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Fair Shares and Selective Attention[†]

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Attitudes toward fairness and redistribution differ along socioeconomic lines. To understand their formation, we conduct a large-scale experiment on attention to merit and luck and the effect of attention on fairness decisions. Randomly advantaged subjects pay less attention to information about true merit and retain more economic surplus, and this effect persists in subsequent impartial decisions. Attention also has a causal role: encouraging subjects to look at merit reduces the effect of an advantaged position on allocations. This suggests that attention-based policy interventions may be effective in reducing polarized views on inequality. (*JEL* C91, D63, D83)

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 (Östling 2009). Affluent Americans are more likely than average Americans to believe that inequalities result from hard work and intelligence rather than from luck (Suhay, Klasnja, and Rivero 2021) 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 promarket views (Di Tella, Galiant and Schargrodsky 2007), and the random allocation of an economic advantage to laboratory subjects causes them to redistribute less to unfortunate peers (Konow 2000; Deffains, Espinosa, and Thöni 2016). In contrast, random shocks that worsen people's economic situations, like sickness and disability, increase the moral appeal of equality (Hvidberg, Kreiner, and Stantcheva 2023). These diverging views about the origin of economic success have been linked to recent political conflict in Western societies (Sandel 2020; Gethin, Martínez-Toledano, and Piketty 2022).

In this paper, we study the role of visual attention in the formation of attitudes toward merit and redistribution. Attention is the filter through which people

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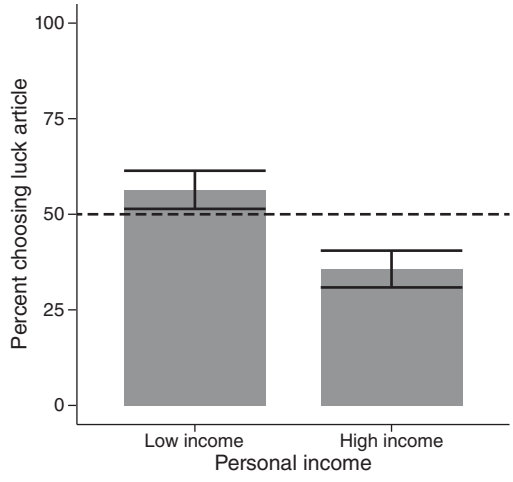

FIGURE 1. CHOICE TO LEARN ABOUT THE ROLE OF LUCK BY INCOME LEVEL

Notes: Choice of article split by income level, with "Low income" defined as $<$ £10.000, and "High income" as $>$ £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 percent confidence intervals.

understand their environment and may reflect a person's motives or background. For instance, citizens of different socioeconomic status may pay attention to news media that provide different narratives about the nature and origin of inequality. We ask how socioeconomic 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 toward meritocracy and economic success and help understand the competition for attention by activists and politicians.

Before describing our main investigation, we motivate our research questions with survey evidence on the relation between socioeconomic 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 socioeconomic status would be 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 percent of high-income participants chose to look at the luck article, compared to 56.4 percent of the low-income participants ($\chi^2 = 32.31, p < 0.001$). Higher income also has a strong, negative correlation with positive attitudes toward redistribution (Kendall rank correlation $\tau = -0.31, p < 0.001$).[1]

These results suggest an interplay between economic status, attention to merit and luck, and attitudes toward redistribution. To rigorously investigate the causal

[1] Details about the implementation and outcomes of the survey are in online Appendix B.1.

links between these variables, we perform a series of large online experiments in which we manipulate both economic advantage and attention. 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" ($N = 600$) and divide the surplus generated by two participants, one with *Advantaged* Status and one with *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 and manipulate their visual attention to the sources of the surplus. Dictators can uncover two sources of information. First, "outcome" information mirrors information most typically available to us. It shows the total contribution of each participant to the surplus, thus combining merit (correct answers) and luck (the randomly determined pay rate). 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 the key locus of competition for attention. Moreover, dwell time determines how intensively attributes are considered, and neuroeconomic studies show that gaze patterns predict choice (Just and Carpenter 1980; Orquin and Mueller Loose 2013; Krajbich 2019; Bordalo, Gennaioli, and Shleifer 2021). Dwell time thus allows us to study the intensive margin of attention, contrasting with studies on rational inattention or information avoidance that typically focus on binary decisions to resolve uncertainty (Gabaix 2018; Golman and Loewenstein 2018; Dana, Weber and Kuang 2007; Grossman and Van der Weele 2018). 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 merit or outcome.

We find a clear effect of both our Status and attention manipulations, as well as an interaction between the two. First, 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 the 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 to effort and luck. 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.

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 driven almost exclusively by *Advantaged* dictators and 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 around 40 percent when dictators' own income is at stake. This effect is driven by changes in dwell time—and not by completely avoiding some information as in previous literature on the topic (e.g., Dana, Weber, and Kuang 2007). We can also rule out experimenter demand effects or processing errors as psychological mechanisms behind the results. Instead, we show that attention causes subjects to change their views of what is appropriate or fair in these division problems in ways that carry over to the Impartial trials.

We go beyond previous literature on self-serving bias in attitudes toward redistribution and merit by studying its cognitive underpinnings. We show that selective attention can help explain the development of such biases. Moreover, our interventions demonstrate the causal role of attention in redistribution and fairness decisions. Our focus on dwell time shows the importance of considering the intensive margin of attention. It suggests interventions based on visual attention are effective as a lever to influence such decisions. As such, our study opens a new window on socioeconomic cleavages in attitudes toward 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.

I. Literature Review

Our research relates to several strands of literature. First, we contribute to a behavioral literature on the role of merit in redistribution. A number of laboratory experiments show that participants are more willing to redress inequalities based on luck rather than merit (Krawczyk 2010; Cappelen et al. 2013; Durante, Putterman, and van der Weele 2014; Lefgren, Sims, and Stoddard 2016; Cappelen et al. 2023; Bortolotti et al. 2024; Buser et al. 2020). Almås, Cappelen, and Tungodden (2020) have shown that this tendency is robust across countries, even if there are differences in the overall tendency to redistribute, although Jakiela (2015) finds that the distinction between merit and luck is less strong in rural villages with strong egalitarian norms. 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, Espinosa, and Thöni (2016) use similar designs and replicate these main results. Espinosa, Deffains, and Thöni (2020) show that the bias is robust to ex post information provision that highlights 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. The literature on visual attention assumes that people look at information that they are thinking about, and we particularly relate to a literature that links choice to various attentional mechanisms (surveyed in Just and Carpenter 1980; Engelmann, Hirmas, and van der Weele 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, Gennaioli, and Shleifer 2012 2021).

In our study, we manipulate attention in a way that preserves the option to direct attention, consistent with top-down control. However, by making certain information relatively easier to access we also affect attention in a bottom-up way. These subtle attentional manipulations may act primarily on "difficult" or conflicted decisions.[2] 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, Leber, and Krajbich 2019). We examine how attentional patterns developed in choices with one's own payoff at stake spillover into impartial decisions, which relates to this literature on attentional history and habits. 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 an emerging literature on the role of attention in pro-social decisions. One such line of research has focused on the phenomenon of information avoidance and selective search in moral situations (Dana, Weber, and Kuang 2007; Grossman and Van der Weele 2017). In these studies, participants choose whether or not to resolve uncertainty about the consequences of their decisions on others. In

[2] 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 more difficult choices where the options are closer in value (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).

contrast to this binary reveal/avoid decision, we look at a more continuous measure of attention, namely visual attention or dwell time.[3] This setting is more realistic in capturing situations where people are exposed to many different perspectives and types of information. Indeed, our study shows that avoidance is very low, and that even if people reveal all information about payoffs, the *length* of the relative dwell time on that information affects their choice as it affects the weight on different types of information.

A separate line of attention literature in social decision-making uses eye-tracking and mouselab technology to focus on continuous attention, but without the element of merit and luck in determining fairness. Fiedler et al. (2013) show correlations between eye movements and social preferences in social allocation problems. These correlations are replicated in mouselabWEB by Bieleke, Dohmen, and Gollwitzer (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, Hausfeld, and Renerte 2022). 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 predetermined amount of time. This exogenous variation can explain about 11 percent of the variation in visual attention and about 1 percent of changes in choice. Other results have shown correlations of attention with loss-framing (Fiedler and Hillenbrand 2020) and in-group bias (Rahal, Fiedler, and De Dreu 2020; Fischbacher et al. 2023) 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 toward economic inequality. They show that political ideology affects whether people detect inequalities in everyday situations, whereas we focus on the determinants of inequality.

II. 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 (i) replicate previous findings on the relationship between economic status and attitudes toward redistribution and (ii) establish a causal relationship between economic status and attention. Experiment 2 generated

[3] Chen and Heese (2021) study sequential binary decisions with partial information revelation. In this setting, one never fully learns the state of the world.

Table 1—Overview of Treatments and Number of Dictators

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

Notes: Overview of the treatments in our 3 × 2 design. The data for the *Free Focus* treatments come 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.

data for the *Merit* and *Outcome Focus* treatments, and allows us to (iii) investigate the causal relationship between attention and attitudes toward redistribution.

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 were collected between the thirteenth and nineteenth of July 2020. For Experiment 2, we recruited 400 dictators and 600 recipients from Prolific.co.[4] The data were collected between the twenty-third and thirtieth 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. Overall 1,500 participants completed the study in the role of either dictator or recipient. In our analyses, we focus on the attention and allocation decisions of the 600 participants assigned the role of dictator.[5] 56 percent of the participants are male and the average age is 25 years. Online Appendix B.2 displays more details of participants demographics and shows that attrition between the two experimental days is minimal and balanced across Status treatments.

A. *Day 1: Surplus Generation*

On Day 1, participants completed eight sets of real effort tasks. In each task set, participants had a limited time period to complete as many tasks as possible. There were four 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 eight 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 stage. The high pay rate was always three times the low

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

[5] None of these dictators took part in the motivating survey discussed in the introduction.

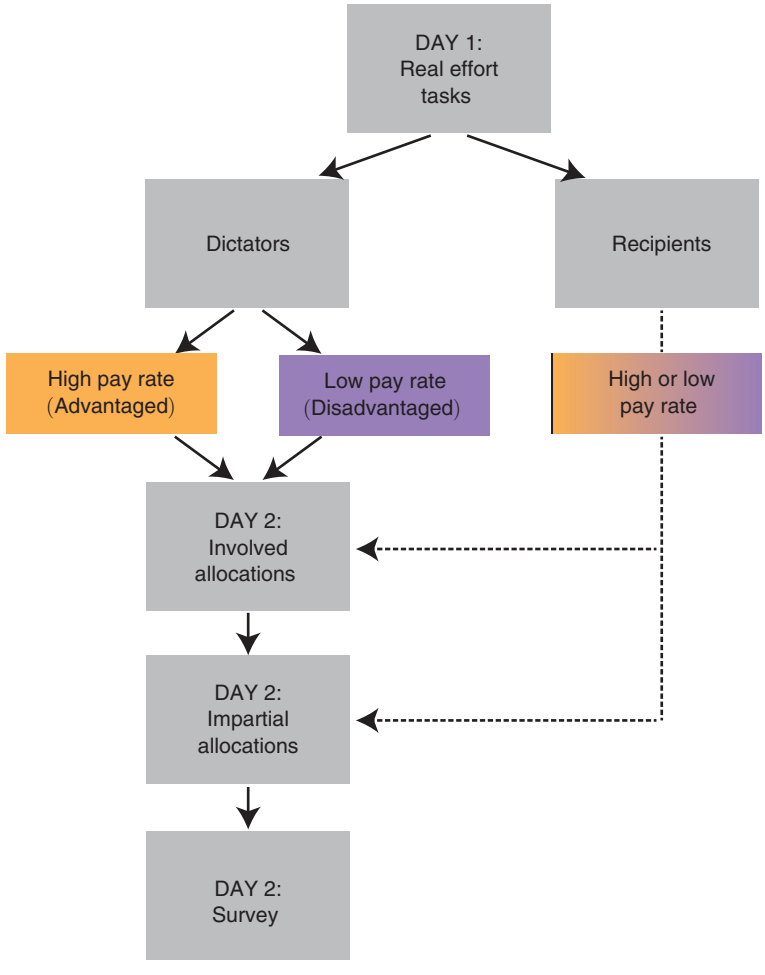

Figure 2. Timeline for Day 1 and 2 for Both Experiments

pay rate, but pay rates were calibrated (based on pilot data[6]) 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 toward the recipients as they would not continue to Day 2. Since all participants

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

were given the same information and were not informed of their pay rate at this stage, the information should not affect participants differentially.

B. *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 three 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 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 eight task sets underlying the common account in each of the trials.

We matched *Advantaged* dictators with *Disadvantaged* recipients and, vice versa, *Disadvantaged* dictators with *Advantaged* recipients. The dictators were made aware of these inequalities in the instructions, and we checked their understanding with a comprehension question. The explicit and consistent allocation of relative advantage throughout the experiment mimics systematic advantages like those due to the socioeconomic position of parents. It allows us to investigate how such advantages affect attention to merit and luck information. We created trials such that dictators outperformed recipients on 50 percent of the trials 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.[7]

[7] The fixed order that allows us to examine spillover from Involved to Impartial decisions also limits the interpretation of Impartial allocations because there is a time confound between later decisions and decision type. The results might be different if Impartial decisions were made first. For example, if participants weigh their own Status

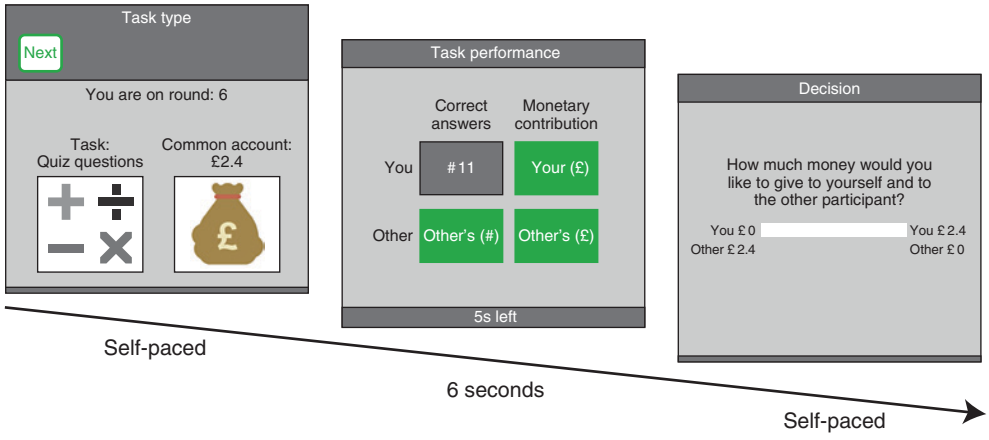

FIGURE 3. INFORMATION SEQUENCE

Notes: 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 six 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. For illustrative purposes, the font size of the text is increased in these images.

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 at the time of the study, 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.[8]

C. *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 eight task sets were used approximately equally across the 40

less in Impartial decisions and there is cognitive dissonance to shifting fairness strategies, this could reduce the self-serving bias also in the Involved decisions, leading to overall more similar attention and allocations regardless of Status. Alternatively, participants could shift their fairness rules even in Impartial decisions if they anticipated the effect on Involved decisions. Such order effects have been investigated in allocation decisions without luck by Dengler-Roscher et al. (2018) with some evidence suggesting that putting Impartial decisions before Involved reduces self-serving bias.

[8] We preassigned 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.

trials. Dictators could spend as much time as they wanted on this screen. Next, dictators had six seconds during which they could reveal information about the number of correct questions each participant answered in the task—merit information—and the monetary contribution of each member of the pair to the account—outcome information. 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).[9] This information was divided into four boxes labeled 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.[10]

D. *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 six-second time limit. The six-second 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 to the experiment the tight attentional constraints that permeate real life (Gabaix 2018). The six-second limit is in line with prior research that uses limits as low as three seconds for decisions with two pieces of information to understand the impact of attention on choice, doubled to six seconds for four 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). In the *Merit Focus* treatment, outcome information was more restricted than merit information and vice versa in the *Outcome Focus* treatment. These restrictions were designed to shift

[9] In particular, outcome information is reflective of information typically available outside of the lab, as it incorporates both merit and luck (for example, one's income can be inferred with some approximation from his/her lifestyle). 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. We exclude pure luck information because the pay differential for *Advantaged* and *Disadvantaged* is constant across trials and known in advance.

[10] Given this setup, one might be concerned that participants do not need to open all the boxes to obtain the information they need. For example, a participant that remembers the pay differential can calculate merit from outcome and vice versa. However, this is a complex and effortful calculation. In providing all the information, we make it easier for the participants to implement the different fairness rules without relying on their memory and arithmetic abilities. Indeed, Section IVE shows that almost all participants open every box. In any case, if participants indeed calculate the content of the boxes they do not see, we will *underestimate* the effect of attention on allocations.

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 milliseconds each. The other two boxes could be opened for no more than 1600 milliseconds each. The total maximum of four seconds spent on box information was chosen to closely match the average time spent on information from the *Free Focus* experiment, which was 3.8 seconds. The 400-millisecond constraint was chosen because information can still be processed and remembered for later use at this timing, whereas timings of 200 milliseconds or lower may actually be restrictive for recognition (DiCarlo, Zoccolan, and Rust 2012; Potter 1976). Prior attention manipulations have used minimum dwell times of 250 milliseconds and 300 milliseconds (Armel, Beaumel, and Rangel 2008; Pärnamets et al. 2015; Pachur et al. 2018; Fisher 2021).

Participants were not required to look at any information: they could choose the sequence and which information to reveal, some information was simply available for a longer time if participants chose to reveal it for longer. Boxes could still be opened multiple times within the six-second 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 six seconds were 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.[11] 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 IVC.). 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 IVD.).

E. *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

[11] For instance, in the *Merit Focus* treatment, 14 of the 20 decisions restricted outcome information to 400 milliseconds and merit information to 1600 milliseconds. This enabled participants to look longer at merit. In the remaining six trials, the 400-millisecond restrictions were placed either on merit information (two decisions), *Advantaged* member information (two decisions), or *Disadvantaged* member information (two decisions). In contrast, in the *Outcome Focus* treatment, 14 trials restricted merit information to 400 milliseconds, while the remaining six trials split the 400-millisecond restrictions evenly between the other information dimensions.

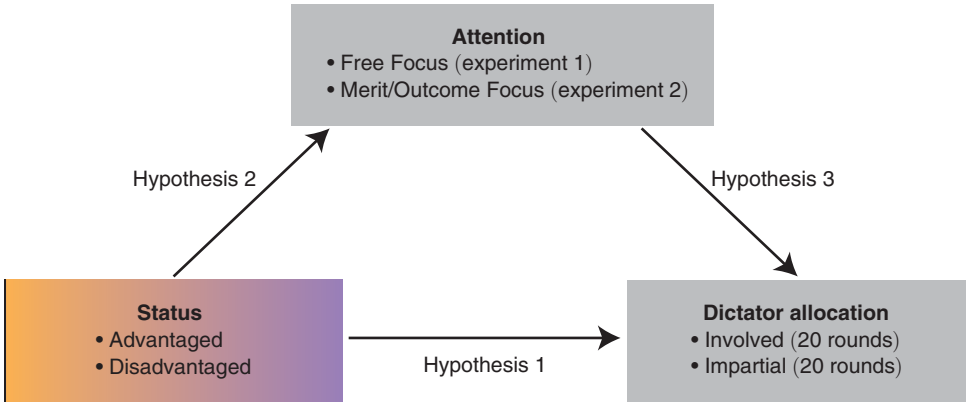

FIGURE 4. FRAMEWORK FOR THE EXPERIMENTAL DESIGN AND HYPOTHESES

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.

F. *Hypotheses*

Our overall aim is to characterize the role of attention in redistributive decisions and self-serving bias—induced by our 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 online Appendix C.

The first 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 1 (Self-Serving Bias): *In the Involved condition, Advantaged dictators give less money to the recipients and more money to themselves than Disadvantaged dictators.*

The second 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 2 (Selective 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 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 I, showing that merit is an important criterion in redistribution and that exogenous changes in salience or dwell time can affect choice.

HYPOTHESIS 3 (Attention Impacts Allocations): *In the Involved condition, increased attention to merit in the Merit Focus treatment leads to a reduction in giving to Advantaged recipients compared to the Outcome Focus treatment.*

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.*

To give further backing to these hypotheses, Section IVA discusses a formal model of attention and fairness.

Attention Measure.—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. In Section IVE, we look at alternative measures like information avoidance. As a measure of selective attention, we use the *difference* between these two dwell times, which we will shorthand with "ΔAttention," that is,

$$\begin{aligned}\Delta Attention \;:=\; &Dwell\ time\ on\ merit\ information\\ &-\ Dwell\ time\ on\ outcome\ information,\end{aligned}$$

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 both pieces of information are necessary to make an informed comparison.

In keeping with the literature, in our main analysis, we disregard dwell times when a box is opened for less than 200 milliseconds, as this is considered too short to fully process information (Willemsen and Johnson 2019; Pachur et al. 2018; DiCarlo, Zoccolan, and Rust 2012). Nevertheless, in online Appendix B.10, we show that our results are robust to using a threshold of 100 milliseconds or including all dwell times regardless of length. Furthermore, in our main specifications, we do not control for the total dwell time of individuals, which is an endogenous regressor that could bias the estimated effect sizes. However, in online Appendix Table A.6, 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.

III. Results

We first give an overview of the data and our main treatment effects before we delve into more details of the different experiments and the interactions between our treatments.

A. *Summary Statistics*

We start by evaluating 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 summarizes the means of the most important outcome variables.[12] First, the share of the surplus given to *Advantaged* members averaged over both dictator types was 56 percent for Involved allocations and 54 percent for Impartial allocations in Experiment 1 and 55 percent for Involved allocations and 54 percent for Impartial allocations in Experiment 2. Dictators kept the entire surplus for themselves in only 5.6 percent of the decisions, in accordance with previous findings that dictators respect earned income (Cappelen, Sørensen, and Tungodden 2010; Rodriguez-Lara and Moreno-Garrido 2012).

Second, participants engaged with merit and outcome information before making their allocations. In the *Free Focus* treatment, they spent on average 3.8 seconds of the available six seconds revealing information in both Involved and Impartial decisions, which amounts to about 2.5 minutes of search time over the entire experiment. Furthermore, pooling across Involved and Impartial decisions, information

[12] Each treatment should have 2000 observations, but fewer than 1.5 percent 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 that the number of nonrecordings is low and spread across treatments and participants, it is unlikely to affect our results.

Table 2—Summary Statistics

		Free Focus		*Merit Focus*		*Outcome Focus*	
		Adv.	*Dis.*	*Adv.*	*Dis.*	*Adv.*	*Dis.*
Panel A: Involved trials							
Allocation	% given to *Adv*.	61.5%	50.4%	59.1%	48.2%	64.1%	48.4%
	% given to self	61.5%	49.5%	59.1%	51.7%	64.1%	51.6%
Attention	Merit info (s)	1.69	1.82	1.40	1.32	0.86	0.88
	Outcome info (s)	2.14	1.91	0.88	0.85	1.55	1.40
	ΔAttention (s)	−0.45	−0.09	0.52	0.47	−0.69	−0.52
Observations		1,992	1,993	1,986	1,986	1,986	1,984
Panel B. Impartial trials							
Allocation	% given to *Adv.*	56.3%	52.0%	54.5%	52.5%	56.5%	52.1%
Attention	Merit info (s)	1.97	2.09	1.51	1.37	0.82	0.90
	Outcome info (s)	1.92	1.58	0.77	0.68	1.27	1.12
	ΔAttention (s)	0.059	0.51	0.75	0.69	−0.45	−0.22
Observations		1,990	1,990	1,987	1,988	1,978	1,986

seeking was equally distributed between information about correct answers (merit) and monetary contribution (outcome).[13] 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, but participants still engaged with the information nevertheless.

B. *Main Treatment Effects*

Our main treatment effects are captured in Table 3, providing a test of our four hypotheses using regression analyses with standard errors clustered at the individual level.

Hypothesis 1: Self-Serving Bias.— Status has a large effect on allocations in the Involved trials. Table 3, column 1 regresses the share allocated to *Advantaged* subjects on a dummy for the *Advantaged*. It shows that the *Advantaged* subjects receive 10 percentage points (roughly 20 percent) more of the pie from the *Advantaged*

[13] We 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 compares the proportion of option-wise (within self-performance or within other performance) transitions to attribute-wise transitions (comparing self and other merit or self and other outcome) 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 $= -0.49$; and *Constrained Focus* Impartial $= -0.54$, supporting a focus on attributes in the analyses. That said, examining dwell time on each box reveals that *Advantaged* dictators look more at self outcome than other outcome in the *Free Focus* Involved trials (rank-sum test $p = 0.02$). However, this increased attention on self does not hold for merit information, is not exhibited by *Disadvantaged* dictators, and is not significant in the *Constrained Focus* treatments or Impartial trials.

Table 3—Overview of the Main Treatment Effects

	Hypothesis 1 % given to *Adv.*		Hypothesis 2 ΔAttention		Hypothesis 3 % given to *Adv.*	
	Involved	Impartial	Involved	Impartial	Involved	Impartial
	(1)	(2)	(3)	(4)	(5)	(6)
Advantaged	10.0	3.45	−0.15	−0.21		
	(1.00)	(0.70)	(0.075)	(0.10)		
Outcome Focus					2.93	0.82
					(1.36)	(0.84)
Experiments	*Constrained Focus* and *Free Focus*				*Constrained Focus* only	
Observations	11,927	11,919	11,927	11,919	7,942	7,939

Notes: All models are linear regressions. Data: columns 1, 3, and 5, Involved trials; columns 2, 4, and 6, Impartial trials; and columns 5 and 6 exclude the dictators from the *Free Focus* treatment. Dependent variables: in columns 1, 2, 5, and 6, the percentage of the pie allocated to the *Advantaged* member of the pair; in columns 3 and 4: difference in dwell time between merit and outcome information. Standard errors clustered by participant in parentheses. List of controls common to all regressions: *age*, *gender* (man, woman, other), *political affiliation* (5 categories), *education* (6 categories), *income* (7 categories), and *continent* (4 categories). In addition, columns 1, 2, 5, and 6 include the share of correct answers coming from the *Advantaged* member over the total number of correct answers of the pair, task type (4 categories).

dictators (that is from themselves) than from the *Disadvantaged* dictators (rank-sum test of average allocations $p < 0.001$). The impact of being *Advantaged* is also apparent in the share dictators kept for themselves. For instance, in the *Free Focus* treatment—arguably the cleanest test of Hypothesis 1—*Advantaged* dictators kept 61.5 percent of the pie compared to slightly less than 50 percent for *Disadvantaged* dictators ($p < 0.001$, rank-sum test). In fact, because the *Disadvantaged* dictators are so close to splitting the surplus 50–50, the two ways of looking at the division are almost equivalent.

The average division around 50 percent by the *Disadvantaged* does not mean they are always splitting the surplus evenly. Allocations by both the *Advantaged* and *Disadvantaged* change with the number of correct answers given by each member of the pair, something we control for in all our regressions of allocation decisions. Online Appendix Figure A.2 shows a histogram of dictators' allocations in the *Free Focus* treatment, revealing substantial heterogeneity. Moreover columns 3 and 4 of online Appendix Table A.5 show that the share of the pie going to *Advantaged* dictators strongly and significantly increases with their share of correct answers (see also online Appendix Figure A.3).

Allocation differences by Status persist into the Impartial trials. Even if their own income is not at stake, *Advantaged* dictators allocate significantly more to the *Advantaged* members of the pair, as column 2 of Table 3 shows. The differences in Impartial allocations are statistically significant but quantitatively smaller than in the Involved trials, accounting for less than half of status bias.[14] This result seems to indicate that Status shifts internalized fairness norms, something we investigate in more detail in our companion paper (Amasino, Pace, and van der Weele 2023).

[14] Online Appendix Table A.4 formally shows that the difference in allocation between *Advantaged* and *Disadvantaged* shrinks in the Impartial trials.

Combined, these results replicate prior work on behavioral allocation biases whereby participants randomly assigned a higher pay rate keep more for themselves (Konow 2000; Rodriguez-Lara and Moreno-Garrido 2012; Deffains, Espinosa, and Thöni 2016).

Hypothesis 2: Selective Attention.—To investigate selective attention, we compare our attention measure, ΔAttention, across Status treatments. Table 3, column *3* regresses ΔAttention on a dummy for the *Advantaged*, finding that *Advantaged* dictators have lower ΔAttention ($p = 0.048$) and hence pay relatively less attention to merit and more to outcome than *Disadvantaged* dictators. This confirms Hypothesis *2* and shows that *Advantaged* dictators prefer information on performance that includes their artificial advantage. Column *4* shows that attentional patterns formed in the Involved condition spill over into the Impartial trials, where the effect is even slightly larger ($p = 0.038$).

Hypothesis 3 Attention Impacts Allocations.—To investigate the causal impact of attention, we compare allocations to the *Advantaged* in the *Outcome* and *Merit Focus* treatments. Participants gave 53.6 percent of the surplus to the *Advantaged* members of the pair in the *Merit Focus* treatment compared to 56.2 percent in the *Outcome Focus* treatment, a significant difference (rank-sum test $p = 0.028$). Columns *5* and *6* of Table *3* include data only from the *Constrained Focus* treatments (Experiment *2*) and show regressions of the share of the allocation to the *Advantaged* on a dummy for the *Outcome Focus* treatments. The coefficient for the dummy indicates that *Advantaged* members receive 2.93 percentage points more in the *Outcome Focus* treatment ($p = 0.033$). Thus, in line with Hypothesis *3*, attention plays a causal role in allocations.[15] In Section IVD, we show that the effect in both types of trials is almost entirely driven by the *Advantaged* dictators and discuss the reasons for this result.

In the Impartial trials, where the dictator's own payoff is not at stake, the difference between *Outcome Focus* and *Merit Focus* on allocations is smaller than in the Involved trials. Participants in the *Merit Focus* treatment gave 53.5 percent of the surplus to the *Advantaged* members of the pair compared with 54.3 percent in the *Outcome Focus* treatment (rank-sum test $p = 0.33$; Table 3, column 6).

In short, we find evidence for all our main hypotheses. Below we discuss the determinants of attention and allocations in more detail, and investigate interaction effects between our treatment dimensions.

C. *Determinants of Attention*

We now investigate how attention varies across all of our six treatments. Figure 5, panel A provides visual evidence of the average level of ΔAttention across our treatments. Table 4 provides corresponding statistical evidence in the form of OLS

[15] Participants in the *Constrained Focus* treatment could have put more effort in looking at the information that was restricted in the majority of rounds, which would reduce the effect of attention. However, this asymmetric effort is implausible: only a minority of subjects were able to identify which type of information was restricted when asked after the experiment; see footnote 18.

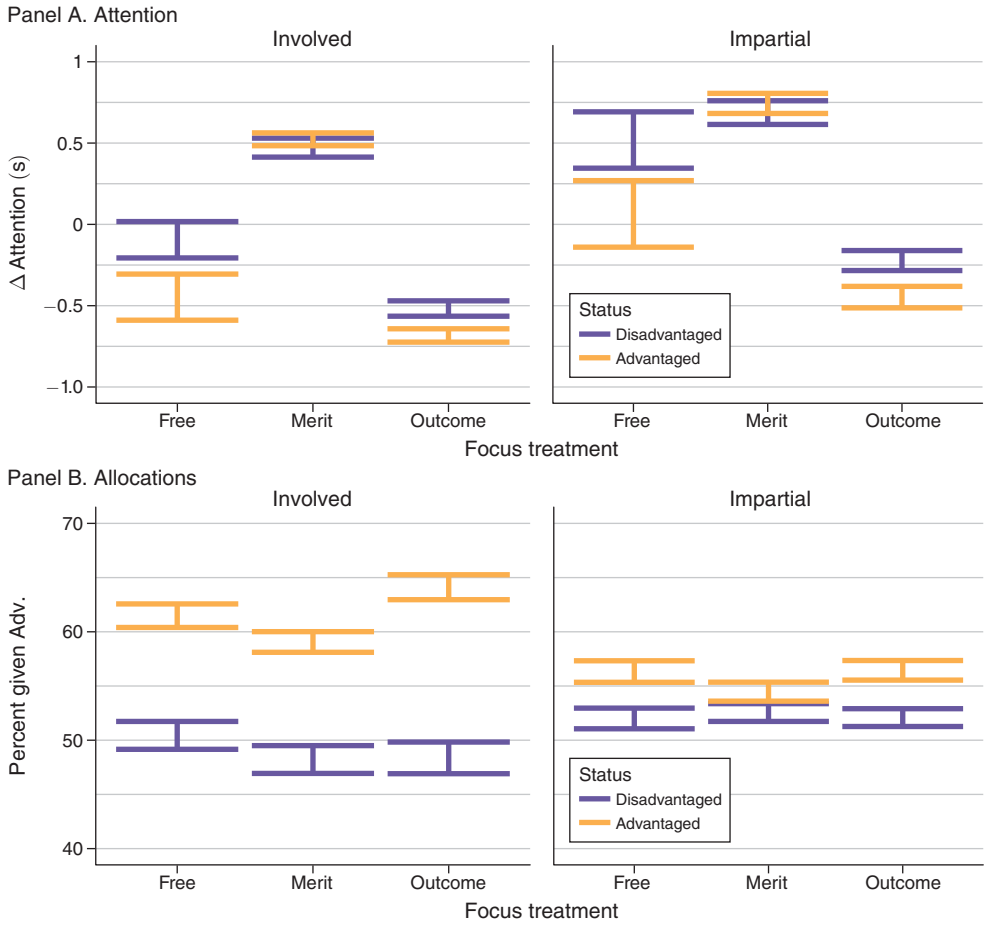

FIGURE 5. OVERVIEW OF TREATMENT EFFECTS ON ATTENTION AND ALLOCATIONS

Notes: The effect of *Merit* and *Outcome Focus* and Status on allocations and attention, shown separately for Involved and Impartial trials. The error bars represent the standard errors.

regressions, where we regress our main outcome variables on the treatment dummies. Columns 1 and 2 have ΔAttention as an outcome variable, whereas columns 3 and 4 focus on allocations (see next section). Since the table does not include a constant, the coefficients for the three attention treatments represent the baseline levels of the attention and allocation variables for the *Disadvantaged* dictators. The interactions terms with *Advantaged* dummy show the change in ΔAttention for the *Advantaged* dictators.[16]

[16] Table 4 deviates from the preregistered analysis in not including the demographic and task type controls. We made this deviation to make the coefficients for *Free Focus*, *Merit Focus*, and *Outcome Focus* easier to interpret. In the current specification, these coefficients give the average value of the dependent variable for the *Disadvantaged* dictators in these treatment. All the results presented in the table replicate if we include the controls, as online Appendix Table A.5 shows.

Table 4—Interactions between Status and Focus Treatments

	ΔAttention		% given to *Adv.*	
	Involved	Impartial	Involved	Impartial
	(1)	(2)	(3)	(4)
Free Focus	−0.090	0.51	50.4	52.0
	(0.11)	(0.17)	(1.27)	(0.95)
Free Focus × *Adv.*	−0.36	−0.46	11.1	4.35
	(0.18)	(0.27)	(1.66)	(1.37)
Merit Focus	0.47	0.69	48.2	52.5
	(0.058)	(0.072)	(1.27)	(0.81)
Merit Focus × *Adv.*	0.051	0.056	10.8	1.93
	(0.070)	(0.095)	(1.58)	(1.19)
Outcome Focus	−0.52	−0.22	48.4	52.1
	(0.047)	(0.061)	(1.45)	(0.82)
Outcome Focus × *Adv.*	−0.17	−0.23	15.8	4.38
	(0.063)	(0.090)	(1.85)	(1.22)
Observations	11,927	11,919	11,927	11,919

Notes: All models are linear regressions. The models do *not* include a constant. Data from all dictators, Involved trials in columns 1 and 3, and Impartial trials in columns 2 and 4. Dependent variable in columns 1 and 2: difference in dwell time between merit and outcome information. Dependent variable, columns 3 and 4: the percentage of the pie allocated to the *Advantaged* member of the pair. Standard errors clustered by participant in parentheses.

We first look at the *Free Focus* treatment, arguably the best test for selective attention, as it did not feature any restrictions on attention. The left panel of Figure 5, panel A shows that *Advantaged* dictators spent about 350 milliseconds longer on outcome information than *Disadvantaged* dictators, resulting in a more negative ΔAttention in Involved trials (rank-sum test of average dwell time $p = 0.010$). Column 1 of Table 4 mirrors this result ($p < 0.046$). This difference in attention by Status is a result of the attention patterns diverging over time. Column 1 of online Appendix Table A.7 regresses ΔAttention in the Involved rounds of the *Free Focus* treatment on the participant Status, the round number, and the interaction between Status and round number. There is no significant difference in ΔAttention for the first round ($p = 0.72$), but *Disadvantaged* dictators pay relatively more attention to the merit information as the rounds progress ($p = 0.039$), whereas *Advantaged* dictators pay more attention to outcome information with time ($p = 0.09$). These diverging attention trends make the coefficient for the interaction term negative and statistically significant ($p = 0.007$), which indicates that the gap in ΔAttention between *Disadvantaged* and *Advantaged* dictators grows as the rounds progress.[17]

Turning to the Impartial decisions, the right panel of Figure 5, panel A shows that the effect of Status on attention persists in the Impartial trials, although with larger variance (rank-sum test $p = 0.045$). Table 4, column 2 shows that this effect is significant at the 10 percent level ($p = 0.094$), but not in regressions

[17]The gradual increase of the difference in attention happens as participants spend less time looking at information over the course of multiple trials ($p < 0.001$, *t*-test). These two trends taken together suggest that, in later trials, participants have a better idea of which information is most important for them, and they focus their attention on it.

with demographic controls as shown in online Appendix Table A.5. These mixed results in the Impartial trials may be due to the fact that attention was more variable (higher standard deviations in column 2 than in column 1 of Table 4) and information avoidance (see subsection IVE.) was higher than in the Involved trials. These two facts suggest that participants may have cared less about the information when their own payoff was not at stake.

Turning to the *Merit Focus* and *Outcome Focus* treatment, we confirm that the constraints in these treatments were effective in actually shifting attention. Table 2 shows that the percentage of time spent looking at merit information was 47 percent in the *Free Focus* treatment and that the *Merit Focus* treatment increased the percentage of time looking at merit to 60 percent, whereas the *Outcome Focus* treatment decreased it to 43 percent. This translates to a difference between *Merit Focus* and *Outcome Focus* in ΔAttention of around one second in both Involved and Impartial trials as shown in Figure 5, panel A (rank-sum tests $p < 0.001$). The impact of Status on attention is present in the *Outcome Focus* treatment (rank-sum test $p = 0.013$) but not the *Merit Focus* treatment (rank-sum test $p = 0.41$). Column 1 of Table 4 replicates these results with a regression. Given the constraints we imposed, it is perhaps not surprising that the Status differences in attention are less pronounced.

Finally, we note that both *Advantaged* and *Disadvantaged* participants spent relatively more time on merit information in the Impartial trials. While we did not hypothesize this pattern, it is consistent across experiments and suggests that merit information was considered relatively more important in the absence of self-interest motives.

D. *Determinants of Allocations*

We turn to determinants of allocations, as measured by the share given to the *Advantaged* dictator. Average allocations across all six treatments are illustrated in Figure 5, panel B and in columns 3 and 4 of Table 4.

Above, we have already established the causal effect of Status and the *Outcome Focus* treatment on the share given to *Advantaged*. Figure 5 disaggregates this result. In the Involved trials, the effect of *Advantaged* is robust across all three attention treatments (rank-sum test $p < 0.001$ in each case). Indeed, in Table 4, column 3, the interaction term for *Advantaged* is highly significant in each treatment. However, the size of the status effect fluctuates: it is lowest in the *Merit Focus* treatment at 10.8 percentage points and highest in the *Outcome Focus* treatment at 15.8 percentage points. This suggests that the difference in allocations between the *Constrained Focus* treatments documented in Table 3 is driven by the *Advantaged* dictators, who shift their allocations by almost 5 percentage points (or 0.58 of a standard deviation) between *Merit* and *Outcome Focus*—a substantial effect also compared to the 0.2 percentage point shift by *Disadvantaged* dictators. Online Appendix Table A.8, column 1 formally confirms that there is a positive interaction between Status and the Focus treatments on allocations, which is significant at the 10 percent level ($p < 0.067$). Section IVA introduces a theoretical model that can capture this interaction and discusses the intuition behind it.

In the Impartial trials, splitting up the effect of *Constrained Focus* treatments by Status shows a similar pattern. Table 4, column 4 shows that Status differences in allocations persist into the *Outcome Focus* treatment, but go away in the *Merit Focus* treatment. These findings mirror the effects of attention documented above and are in line with the idea that *Advantaged* dictators struggle to justify their higher share when they are forced to focus on merit information.

Quantifying the Impact of Dwell Time.—To get a better sense of the quantitative importance of dwell time, we investigate how increasing ΔAttention by a given amount, say one second, affects allocations. A one-second increase (reallocating 500 milliseconds from merit to outcome information) implies a shift equivalent to 23 percent of the average dwell time in the *Constrained Focus* treatment. Such an increase in ΔAttention is similar to the one produced by the *Outcome Focus* treatment, so it does not involve an extrapolation of our treatment effects.

Our analysis is based on a two-stage instrumental variable regression, where we instrument dwell time with the Focus treatment to which the subject is assigned, pooling the data at the subject level.[18] Online Appendix Table A.3 shows the result of the second-stage regressions. Column 1 shows that increasing ΔAttention by one second leads to a 2.6 percentage point decrease in allocations to the *Advantaged* members. Moreover, to compute the impact of a one-second change in ΔAttention on the effect of Status, we repeat the IV analysis separately for the *Advantaged* and *Disadvantaged* dictators in columns 2 and 3 of online Appendix Table A.3. Increasing ΔAttention by one second cuts the share that the *Advantaged* keep for themselves by 4.1 percentage points ($p < 0.001$), whereas it cuts the share that *Disadvantaged* dictators give to *Advantaged* recipients only by 0.1 percentage points, a negligible and insignificant effect. Thus, changing ΔAttention by one second reduces the gap between the allocation of *Advantaged* and *Disadvantaged* dictators by 4 percentage points ($p = 0.087$).[19] We conclude that reallocating 500 milliseconds (or 23 percent) of dwell time from outcome to merit information reduces the effect of Status on Allocation in the *Free Focus* treatment by 36 percent.

Panel B of online Appendix Table A.3 reports the corresponding results for the Impartial trials. Compared to the Involved trials, the results go in the same direction, but with a smaller effect size and less statistical significance. In particular, column 3 shows that increasing ΔAttention by one second cuts the share that the *Advantaged*

[18] The *F*-statistic of our first stage is above 550, indicating a strong instrument and a minimal expected bias in the estimates. The exclusion restriction—that attention constraints only affect allocations via dwell time—is in line with standard models of attention like drift diffusion models, which focus on dwell time as the exclusive variable (Krajbich, Armel, and Rangel 2010). We can also exclude that our restrictions have a demand effect—see Section IVC. Furthermore, the time limit on at least one box is binding in 90.5 percent 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. Pooling the data at the individual level is necessary because the instrument—the Focus treatment—varies between but not within subjects. As such, in the second stage, a participants' predicted ΔAttention is the same in every round.

[19] To obtain this *p*-value, we run an IV regression with all the data from the *Constrained Focus* treatments. In it, we included ΔAttention and its interaction term with Status, and we used the *Outcome Focus* treatment and the interaction between the Status and *Outcome Focus* treatments as instruments. We then test whether the interaction term is different from zero. Column 3 of Table A.8 in the online Appendix reports this IV estimation.

dictators give to the *Advantaged* recipients by 1.5 percentage points, a marginally significant difference ($p = 0.093$) and that the effect of Status on allocation goes down by 2.4 percentage points or 54 percent of the effect of Status on allocation found in column 4 of Table 4 ($p = 0.077$). Thus, shifting less than a quarter of the attention can eliminate more than half of the self-serving biases in allocation, as measured by the effect of Status on Impartial allocations (Konow 2000). This is a large effect, and future research should investigate the robustness of this result.

How much does endogenous attention change allocation decisions?.—Here, we use the results of the *Constrained Focus* treatments to estimate the impact of voluntary changes in attention in the *Free Focus* treatments. We focus on *Advantaged* dictators, as they are most affected by the shifts in attention. We perform a simple back-of-the-envelope calculation, using the fact that *Advantaged* dictators keep 4.1 percentage points less in the Involved trials if ΔAttention increases by one second (online Appendix Table A.3, column 3). Moreover, from online Appendix Table A.3, we know that ΔAttention is 0.35 seconds lower for the *Advantaged* dictators than for *Disadvantaged* dictators in the *Free Focus* treatment. Multiplying these two numbers, we predict that the endogenous shift in ΔAttention in the *Free Focus* treatments causes the *Advantaged* dictators to keep 1.43 percentage points less of the surplus. This drop is equal to 13 percent of the difference in allocations between *Advantaged* and *Disadvantaged* dictators in the Involved allocations of the *Free Focus* treatment. If we repeat the same calculations for the Impartial allocations, we find that the *Advantaged* dictators would have given 0.68 percentage points (or 16 percent) less to the *Advantaged* member of the pair if they had looked at the information as the *Disadvantaged* dictators did.

Of course these are crude calculations, as they assume that the effect of ΔAttention on behavior is linear and equally large across the different Focus treatments. Nevertheless, they suggest that selective attention has a nonnegligible impact on behavior, even if it does not explain the bulk of the self-serving bias.

IV. Discussion

In this section, we discuss the interpretation of our results. First, we introduce a theoretical model to guide the interpretation of our results. Then we show the impact of attention on adherence to fairness criteria. Finally, we discuss and rule out potential confounds including experimenter demand effects and processing errors and discuss other attention measures.

A. *Theoretical Interpretation*

To further guide the interpretation of our results, we provide a theoretical model in online Appendix A. Building on Konow (2000) and Cappelen et al. (2007), we assume dictators feel guilty about keeping more than their fair share, which is determined by subjectively applying fairness criteria. Following the literature, we consider three distinct fairness criteria (Cappelen et al. 2007; Bortolotti et al. 2024; Almås, Cappelen, and Tungodden 2020). 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, whereas the *Egalitarian* criterion can be implemented in the absence of any information.

We introduce attention into this framework and assume that paying attention to a fairness criterion or the information associated with this criterion increases its subjective weight. Dictators trade off attending to the criterion that reduces their guilt from keeping the money and a psychological cost of distorting attention. The extended model can generate our three main hypotheses. Moreover, under some mild additional assumptions, the model can also explain the observed, but not hypothesized, asymmetry between *Advantaged* and *Disadvantaged* dictators. The intuition for this last result comes from the way Status affects the optimality of different fairness criteria and is similar to that in Hochleitner (2022). In particular, the egalitarian criterion is often the most lucrative for *Disadvantaged*, but almost never for the *Advantaged*.[20] Hence, the *Disadvantaged* dictators are better off placing a higher subjective weight on the egalitarian criterion than the *Advantaged* dictators. Since the egalitarian split can be achieved without paying attention to any performance information (except the total surplus), this makes *Disadvantaged* dictators' decisions somewhat inelastic to attentional shifts. By contrast, the *Advantaged* dictators reduce guilt by placing a high weight on the libertarian criterion. Doing so requires them to spend enough time on outcome information, making them relatively responsive to attentional constraints in this dimension.

B. *Does Attention Affect Perceptions of Fairness?*

One way in which attention may change behavior is through the perception or internalization of normative fairness criteria. For instance, participants for whom merit information is available relatively longer may be more likely to consider this information as ethically relevant for their allocation. To investigate this mechanism, we look at dictator adherence to the three criteria described in the previous subsection.

Our main measure of fairness perceptions are dictator allocations in the Impartial trials, which eliminate considerations of personal gain. 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 percent of the total surplus size. Defined in this way, 20 percent of the choices are egalitarian, 35 percent are meritocratic, and 23 percent are libertarian.[21] Online Appendix Figure A.8 displays the fairness criteria and the choices that fit within them.

[20] This statement is true if the *Disadvantaged* don't answer many more questions correctly than the *Advantaged*, a condition that is almost always satisfied in our experiment.

[21] For example, we consider any allocation for which a member of the pair receives between 45 percent and 55 percent of the surplus to be consistent with the egalitarian criterion. Using these definitions, 78 percent of the allocations are consistent with at least 1 fairness criterion, and 66 percent of the allocations are consistent with

To investigate the impact of attention on this fairness measure, we define a dummy that takes a value of 1 if the Impartial allocation adheres to the relevant fairness criteria and regress this on the *Outcome Focus* treatment in the *Constrained Focus* treatments. Because we found above that *Advantaged* dictators are more susceptible to the *Outcome Focus* treatments, we show the aggregate effect as well as the effect split by Status.

The left panel of Figure 6 shows the results of this analysis, the details of which can be found in online Appendix Table A.15. The *Outcome Focus* treatment (relative to the *Merit Focus* treatment) causes a modest shift toward more libertarian choices and away from meritocratic choices, effects that are significant only for the *Advantaged* dictators. The *Outcome Focus* treatment does not significantly shift the egalitarian criterion (at the 5 percent level) for either group of dictators.

To further investigate these patterns, we look at a secondary measure of fairness, namely dictators' ratings of "moral appropriateness" of the different fairness norms, measured on a Likert scale from 1 to 4. The right panel of Figure 6 shows the coefficient of ordered logit regressions, where the dependent variable is the dictator endorsement of the relevant fairness criteria (see online Appendix Table A.16 for the associated regressions). The results follow the same pattern as those of the fairness allocations with the *Outcome Focus* treatment leading to a modest shift toward libertarian norms and away from meritocratic norms for *Advantaged* dictators. However, these results are noisy and statistically significant only for the shift away from meritocratic norms.[22]

An interesting further question is how the Status manipulation affects allocations and fairness views in the Impartial trials. Table A.14 in the online Appendix shows evidence that *Advantaged* dictators are about 6 percentage points less likely to make an egalitarian split and about 10 percentage points more likely to make a libertarian split. These estimates support the idea that dictators adopt self-serving views of fairness. For reasons of space, we leave a detailed examination of this effect and the relation to our secondary elicitations of fairness views to a companion paper (Amasino, Pace, and van der Weele 2023).

C. *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 20 decision rounds featured attention manipulations that were orthogonal to that of the treatment, as described in Section IID. In addition, our questionnaire

only 1 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.

[22] In addition, we asked for dictators' expectations of others' endorsement of the same norms ("social appropriateness"), using the method by Krupka and Weber (2013). Figure A.9 in the online Appendix presents an analysis of this variable, analogous to the left panel of Figure 6. We find similar asymmetries between the *Advantaged* and *Disadvantaged* dictators in the response to the *Outcome Focus* treatment, but effects are noisier and not significant. This may reflect measurement error as well as participants' uncertainty about how other participants evaluated fairness norms.

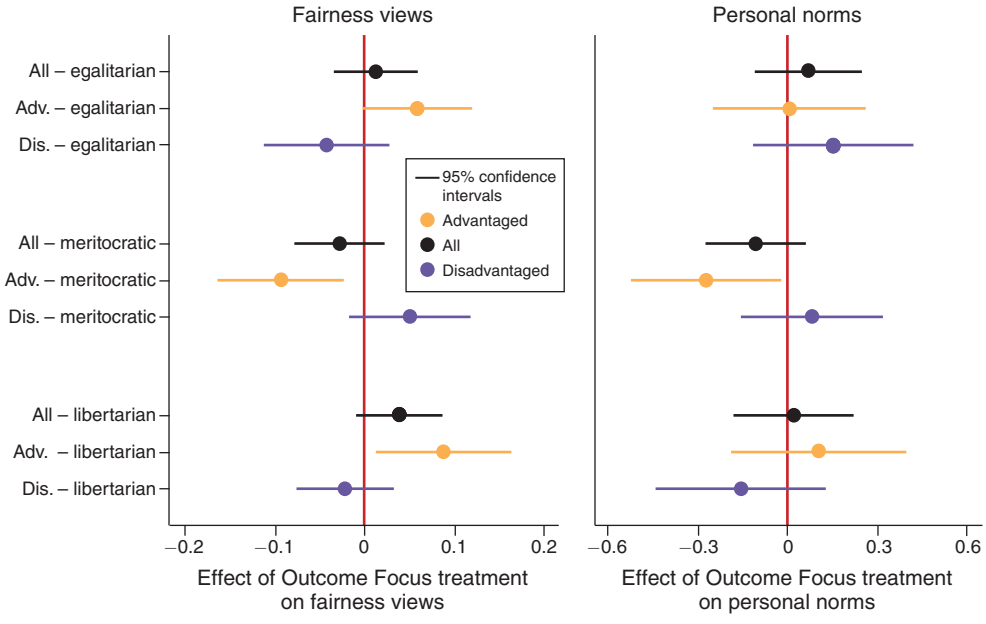

FIGURE 6.

Notes: The effect of the *Outcome Focus* treatment of Impartial choices (left panel) and Personal norms (right panel). For the left panel, the dependent variable is adherence to fairness criteria in dictator allocations in the Impartial trials. Pictured effects represent coefficients of dummy of *Outcome Focus* in linear regression models. The 95 percent confidence intervals are computed using standard errors clustered at the dictator level. For the right panel, the dependent variable is the dictators' ratings about the moral appropriateness of redistributing according to the different fairness criteria. Pictured effects represent coefficients of dummy of *Outcome Focus* in ordered logit models. The 95 percent confidence intervals are computed using robust standard errors.

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 percent of participants said they did not detect a systematic difference in box closing times. Overall, only 20 percent guessed the restrictions on both boxes correctly, a further 5.5 percent guessed one box correctly, and 8.5 percent 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 percent of participants who did not detect any difference in closing time. Table A.17 in the online Appendix provides the results of this analysis. We replicate our finding that attention changes allocation decisions. If anything, the results are *stronger* in this subsample. This further demonstrates that experimenter demand effects did not drive our results.

D. *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. The attention recognition literature suggests that recognition and memory consolidation for more complex scenes only takes up to 400 milliseconds, and other processing studies have used dwell times of 250 milliseconds or mouselab box times of 300 milliseconds (Potter 1976; Potter et al. 2014; Armel, Beaumel, and Rangel 2008; Milosavljevic et al. 2012; Pärnamets et al. 2015; Pachur et al. 2018; Ghaffari and Fiedler 2018; Fisher 2021). Therefore, 400 milliseconds 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 online Appendix Tables A.3 and A.14). 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 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 6 rounds, the restrictions were randomly allocated to other dimensions (see Section IID.). 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. Online Appendix Table A.18 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. This shows attention in any single trial does not have a strong influence on behavior but rather that it is the sustained push to attention over multiple trials that produces the effect of the *Outcome Focus* treatment.

Third, we can check whether the *Outcome Focus* treatment changes people's beliefs about the relative performance of the *Advantaged* and *Disadvantaged* dictators by examining two questions that were asked after the allocation decisions. The first question asked dictators to estimate the percentage of rounds in which the *Disadvantaged* member of the pair had a higher monetary contribution (outcome) than the *Advantaged* one. The second question asked dictators to report the number of rounds in which the recipients they were matched with answered more questions correctly (merit). We rewarded accurate answers with a £1 bonus. Table A.11 in the online Appendix shows that there are no significant differences in the beliefs in the

Merit and *Outcome Focus* treatments. These null results suggest that the attention manipulation did not alter the dictators' perceptions of the relative contribution of the recipients.

In summary, the attention manipulation did not prevent subjects from making any particular allocation. Of course, dwell times may have affected the ease with which subjects could incorporate information into decisions, but this is exactly the point of studying this variable.

E. *Information Avoidance*

Our focus is on continuous measures of dwell time and relative attention, wherein participants have processed all relevant information, but simply place different weights according to the time spent on it. This is qualitatively different from a previous literature looking at binary information avoidance designs, where participants do not have access to the information they avoid. In such cases, information avoidance may signal that participants decide independently of merit or outcome and thus have no use for the information—or that they want to avoid information in order not to face psychological conflicts from taking the most money for themselves (Dana, Weber, and Kuang 2007; Grossman and Van der Weele 2017).

We find that information avoidance does not play an important role in our experiment. Dictators open all the boxes in 85 percent of Involved trials, and avoidance of either type of information is lower than 10 percent on aggregate. In the Impartial trials, information avoidance is higher, but subjects still open all boxes in 70 percent of trials. In addition, there are no clear self-serving patterns in the avoidance behavior, as we discuss in online Appendix B.17. To confirm that avoidance does not drive our results, we replicate all our findings excluding trials with avoidance and collapsing the data at the individual level. In the online Appendix, Tables A.20 and A.21 show that all our results hold in these restricted data. Thus, it appears that selective attention occurs on the intensive rather than the extensive margin.

F. *Other Results Discussed in the Online Appendix*

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). Online Appendix B.12 replicates our findings using these other measures.

V. Conclusion

In this paper, we show that economic advantage causes selective attention, as it reduces how long people dwell on information about merit. Furthermore, we demonstrate the causal impact of dwell time on behavior, as biased attention increases the amount of money people allocate to themselves or other similarly advantaged individuals. 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 and fewer meritocratic allocations among *Advantaged* dictators. As underlying psychological mechanisms, we can rule out experimenter demand effects and processing errors, and we find evidence that sustained attentional manipulation affects the formation of fairness views. We go beyond previous literature on information avoidance, as we show that it is dwell time or the intensity of attention that drives our results.

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 toward redistribution.

These results show the importance of attention to effort and luck for redistributive behavior. More research is needed to determine the ecological validity of these claims. Evidence on self-serving biases in the laboratory have been confirmed in natural experiments (Di Tella, Galiant, and Schargrodsky 2007; Hvidberg, Kreiner, and Stantcheva 2023; Schwardmann, Tripodi, and Van der Weele 2022), so future research could establish whether the same is true for the attention channel identified in this paper. Given the complex experimental design and multiple analyses, more research examining the relationships between status, attention, and allocations across different contexts are needed to confirm the robustness of our findings.

Extrapolating for a moment beyond the laboratory, selective attention may explain why groups have different views on the nature and desirability of inequality and provide insights for a current debate about the role of meritocracy in Western society. For instance, elites' attentional habits may cement views that wealth differences are earned, explaining the findings of recent surveys. It can also explain why elites 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.

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