** “Relative visual saliency differences induce sizable bias in consumer choice,” *Journal of Consumer Psychology*, 2012, *22* (1), 67–74.
- Mullett, Timothy L and Neil Stewart,** “Implications of visual attention phenomena for models of preferential choice.,” *Decision*, 2016, *3* (4), 231.
- Orquin, Jacob L. and Simone Mueller Loose,** “Attention and choice: A review on eye movements in decision making,” *Acta Psychologica*, September 2013, *144* (1), 190–206.
- Ostling, Robert,** “Economic Influences on Moral Values,” *The B.E. Journal of Economic Analysis & Policy*, January 2009, *9* (1).
- Pachur, Thorsten, Michael Schulte-Mecklenbeck, Ryan O. Murphy, and Ralph Hertwig,** “Prospect theory reflects selective allocation of attention,” *Journal of Experimental Psychology: General*, 2018, *147* (2), 147–169.
- Piff, Paul K., Dylan Wiwad, Angela R. Robinson, Lara B. Aknin, Brett Mercier, and Azim Shariff,** “Shifting attributions for poverty motivates opposition to inequality and enhances egalitarianism,” *Nature Human Behaviour*, May 2020, *4* (5), 496–505.
- Potter, Mary C.,** “Short-term conceptual memory for pictures.,” *Journal of experimental psychology: human learning and memory*, 1976, *2* (5), 509.
- Potter, Mary C, Brad Wyble, Carl Erick Hagmann, and Emily S McCourt,** “Detecting meaning in RSVP at 13 ms per picture,” *Attention, Perception, & Psychophysics*, 2014, *76* (2), 270–279.
- Pärnamets, Philip, Petter Johansson, Lars Hall, Christian Balkenius, Michael J. Spivey, and Daniel C. Richardson,** “Biasing moral decisions by exploiting the dynamics of eye gaze,” *Proceedings of the National Academy of Sciences*, 2015, *112* (13), 4170–4175.
- Rahal, Rima-Maria and Susann Fiedler,** “Understanding cognitive and affective mechanisms in social psychology through eye-tracking,” *Journal of Experimental Social Psychology*, 2019, *85*, 103842.
- , – , 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.

- Rodriguez-Lara, Ismael and Luis Moreno-Garrido**, “Self-interest and fairness: self-serving choices of justice principles,” *Experimental Economics*, 2012, 15 (1), 158–175.
- Sandel, Michael J.**, *The Tyranny of Merit: What’s become of the common good?*, Allen Lane London, 2020.
- Shimojo, Shinsuke, Claudiu Simion, Eiko Shimojo, and Christian Scheier**, “Gaze bias both reflects and influences preference,” *Nature Neuroscience*, December 2003, 6 (12), 1317–1322.
- Smith, Stephanie M and Ian Krajbich**, “Attention and choice across domains.,” *Journal of Experimental Psychology: General*, 2018, 147 (12), 1810.
- and —, “Gaze amplifies value in decision making,” *Psychological science*, 2019, 30 (1), 116–128.
- Suhay, Elizabeth, Marko Klasnja, and Gonzalo Rivero**, “Ideology of Affluence: Rich Americans’ Explanations for Inequality and Attitudes toward Redistribution,” 2020.
- Tella, Rafael Di, Sebastian Galiant, and Ernesto Schargrodsky**, “The Formation of Beliefs: Evidence from the Allocation of Land Titles to Squatters,” *The Quarterly Journal of Economics*, February 2007, 122 (1), 209–241.
- Theeuwes, Jan**, “Goal-driven, stimulus-driven, and history-driven selection,” *Current opinion in psychology*, 2019, 29, 97–101.
- Waldfoegel, Hannah B., Jennifer Sheehy-Skeffington, Oliver P. Hauser, Arnold K. Ho, and Nour S. Kteily**, “Ideology selectively shapes attention to inequality,” *Proceedings of the National Academy of Sciences*, April 2021, 118 (14).
- Willemsen, Martijn C. and Eric J. Johnson**, “Observing Cognition with MouselabWEB,” in “A handbook of process tracing methods” 2019, pp. 76–95.

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%.¹⁸

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

¹⁸Despite 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 A.1. 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 A.1: Survey results.

	Article Choice	Article Choice	Redistribution Attitude	Redistribution Attitude
Personal Income (High)	-0.22*** (0.038)		-1.40*** (0.14)	
Personal Income (10,000s of pounds)		-0.0020*** (0.00035)		-0.013*** (0.0013)
Age	0.0045** (0.0016)	0.0046** (0.0016)	-0.026*** (0.0061)	-0.025*** (0.0061)
Gender (Woman)	0.033 (0.038)	0.036 (0.038)	0.20 (0.14)	0.21 (0.14)
Gender (Other)	0.12 (0.12)	0.13 (0.55)	1.79*** (0.48)	1.85*** (0.48)
Gender (Prefer not to answer)	-0.014 (0.36)	-0.0021 (0.36)	2.70* (1.30)	2.76* (1.30)
Observations	767	767	767	767

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. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

A.2 Effect of Status on Attention Controlling for Total Dwell Time

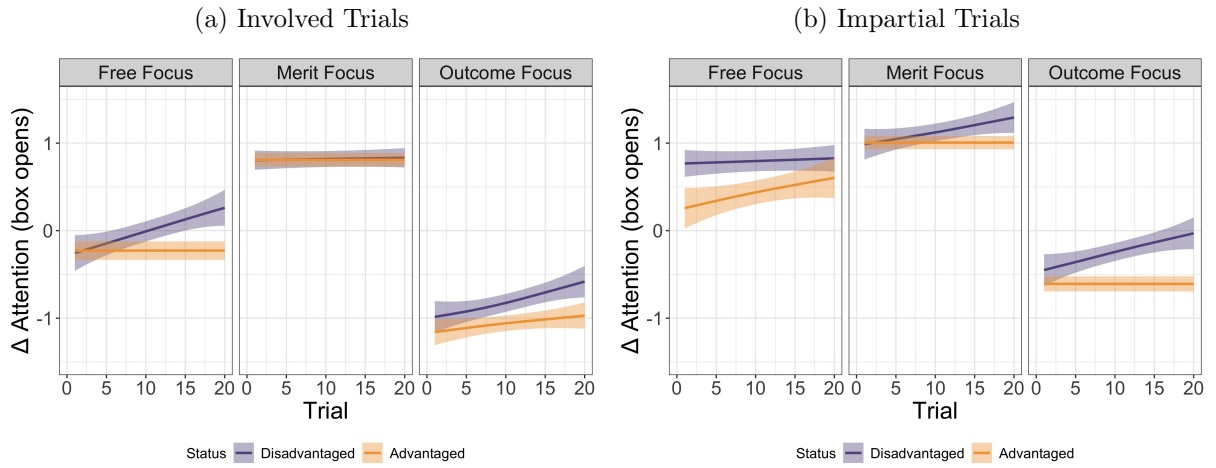
Table A.2: Differences in Attention.

Panel A: Involved Trials					
	(1) All data Δ Attention	(2) Free focus Δ Attention	(3) Free focus Δ Attention	(4) Mer. Focus Δ Attention	(5) Out. Focus Δ Attention
Advantaged	-0.13 ⁺ (0.072)	-0.29 ⁺ (0.16)	0.057 (0.18)	-0.017 (0.055)	-0.088* (0.041)
Round			0.014 (0.011)		
Advantaged * Round			-0.033* (0.013)		
Observations	11930	3988	3988	3972	3970
Panel B: Impartial Trials					
	All data Δ Attention	Free focus Δ Attention	Free focus Δ Attention	Mer. Focus Δ Attention	Out. Focus Δ Attention
Advantaged	-0.21* (0.100)	-0.37 (0.25)	-0.42 (0.26)	-0.081 (0.084)	-0.20** (0.062)
Round			0.0035 (0.0084)		
Advantaged * Round			0.0050 (0.012)		
Observations	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 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 A.1: The dynamics of attention (box opens) by treatment



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.

Figure A.2: The relationship between attention (box opens) and allocations

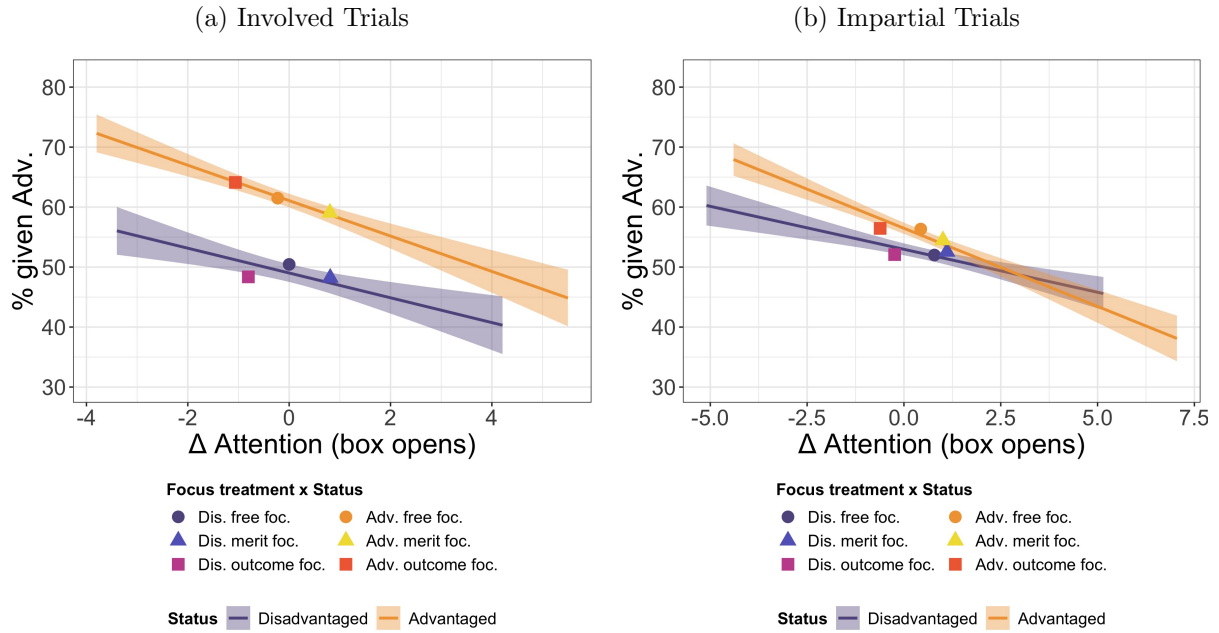
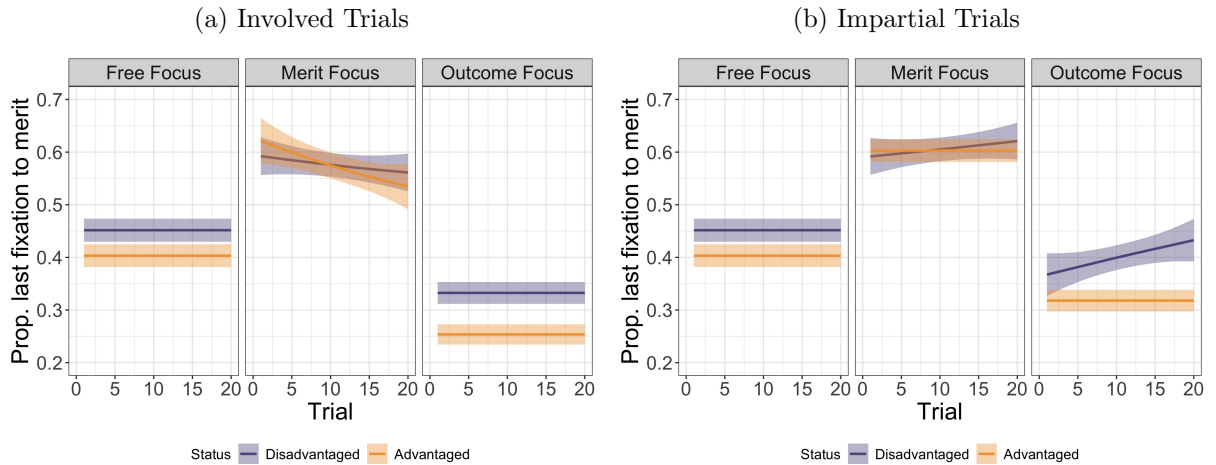


Illustration of the influence of attention (number of times merit - outcome information boxes were opened) on allocations, split by Status. The lines include data from both Free Focus and Constrained Focus treatments and are linearly smoothed with shaded 95% confidence bands. The mean attention and allocations are split by information condition.

Figure A.3: The dynamics of attention (final fixation) by treatment



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 A.3: Differences in Attention - Number of Boxes

Panel A: Involved Trials					
	(1)	(2)	(3)	(4)	(5)
	All data	Free focus	Free focus	Mer. Focus	Out. Focus
	Δ Boxes	Δ Boxes	Δ Boxes	Δ Boxes	Δ Boxes
Advantaged	0.15 (0.11)	0.17 (0.21)	-0.11 (0.25)	-0.024 (0.15)	0.26* (0.12)
Round			-0.030** (0.012)		
Advantaged * Round			0.026+ (0.015)		
Observations	11930	3988	3988	3972	3970
Panel B: Impartial Trials					
	All data	Free focus	Free focus	Mer. Focus	Out. Focus
	Δ Boxes	Δ Boxes	Δ Boxes	Δ Boxes	Δ Boxes
Advantaged	0.28* (0.13)	0.24 (0.29)	0.34 (0.32)	0.082 (0.17)	0.46** (0.18)
Round			-0.013 (0.0095)		
Advantaged * Round			-0.0096 (0.014)		
Observations	11923	3984	3984	3975	3964

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 A.4: Differences in Attention - Last Fixation.

Panel A: Involved Trials					
	(1)	(2)	(3)	(4)	(5)
	All data	Free focus	Free focus	Mer. Focus	Out. Focus
	Δ Boxes	Δ Boxes	Δ Boxes	Δ Boxes	Δ Boxes
Advantaged	0.038 ⁺ (0.021)	0.031 (0.032)	0.0040 (0.043)	0.0084 (0.033)	0.084* (0.033)
Round			-0.0013 (0.0020)		
Advantaged * Round			0.0026 (0.0029)		
Observations	11791	3953	3953	3909	3929
Panel B: Impartial Trials					
	All data	Free focus	Free focus	Mer. Focus	Out. Focus
	Δ Boxes	Δ Boxes	Δ Boxes	Δ Boxes	Δ Boxes
Advantaged	0.041 ⁺ (0.023)	0.031 (0.032)	0.0041 (0.043)	0.015 (0.036)	0.11** (0.040)
Round			-0.0010 (0.0020)		
Advantaged * Round			0.0025 (0.0028)		
Observations	11729	3949	3949	3887	3893

All models are linear regressions with the dependent variable equal to 1 if the last fixation is 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 A.4: The relationship between attention (final fixation) and allocations

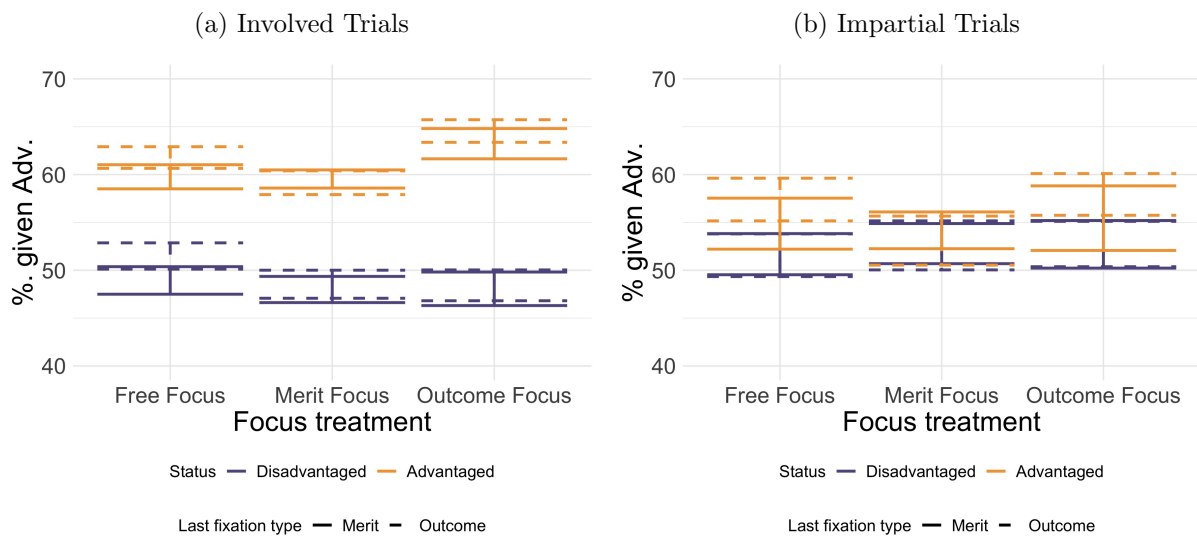


Illustration of the influence of attention (final fixation on merit vs. outcome information) on allocations, split by Status and Focus treatment. 95% confidence intervals are plotted.

A.4 The effect of attention on allocation for subjects that did not realize that some boxes were more likely to be restricted than others

Table A.5: The Effect of Attention on Allocation.

Panel A: Involved Trials, Constrained Focus only				
	(1)	(2)	(3)	(4)
	All data % to Adv.	Disadvantaged % to Adv.	Advantaged % to Adv.	All Data % to Adv.
Outcome Focus	5.07** (1.76)	2.69 (2.56)	6.31** (1.88)	
Δ Attention				-4.70** (1.63)
Observations	4867	2478	2389	245
F-statistic - first stage				552
Panel B: Impartial Trials, Constrained Focus only				
	All data % to Adv.	Disadvantaged % to Adv.	Advantaged % to Adv.	All Data % to Adv.
Outcome Focus	1.81 (1.11)	-0.35 (1.55)	3.28* (1.43)	
Δ Attention				-1.67+ (0.99)
Observations	4871	2486	2385	245
F-statistic - first stage				244

The dependent variable is the percentage of the pie allocated to the Advantaged member of the pair. Standard errors clustered by participant in parentheses. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Data include only subjects that said that they did not notice any information box to me more likely to be restricted. 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

A.5 Within-Subject Variation in Attention Restrictions

Table A.6: Percentage given to the advantaged conditional of the trial and focus type.

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

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. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Data from Outcome Focus and Merit Focus treatments. All models are linear regressions. The second column 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

CONFIDENTIAL - FOR PEER-REVIEW ONLY

Tracking fairness (#44417)

Created: 07/12/2020 01:54 AM (PT)

Shared: 11/03/2020 12:17 PM (PT)

This pre-registration is not yet public. This anonymized copy (without author names) was created by the author(s) to use during peer-review. A non-anonymized version (containing author names) will become publicly available only if an author makes it public. Until that happens the contents of this pre-registration are confidential.

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

screen.

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).

CONFIDENTIAL - FOR PEER-REVIEW ONLY

Tracking fairness - attention manipulation (#52512)

Created: 11/18/2020 09:42 AM (PT)

Shared: 02/16/2021 06:11 AM (PT)

This pre-registration is not yet public. This anonymized copy (without author names) was created by the author(s) to use during peer-review. A non-anonymized version (containing author names) will become publicly available only if an author makes it public. Until that happens the contents of this pre-registration are confidential.

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 (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.

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

<p style="text-align: center;">Welcome!</p> <p style="text-align: center;">This is a study conducted by researchers at the University of Amsterdam.</p> <p style="text-align: center;"> UNIVERSITEIT VAN AMSTERDAM</p>	<p style="text-align: center;">All the tasks in this study are not mobile-compatible.</p> <p style="text-align: center;">You can only participate with a desktop or laptop.</p>
<p style="text-align: center;">This study consists of two sessions:</p> <ul style="list-style-type: none">• Session 1 lasts about 34 minutes• Session 2 lasts about 55 minutes <p style="text-align: center;">The total reward for completing the two sessions is £9:</p> <ul style="list-style-type: none">• £2.85 for session 1• £6.15 for session 2	<p style="text-align: center;">You need to complete both sessions for your submission to be approved and paid.</p> <p style="text-align: center;">You must complete Session 2 between Tuesday 24th November at 1.00 pm (CET) and Wednesday 25th November at 10.00 pm (CET).</p> <p style="text-align: center;">We will send you a reminder on Prolific when Session 2 will be open.</p>

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.



UNIVERSITEIT VAN AMSTERDAM

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

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.



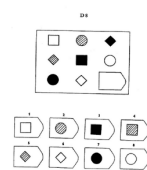
In each task, you are matched with a different participant

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.

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]

<p style="text-align: center;">Tasks 1 and 2</p> <p>You need to move a slider to a predetermined value. You have 2 minutes to move as many sliders as you can.</p> <div style="display: flex; justify-content: space-around; align-items: flex-start;"> <div style="text-align: center;"> <p>Incorrect slider</p> <p>2) Set the slider to 234</p>  <p>The value of the slider is 179</p> </div> <div style="text-align: center;"> <p>Correct slider</p> <p>2) Set the slider to 234</p>  <p>The value of the slider is 234</p> </div> </div> <p>You get a reward per each slider you set to the correct value.</p> <p>High Reward: £0.30 Low Reward: £0.10</p>	<p style="text-align: center;">Tasks 3 and 4</p> <p>You need to answer a quiz. You have 4 minutes to answer as many questions as you can.</p> <p>You get a reward per each correct answer.</p> <p>High Reward: £0.90 Low Reward: £0.30</p>																									
<p style="text-align: center;">Tasks 5 and 6</p> <p>You need to count zeros in a table. You have 3 minutes to count the zeros in as many tables as you can.</p> <div style="text-align: center; border: 1px solid gray; padding: 5px; width: fit-content; margin: 0 auto;"> <table style="border-collapse: collapse; text-align: center;"> <tbody> <tr><td>7</td><td>0</td><td>9</td><td>1</td><td>6</td></tr> <tr><td>7</td><td>0</td><td>0</td><td>4</td><td>6</td></tr> <tr><td>8</td><td>0</td><td>7</td><td>0</td><td>0</td></tr> <tr><td>0</td><td>0</td><td>7</td><td>7</td><td>0</td></tr> <tr><td>0</td><td>6</td><td>0</td><td>3</td><td>0</td></tr> </tbody> </table> </div> <p>You get a reward each time you correctly state the number of zeros.</p> <p>High Reward: £0.30 Low Reward: £0.10</p>	7	0	9	1	6	7	0	0	4	6	8	0	7	0	0	0	0	7	7	0	0	6	0	3	0	<p style="text-align: center;">Tasks 7 and 8</p> <p>You need to find the missing tile in a figure. You have 3 minutes to complete as many figures as you can.</p> <div style="text-align: center;">  </div> <p>You get a reward each time you select the correct tile.</p> <p>High Reward: £0.60 Low Reward: £0.20</p>
7	0	9	1	6																						
7	0	0	4	6																						
8	0	7	0	0																						
0	0	7	7	0																						
0	6	0	3	0																						

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.



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.



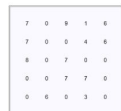
Welcome to Session 2 of this study.

This session consists of three parts that will last for approximately **55** minutes.

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



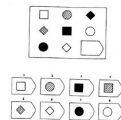
Moving Sliders Task





Counting Zeros Task



Quiz Questions Task



Missing Tiles Task

<p style="text-align: center;">Part 1</p>	<p>We randomly assigned participants to one of these groups:</p> <div style="display: flex; justify-content: space-around; align-items: center;"> <div style="text-align: center;"> <p>High Reward per correct answer Group</p>  </div> <div style="text-align: center;"> <p>Low Reward per correct answer Group</p>  </div> </div>
<p style="text-align: center;">You are assigned to the HIGH reward per correct answer.</p>	<p>You will complete 20 rounds of a division task which we will explain in the next slides.</p> <p>You are always matched with someone new for every round.</p> <p>All the other participants you are matched with will get the LOW reward per correct answer.</p>

The second row shows the instructions for Advantaged participants, whereas Disadvantaged participants saw HIGH and LOW switched across slides. Disadvantaged participants were instructed that that they were assigned the LOW, and the other participants the HIGH, reward per correct answer.

Comprehension questions:

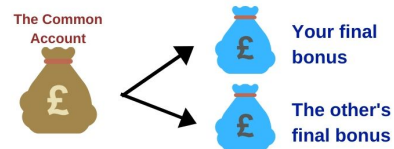
1. Which reward condition were you and the other participants you are matched with assigned to? **MULTIPLE CHOICE** [Advantaged: You: High reward, Others: Low reward; Disadvantaged: You: Low reward, Others: High reward;]
2. What determines the common account on each round? **MULTIPLE CHOICE** [The combined 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 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.

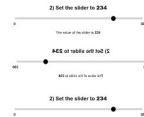
You will be paid the money you allocated for yourself, and the other participant will receive the money you allocated to them.

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**.

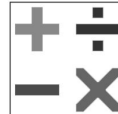
First, you will see **which task** from Session 1 determined the common account for this round.



Moving Sliders Task

7	0	9	1	6
7	0	0	4	6
8	0	7	0	0
0	0	7	7	0
0	6	0	3	0

Counting Zeros Task



Quiz Questions Task



Missing Tiles Task

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

Task: Counting Zeros

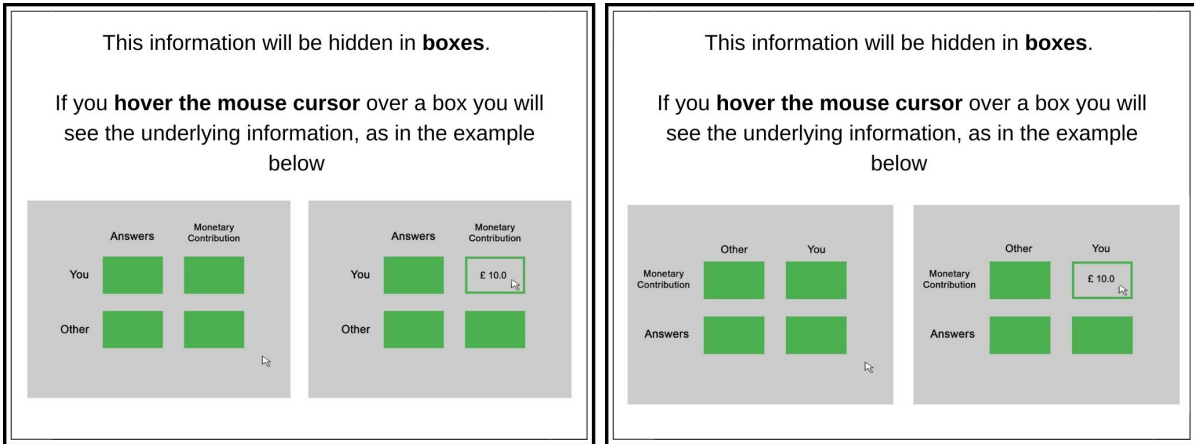
7	0	9	1	6
7	0	0	4	6
8	0	7	0	0
0	0	7	7	0
0	6	0	3	0

Common account: £10



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):
correct answers x 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.

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

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 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.

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]

Part 2

Thank you for completing Part 1 of the Session 2.

Now, we will proceed to Part 2 of this study.

In this part of the study, you will do the same division task as the previous part.

However, you will divide common accounts for **two other, anonymous players** instead:

Player High and Player Low

Just like you, both Player High and Player Low have also completed the series of tasks online in Session 1.

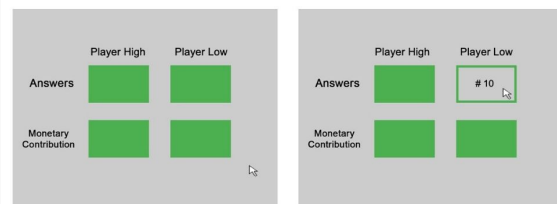
Player **High** receives a **HIGH Reward** per correct answer.

Player **Low** receives a **LOW Reward** per correct answer.

As in Part 1, you will be presented with information boxes with Player High's and Player Low's number of correct answers and earnings for **6 seconds**.

And you can **hover the mouse cursor over a box** to see the underlying information.

This is the information layout you will see



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.

Social Norms

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?¹⁹

¹⁹We asked these two questions only in the Constrained Focus treatments.

Part 3.2

In the questions below, *morally appropriate* refers to an action that is "correct", "fair", or "ethical" according to your values and morality.

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?²⁰

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]

²⁰The order of the norms questions is randomized at the individual level and it is consistent across the different elicitation screens. 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

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.

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

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