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Abstract

Modern societies are characterized by widespread disparities in opportunities, which play a crucial role in creating income inequality. This paper investigates how individuals handle income inequality arising from these unequal opportunities. We report from a large-scale experimental study involving general populations in the United States and Scandinavia, where participants make consequential redistributive decisions as third-party 'spectators' for workers who faced unequal opportunities. Our findings provide strong evidence that a significant majority of people are willing to accept inequality caused by unequal opportunities, a position that markedly contrasts with their responses to inequality caused by luck. Two distinct forces drive greater acceptance of inequality under unequal opportunities: the tendency to mistakenly attribute the impact of unequal opportunities to inherent productivity, and the moral relevance attributed to choice differences caused by unequal opportunities. We further demonstrate a clear societal and political divide in responses to unequal opportunities, with Americans and right-wing voters exhibiting a greater acceptance of the resulting inequality, reflecting both differences in fairness views and attribution biases in these populations.

Keywords: Unequal opportunities; Inequality acceptance; Attribution bias; Fairness views.

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1 Introduction

Unequal opportunities are a stark reality in modern societies (Chetty et al., 2014), and shape individual choices in the labor market, education, health, and other aspects of life. As a result, unequal opportunities create income inequality between people who have made different choices only because they faced different circumstances beyond their own control (Bowles, 1973; Conlisk, 1974; Corak, 2013; Jusot et al., 2013). How do people handle such income inequality that ultimately are a product of unequal opportunities? This paper provides novel evidence on this question from a large-scale experimental study engaging about 8000 participants from the general populations of the US and Scandinavia (Denmark, Norway, and Sweden).

Handling income inequality in a society characterized by unequal opportunities presents two profound challenges. First, it is typically not straightforward to determine the extent to which an income inequality reflects differences in opportunities, and people may exhibit attribution bias and mistakenly credit the inequality to differences in a person's productivity and preference (Jones and Harris, 1967). Second, the fact that people with different opportunities make different choices raises the question of whether such choices are morally relevant. For example, is someone who exerts more effort because they have greater opportunities more deserving of reward (Roemer, 1998)?

To study fairness views in an environment where income inequality stems from unequal opportunities, we designed an experiment in which individuals, termed *workers*, accumulated earnings from a real effort task. In the baseline treatment, *Unequal Opportunities*, workers were randomly assigned to different opportunities: *advantaged* workers earned more per completed task than *disadvantaged* workers. Motivated by the higher reward, advantaged workers worked longer and completed more tasks than disadvantaged workers. We then recruited people from the general populations in the US and Scandinavia to act as third-party *spectators* and make consequential decisions about whether to redistribute earnings between two workers who had faced unequal opportunities. The spectators were informed that the two workers did equally well compared to workers who had the same opportunity as themselves, and that they were expected to perform equally well if they faced the same opportunities. In this situation, do spectators choose to equalize the earnings between the two workers—recognizing that the inequality is ultimately a product of unequal opportunities—or do they opt to reward performance by allocating more to the advantaged worker, even though this greater performance is a result of advantageous opportunities?

We find that a significant majority of spectators accept inequality resulting from

unequal opportunities. Specifically, 81% of spectators assigned a higher income to the advantaged worker than to the disadvantaged worker, with almost half choosing not to redistribute any income at all. Only 16% of spectators chose to equalize the income between the two workers. Overall, the spectators reduced inequality by about a quarter as measured by the Gini coefficient: from 0.52 before redistribution to 0.39 after redistribution.

To investigate whether the low level of redistribution under unequal opportunities stems from a general acceptance of inequality, we compare the Unequal Opportunities treatment to an *Outcome Luck* treatment. In this treatment, both workers receive the same piece-rate, work for an identical duration, and complete an equal number of tasks. Outcome luck generates earnings inequality by randomly awarding a bonus payment to one worker but not the other. We calibrate this bonus to produce the same level of before-redistribution inequality as observed in the Unequal Opportunities treatment. Our findings reveal a stark contrast in spectator behaviors when inequality is caused by outcome luck versus unequal opportunities. In the Outcome Luck treatment, more than half of the spectators chose to equalize the income of the two workers, and the overall inequality as measured by the Gini coefficient is more than halved: from 0.52 before redistribution to 0.19 after redistribution. Hence, the high level of inequality acceptance observed under unequal opportunities is not an outcome of a general acceptance of inequality, but rather a specific response to inequality generated by unequal opportunities.

Having established widespread acceptance of inequality resulting from unequal opportunities, we explore the underlying mechanisms. We show that two pivotal factors contribute to this acceptance: attribution bias and a prevailing preference to reward performance, irrespective of its underlying cause.

First, people tend to misattribute the impact of advantageous opportunities to the innate productivity of the advantaged worker. To assess whether attribution bias results in an overestimation of the advantaged worker's productivity in the present study, we conducted an incentivized belief elicitation, which measured spectators' beliefs about the two workers' performances in a condition with equal opportunity. We find that almost half of the spectators exhibited attribution bias and believed that the advantaged worker outperformed the disadvantaged worker under equal opportunity. On average, spectators overestimated the advantaged worker's performance by 43% compared to the disadvantaged worker under an equal piece-rate.

To establish a causal link between attribution bias and acceptance of inequality under unequal opportunities, we implemented a third treatment, *Unequal Opportunities with Limited Information*. Here, we omitted information about how well

the workers had performed relative to others with the same piece-rate. We find that the absence of this information exacerbates the attribution bias in favor of the advantaged worker and leads to an even greater acceptance of inequality among the spectators. Hence, attribution bias significantly contributes to inequality acceptance under unequal opportunities.

Second, to examine the role of a preference for rewarding performance induced by unequal opportunities, we examine those spectators who do not exhibit an attribution bias and thus recognize that the observed earnings inequality is a result of unequal opportunities. We show that even among this group of spectators, there is a significant increase in inequality acceptance when we compare the Unequal Opportunities treatment to the Outcome Luck treatment. Hence, a preference for rewarding performance, regardless of its origin, also significantly contributes to inequality acceptance under unequal opportunities.

Next, we expand our analysis to compare the tolerance of inequality caused by unequal opportunities across different societies. We focus on the US and Scandinavia because they represent opposite extremes in terms of both income inequality and unequal opportunities among developed countries, as depicted by the Great Gatsby curve (Corak, 2013; Durlauf et al., 2022). Are these structural differences also reflected in different attitudes towards income inequality resulting from unequal opportunities?

We observe systematic differences between the US and Scandinavia. Faced with unequal opportunities, Scandinavians tend to implement significantly less inequality compared to Americans, which reflects both differences in attribution bias and in the preference for rewarding performance resulting from unequal opportunities. Americans are more prone to misattribute the effects of unequal opportunities to the innate productivity of workers than Scandinavians, and Americans have a stronger preference for rewarding performance induced by unequal opportunities.

We also observe similar differences in inequality acceptance across political orientations. Republicans accept more inequality than Democrats under unequal opportunities, reflecting that Republicans are more prone to exhibit attribution bias and more inclined to reward performance arising from unequal opportunities. Comparable differences are observed in Scandinavia when comparing spectators with right-wing political orientation to spectators with other political orientations.

Our paper contributes to the growing empirical literature studying inequality acceptance under unequal opportunities. First, a leading normative view on dealing with inequality caused by unequal opportunities is the principle of equality of opportunity (EOp) or responsibility-sensitive egalitarianism. This concept posits that inequality should be accepted when they arise from individual choices and

efforts, but not when they result from sheer luck or opportunities beyond one's control (Rawls, 1971; Cohen, 1989; Roemer, 1993, 1998). Previous empirical research on popular support for EOp, such as the studies conducted by Alesina et al. (2018) and those based on the International Social Survey Programme (e.g., Steele (2015)), has consistently shown broad endorsement of EOp in the US and Europe. These studies, taking an *ex ante* perspective, find that people generally favor a society where the set of opportunities people start with is made equal. Our study takes a different angle by examining the willingness to enforce equality of opportunity *ex post* through compensation when unequal opportunities have already led to income inequality. Contrary to the broad support for *ex ante* equality of opportunity, we find a prevailing reluctance to reduce inequality through redistribution *ex post*, offering a potential explanation for why public support for EOp does not translate into practical measures to reduce inequality caused by unequal opportunities.

The paper contributes to the literature on social preferences (Andreoni and Miller, 2002; Bartling et al., 2015; Bellemare et al., 2008; Bolton and Ockenfels, 2000; Cappelen and Tungodden, 2019; Charness and Rabin, 2002; Engelmann and Strobel, 2004; Exley and Kessler, 2024; Fehr et al., 1993; Fehr and Vollmann, 2022; Fehr and Schmidt, 2001; Rabin, 1993), and provides new insights on how the source of inequality shapes inequality acceptance (Akbaş et al., 2019; Alesina et al., 2001; Almås et al., 2020; Barr et al., 2023; Balafoutas et al., 2013; Fong, 2001; Konow, 1996, 2000, 2009; Cappelen et al., 2007, 2013, 2022; Cassar and Klein, 2019; Durante et al., 2014; Krawczyk, 2010; Mollerstrom et al., 2015*b*; Müller and Renes, 2021; Sugden and Wang, 2020; Konow, 2000; Cappelen et al., 2007, 2010, 2013; Möllerström et al., 2015*a*; Almås et al., 2020). Our work relates particularly to a contemporaneous set of compelling papers that explore the role of unequal opportunities for inequality acceptance (Andre, 2024; Bhattacharya and Mollerstrom, 2022; Dong et al., 2022; Preuss et al., 2022). Most of these papers study economic environments in which there is no endogenous choice of effort, while our paper focuses on an economic environment where unequal opportunities cause disadvantaged workers to exert less effort than advantaged workers. In this respect, we are most closely related to the important study by Andre (2024), who provides evidence that spectators do not take into account that workers have faced unequal opportunities when rewarding effort. He shows that this partly reflects uncertainty about what would have happened under equal opportunity and partly that they find it fair to reward effort even when opportunities are unequal. In contrast, we focus on understanding the extent to which and why people differentiate between inequality caused entirely by unequal opportunities and inequality caused by outcome luck, which are both external forces beyond workers' control. In a novel experimental design, we manipulate the nature of the inequality and isolate the

effect of unequal opportunities on the effort choice. We show that people are more accepting of inequality caused by unequal opportunities than inequality caused by outcome luck, and provide evidence that this reflects both attribution bias and, consistent with Andre (2024), fairness preferences. Finally, we show, using large-scale general population samples, that acceptance of inequality caused by unequal opportunities is widespread, but also that the level of attribution bias and fairness preferences vary systematically across societies and along the political spectrum.

Our study also addresses the literature on attribution bias in economics, which has shown that people systematically overestimate the influence of persistent personal characteristics over situational factors (Jones and Harris, 1967; Ross, 1977; Haggag et al., 2019, 2021; Han et al., 2022). We find that a significant share of individuals misattribute the impact of unequal opportunities to the innate productivity of those with advantageous opportunities, leading to an overestimation of their productivity. Our research contributes new insights into the role of attribution bias in redistribution choices, demonstrating how this misattribution can inflate perceptions of productivity for those in advantageous positions and contribute to higher inequality acceptance. Additionally, our findings indicate that providing comparative information about individuals with similar opportunities can mitigate attribution bias, subsequently reducing the acceptance of inequality under unequal opportunities. The notable differences in attribution bias between Americans and Scandinavians, as well as across the political spectrum, underscore the need for further research in this area.

Finally, we contribute to the cross-country comparison of attitudes and beliefs toward redistribution (Fong, 2001; Alesina and Glaeser, 2004; Alesina and Angeletos, 2005; Ashok et al., 2015; Alesina et al., 2018; Almås et al., 2020). While previous literature focuses on the comparison of fairness preferences under equal opportunity, our study compares the redistribution behaviors of Americans and Scandinavians under unequal opportunities, demonstrating that Americans are more accepting of inequality caused by such circumstances. In the existing literature, two types of redistribution-relevant beliefs are widely studied: beliefs about upward mobility (Alesina and La Ferrara, 2005; Alesina et al., 2018) and beliefs about the causes of inequality (Fong, 2001; Alesina and Angeletos, 2005; Benabou and Tirole, 2006; Mijs, 2021; Fehr and Vollmann, 2022). Americans are often found to be more optimistic about upward mobility and more likely to believe that inequality is caused by meritocratic factors, such as effort and talent, even though these beliefs are not always aligned with reality (Chetty et al., 2014; Alesina et al., 2018). We add to this literature by offering a potential explanation for this divergence in beliefs: Americans and Scandinavians draw different inferences from the same evidence. Our findings suggest that Americans are more likely to un-

derestimate the impact of external circumstances, namely unequal opportunities, and to misattribute its impact to the enduring character of others, thereby interpreting inequality as more meritocratic. In doing so, we also show that the two redistribution-relevant beliefs are deeply interconnected: underestimating the impact of unequal opportunities can lead one to overestimate the role of meritocratic factors in existing inequality, fostering the belief that moving up is possible because disadvantaged circumstances can be overcome by talent or effort.

The remainder of the paper is organized as follows: Section 2 provides a simple conceptual framework to guide the interpretation of our results. Section 3 presents the experimental design. Section 4 reports the main results. Section 5 explores the underlying mechanisms that make people accept inequality caused by unequal opportunities. Section 6 estimates the prevalence of different fairness views. Section 7 reports heterogeneity analyses across societies and along the political spectrum. Section 8 concludes. Additional analysis and details about the study are provided in the Online Appendix.

2 Conceptual Framework

Here, we present a simple conceptual framework to illustrate the economic environment and the decision problem of the spectator.

The nature of the inequality: A worker chooses how much to work (effort), h, to maximize the utility function $U(e, l)$, where e is earnings, $l = T - h$ is leisure time, and T is the overall time constraint.

The worker earns a piece-rate ω for each task completed when working and a non-work compensation normalized to one for each unit of leisure. Let $\theta f(h)$ denote the number of tasks completed (performance) by a worker with productivity $\theta > 0$ given work time h, where $f'(h) > 0$ and $f''(h) < 0$. Earnings are given by $e(h) = \omega\theta f(h) + (T - h)$.

It follows that the optimal choice of work time (interior solution) is determined by the worker's preferences (U_l/U_e), the worker's productivity (θ), and the piece-rate (ω):

$$U_l/U_e = \omega \cdot \theta f'(h^*) - 1 \tag{1}$$

To study how people handle inequality caused entirely by unequal opportunities, the spectator considers two workers with the same preferences and the same productivity but assigned different piece-rates: One worker is *advantaged* (a) and earns a high piece-rate ω_H, the other worker is *disadvantaged* (da) and earns a

low piece-rate ω_L. The unequal opportunities lead them to make different effort choices and achieve different performances and earnings - they would have made the same choice of effort and had the same performance and earnings if they had the same piece-rate.

We focus on a situation where the workers' utility function is strictly increasing in earnings ($U_e > 0$), but there is no intrinsic utility from leisure ($U_l = 0$). In this situation, the cost of effort is given by the non-work compensation, and the workers stop working when $\omega \cdot \theta f'(h^*) = 1$. Hence, the advantaged worker exerts more effort and has greater performance and earnings than the disadvantaged worker.

Spectator choice: The spectator decides whether to redistribute the earnings between the advantaged worker (a) and the disadvantaged worker (da). There is no cost of redistribution, $e_a + e_{da} = y_a + y_{da}$, where y_a and y_{da} are the incomes of the advantaged worker and the disadvantaged worker after redistribution. We assume that the spectator has a preference for fairness, as captured by the following simple utility function (Almås et al., 2020): $V(y_a) = -(y_a - m_a)^2$, where m_a represents the spectator's view of what is the fair income to the advantaged worker. It follows directly that the spectator implements what they consider fair, $y_a = m_a$.

3 Experimental Design

Here, we provide an overview of the experimental design, which follows closely the conceptual framework.[1] There are two types of participants in the experiment: workers, who perform simple real-effort tasks and accumulate earnings, and spectators, who make real redistribution decisions about the earnings of a pair of workers. We first present the samples and the experiment procedures for the main treatment, Unequal Opportunities. Subsequently, we outline how the two other treatments, Outcome Luck and Unequal Opportunities with Limited Information, differ from the main treatment. Additional details on the experimental design are provided in Appendix B.1.

3.1 Workers

Sample: Workers are recruited from the population of US residents on the international online data collection platform Prolific. Our final sample contains a total of 1,068 workers, grouped into 534 pairs. Data collection for the workers took place between December 2021 and June 2022.

[1] The study was pre-registered at the AEA RCT Registry under AEARCTR-0009740.

Earnings: Workers accumulate earnings in a 30-minute session. Workers allocate their time between work and leisure. The work is to complete simple counting tasks (Abeler et al., 2011), for which they earn a piece-rate for each correctly completed task. The difficulty of the real-effort task increases with each task, which ensures the concavity of the production function. The workers can choose to stop working at any point, and earn a non-work compensation of \$0.25 per minute of leisure. A worker's total *earnings* are defined as the sum of their earnings from working and their non-work compensation.

Unequal opportunities: Workers are randomly assigned, with equal probability, to either a *high piece-rate* of \$0.6 per completed task or a *low piece-rate* of \$0.1 per completed task. The piece-rate is announced to the worker at the beginning of the session, and the worker is unaware of the existence of the other piece-rate. We refer to the worker who earns a high piece-rate as the *advantaged* worker and the worker who earns a low piece-rate as the *disadvantaged* worker.

Preferences and productivity: To ensure that earnings inequality results solely from unequal opportunities, not differences in preference or productivity, we minimize the intrinsic utility workers derive from leisure and match workers with the same productivity.

To make the intrinsic leisure utility as small as possible, leisure time involves a minimally engaging activity. The workers must remain on the study website to qualify for the non-work compensation during the leisure time. This requirement is enforced through frequent attention checks that appear at regular intervals. Hence, the cost of effort is given by the non-work compensation, which is the same for all workers.

To equalize productivity of the two workers, we match advantaged workers and disadvantaged workers with the same *within-group ranking*. A worker's within-group ranking is their ranking in terms of performance among 100 workers who were assigned the same piece-rate. The workers were randomly assigned to a piece-rate, and it thus follows from the conceptual framework that an advantaged worker and a disadvantaged worker with the same within-group ranking have the same productivity.

Test of experimental design: Here, we provide evidence showing that the experimental design succeeded in creating an economic environment in which unequal opportunities caused advantaged workers to complete more tasks than the disadvantaged workers, and that this inequality was only caused by unequal opportunities.

In the left panel of Figure 1, we show the number of completed tasks for advan-

taged and disadvantaged workers with the same within-group ranking, across the performance distribution. For all within-group ranks, we observe, consistent with the conceptual framework, that the advantaged workers complete more tasks than the disadvantaged workers. On average, advantaged workers worked more than three times as many minutes (18 vs. 5 minutes), completed more than twice as many tasks (40 vs. 17 tasks), and earned more than three time as much ($25.9 versus $7.9) as the disadvantaged workers.

In contrast, in the right panel of Figure 1, we show that workers with the same within-group rank had exactly the same performance when they had equal opportunity. The session with equal opportunity was conducted prior to the unequal opportunities session.[2] It is identical to the unequal opportunities session, except that all workers are paid an equal piece-rate of $0.1.

Taken together, the two panels in Figure 1 show that the experimental design succeeded in establishing an inequality that was only driven by unequal opportunities, and not by differences in preferences or productivity.

3.2 Spectators

Sample: We recruit participants as spectators from the general populations of the US and Scandinavia (Denmark, Norway, and Sweden) using the survey agency Norstat. The main sample consists of 6,824 spectators, with 3,051 from the US and 3,773 from Scandinavia.[3] Data collection for the spectator side took place between June and September 2022.

Table A.2 provides the descriptive characteristics of the main sample. Overall, the sample is quite balanced on gender, with slightly more female respondents, and diverse in terms of age, education, and income levels.

Spectator - the nature of the inequality: Each spectator is paired with two workers, one advantaged and one disadvantaged. The two workers have the same within-group ranking, but the advantaged worker has worked longer, completed more tasks, and has higher earnings.

Spectator - information: The spectators are informed that the workers have

[2]The equal opportunity session was implemented to allow us to elicit incentivized beliefs about worker productivity from the spectators, and the workers were paid their earnings from this session.

[3]In total, 8,041 respondents completed the study. We exclude, as pre-specified, the 1,059 respondents who failed the attention check. We also exclude 158 respondents who faced worker pairs in the Unequal Opportunities treatments who did not fulfill the design criteria that the advantaged worker in the pair has worked longer, completed more tasks, and has higher earnings. Our results remain unchanged to including all 8,041 respondents, as shown in Appendix C.2.

been randomly assigned to a high piece-rate or to a low piece-rate, and given information about the workers' piece-rates, number of tasks completed, working time, total earnings, and within-group ranking (see Figure D.8).

We convey to the spectators that the inequality between the two workers is caused by unequal opportunities, not by differences in productivity or preferences. In particular, we inform them that two workers with the same within-group ranking are in expectation equally productive, and that workers typically stop working when their average earnings per minute from work is below the rate of non-work compensation.

Spectator choice: Spectators decide on the distribution of *income* between the advantaged worker and the disadvantaged worker. Each spectator makes a single redistribution decision for one pair of workers. Each pair of workers is assigned to on average 15 different spectators from whom one decision is randomly implemented.

Belief elicitation: After the spectators have made their redistribution decision, we implement an incentivized elicitation of their beliefs about each of the two workers' performance in the session with equal opportunity.[4]

Survey: At the end of the session, spectators complete a non-incentivized survey about their socioeconomic characteristics, including education, income, age, gender, and region of residence, and their general view on redistribution and political orientation.

3.3 Treatments

We have so far outlined the structure of the Unequal Opportunities treatment. In the two other treatments, the spectators consider the same inequality in earnings, but we vary the source of the inequality in the Outcome Luck treatment and the information given to the spectators in the Unequal Opportunities with Limited Information treatment.

Outcome Luck: In this treatment, the spectators are given the same information about the workers as in the Unequal Opportunities treatment, but the source of the earnings inequality is luck. Each worker is paid a \$0.1 piece-rate, and workers with the same productivity therefore make the same effort choice and have equal performance. As in the Unequal Opportunities treatment, we rank workers by performance and match workers with the same performance. The earnings inequality is generated by randomly assigning one worker (the advantaged) in

[4]The spectator is paid a bonus of \$2 if their belief estimate is within a two-task margin of the actual performance. We randomly select one of the two beliefs for payment.

each pair an unannounced bonus payment at the end of the 30-minutes session, whereas the other worker (the disadvantaged) receives no bonus. We calibrate the bonus such that the earnings inequality matches exactly the earnings inequality in the Unequal Opportunities treatment.

Unequal Opportunities with Limited Information: In this treatment, the source of inequality is the same as in the Unequal Opportunities treatment, but the spectators are not informed about the within-group ranking of the two workers. Otherwise, they are given the same information as in the Unequal Opportunities treatment.

4 Results: Inequality Acceptance

We start by considering how spectators handle earnings inequality in the Unequal Opportunities treatment, before we compare it to inequality acceptance in the Outcome Luck treatment.

4.1 Unequal Opportunities

In Figure 2, we provide an overview of the spectator choices when the spectators consider an inequality caused by unequal opportunities and have full information about the nature of the inequality.

In the upper panel, we report the share of spectators who allocate more income to the advantaged worker than to the disadvantaged worker, the share of spectators who equalize, and the share of spectators who reverse the earnings inequality. We observe that a large majority of the spectators (81%) find it fair that the advantaged worker has a higher income than the disadvantaged worker when the inequality is caused by unequal opportunities. In contrast, only a small minority of the spectators (16%) choose to equalize the income of the two workers.[5] Very few spectators (4%) reverse the inequality in favor of the disadvantaged worker. On average, the spectators allocate an income to the advantaged worker that is more than double that of the disadvantaged worker, \$23.2 vs. \$10.6.

The lower panel shows the after-redistribution income inequality across the range of before-redistribution earnings inequality, where both inequalities are measured by the Gini coefficient.[6] We observe that the acceptance of inequality caused by

[5]We define a spectator to equalize the incomes of the two workers if they implement an income differences of less than \$0.5. Our results are not sensitive to this cutoff, as shown in Appendix Table C.19.

[6]The Gini coefficient is given by $\left|\frac{y_L - y_H}{y_L + y_H}\right| \in \{0, 1\}$, where y_L, y_H are the incomes of the two

unequal opportunities applies at all levels of before-redistribution inequality, with a majority of the spectators accepting all of the inequality or even increasing it (57%). Overall, there is limited redistribution from the advantaged worker to the disadvantaged worker: the spectators reduce the inequality from a before-redistribution Gini coefficient of 0.52 to a after-redistribution Gini coefficient of 0.39 .

We can thus state our first main finding:

Result 1: A large majority of spectators accept earnings inequality caused by unequal opportunities, allocating more income to the advantaged worker than to the disadvantaged worker.

4.2 Unequal Opportunities versus Outcome Luck

In this part, we explore whether inequality acceptance under unequal opportunities is caused by unequal opportunities inducing advantaged and disadvantaged workers to make different effort and performance choices. For this purpose, we compare inequality acceptance in the Unequal Opportunities treatment and the Outcome Luck treatment, which only differ in the nature of the inequality. In both treatments, the inequality is caused by factors outside the workers' control, but only in Unequal Opportunities the advantaged worker is incentivized to exert more effort than the disadvantaged worker.

The top panels of Figure 3 report the average after-redistribution Gini coefficient (left panel) and the share of spectators who equalize the income of the two workers (right panel) in the Unequal Opportunities and Outcome Luck treatments. In the left panel, we observe that spectators in the Outcome Luck treatment, who face the same before-redistribution inequality as the spectators in the Unequal Opportunities, reduce the after-redistribution Gini coefficient to about half of what is implemented when there are unequal opportunities: 0.19 versus 0.39 ($p < 0.001$). In the right panel, we show that the lower inequality acceptance in the Outcome Luck treatment causes a much larger share of the spectators to equalize than in the Unequal Opportunities treatment: 55% versus 16% ($p < 0.001$).

The bottom panels of Figure 3 show the after-redistribution Gini coefficients (left panel) and the share of spectators who equalize (right panel) across the distribution of worker pair productivity. We observe that the after-redistribution Gini coefficient in the Unequal Opportunities treatment is higher than in the Outcome Luck treatment across productivity levels, and the share equalizing is lower. Hence, the

workers. The Gini coefficient is equal to 1 if one worker is allocated all the income, and is equal to 0 if the workers are allocated the same income.

increased inequality acceptance in the Unequal Opportunities treatment compared to the Outcome Luck treatment is not driven by spectators becoming particularly accepting of inequality when workers are highly productive or unproductive.

Finally, in Appendix Table A.3 we report the corresponding regression analyses. We show that the results are robust to controlling for the characteristics of the spectator (society of residence, gender, age, education, income, and political orientation) and worker-pair fixed effects.

We summarize this analysis as our second main finding:

Result 2: Spectators are more willing to accept inequality caused by unequal opportunities than inequality caused by outcome luck.

5 Mechanisms

We have shown that the majority of people accept some inequality caused by unequal opportunities but are much less accepting of the same inequality when it is caused by outcome luck. We explore two mechanisms that may explain why people handle the same inequality differently in these two situations. First, in the Unequal Opportunities treatment, spectators may misattribute the performance difference to the advantaged worker being more productive than the disadvantaged worker rather than to unequal opportunities. In the Outcome Luck treatment, there is no scope for such misattribution because the two workers have the same performance. Second, even if spectators are unbiased and recognize that the performance difference is driven by unequal opportunities, they may still find it fair that the better-performing worker receives a higher income.

5.1 Attribution Bias

Attribution bias, or the fundamental attribution error, is the tendency to overestimate the influence of personal characteristics in explaining individual behaviors, and underestimate the influence of external factors (Jones and Harris, 1967; Ross, 1977; Haggag et al., 2019, 2021; Han et al., 2022). In our context, the performance difference between the two workers is, by design, driven by unequal opportunities, where the difference in piece-rate makes the advantaged worker exert more effort than the disadvantaged worker. However, an attribution-biased spectator may underestimate the influence of unequal opportunities (external factor), and attribute the performance difference to differences in worker productivity (personal characteristics). Consequently, an attribution-biased spectator may infer that the advantaged worker is more productive than the disadvantaged worker based on the observed performance difference under unequal opportunities.

To test whether spectators overestimate the productivity of the advantaged worker under unequal opportunities, we elicit their beliefs about the performance of the two workers when they have equal opportunity. Attribution bias is defined as the ratio of the *believed* performance difference under *equal opportunity* to the *observed* performance difference under *unequal opportunities*.[7] It measures the extent to which spectators attribute the performance differences caused by unequal opportunities to differences in worker productivity. An attribution bias of 0 means the spectator believes there is no performance difference under equal opportunity, indicating that the spectator attributes all of the performance difference to unequal opportunities. An attribution bias of 1 means the spectator believes the performance difference to be the same under equal opportunity as under unequal opportunities, indicating that the spectator attributes all of the performance difference to a difference in worker productivity.

Both the advantaged worker and the disadvantaged worker complete 15 tasks when they have equal opportunity and are assigned the low piece-rate, proving that they have the same productivity. The top panel of Figure 4 shows the beliefs of spectators about the number of tasks completed by the two workers under equal opportunity, which measures their beliefs about the productivity of the two workers. On average, spectators believe that the advantaged worker completed 25 tasks and that the disadvantaged worker completed 17 tasks when both were assigned the low piece-rate ($p<0.001$). The tendency to believe that the advantaged worker performs better under equal opportunity persists across the productivity distribution. Regardless of the within-group ranking of the worker pair, spectators believe that the advantaged worker completed around 30% more tasks than the disadvantaged worker with the same within-group ranking. Taken together, the top panel demonstrates that spectators are, on average, attribution-biased and misattribute some of the performance difference under unequal opportunities to differences in worker productivity.

The bottom panel of Figure 4 shows the distribution of the attribution bias. About half of the spectators (48%) are not attribution biased, and believe that the two workers perform the same under equal opportunity. In contrast, 20% of spectators are fully attribution-biased, and believe that the performance difference between the advantaged and disadvantaged worker under unequal opportunities would re-

[7] Formally, the we measure the attribution bias as $ab = \frac{\tilde{p}_H(\omega_L) - \tilde{p}_L(\omega_L)}{p_H(\omega_H) - p_L(\omega_L)}$, where $p_H(\omega_H)$, $p_L(\omega_L)$ are the number of tasks completed by the advantaged worker under the high piece-rate and by the disadvantaged worker under the low piece-rate when there are unequal opportunities, and $\tilde{p}_H(\omega_L), \tilde{p}_L(\omega_L)$ are the spectator's beliefs about the number of tasks completed by the two workers under the low piece-rate when there is equal opportunity. In line with how we incentivized the spectators in the belief elicitation, we define a spectator as unbiased if the difference in the beliefs about the two workers is two tasks or less. Our results are robust to alternative cutoffs.

main the same under equal opportunity. A similar share of spectators, 21%, are partially attribution-biased, and believe that the advantaged worker performs better under equal opportunity than the disadvantaged worker, but also that the performance difference is smaller than under unequal opportunities. Taken together, these findings provide strong evidence of attribution bias among a substantial share of spectators.

Result 3: A substantial share of spectators are attribution-biased and believe the advantaged worker is more productive than the disadvantaged worker.

We now turn to study whether the attribution bias contributes to inequality acceptance under unequal opportunities. To identify a causal effect of the attribution bias on inequality acceptance, we introduce the *Unequal Opportunities with Limited Information* treatment. This treatment is identical to the Unequal Opportunities Full Information treatment, except that we do not provide the within-group ranking information to the spectators. Spectators therefore have to make inferences about the productivity of the two workers under greater uncertainty than in the Unequal Opportunities treatment, leaving more room for attribution bias.

In the left panel of Figure 5, we show that providing information about the within-group ranking of the two workers causes a decrease in the attribution bias. The average size of attribution bias decreases by about 25%, from attributing 42% of the observed performance difference under unequal opportunities to productivity to 32% ($p<0.001$), as we move from the Limited Information treatment to the Full Information treatment. The share of unbiased spectators also increases significantly when providing this information, from 28% to 48% ($p<0.001$). In the right panel, we show the treatment effect on inequality acceptance. We observe that providing the within-group rank of the two workers causes a decrease in the after-redistribution Gini by about 10%, from 0.43 to 0.39, and an increase in the share of spectators equalizing, from 9% to 16%. Taken together, the evidence suggests that attribution bias contributes to inequality acceptance when there are unequal opportunities.

The treatment effect on the share of spectators equalizing (right panel) is about one-third of the treatment effect on the the share of unbiased spectators (left panel), and it is consistent with some spectators accepting inequality when they have limited information because they are attribution biased, but not accepting it when they have complete information and are unbiased. Consistent with this, we find that the treatment effect on the share of participants who are unbiased and equalize is almost identical to the treatment effect on the share of participants who equalize, 6.8% versus 7.2%.

In Appendix Table A.4, we report the corresponding regression analysis, which shows that the findings are robust to controlling for the background characteristics of the spectator and worker-pair fixed effects. Hence, we can state the following result:

Result 4: Attribution bias contributes to inequality acceptance under unequal opportunities.

5.2 Inequality acceptance among unbiased spectators

Half of the spectators are not attribution-biased in the Unequal Opportunities treatment, as shown in Figure 4, and fully recognize that the two workers are equally productive and that the performance difference is entirely driven by unequal opportunities. To study whether the performance difference still contributes to inequality acceptance, we compare the redistribution behavior of the unbiased spectators in the Unequal Opportunities treatment and the unbiased spectators in the Outcome Luck treatment.[8]

Figure 6 reports inequality acceptance in the unbiased sample in the two treatments. In the upper left panel, we observe that the after-redistribution Gini coefficient among the unbiased spectators under unequal opportunities is about twice the after-redistribution Gini coefficient among the unbiased spectators under outcome luck, 0.35 versus 0.17 ($p < 0.001$). In the upper right panel, we see a corresponding reduction in the share of spectators that equalize the income between the two workers, 22% versus 61% ($p < 0.001$). In the lower panels, we show that these patterns are robust across the productivity distribution, where we at all levels observe a significant increase in the after-redistribution Gini coefficient and a large decrease in the share of spectators equalizing when comparing the Unequal Opportunities treatment to the Outcome Luck treatment. Taken together, the findings demonstrate that unbiased spectators are more willing to accept inequality caused by unequal opportunities than inequality caused by outcome luck, which shows that they find the performance difference to be relevant for whether they accept an inequality.

[8]Details about how we elicited the attribution bias in the Outcome Luck treatment is provided in Appendix B.1.

Result 5: Even among spectators who recognize that performance differences are entirely caused by unequal opportunities, we observe greater inequality acceptance under unequal opportunities compared to outcome luck.

6 Fairness Views

There are three salient fairness views in the economic environments that we study: libertarianism, egalitarianism, and meritocracy (Almås et al., 2020).

Definitions. The three fairness views can be defined as follows (see also Appendix B.2):

- **Libertarianism:** Income inequality is fair, regardless of the source of inequality.
- **Egalitarianism:** Income inequality is unfair, regardless of the source of inequality.
- **Meritocracy:** If one person has greater merit than another person, then it is fair that this person has greater income; if they have the same merit, then they should have the same income.

A spectator motivated by the libertarian fairness view would accept inequality both when it is caused by unequal opportunities and when it is caused by outcome luck, while a spectator motivated by the egalitarian fairness view would always equalize. In contrast, a meritocratic spectator would accept inequality only if they consider the advantaged worker to have greater merit than the disadvantaged worker. In our setting, there are two versions of meritocracy, defined by whether it is productivity or performance that is considered meritorious:

- **Equal-Opportunity Meritocracy:** If one person has greater productivity than another person, then it is fair that this person has greater income; if they have the same productivity, then they should have the same income.
- **Unconditional Meritocracy:** If one person has greater performance than another person, then it is fair that this person has greater income; if they have the same performance, then they should have the same income.

Equal-Opportunity meritocracy views an income inequality to be fair only when it reflects differences in performance that would arise under equal opportunity, which in the present study would be the case if the inequality reflects that one person is more productive than another person. A spectator with this fairness view would thus equalize if they are unbiased and recognize that the workers are equally productive, but accept some inequality if they are biased and believe that

the advantaged worker is more productive than the disadvantaged worker. Unconditional meritocracy would only focus on performance and find it fair that the advantaged worker has more income than the disadvantaged worker, even if they recognize that the performance difference is entirely driven by unequal opportunities. Thus only equal-opportunity meritocrats would take into account their beliefs about the productivity of the advantaged worker and the disadvantaged worker. For the other spectators, these beliefs are not relevant for their fairness considerations.

Estimating prevalence of fairness views. To estimate the prevalence of different fairness views among the spectators, we exploit the between-subject design and the identified treatment differences. This approach allows us to estimate the share of spectators motivated by each fairness view with minimal assumptions, as we outline in more detail in Appendix B.2.

Figure 7 provides an overview of the estimated shares of spectators adhering to each fairness view. We find that 29% of spectators can be classified as libertarians, 5% as egalitarians, and 50% as meritocrats, which means that these three fairness types can account for the large majority of spectator choices. Among meritocrats, we estimate that at least 7% are equal-opportunity meritocrats and 19% are unconditional meritocrats. The remaining meritocrats (24%) could be of either type.

Understanding unconditional meritocrats. The substantial share of unconditional meritocrats contributes to explain the high level of inequality acceptance under unequal opportunities. These spectators reward the advantaged worker for their performance despite knowing that the performance difference is entirely driven by opportunities. To gain a deeper understanding of their behavior, we examine in greater detail how much inequality they accept between the advantaged worker and the disadvantaged worker under unequal opportunities.

In our environment, spectators know both the performance and the working time of the two workers, allowing them to follow a rule of distributing income in proportion to performance or effort. However, we find that most unconditional meritocrats do not follow a proportional rule.

Instead, we observe two main patterns of behavior among unconditional meritocrats: First, a significant share of unconditional meritocrats do not redistribute at all when there is a performance difference, suggesting they view the earnings inequality as fair when choices are involved. Comparing the unbiased samples in the Unequal Opportunities and Outcome Luck treatments, we find an 18 percentage point increase in spectators choosing not to redistribute under Unequal Opportunities. This translates into a 9 percentage point increase in no redistribution at

the population level and accounts for about 50% of unconditional meritocrats.

Second, among those who do redistribute, many appear to follow what we call the "substitution effect principle". According to this principle, the advantaged worker should keep earnings from additional effort relative to the disadvantaged worker, but not additional earnings from the higher piece-rate for the same level of effort. Figure 8 shows that the substitution effect principle aligns more closely with actual redistribution decisions than the proportional rules, especially for low and medium worker performance levels.

To summarize, we find limited support for the proportionality principle among unconditional meritocrats. Instead, some view all earnings inequality as fair when choices are involved, while others attempt to isolate and reward earnings directly attributable to differential effort.

7 Heterogeneity

In this section, we first explore differences in inequality acceptance under unequal opportunities between American and Scandinavian spectators, followed by analyzing decisions by political orientations.

US vs. Scandinavia

We find systematic differences between the two societies in the redistribution choices under unequal opportunities. As shown in Figure 9, spectators in the US accept substantially more inequality than spectators in Scandinavia under unequal opportunities. Comparing the distributions of after-redistribution Gini coefficients under unequal opportunities, we identify two main patterns: First, spectators in Scandinavia choose to equalize income between the two workers more than twice as often as American spectators (21% vs. 9%). They are also more likely to redistribute some amount towards the disadvantaged worker without fully equalizing (29% among Scandinavians vs. 18% among Americans). Second, American spectators are much more likely than their Scandinavian counterparts to make no redistribution (61% among Americans vs. 38% among Scandinavians). Together, these differences imply a large reduction in the average Gini after redistribution in Scandinavia (from 0.52 to 0.35) and a small reduction in the US (from 0.52 to 0.44, $p < 0.001$).

While it is not unexpected that Scandinavians accept less inequality than Americans, the mechanisms behind the differences are novel: differences in attribution biases and differences in fairness views on performance differences caused by unequal opportunities. As seen in Appendix Figure A.1, the differences in attribution

bias are substantial between the two societies. Focusing on the Full Information treatment, we find that while more than half (59%) of Scandinavians hold unbiased belief, only 36% of Americans share this view. Notably, 34% of Americans believe that the performance difference under unequal opportunities would persist under equal opportunity, indicating completely attributing the observed performance difference to differences in productivity.[9] This difference in beliefs is particularly striking because the same evidence is presented to spectators in both societies, yet they draw very different conclusions regarding the productivity of the two workers.

Analyzing redistribution decisions among the sub-sample of unbiased spectators indicates that preferences to reward performance caused by unequal opportunities also differ between the two societies. Unbiased spectators, who fully realize that the two workers are otherwise identical except for the opportunities, reduce the after-redistribution Gini to 0.33 in Scandinavia, whereas unbiased spectators in the US implement a significantly higher Gini of 0.40 ($p < 0.001$).

Having established that the societal difference results from both differences in attribution bias and fairness views, we estimate the contribution of each factor through comparing the difference in after-redistribution Gini with and without controlling for the attribution bias. We present the regression results in Appendix Figure A.2. The leftmost panel shows that controlling for the level of the attribution bias reduces the coefficient for the US by approximately 23% (from 0.08 to 0.06) across our two treatments. Thus, at least 23% of the differences between the two societies in inequality acceptance under unequal opportunities can be explained by how they attribute performances to innate productivity versus external circumstances, and its effect can be larger if we take into account the measurement errors in the belief elicitation. We attribute the remaining societal differences to divergences in fairness views.

Lastly, to examine the fairness view divergence between the two societies more closely, we estimate the share of each fairness view by society and display them in Appendix Figure A.3. Consistent with previous research (Alesina and Glaeser, 2004; Almås et al., 2020), we find a substantially greater share of libertarians in the US (40% vs. 21%) and a small, but greater share of egalitarians in Scandinavia (6% vs. 4%). Meritocrats form a large share in both societies, yet a much larger one in Scandinavia than in the US (59% vs. 39%).

Among the meritocrats we can cleanly identify as equal-opportunity or uncondi-

[9]Reducing the information available to the spectators in the Limited Information treatment shifts individuals in both the US and Scandinavia away from holding unbiased beliefs, but the shift is smaller in the US than in Scandinavia, as can be seen in Appendix Table A.11.

tional, there are more equal-opportunity meritocrats in Scandinavia than in the US (10% vs. 4%), and they make up a larger proportion of the identifiable meritocrats (40% vs. 33%). This difference in composition suggests that there are differences in the definition of merit between the two societies, and meritocrats in the US are more likely to view effort and performance caused by unequal opportunities as morally relevant.

We summarize these findings in Result 6.

Result 6: Americans are systematically more willing to accept inequality caused by unequal opportunities than Scandinavians. The difference between the two societies is partially caused by Americans' higher tendency to be attribution-biased.

Political Orientation

The bottom panel of Figure 9 shows that differences in redistribution under unequal opportunities are evident not only between Scandinavia and the US but also across political orientations within these societies. Mirroring the societal comparison, right-wing spectators accept more inequality than not right-wing spectators.[10] Starting with the same before-redistribution earnings inequality, right-wing spectators redistribute to achieve an after-redistribution Gini of 0.44, while spectators that are not right-wing reduces it to 0.39. Right-wing spectators are both less likely to fully equalize the income of the two workers and less likely to partially redistribute from the advantaged worker to the disadvantaged worker than their not right-wing counterparts.

In the US, the difference between right-wing and not right-wing in redistribution under unequal opportunities is a result of both differences in attribution bias and in fairness views. Republicans are more likely to misattribute the higher production of the advantaged worker to their productivity than non-Republicans. Appendix Figure A.1 shows that the most common belief among Republicans is fully biased, attributing the observed difference in performance solely to the advantaged worker's higher productivity. In contrast, the most common belief among non-Republicans is unbiased. Regarding preferences, unbiased Republicans are more willing to accept inequality than unbiased non-Republicans (0.39 vs. 0.35 after-redistribution Gini coefficients, $p < 0.001$), indicating that Republicans have a stronger preference for rewarding performance caused by unequal opportunities. Results in Appendix Figure A.2 indicate that the disparity between Republicans

[10]While right-wing spectators are clearly defined as Republicans in our American sample, the definition of right-wing is less straightforward in a Scandinavian context. We provide the detailed definition of right-wing in Scandinavia in Appendix Table A.1.

and non-Republicans is primarily caused by differences in fairness views, which account for 88% of the difference in redistribution, while differences in attribution bias explain the remaining 12%. In addition, the figure shows that the differences in redistribution under unequal opportunities across the political spectrum are not explained by demographic differences among individuals of different political orientation.

Interestingly, higher inequality acceptance among the right-wing in Scandinavia is solely caused by their greater willingness to reward performance induced by advantageous opportunities than their counterparts. Right-wing spectators and those of other political orientations in Scandinavia are equally likely to hold biased beliefs, and controlling for belief bias has no significant impact on after-redistribution Gini between the two groups (see the leftmost column of Appendix Figure A.2). This result suggests a difference in the nature of right-wing politics between the two societies.

Result 7: Right-wing spectators are more accepting of inequality caused by unequal opportunities than non-right-wing spectators. While the higher inequality acceptance among right-wing in the US is partly driven by a greater tendency towards attribution bias, the higher inequality acceptance among Scandinavian right-wing primarily reflects differences in fairness views.

8 Conclusion

In this paper, we investigate how individuals handle income inequality arising from unequal opportunities using a large-scale experimental study involving general population from the United States and Scandinavia. Our findings provide strong evidence that a significant majority of people are willing to accept inequality caused entirely by unequal opportunities. This position contrasts sharply with their reactions to inequality caused by outcome luck. The acceptance of inequality under unequal opportunities is driven by two distinct forces: the tendency to mistakenly attribute the impact of unequal opportunities to inherent productivity, and the moral relevance attributed to performance differences caused by unequal opportunities.

We demonstrate that people tend to overestimate the advantaged worker's productivity and underestimate the impact of unequal opportunities on individual choices. Providing counterfactual information in the form of workers' performance rankings relative to other workers with the same opportunity can effectively reduce this bias and consequentially reduce people's tolerance of inequality caused by unequal opportunities. However, even with the provision of such information, nearly

half of the spectators in our study still exhibit this attribution bias. This finding has significant implications for implementing equality of opportunity in practice, as it suggests that even when provided with complete information, individuals may underestimate the contribution of unequal opportunities to income inequality. Consequently, this bias can lead to an inflated perception of the productivity of those in advantageous positions, potentially perpetuating inequality.

Our study also uncovers clear societal and political differences in responses to inequality caused by unequal opportunities. Americans and right-wing voters exhibit a greater acceptance of resulting inequality compared to their Scandinavian and left-wing counterparts. Notably, our findings suggest that the higher acceptance of inequality under unequal opportunities among Americans can be partly attributed to their greater propensity for attribution bias. This insight provides a new perspective on the observed differences in existing inequality between the two societies, suggesting that they may not solely stem from different fairness views but also from how people draw different inferences from the same information. Americans are more inclined to underestimating the role of external circumstances and overestimating the importance of individual characteristics when assessing outcomes.

These findings contribute significantly to understanding the discrepancy between the widespread public support for equality of opportunity and the lack of practical measures to reduce inequality caused by unequal opportunities. Our results suggest that even when individuals endorse equality of opportunities *ex ante*, they may be reluctant to reduce inequality through redistribution *ex post* when unequal opportunities have already led to income inequality. This suggests that fairness perceptions are not entirely independent of existing inequality levels. Our study thus highlights the importance of addressing unequal opportunities early, before they translate into significant income inequality that become more challenging to reduce due to attribution bias and fairness views.

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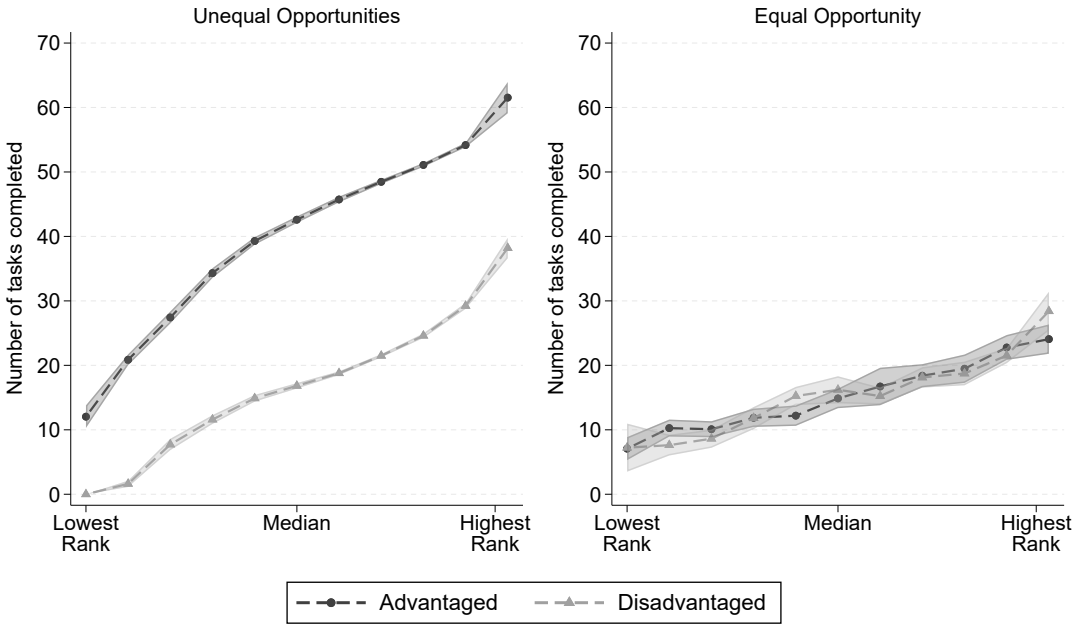

Figure 1: Worker performance: Unequal vs. equal opportunities

NOTE: Average performance of advantaged and disadvantaged workers across the worker performance distribution. Performance is defined as the number of tasks completed in 30 minutes. "Advantaged" represents the worker who receives the high piece-rate of \$0.6 and "Disadvantaged" represents the worker who receives the low piece-rate of \$0.1. Workers are ranked 1 to 100 according to their performance among workers who received the same piece-rate, i.e. with the same opportunity. The left panel shows the performance with unequal opportunities, whereas the the right panel shows the performance with equal opportunity.

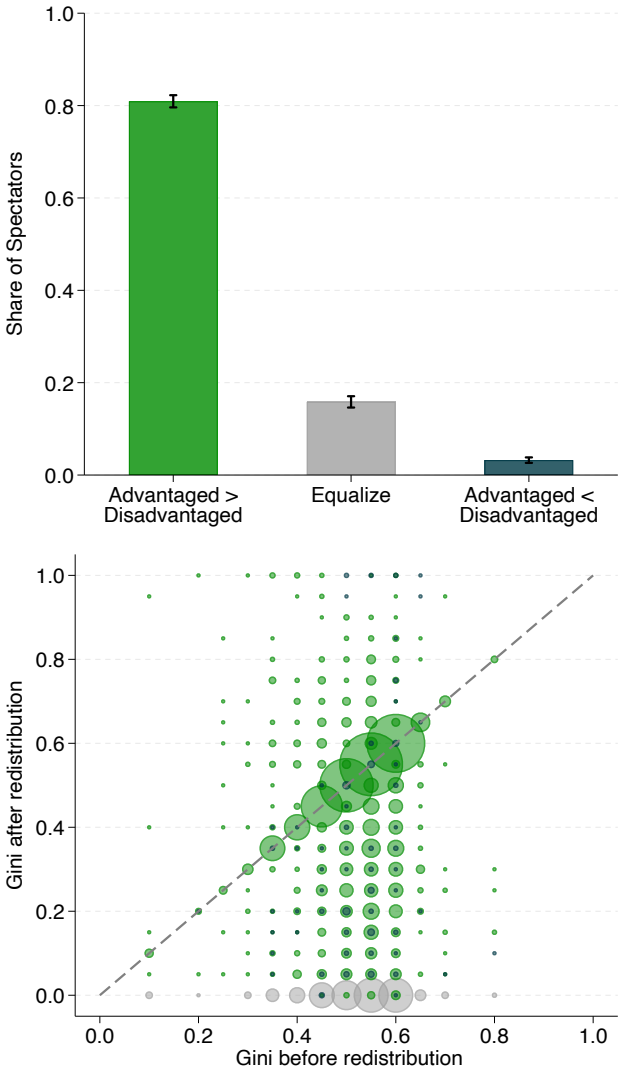

Figure 2: Inequality acceptance under unequal opportunities

NOTE: Inequality acceptance under unequal opportunities. The upper panel shows the shares of spectators who allocate more income to one of the two workers and the share who equalize the incomes. "Advantaged>Disadvantaged" represents the share of workers who allocate more to the advantaged worker than to the disadvantaged worker after redistribution. "Equalize" represents the share of spectators who equalize the income of the two workers through redistribution, where equalization is defined as an income difference smaller than \$0.5. "Advantaged<Disadvantaged" represents the share of spectators who make the disadvantaged worker have a higher income than the advantaged. The lower panel shows the distributions of Gini coefficient after redistribution at various levels of inequality before redistribution. The size of each bubbles on the graph represents the number of spectators making a particular redistribution choice. Specifically, bubbles on the 45-degree line denote no redistribution, bubbles on the x-axis denote equalized incomes of the two workers, bubbles between the 45-degree line and the x-axis denote partial redistribution which reduces the inequality, and bubbles above the 45-degree line denote an increase in inequality.

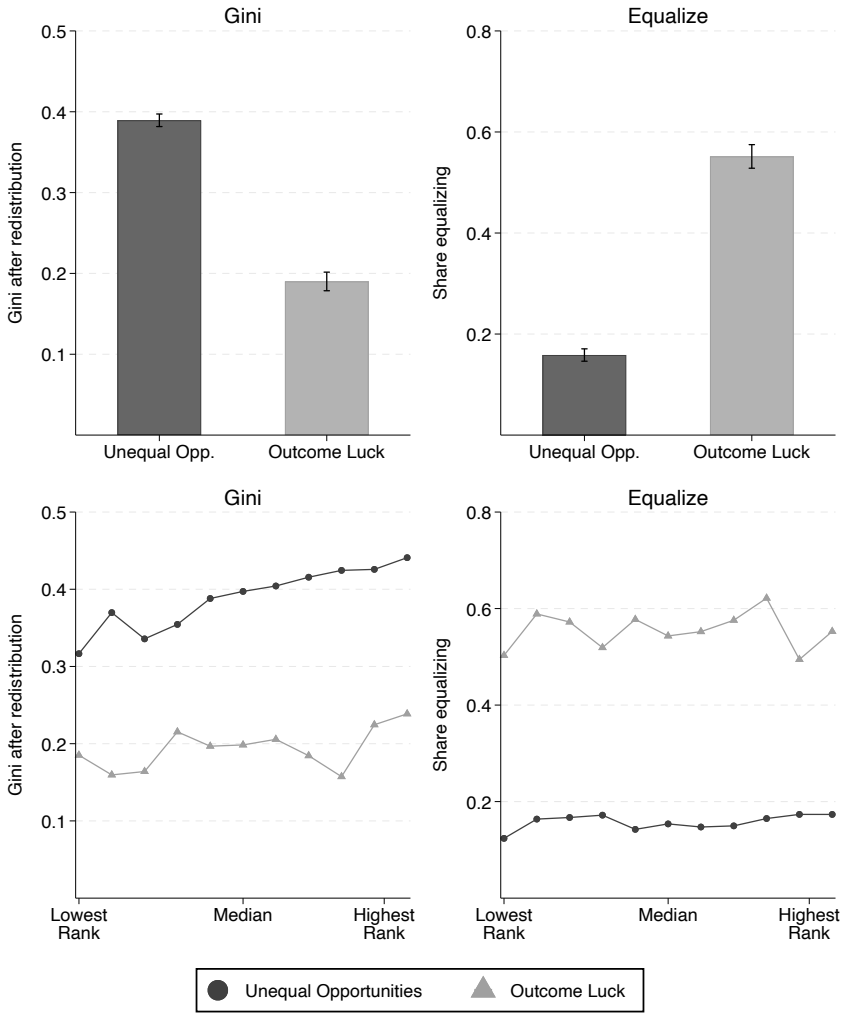

Figure 3: Unequal Opportunities vs. Outcome Luck

NOTE: Comparison of spectators' inequality acceptance in the Unequal Opportunities and Outcome Luck treatments. Top-left panel displays the after-redistribution Gini coefficient and the top-right panel shows the proportion of spectators who equalize earnings between the two workers. The bottom panels display the two inequality acceptance measures across the performances of workers.

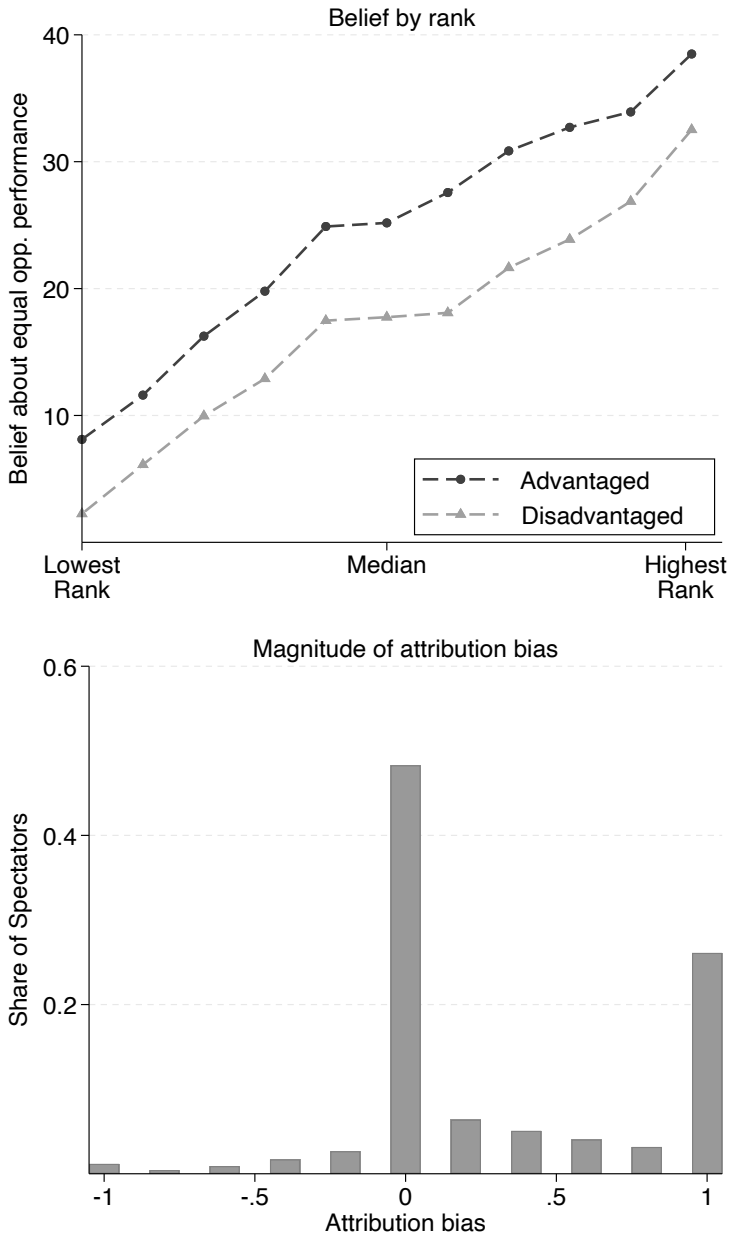

Figure 4: Beliefs about worker productivity

NOTE: Spectators' beliefs about the productivity of an advantaged worker and an disadvantaged worker under unequal opportunities. Spectators are asked to estimate the number of tasks completed by the two workers when they had the same opportunity. The top panel shows the spectators' beliefs on equal-opportunity production of the two workers separately. The bottom panel illustrates the distribution of the magnitude of attribution bias, defined as the ratio of the believed performance difference under equal opportunity to the observed performance difference under unequal opportunities.

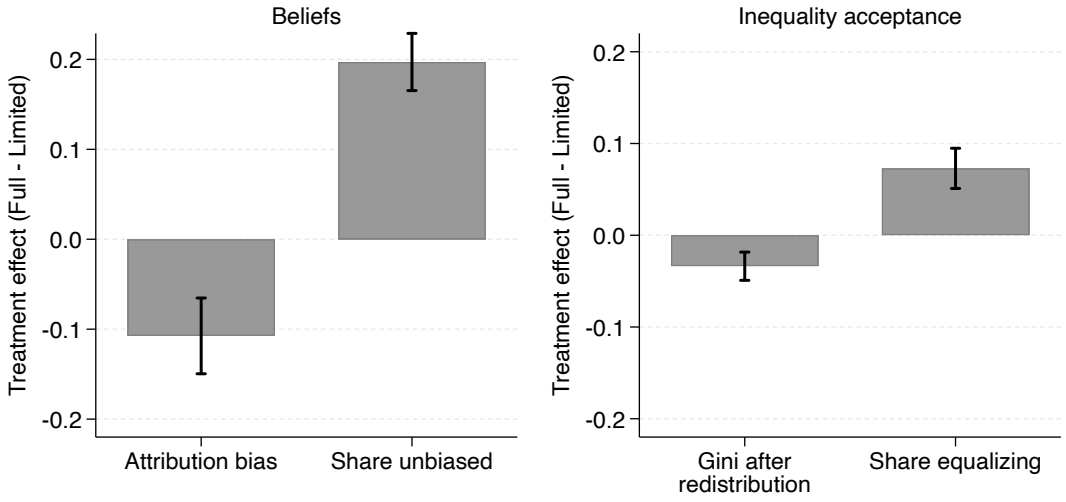

Figure 5: Treatment effects: Limited vs. Full Information

NOTE: Treatment differences between the Unequal Opportunities with Limited Information and with Full Information treatments, focusing on spectators' beliefs and their acceptance of inequality. The left panel displays the treatment effect on beliefs, as measured by the magnitude of attribution bias, defined as the ratio of the believed performance difference under equal opportunity to the observed performance difference under unequal opportunities, and the share of unbiased spectators. The right panel illustrates the treatment effect on implemented inequality, measured by both Gini after redistribution and the share of equalizing spectators.

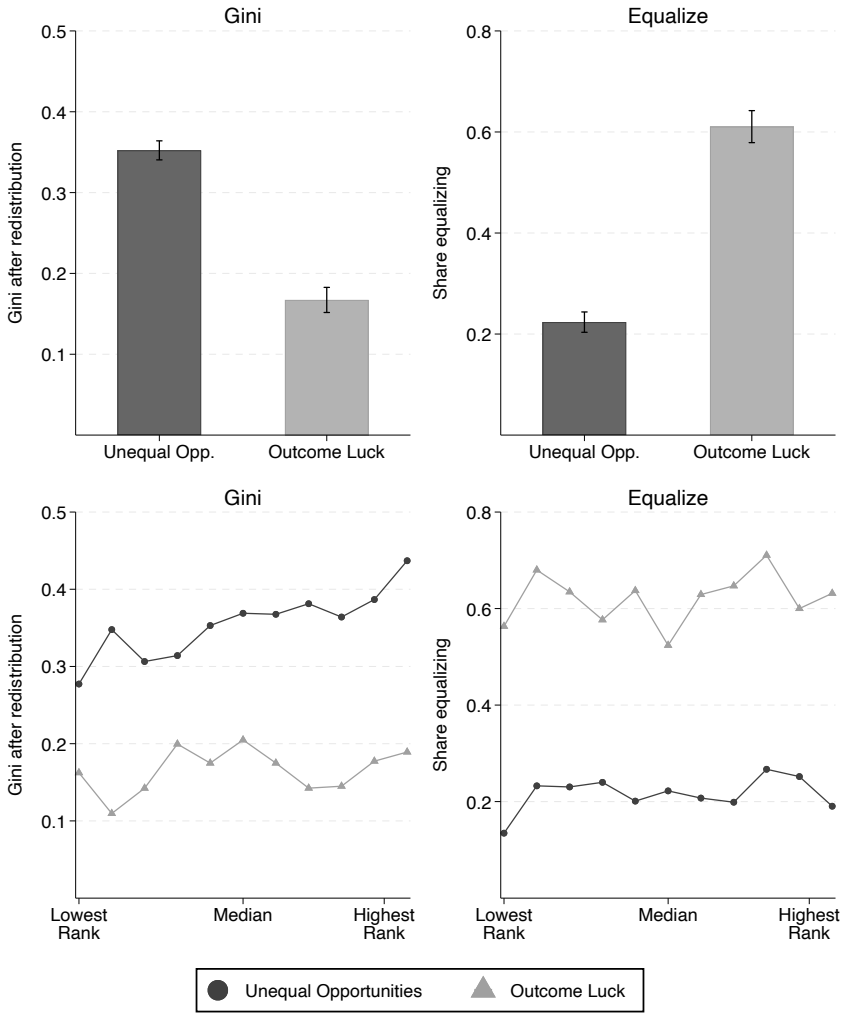

Figure 6: Unbiased spectators: Unequal Opportunities vs. Outcome Luck

NOTE: Comparison of pectators' inequality acceptance in the Unequal Opportunities and Outcome Luck treatments. The samples are restricted to spectators who are not attribution biased. Top-left panel shows the after-redistribution Gini of unbiased spectators and the top-right panel focuses on the proportion of unbiased spectators who equalize incomes between the two workers. The bottom panels display the two inequality acceptance measures across the performances of workers.

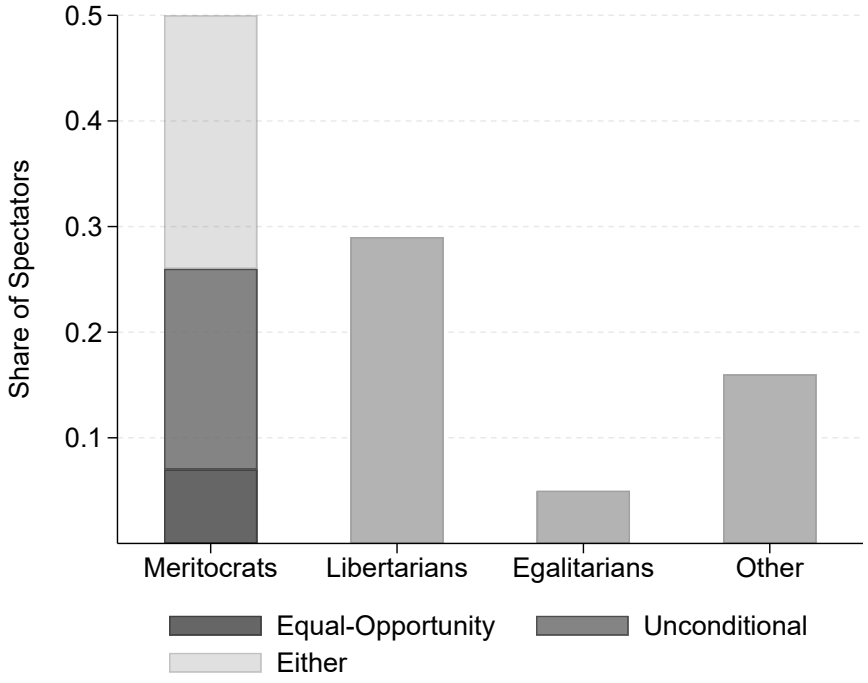

Figure 7: Prevalence of fairness views

NOTE: Estimated prevalence of different fairness views among spectators in our main sample. The share of libertarians are estimated as spectators who do not redistribute in the Outcome Luck treatment. The share of egalitarians are estimated as spectators who equalize the earnings of the two workers while holding biased beliefs in the Unequal Opportunities treatment. Meritocrats are the remaining spectators who equalize in the Outcome Luck treatment. Equal-Opportunity Meritocrats are estimated using the increase in the share of spectators who equalize between the Limited and Full Information treatments. The share of Unconditional Meritocrats is the decrease in the share of unbiased and equalizing spectators between the Outcome Luck and the Unequal Opportunities treatment. The "Other" category consists of spectators whose behavior cannot be rationalized by any of the standard fairness views under Outcome Luck.

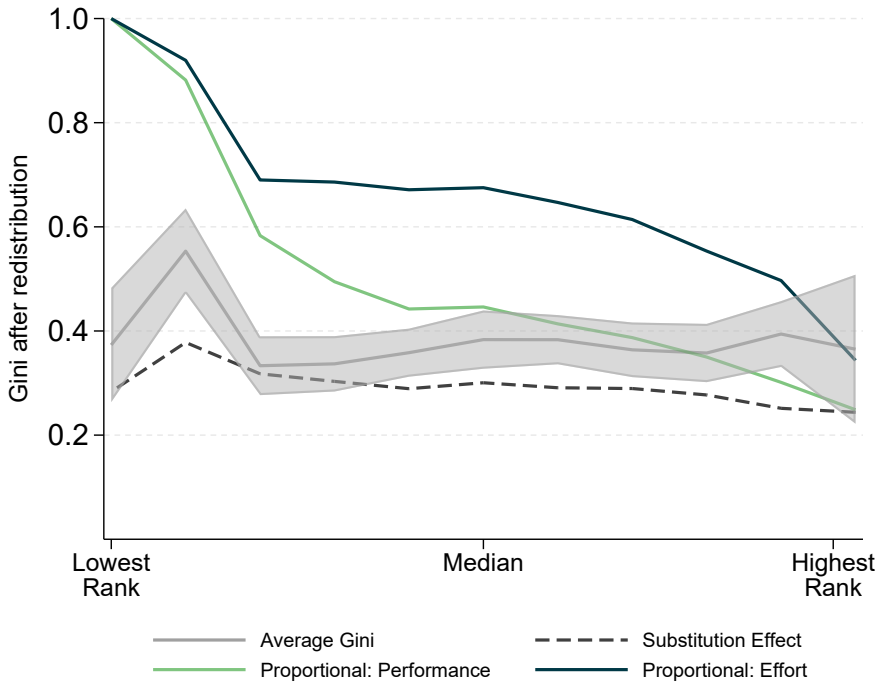

Figure 8: Rules of redistribution: Predicted vs. actual after-redistribution Gini

NOTE: Comparison of actual Gini after redistribution with the predicted Gini after redistribution according to different rules of redistribution at different levels of worker performance. We include spectators who hold unbiased beliefs and partially redistribute. The solid gray line represents the average Gini after redistribution, including a 95% confidence interval. The dashed line denotes the predicted Gini after redistribution if spectators choose to accept the inequality caused by the substitution effect, namely the extra earnings generated by the advantaged worker's additional effort. The blue and green lines are the predicted after-redistribution Gini using the proportional rule based on effort (time worked) and performance (number of tasks completed) respectively.

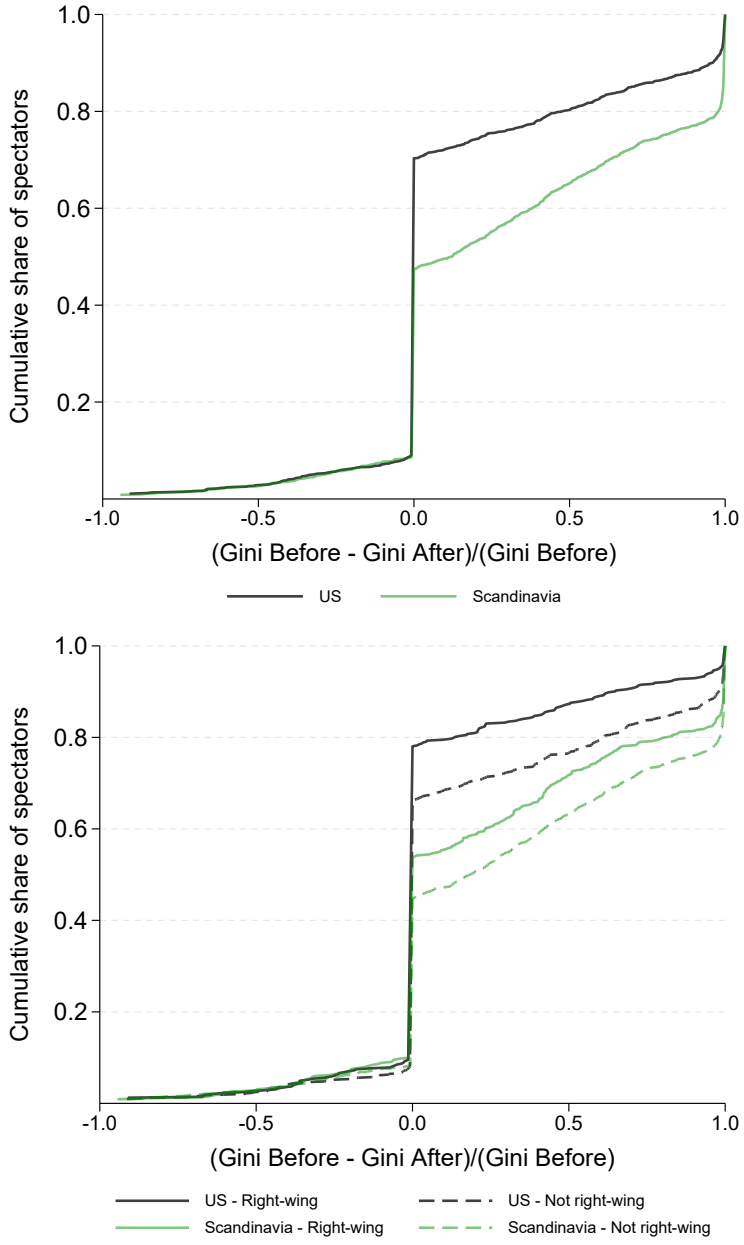

Figure 9: Inequality acceptance under unequal opportunities by societies and political orientations

NOTE: Distributions of the change in Gini coefficient after redistribution among spectators under unequal opportunities, pooling the full information and the limited informtion treatments. The top panel displays the distributions in the US and Scandinavia; the bottom panel displays the right-wing and non-right-wing spectators in the two societies. We standardize the change in Gini coefficient by dividing it by before-redistribution Gini coefficient so that 1 implies full removal of inequality and 0 implies no redistribution.

A Appendix: Additional Figures & Tables

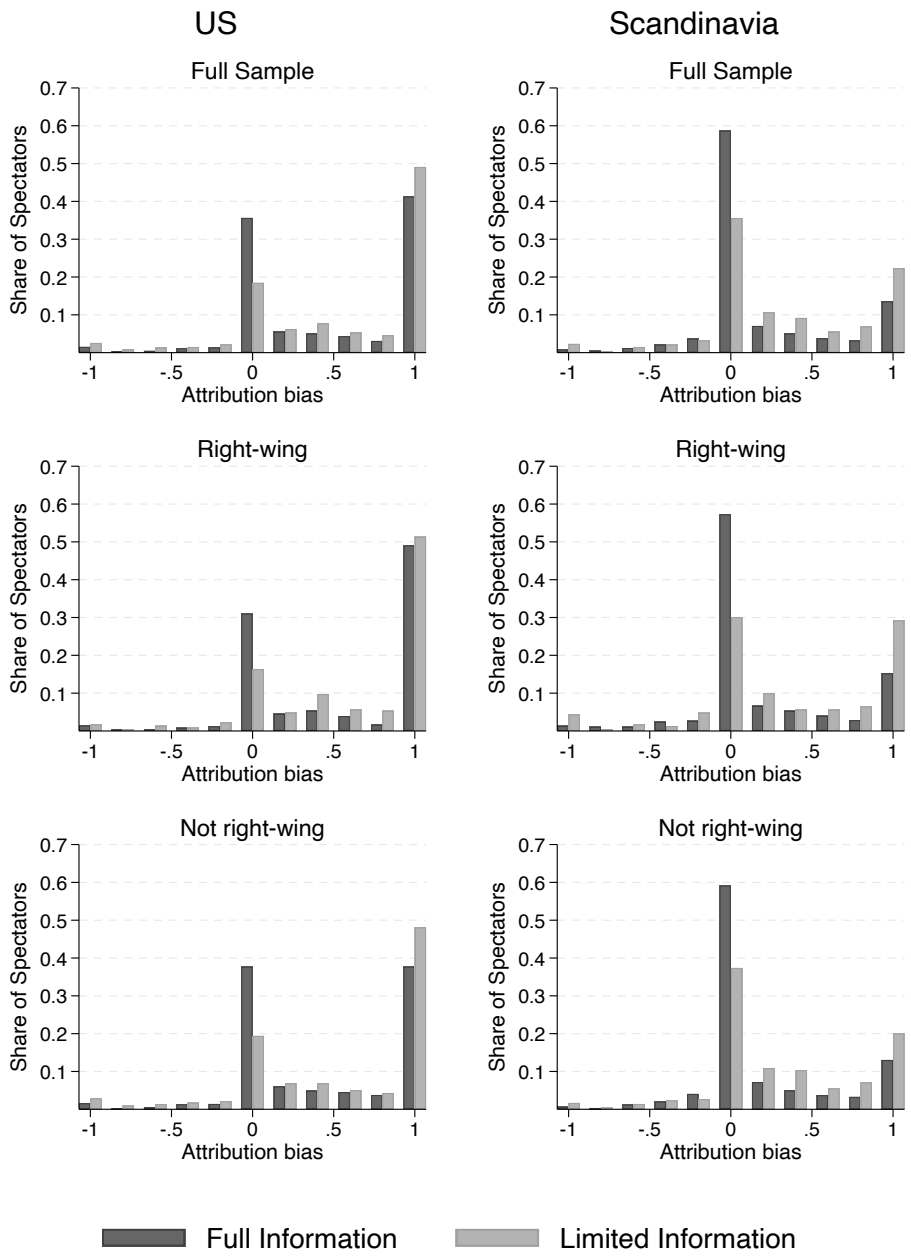

Figure A.1: Attribution bias by treatments, societies and political orientation.

NOTE: Distribution of attribution bias split by society (US versus Scandinavia), political orientation (right-wing versus not right-wing) and treatment (Unequal Opportunities with Full versus Limited Information). Definitions of right-wing political orientation can be found in Table A.1.

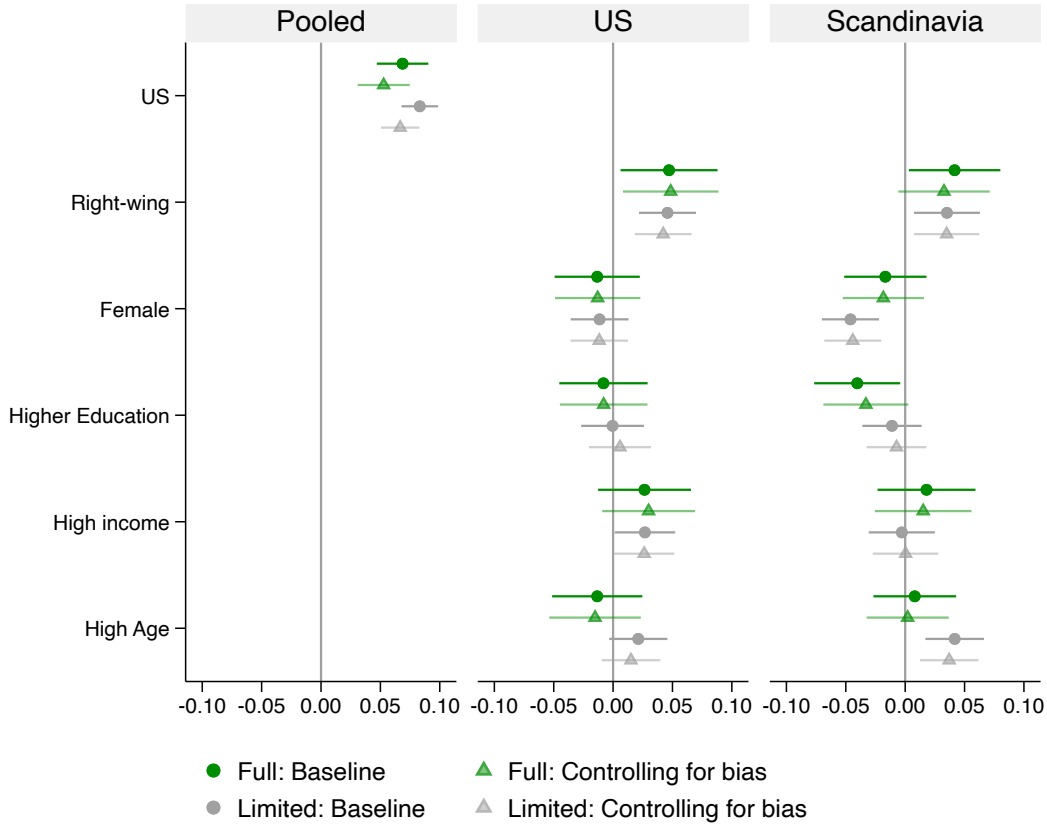

Figure A.2: Determinants of inequality acceptance under unequal opportunities

NOTE: OLS regression coefficients of demographic variables on after-redistribution Gini coefficients under unequal opportunities, with full and limited information. It also displays the regression coefficients with and without controlling for attribution bias. In all regressions we use worker-pair fixed effects.

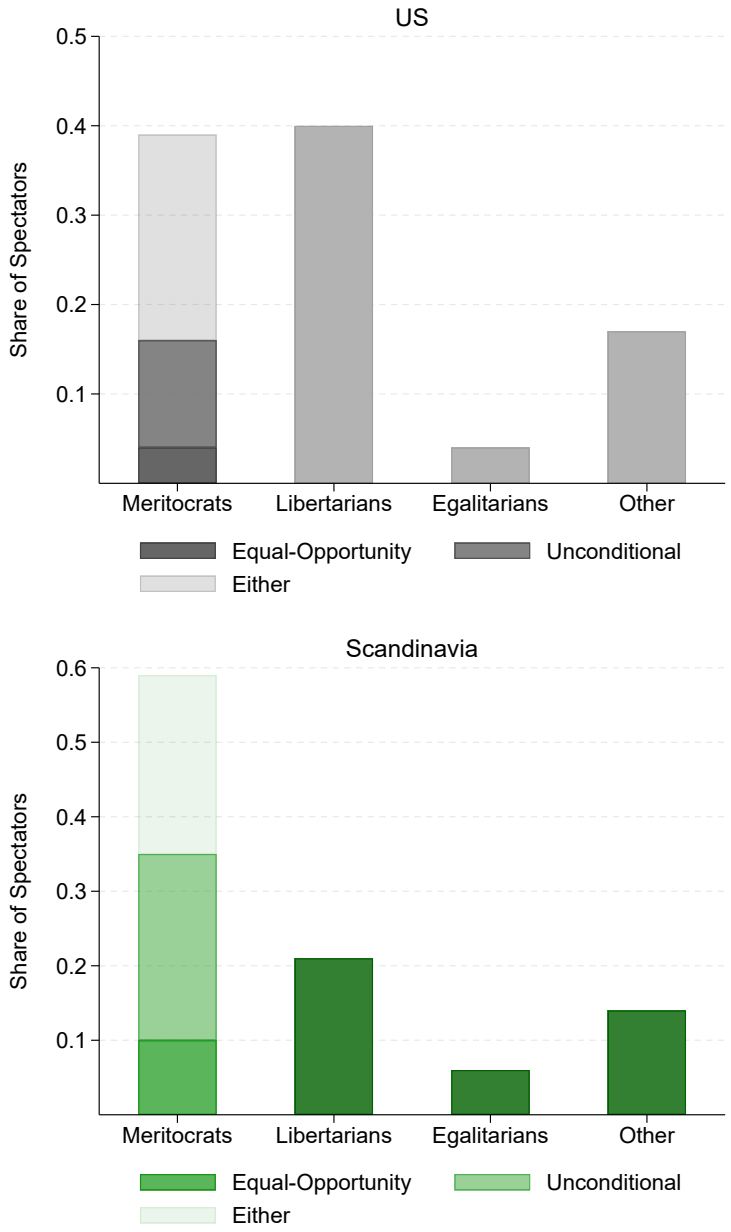

Figure A.3: Prevalence of fairness views by societies

NOTE: The prevalence of fairness views in both societies. See Figure 7 for details.

Table A.1: Definitions of variables collected in the survey

Variable	United States	Scandinavia		
		Denmark	Norway	Sweden
Education (those classified as higher education in bold)				
	Some high school	Folkeskole	Grunnskole	Grundskola
	High school or equivalent	Gymnasium eller ungdomsuddannelse	Videregående	Gymnasium
	Some college	Universitetsstudier, men ingen eksamen	Universitetsstudier, men ingen eksamen	Högskole- eller universitetstudier, men ingen examen
	Associates degree	**Bacheloruddannelse**	**Bachelorutdannelse**	**Kandidatexamen**
	College degree	**Masteruddannelse**	**Masterutdannelse**	**Masterexamen**
	Postgraduate degree	**Ph.D.-grad**	**Ph.D.-grad**	**Doktorsexamen**
	Doctorate	Andet	Annet	Annan
	Other			
	Prefer not to say	Foretrækker ikke at svare	Foretrekker ikke å svare	Föredrar att inte svara
Income (those classified as above median in bold)				
	Less than $10,000	0-100.000 DKK	0-100.000 NOK	0-100.000 SEK
	$10,001-$20,000	100.001-200.000 DKK	100.001-200.000 NOK	100.001-200.000 SEK
	$20,001-$30,000	200.001-300.000 DKK	200.001-300.000 NOK	200.001-300.000 SEK
	$30,001-$40,000	300.001-400.000 DKK	300.001-400.000 NOK	300.001-400.000 SEK
	$40,001-$50,000	**400.001-500.000 DKK**	400.001-500.000 NOK	**400.001-500.000 SEK**
	$50,001-$60,000	**500.001-600.000 DKK**	500.001-600.000 NOK	**500.001-600.000 SEK**
	$60,001-$70,000	**600.001-700.000 DKK**	**600.001-700.000 NOK**	**600.001-700.000 SEK**
	$70,001-$80,000	**700.001-800.000 DKK**	**700.001-800.000 NOK**	**700.001-800.000 SEK**
	$80,001-$90,000	**800.001-900.000 DKK**	**800.001-900.000 NOK**	**800.001-900.000 SEK**
	$90,001-$100,000	**900.001-1.000.000 DKK**	**900.001-1.000.000 NOK**	**900.001-1.000.000 SEK**
	$100,001-$110,000	**1.000.001-1.100.000 DKK**	**1.000.001-1.100.000 NOK**	**1.000.001-1.100.000 SEK**
	$110,001-$120,000	**1.100.001-1.200.000 DKK**	**1.100.001-1.200.000 NOK**	**1.100.001-1.200.000 SEK**
	$120,001-$130,000	**1.200.001-1.300.000 DKK**	**1.200.001-1.300.000 NOK**	**1.200.001-1.300.000 SEK**
	$130,001-$140,000	**1.300.001-1.400.000 DKK**	**1.300.001-1.400.000 NOK**	**1.300.001-1.400.000 SEK**
	$140,001-$150,000	**1.400.001-1.500.000 DKK**	**1.400.001-1.500.000 NOK**	**1.400.001-1.500.000 SEK**
	$150,001 or more	**1.500.001 DKK eller derover**	**1.500.001 NOK eller mer**	**1.500.001 SEK eller mer**
	Prefer not to say	Foretrækker ikke at svare	Foretrekker ikke å svare	Föredrar att inte svara
Political Orientation (those classified as right-wing in bold				
	The Democratic Party	Socialdemokratiet (A)	Arbeiderpartiet (AP)	Socialdemokraterna (S)
	The Republican Party	Venstre (V)	**Høyre (H)**	**Moderaterna (M)**
	Another party	**Dansk Folkeparti (O)**	Senterpartiet (Sp)	**Sverigedemokraterna (SD)**
	Do not have the right to vote	SF - Socialistisk Folkeparti (F)	**Fremskrittspartiet (FrP)**	Centerpartiet (C)
	Did not vote	Radikale Venstre (B)	Socialistisk Venstreparti (SV)	Vänsterpartiet (V)
	Prefer not to say	Enhedslisten (Ø)	Rødt (R)	**Kristdemkoraterna (KD)**
		Det Konservative Folkeparti (C)	Venstre (V)	Liberalerna (L)
		Nye Borgerlige (D)	Miljøpartiet De Grønne (MDG)	Miljöpartiet (MP)
		Liberal Alliance (I)	Kristelig Folkeparti (KrF)	Annat
		Frie Grønne (Q)	Pasientfokus (PF)	Har inte rösträtt
		Alternativet (Å)	Annet	Föredrar att inte svara
		Kristendemokraterne (K)	Har ikke stemmerett	
		Annet	Foretrekker ikke å svare	
		Har ikke stemmret		
		Fortrækker ikke at svare		

Table A.2: Sample Demographics

	All	US	Scandinavia	Denmark	Norway	Sweden
Age	48	53	44	46	44	43
Female	55%	58%	52%	56%	47%	52%
High education	44%	46%	43%	43%	55%	31%
High income	39%	49%	31%	27%	31%	37%
Right-wing	28%	32%	25%	16%	24%	35%
Observations	6,824	3,051	3,773	1,233	1,258	1,282

NOTE: The table reports summary statistics for our sample of spectators split by societies and by countries in Scandinavia. Variable definitions can be found in Table A.1.

Table A.3: Inequality Acceptance: Unequal Opportunities vs. Outcome Luck

	(1) Gini	(2) Gini	(3) Equalize	(4) Equalize
Unequal Opportunities	0.195***	0.197***	-0.394***	-0.396***
	(0.008)	(0.008)	(0.015)	(0.015)
US		0.087***		-0.150***
		(0.007)		(0.012)
Right-wing		0.023**		-0.034*
		(0.007)		(0.013)
High education		-0.017*		0.037**
		(0.007)		(0.013)
High income		0.008		-0.004
		(0.007)		(0.013)
Female		-0.047***		0.068***
		(0.007)		(0.012)
High age		0.027***		-0.017
		(0.007)		(0.012)
Constants	0.193***	0.161***	0.552***	0.587***
	(0.006)	(0.009)	(0.013)	(0.017)
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	5154	5154	5154	5154

NOTE: The table reports robust OLS regressions of inequality acceptance using the Outcome Luck treatment as baseline. "Unequal Opportunities" equals 1 if the subject is instead in the Unequal Opportunities treatment. We include two measures of inequality acceptance:"Gini" represents the level of inequality as measured by the Gini coefficient after redistribution, "Equalize" is a indicator variable for equalizing the income of two workers, with a margin of \$0.5 defining what constitutes equalizing. In Columns (2) and (4) we include a set of controls: "Right-wing" equals 1 if the spectator holds a political orientation to the right in their particular country, "High education" is an indicator for holding a bachelors' degree or higher. "High Income" is an indicator for a spectator with an individual income above the median individual income in their corresponding country. "High Income" is an indicator for a spectator with an individual income above the median individual income in their corresponding country."High age" is an indicator variable for being older than the sample median for the country, "Female" equals 1 if the spectator is female. In all specifications, we control for the fixed effect of the worker-pair. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses.

Table A.4: Attribution Bias & Inequality Acceptance: Full vs. Limited Information

(a) Panel A: Attribution Bias

	(1) Unbiased	(2) Unbiased	(3) Attribution bias	(4) Attribution bias
Limited Information	-0.197***	-0.199***	3.335***	3.404***
	(0.016)	(0.016)	(0.405)	(0.385)
Constant	0.481***	0.578***	9.409***	6.303***
	(0.009)	(0.017)	(0.207)	(0.384)
Demographic controls		Yes		Yes
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	5078	5078	5078	5078

(b) Panel B: Inequality Acceptance

	(1) Gini	(2) Gini	(3) Equalize	(4) Equalize
Limited Information	0.034***	0.035***	-0.073***	-0.074***
	(0.008)	(0.008)	(0.011)	(0.011)
Constant	0.392***	0.350***	0.159***	0.202***
	(0.004)	(0.008)	(0.007)	(0.012)
Demographic controls		Yes		Yes
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	5078	5078	5078	5078

NOTE: The table reports robust OLS regressions of attribution bias (Panel A) and inequality acceptance (Panel B) using the Unequal Opportunities Full Information treatment as baseline. "Limited Information" equals 1 if the spectators is instead in the Unequal Opportunities Limited Information treatment. "Unbiased" is an indicator variable for holding unbiased beliefs. "Attribution bias" is the magnitude of attribution bias, defined as the ratio of the believed performance difference under equal opportunities to the observed performance difference under unequal opportunities. "Gini" represents the level of inequality as measured by the Gini coefficient after redistribution, "Equalize" is an indicator variable for equalizing the income of two workers, In Columns (2) and (4) of both panels we include a set of demographic controls, including right-wing, high education, high income, female and high age. In all specifications, we control for the fixed effect of the worker-pair. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses.

Table A.5: Balance table: Unbiased sample

	(1) Unequal Opportunities	(2) Outome Luck	(3) Difference (2)-(1)
US	0.334	0.353	-0.020
	(0.472)	(0.478)	(0.020)
Right-wing	0.248	0.259	-0.010
	(0.432)	(0.438)	(0.018)
High education	0.491	0.504	-0.012
	(0.500)	(0.500)	(0.021)
High income	0.386	0.416	-0.029
	(0.487)	(0.493)	(0.020)
Female	0.550	0.536	0.014
	(0.498)	(0.499)	(0.021)
High age	0.471	0.493	-0.022
	(0.499)	(0.500)	(0.021)
Observations	1646	909	2555

NOTE: The table reports means for the unbiased sample in both Unequal Opportunities and Outcome Luck treatments. Standard deviations in parentheses. Column (3) provides the difference between the two treatments, with standard error in parentheses and stars indicating significant differences. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A.6: Heterogeneity: Inequality acceptance under unequal opportunities

(a) Panel A: Full Sample

	(1) Gini	(2) Gini	(3) Equalize	(4) Equalize
US	0.083***	0.067***	-0.114***	-0.090***
	(0.008)	(0.008)	(0.012)	(0.013)
Attribution bias		Yes		Yes
Observations	3408	3408	3408	3408

(b) Panel B: US

	(1) Gini	(2) Gini	(3) Equalize	(4) Equalize
Right-wing	0.046***	0.042***	-0.061***	-0.056**
	(0.012)	(0.012)	(0.018)	(0.018)
Female	-0.011	-0.012	0.018	0.018
	(0.012)	(0.012)	(0.018)	(0.018)
High education	-0.000	0.006	-0.002	-0.011
	(0.013)	(0.013)	(0.020)	(0.020)
High income	0.027*	0.026*	-0.032	-0.031
	(0.013)	(0.013)	(0.019)	(0.019)
High age	0.021	0.015	0.003	0.012
	(0.012)	(0.013)	(0.018)	(0.019)
Attribution bias		Yes		Yes
Observations	1540	1540	1540	1540

(c) Panel C: Scandinavia

	(1)	(2)	(3)	(4)
	Gini	Gini	Equalize	Equalize
Right-wing	0.035*	0.035*	-0.047*	-0.047*
	(0.014)	(0.014)	(0.024)	(0.024)
Female	-0.046***	-0.044***	0.070**	0.067**
	(0.012)	(0.012)	(0.021)	(0.021)
High education	-0.011	-0.007	0.018	0.011
	(0.013)	(0.013)	(0.022)	(0.022)
High income	-0.003	0.000	-0.007	-0.013
	(0.014)	(0.014)	(0.024)	(0.024)
High age	0.042***	0.037**	-0.045*	-0.037
	(0.013)	(0.013)	(0.022)	(0.022)
Attribution bias		Yes		Yes
Observations	1868	1868	1868	1868

NOTE: The table reports robust OLS regressions for the determinants of inequality acceptance for the Unequal Opportunities treatments (both Full and Limited Information). "Gini" represents the level of inequality as measured by the Gini coefficient after redistribution, "Equalize" is an indicator variable for equalizing the income of two workers, Panel A gives the result for the full sample, controlling only for the society of origin using "US" which equal 1 if the spectator is from the US. Panel B contains the US sample only while Panel C contains the Scandinavian sample. In Panel B and C we include a set of demographic controls as explanatory variables including dummies for right-wing, high education, high income, female and high age. In specification (2) and (4) in all panels, we control for the magnitude of attribution bias of the spectator. In all specifications, we control for the fixed effect of the worker-pair. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$. Standard errors in parentheses.

Table A.8: Heterogeneity: Attribution bias

(a) Panel A: Full Sample

	(1) Unbiased	(2) Attribution bias
US	-0.227***	6.526***
	(0.017)	(0.398)
Worker-pair fixed effects	Yes	Yes
Observations	3408	3408

(b) Panel B: US

	(1) Unbiased	(2) Attribution bias
Right-wing	-0.044	1.756*
	(0.030)	(0.798)
Female	0.002	0.126
	(0.028)	(0.732)
High education	0.164***	-3.048***
	(0.030)	(0.781)
High income	-0.016	0.296
	(0.030)	(0.796)
High age	-0.065*	2.999***
	(0.030)	(0.749)
Worker-pair fixed effects	Yes	Yes
Observations	1540	1540

(c) Panel C: Scandinavia

	(1) Unbiased	(2) Attribution bias
Right-wing	0.011	0.097
	(0.030)	(0.619)
Female	0.001	-0.892
	(0.026)	(0.515)
High education	0.086**	-1.737***
	(0.027)	(0.511)
High income	0.065*	-1.426*
	(0.029)	(0.612)
High age	-0.103***	2.100***
	(0.026)	(0.530)
Worker-pair fixed effects	Yes	Yes
Observations	1868	1868

NOTE: The table reports robust OLS regressions for the determinants of inequality acceptance for the Unequal Opportunities treatments (both Full and Limited Information). "Unbiased" is an indicator variable for holding unbiased beliefs. "Attribution bias" is the magnitude of attribution bias, defined as the ratio of the believed performance difference under equal opportunity to the observed performance difference under unequal opportunities. Panel A gives the result for the full sample, controlling only for the society of origin using "US" which equal 1 if the spectator is from the US Panel B contains the US sample only while Panel C contains the Scandinavian sample. In Panel B and C we include a set of demographic controls, including right-wing, high education, high income, female and high age In all specifications, we control for the fixed effect of the worker-pair. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$. Standard errors in parentheses.

Table A.10: Heterogeneity: Unequal Opportunities vs. Outcome Luck

(a) Panel A: US

	(1) Gini	(2) Gini	(3) Equalize	(4) Equalize
Unequal Opportunities	0.189***	0.191***	-0.346***	-0.349***
	(0.012)	(0.012)	(0.022)	(0.022)
Constants	0.247***	0.249***	0.437***	0.401***
	(0.010)	(0.016)	(0.019)	(0.028)
Demographic controls		Yes		Yes
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	2316	2316	2316	2316

(b) Panel B: Scandinavia

	(1) Gini	(2) Gini	(3) Equalize	(4) Equalize
Unequal Opportunities	0.199***	0.202***	-0.435***	-0.439***
	(0.011)	(0.011)	(0.021)	(0.021)
Constants	0.149***	0.158***	0.647***	0.624***
	(0.008)	(0.012)	(0.017)	(0.024)
Demographic controls		Yes		Yes
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	2838	2838	2838	2838

NOTE: The table reports robust OLS regressions of inequality acceptance using the Outcome Luck treatment as baseline for the US (Panel A) and Scandinavia (Panel B) samples separately. "Unequal Opportunities" equals 1 if the subject is instead in the Unequal Opportunities treatment. We include two measures of inequality acceptance:"Gini" represents the level of inequality as measured by the Gini coefficient after redistribution, "Equalize" is an indicator variable for equalizing the income of two workers, with a margin of \$0.5 defining what constitutes equalizing. In Columns (2) and (4) of both panels we include a set of demographic controls, including right-wing, high education, high income, female and high age In all specifications, we control for the fixed effect of the worker-pair. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses.

Table A.11: Heterogeneity: Full vs. Limited Information

(a) Panel A1: US - Beliefs

	(1)	(2)	(3)	(4)
	Unbiased	Unbiased	Attribution bias	Attribution bias
Limited Information	-0.151***	-0.152***	2.787***	2.750***
	(0.024)	(0.023)	(0.655)	(0.657)
Demographic controls		Yes		Yes
Observations	2275	2275	2275	2275

(b) Panel B1: Scandinavia - Beliefs

	(1)	(2)	(3)	(4)
	Unbiased	Unbiased	Attribution bias	Attribution bias
Limited Information	-0.243***	-0.239***	3.978***	3.886***
	(0.023)	(0.022)	(0.482)	(0.483)
Demographic controls		Yes		Yes
Observations	2803	2803	2803	2803

(c) Panel A2: US - Inequality Acceptance

	(1)	(2)	(3)	(4)
	Gini	Gini	Equalize	Equalize
Limited Information	0.022*	0.023*	-0.042**	-0.044**
	(0.011)	(0.011)	(0.014)	(0.014)
Demographic controls		Yes		Yes
Observations	2275	2275	2275	2275

(d) Panel B2: Scandinavia - Inequality Acceptance

	(1)	(2)	(3)	(4)
	Gini	Gini	Equalize	Equalize
Limited Information	0.038***	0.037***	-0.093***	-0.092***
	(0.011)	(0.011)	(0.018)	(0.017)
Demographic controls		Yes		Yes
Observations	2803	2803	2803	2803

NOTE: The table reports robust OLS regressions of attribution bias (Panel A1 and B1) and inequality acceptance (Panel A2 and B2) using the Unequal Opportunities Full Information treatment as baseline. We run the regressions separately for the US (Panels A1 and A2) and Scandinavia (Panels B1 and B2). "Limited Information" equals 1 if the spectators is instead in the Unequal Opportunities Limited Information treatment. "Unbiased" is an indicator variable for holding unbiased beliefs. "Attribution bias" is the magnitude of attribution bias, defined as the ratio of the believed performance difference under equal opportunity to the observed performance difference under unequal opportunities. "Gini" represents the level of inequality as measured by the Gini coefficient after redistribution, "Equalize" is an indicator variable for equalizing the income of two workers, In Columns (2) and (4) of all panels we include a set of demographic controls, including right-wing, high education, high income, female and high age. In all specifications, we control for the fixed effect of the worker-pair. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses.

Table A.13: Heterogeneity: Inequality acceptance across regions of the US and countries In Scandinavia

(a) Panel A: Regions of the US

	(1)	(2)	(3)	(4)
	Gini	Gini	Gini	Gini
Northeast	0.033	0.041*	0.030	0.037*
	(0.019)	(0.019)	(0.019)	(0.019)
Midwest	0.004	0.004	0.001	0.002
	(0.017)	(0.018)	(0.017)	(0.017)
South	0.021	0.016	0.018	0.013
	(0.016)	(0.016)	(0.016)	(0.016)
Constant	0.421***	0.375***	0.392***	0.350***
	(0.012)	(0.015)	(0.018)	(0.020)
Demographic controls			Yes	Yes
Attribution bias		Yes		Yes
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	1540	1540	1540	1540

(b) Panel B: Countries of Scandinavia

	(1)	(2)	(3)	(4)
	Gini	Gini	Gini	Gini
Norway	-0.024	-0.021	-0.029*	-0.027
	(0.015)	(0.015)	(0.015)	(0.015)
Sweden	0.005	0.011	0.001	0.006
	(0.014)	(0.015)	(0.015)	(0.015)
Constant	0.358***	0.337***	0.363***	0.345***
	(0.010)	(0.012)	(0.015)	(0.017)
Demographic controls			Yes	Yes
Attribution bias		Yes		Yes
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	1868	1868	1868	1868

NOTE: The table reports robust OLS regressions of inequality acceptance in the Unequal Opportunities treatments (both Full and Limited Information). In Panel A we explore heterogeneity across regions of the US (with West as baseline) and in Panel B we explore heterogeneity across countries of Scandinavia (with Denmark as baseline), "Gini" represents the level of inequality as measured by the Gini coefficient after redistribution, In the US regions are defined using the Census bureau definition with four regions. In columns (2) and (4) in all panels, we control for the magnitude of attribution bias of the spectator. In columns (3) and (4) of both panels we include a set of demographic controls, including right-wing, high education, high income, female and high age In all specifications, we control for the fixed effect of the worker-pair. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses.

A.1 US Sample

In this section we reproduce all main figures for the US sample only.

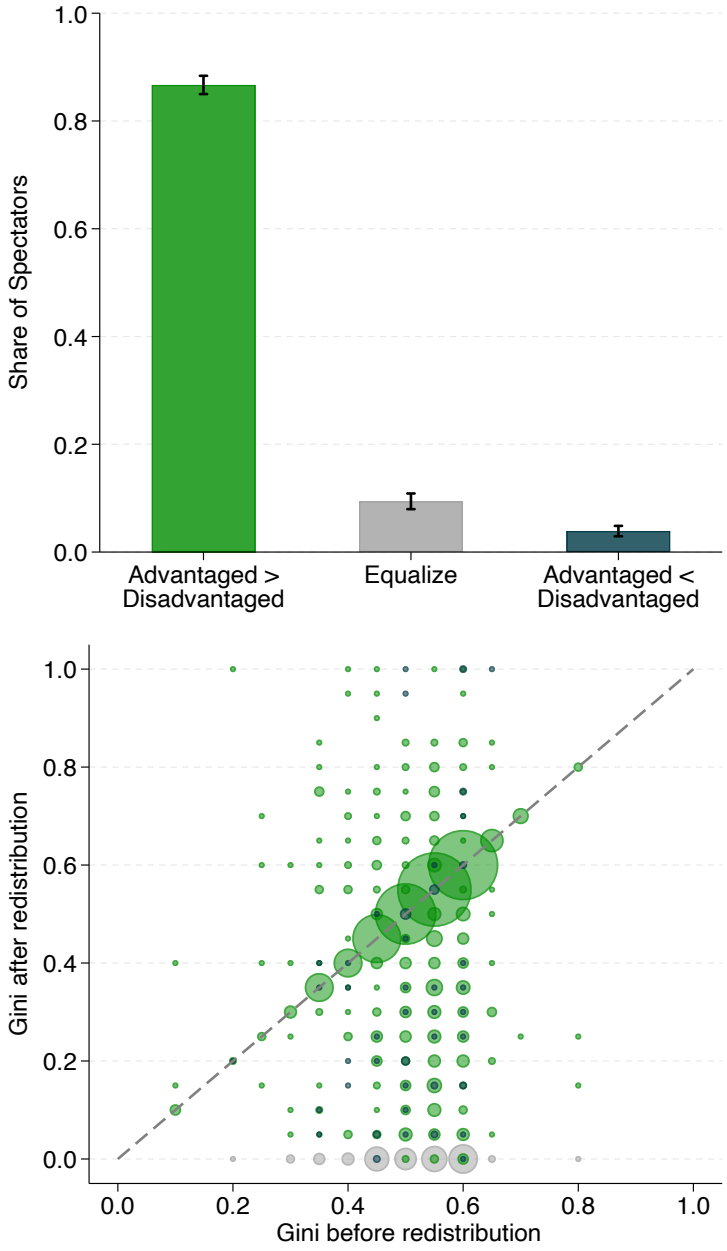

Figure A.4: Inequality acceptance under unequal opportunities (US sample)

NOTE: See Figure 2.

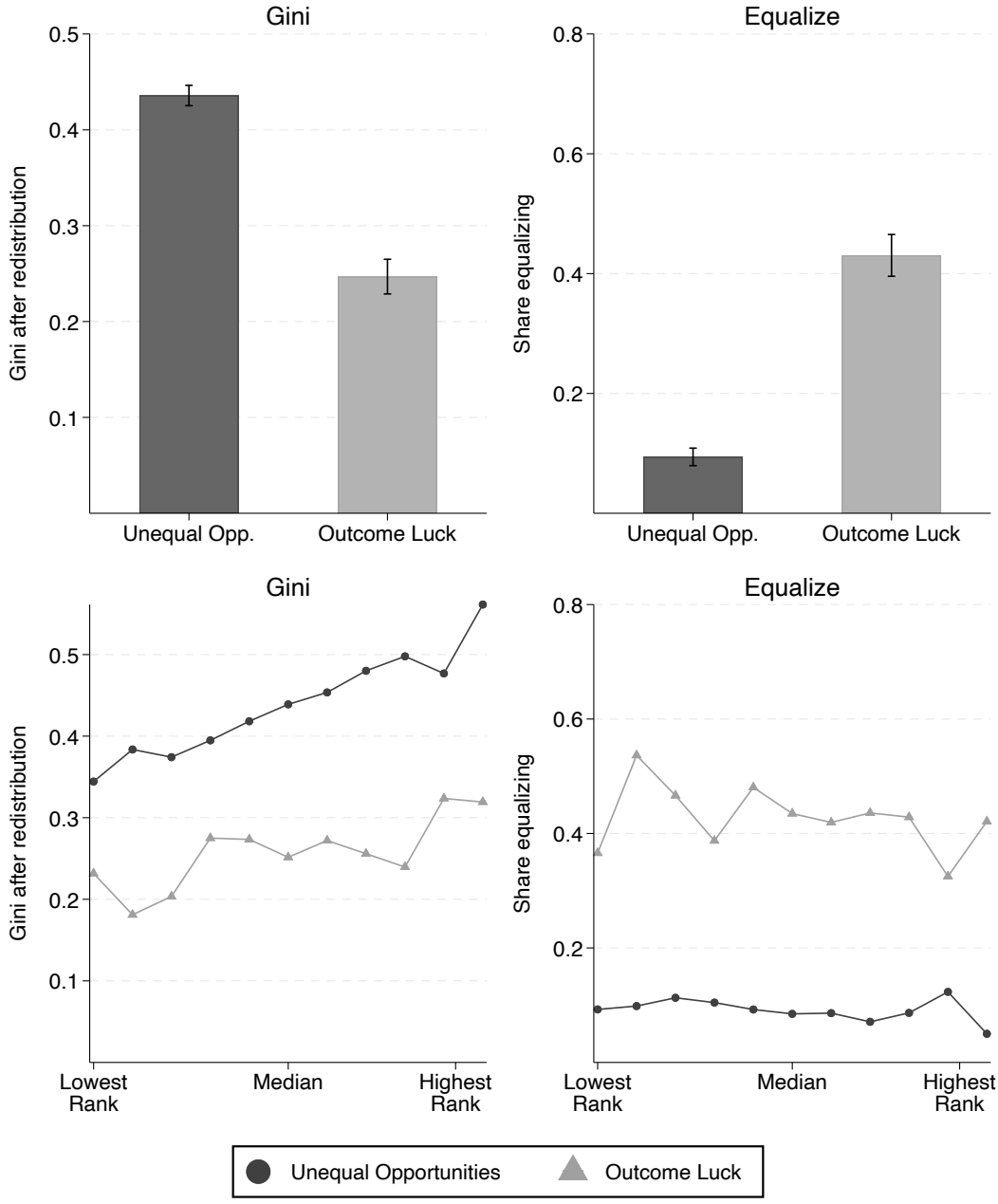

Figure A.5: Unequal Opportunities vs. Outcome Luck

NOTE: See Figure 3.

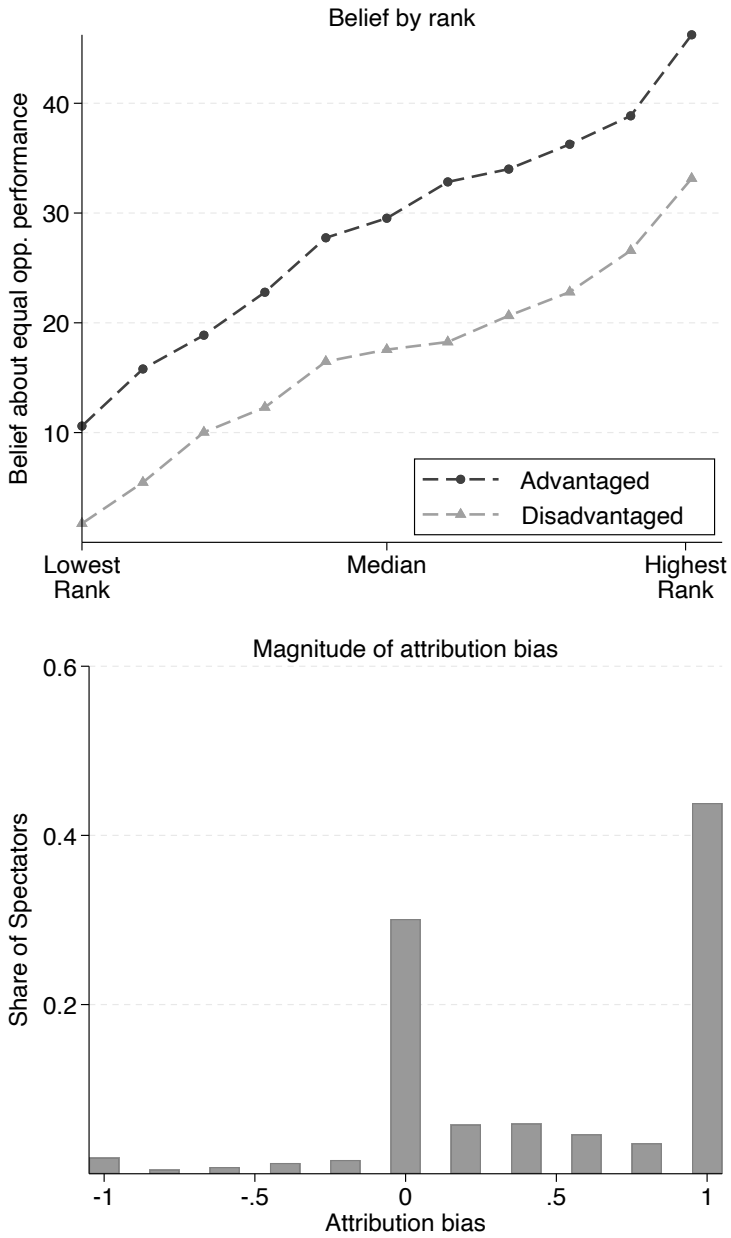

Figure A.6: Beliefs about worker productivity (US sample)

NOTE: See Figure 4.

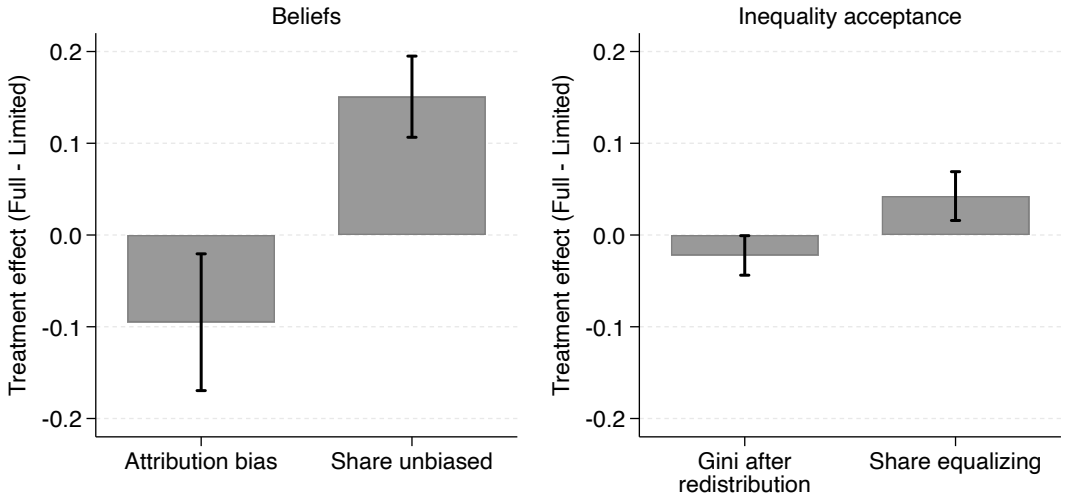

Figure A.7: Treatment effects: Limited vs. Full Information (US sample)

NOTE: See Figure 5

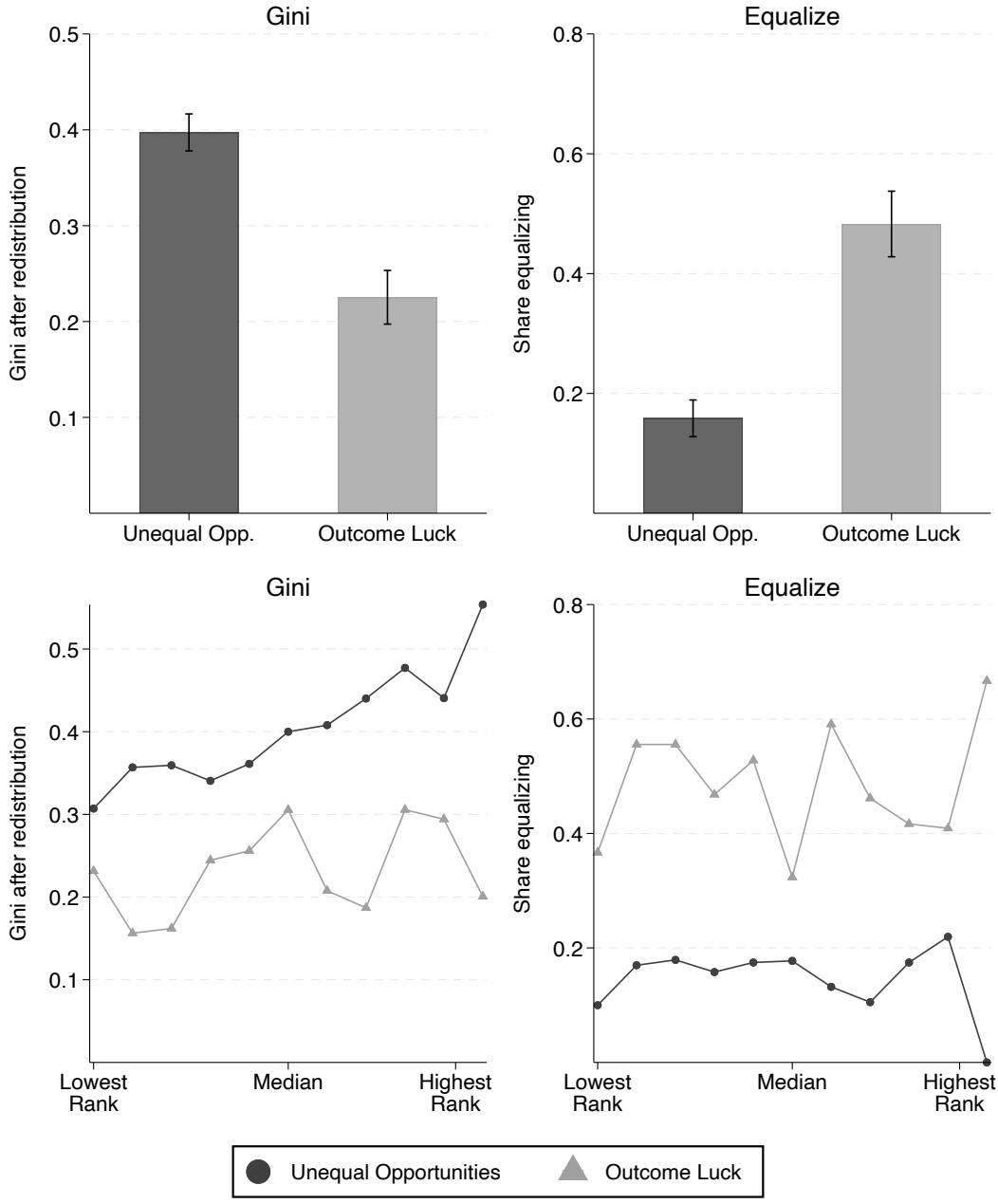

Figure A.8: Unbiased spectators: Unequal Opportunities vs. Outcome Luck (US sample)

NOTE: See Figure 6.

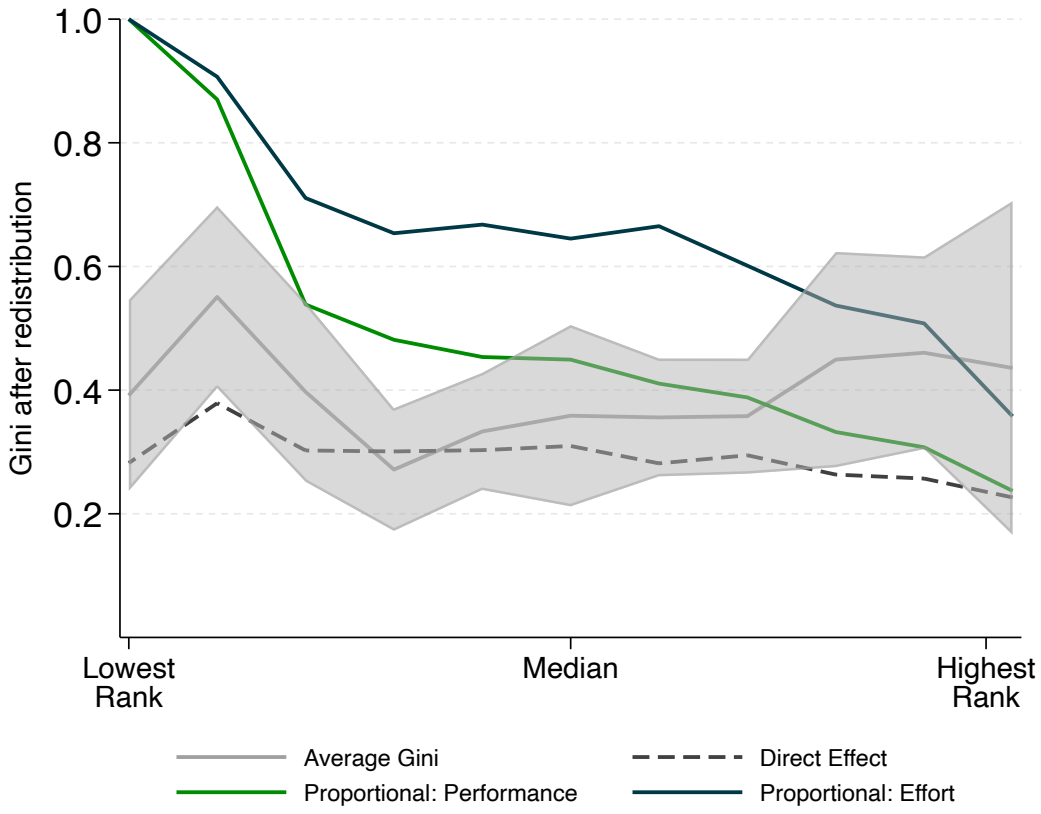

Figure A.9: Rules of redistribution: Predicted vs. actual Gini (US sample)
NOTE: See Figure 8.

A.2 Scandinavia Sample

In this section we reproduce all main figures for the Scandinavia sample only.

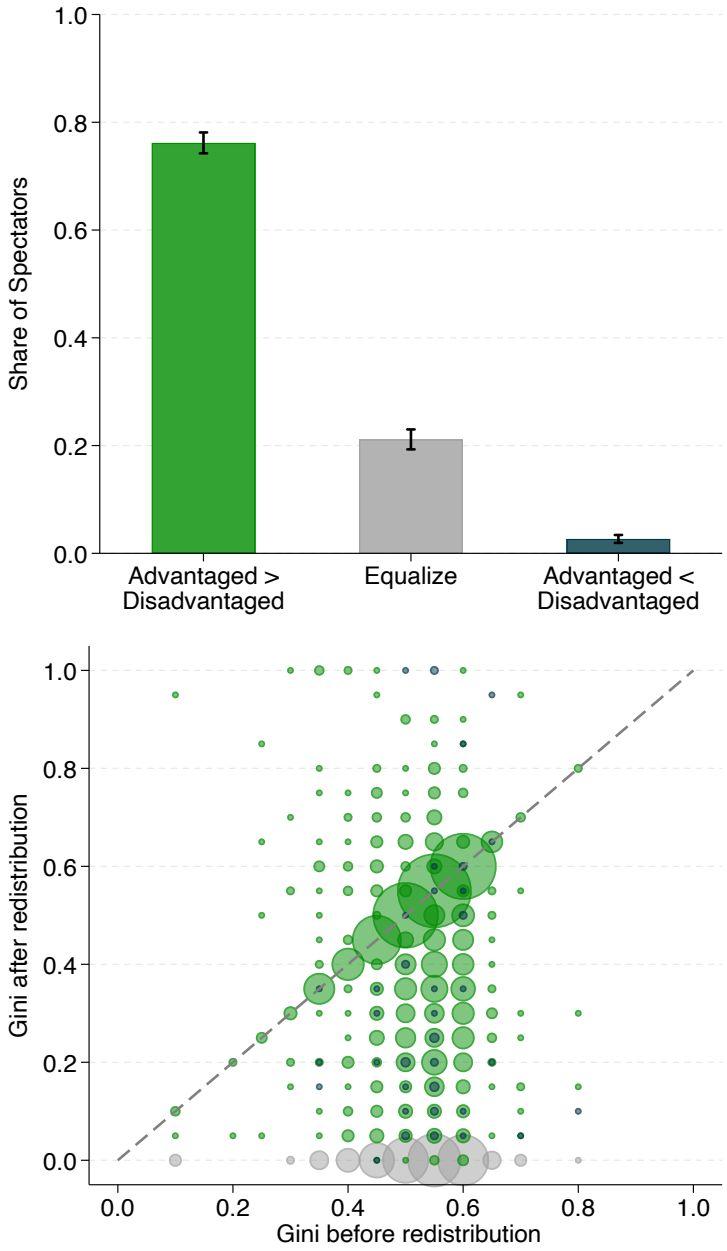

Figure A.10: Inequality acceptance under unequal opportunities (Scandinavia sample)

NOTE: See Figure 2.

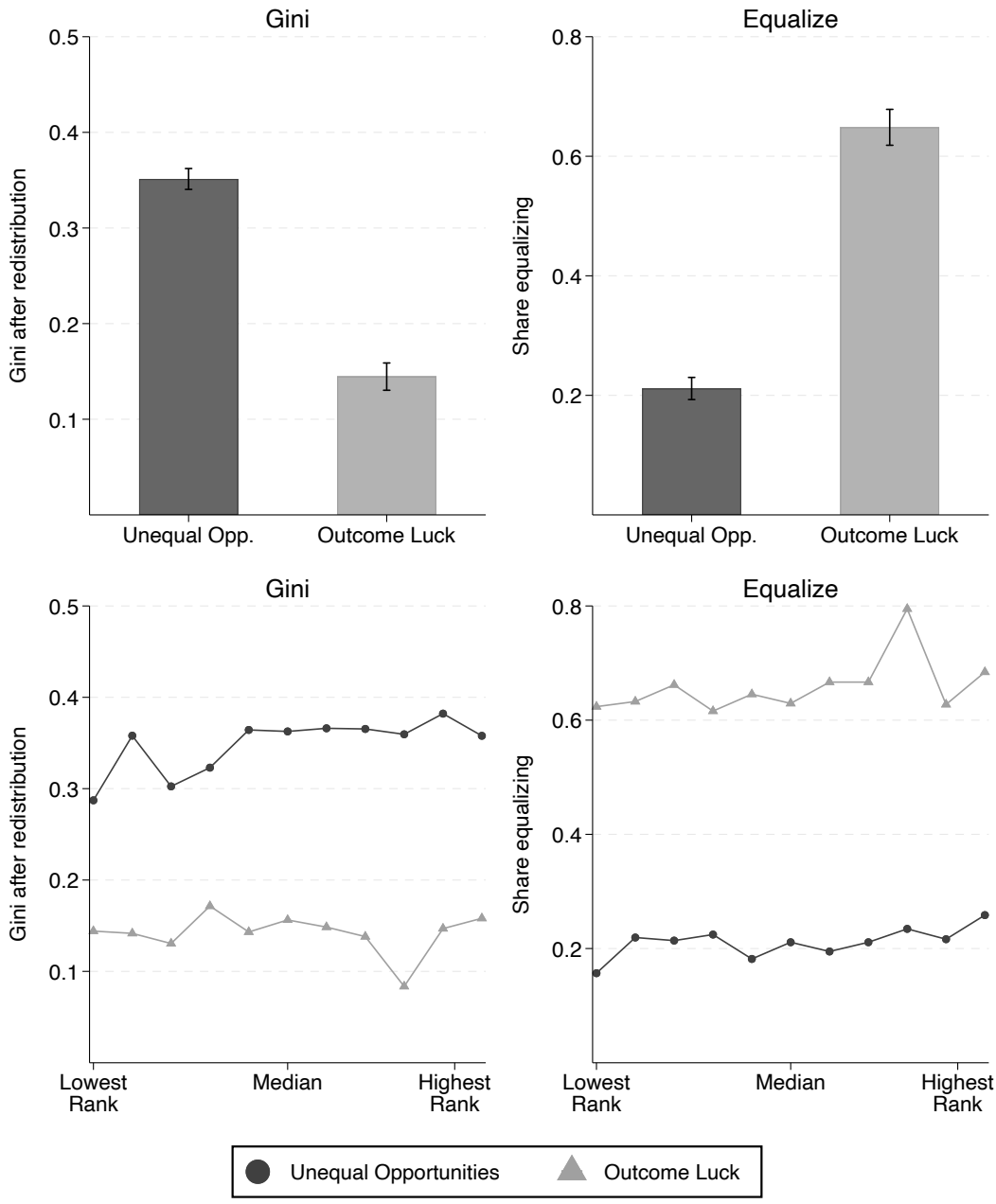

Figure A.11: Unequal Opportunities vs. Outcome Luck (Scandinavia sample)

NOTE: See Figure 3.

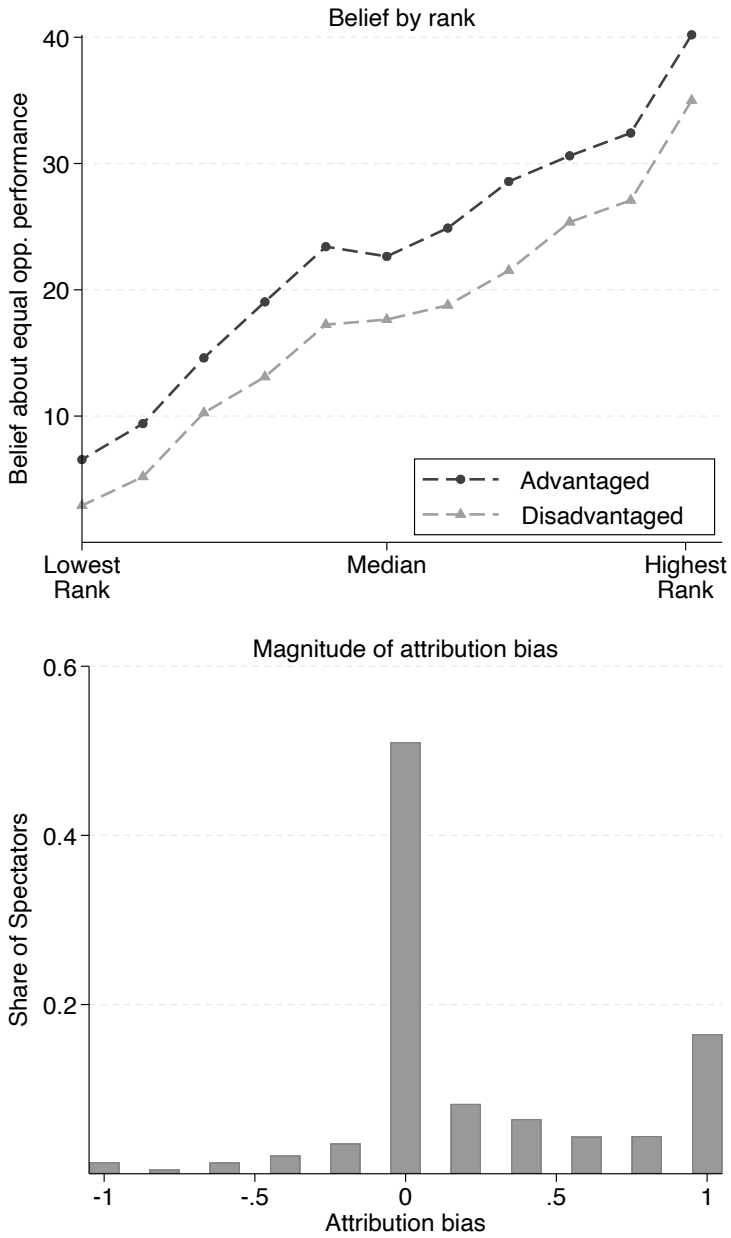

Figure A.12: Beliefs about worker productivity (Scandinavia sample)

NOTE: See Figure 4.

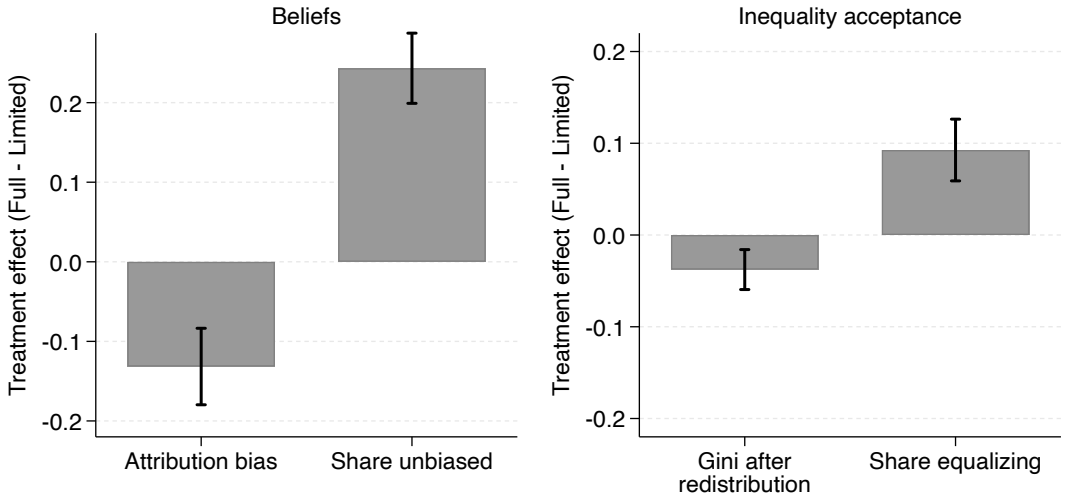

Figure A.13: Treatment effects: Limited vs. Full Information (Scandinavia sample)

NOTE: See Figure 5

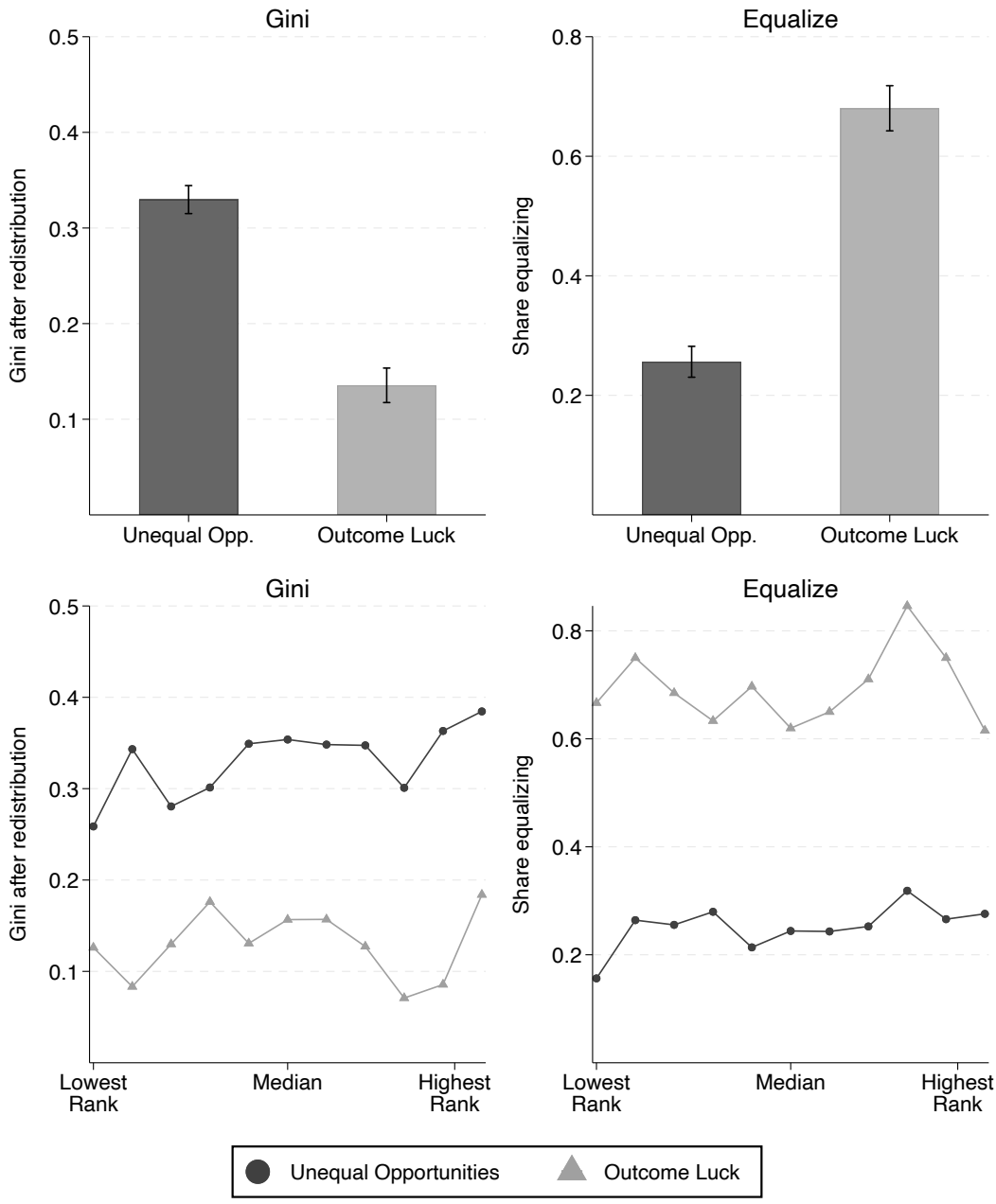

Figure A.14: Unbiased spectators: Unequal Opportunities vs. Outcome Luck (Scandinavia sample)

NOTE: See Figure 6.

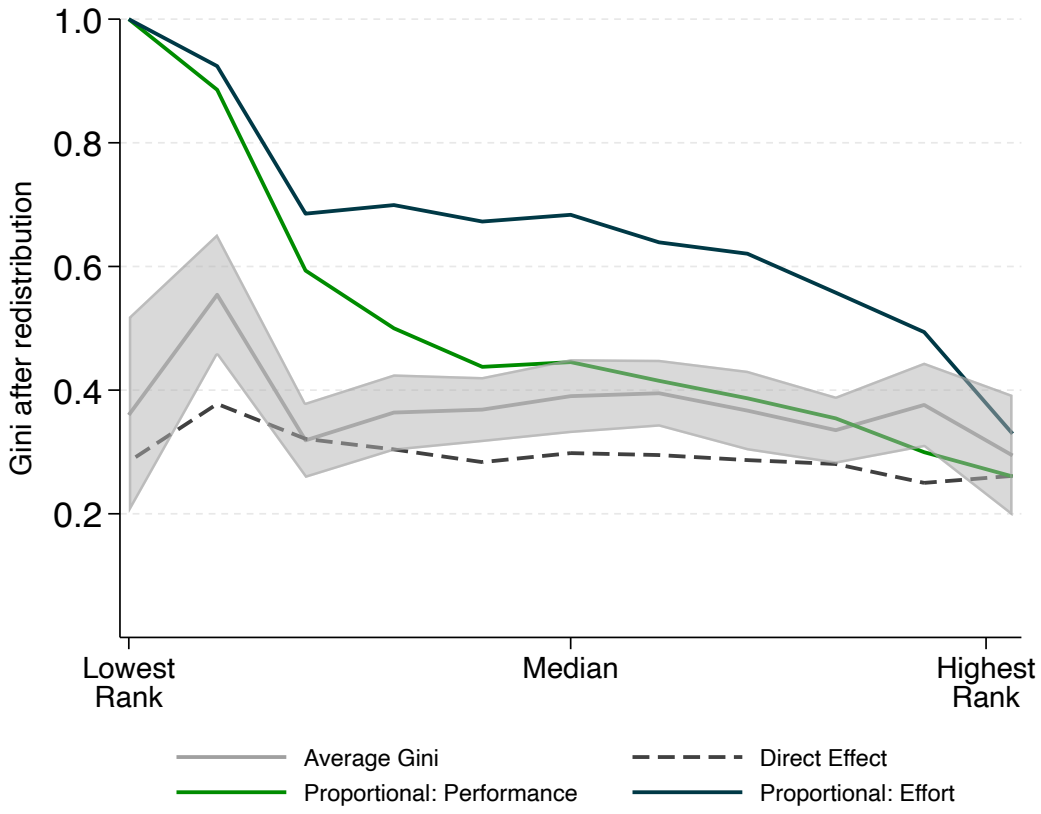

Figure A.15: Rules of redistribution: Predicted vs. actual Gini (Scandinavia sample)
NOTE: See Figure 8.

B Appendix: Further Details on Experimental Design and Fairness Views Estimation

B.1 Further Design Details

B.1.1 Workers, further details

Sample.: In recruiting, we impose two restrictions on our sample to ensure high data quality: Workers must be U.S. residents and they must have an approval rating of at least 99% on previous assignments. In addition, we exclude any participant who fails our comprehension checks, the attention checks or are repeated participants.

Task.: Our subjects perform an adapted version of the Abeler et al. (2011) count-

ing task, where the first table consists of 12-by-2 digits. With each correct answer, another 3 digits are added to the next matrix. If a worker provides the wrong answer to a table, they need to re-do the task. For each digit in a table, there is an equal probability of it taking the value of zero and of one. At any point, workers can choose to stop working. After stopping to work, workers cannot return to working on the task again.

When not working, we require workers to indicate that they are still attentive to the study by responding to attention checks, i.e. by clicking a button that appears on screen roughly once every 60 seconds and remains on screen for 5 seconds. Using continuous attention checks allows us to avoid workers conducting other leisure or work activities when not working. As such, we avoid workers gaining either monetary or non-monetary benefits outside of those generated by the earnings they accumulate in our experiment. Overall, these design choices allow us to align our experimental approach with the conceptual framework, where we assume that workers only derive monetary value from time spent not working. Essentially, by assigning a fixed \$0.25 per minute to leisure time we equalize the value derived from not working among workers.

To make it visible when it is earnings maximizing to stop working, we provide workers with a pop-up reminder when their earnings in the last minute from completing tasks falls below the per minute compensation from not working.

Workers are only informed of their own piece-rate, and not of the existence of another possible rate as we want them to make work-versus-stop-working decisions solely based on the benefit and (opportunity) cost of work. Introducing the social comparison concerns complicates both workers' choices, and more importantly, spectators' redistribution decisions.

Workers are paid a show-up fee of \$10 for participating in the study, which comes on top of any income they earn from their own and spectators' choices.

B.1.2 Spectators, further details

Sample.: Spectators are paid a show-up fee of \$3 for completing the study, and may earn additional income from the belief elicitation task. Following our pre-analysis plan, we exclude inattentive participants: Spectators who complete either the redistribution decision or the belief elicitation task in less than 10 seconds.

Redistribution decisions.: Spectators are provided with detailed information about the workers' performance. Beyond the information (texts and graphics) provided in the main text, we also ensure spectators have understood the information by requiring them to answer comprehension questions with corrections provided

for any inaccuracies.

When making redistribution decisions, spectators are asked to split the combined earnings of the two workers, without any cost of redistribution.

Spectators are informed that workers anticipated a third party's influence on their earnings before the start of the working section, but no information was provided on the identity or the specific decisions of these third parties.

B.1.3 Implementation, further details

We recruit individuals who are residents in either the United States or Scandinavia (Denmark, Norway or Sweden). One notable feature of our design is that we compare the Unequal Opportunities treatment to both the Outcome Luck treatment and the Unequal Opportunities with Limited Information treatment. To ensure that differences between treatments are not driven by differences in the worker sample facing spectators, we let half Unequal Opportunities spectators share the same worker sample with Outcome Luck spectators and, at the same time, let the other half share the same worker sample with Unequal Opportunities with Limited Information spectators. Therefore, we have twice as many spectators in the Unequal Opportunities treatment as in the Outcome Luck and the Unequal Opportunities with Limited Information treatments.

We planned to recruit 4000 spectators each from the U.S. and Scandinavia. In the end, we recruited a total of 8041 spectators. Following our pre-analysis plan, we impose one main exclusion criterion to exclude inattentive participants: spectators who complete either the redistribution decision or belief elicitation screen screen in less than 10 seconds.[11] Our main results remain qualitatively unchanged if we include the non-exclusive sample of 8041 spectators. Please refer to Appendix C.2 for details.

B.1.4 Treatments, further details

Outcome Luck. In addition to the basic information, we also provide a "within-group ranking" to be consistent with the Unequal Opportunities treatment. Here the "group" is the full sample because all workers receive the same piece-rate. A within-group ranking therefore informs spectators of the overall rankings of the two workers compared to all other workers.

We calibrate the random payments to the advantaged workers in the Outcome Luck treatment to create the same income inequality before redistribution. Specifically,

[11] 10 seconds is chosen as the cutoff as it is practically impossible to finish reading the instructions on these two screens within 10 second.

for each worker pair in the Unequal Opportunities treatment, we search for two workers in the Outcome Luck treatment who earned the same as the low piece-rate worker of the pair. For example, if the low piece-rate worker earned \$6 and the high piece-rate worker earned \$22, then we find two workers in the Outcome Luck treatment who both earned \$6. Next, we calibrate the random payment to replicate the inequality among the Unequal Opportunities treatment pair. In the above example, we will randomly assign \$16 to one of the two Outcome Luck workers and generate the (\$6,\$22) inequality. The only difference between the two pairs is that the income inequality is driven by outcome luck for one pair and unequal opportunities for the other pair. We conduct this procedure for each pair in the Unequal Opportunities treatment.

To measure the degree of attribution bias, we also elicit the beliefs regarding equal-opportunity performance in the Outcome Luck treatment. However, since the spectators in this treatment are already informed of the two workers' performance under equal opportunity, we instead present them with the scenario from the Unequal Opportunities treatment and elicit their beliefs regarding the performance of this second pair of workers.

B.1.5 Unbiased spectators in both Unequal Opportunities and Outcome Luck

Directly comparing inequality acceptance of the unbiased sample in the Unequal Opportunities treatment to the entire sample in the Outcome Luck treatment is problematic because the unbiased sample could differ from the entire sample in systematic ways.[12] To address this issue, we conduct a comparison between unbiased spectators in both treatments. In the Outcome Luck treatment, we also elicited spectators' beliefs on two Unequal Opportunities workers' equal-opportunity performance.[13]

Table A.5 shows that unbiased spectators in the two treatments are balanced on all demographic variables. This suggests that even though the unbiased sample is a selected sample, the same selection is applied to both treatments. Thus differences in sample selection are unlikely to explain a difference in inequality acceptance between the unbiased spectators of the two treatments.

Unbiased spectators account for 52% of the sample in Outcome Luck and 48% of

[12]For example, Americans, seniors, and people without higher education are more likely to be biased.

[13]Spectators in the Outcome Luck treatment are also biased in favor of the advantaged worker. On average, they estimated that the advantaged worker completed 9 more tasks than the disadvantaged worker under equal piece-rates, which is not statistically different from Unequal Opportunities spectators.

the sample in Unequal Opportunities. The initial inequality is not significantly across the two samples (0.50 in Unequal Opportunities and 0.52 in Outcome Luck, P-value=0.873).

B.2 Fairness Views: Detailed Definitions and Estimation Methods

This appendix provides more detailed definitions of the fairness views discussed in the main text and elaborates on the methods used to estimate their prevalence.

B.2.1 Detailed Definitions of Fairness Views

- **Libertarian:** Income inequality is fair, regardless of the source of inequality. Formally, this implies no redistribution: $e_i = y_i$, $e_j = y_j$ for all i, j.
- **Egalitarian:** Income inequality is unfair, regardless of the source of inequality. Formally, this implies full equalization of incomes: $y_i = y_j$ for any e_i and e_j.
- **Meritocrat:** If one person has greater merit than another person, then it is fair that this person has greater income; if they have the same merit, then they should have the same income. Formally:

$$\text{if } merit_i(\omega_i) = \max\{merit_i(\omega_i), merit_j(\omega_j)\}, \text{ then } \max\{y_i, y_j\} = y_i$$

  where $i \in \{L, H\}$ and $merit_i(\omega_i)$ is worker i's merit given opportunity ω_i.

Meritocrats are further classified into two categories:

- **Equal-Opportunity Meritocrats:** Define merit as an individual's productivity. Formally:

$$\text{if } \theta_i > \theta_j, \text{ then } y_i > y_j,$$

  where θ_i is worker i's productivity.
- **Unconditional Meritocrats:** Define merit as an individual's actual performance. Formally:

$$\text{if } p_i(\omega_i) > p_j(\omega_j), \text{ then } y_i > y_j,$$

  where $p_i(\omega_i)$ is worker i's performance under their opportunity ω_i.

B.2.2 Estimation Methods

In the Outcome Luck treatment, libertarians choose to not redistribute and egalitarians choose to fully equalize the earnings of the two workers. For meritocrats, the two workers perform the same, are equally productive and make the same effort choice. Thus meritocrats should equalize.

In the Unequal Opportunities treatments, libertarians and egalitarians will redistribute exactly as they do under Outcome Luck. Meritocrats behave differently depending on their definition of merit. Unconditional meritocrats allocate more earnings to the advantaged worker because of her higher performance. Equal-opportunity meritocrats equalize earnings of the two workers if they have the unbiased beliefs because the two workers are by design equally productive. However, they would also allocate more to the advantaged worker if they are attribution biased and believe that the advantaged worker is more productive. Note that equal-opportunity meritocrats are the only group of spectators that behave differently depending on their beliefs about the productivity of the two workers, and as a result the only group of spectators that may redistribute differently under full information versus limited information.

Given how different types of fairness views behave in different treatments, we use the following methods to estimate the prevalence of each fairness view:

- **Libertarians:** Estimated as the share of spectators who do not redistribute in the Outcome Luck treatment.
- **Egalitarians:** Estimated as the share of spectators who equalize the earnings of the two workers while holding biased beliefs in the Unequal Opportunities treatment.
- **Meritocrats:** Estimated using the the share of spectators who equalize in the Outcome Luck treatment minus the share of egalitarians.
  - **Equal-Opportunity Meritocrats:** Estimated using the share of spectators who equalize in the Unequal Opportunities treatment with Full information minus the share of equalizing spectators in the Limited Information treatment. We find a 7 percentage point increase in the share of equalizing spectators when moving from Limited to Full Information. This increase can only be driven by belief-sensitive equal-opportunity meritocrats, providing a lower bound estimate for this group.
  - **Unconditional Meritocrats:** Estimated by comparing the unbiased spectators in the Unequal Opportunities (Full Information) and Outcome Luck treatments, calculating the decrease in equalization in the

Unequal Opportunities treatment. We observe a 39 percentage point decrease in equalization in the Unequal Opportunities treatment. This decrease can only be driven by unconditional meritocrats because all other types who equalize in one treatment would also equalize in the other. Given that 48% of spectators are unbiased, this amounts to approximately 19% of the total population being unconditional meritocrats.

C Appendix: Robustness Analysis

C.1 Excluding non-matched sample

Our experimental design incorporates a matched sample approach to ensure robust comparisons across treatments. The worker pairs in the Unequal Opportunities Full Information (UOFI) treatment are strategically divided into two subgroups:

1. A subgroup matched to the worker pairs in the Outcome Luck (OL) treatment.
2. A subgroup identical to the pairs in the Unequal Opportunities Limited Information (UOLI) treatment.

This design allows for precise treatment comparisons while controlling for worker characteristics. Specifically:

- When comparing UOFI to OL, we utilize only the UOFI subgroup matched to OL pairs.
- When comparing UOFI to UOLI, we use only the UOFI subgroup identical to UOLI pairs.

Formally, let W_{UOFI} be the set of all worker pairs in the UOFI treatment. We can express this as the union of two disjoint subsets:

$$W_{UOFI} = W_{UOFI-OL} \cup W_{UOFI-UOLI}$$

where $W_{UOFI-OL}$ is matched to OL pairs and $W_{UOFI-UOLI}$ is identical to UOLI pairs.

For our analyses:

$$\begin{aligned} \text{UOFI vs. OL comparison:} \quad & W_{UOFI-OL} \text{ vs. } W_{OL} \\ \text{UOFI vs. UOLI comparison:} \quad & W_{UOFI-UOLI} \text{ vs. } W_{UOLI} \end{aligned}$$

This approach ensures that any observed differences in spectator behavior can be attributed solely to the treatment manipulations, rather than to variations in worker pair characteristics.

Table C.1 demonstrates that all main treatment effects reported in Sections 4 and 5 remain consistent when using these matched samples instead of the full UOFI sample. Furthermore, Figures C.1 –C.9 in this appendix reproduce all main figures using only the matched samples, illustrating the robustness of our findings.

Table C.1: Alternative Sample: Main vs. Matched Samples

(a) Panel A: Inequality Acceptance

	(1) Main	(2) Match with OutLuck	(3) Difference (2) vs. OutLuck	(4) Match with OppLim	(5) Difference (4) vs. OppLim
Share Adv. > Disadv.	0.809	0.818	0.416***	0.801	0.085***
	(0.393)	(0.386)	(0.015)	(0.400)	(0.012)
Share equalizing	0.158	0.156	-0.395***	0.161	-0.074***
	(0.365)	(0.363)	(0.015)	(0.367)	(0.011)
Gini before	0.521	0.523	0.002	0.520	0.002
	(0.084)	(0.062)	(0.002)	(0.102)	(0.003)
Gini after	0.389	0.387	0.197***	0.392	0.038***
	(0.232)	(0.224)	(0.008)	(0.239)	(0.008)
Observations	3408	1728	3474	1680	3350

(b) Panel B: Attribution Bias

	(1) Main	(2) Match with OutLuck	(3) Difference (2) vs. OutLuck	(4) Match with OppLim	(5) Difference (4) vs. OppLim
Performance Adv. vs. Disadv.	9.354	9.321	-0.026	9.387	3.468***
	(11.705)	(11.580)	(0.397)	(11.834)	(0.416)
Magnitude attribution bias.	0.316	0.322	-0.001	0.310	0.114***
	(0.550)	(0.523)	(0.018)	(0.578)	(0.021)
Share unbiased	0.483	0.487	-0.033*	0.479	-0.198***
	(0.500)	(0.500)	(0.017)	(0.500)	(0.016)
Share fully biased	0.204	0.202	-0.072***	0.207	0.059***
	(0.403)	(0.402)	(0.014)	(0.405)	(0.015)
Observations	3408	1728	3474	1680	3350

NOTE: The table shows the means for our main outcome variables for our main Unequal Opportunities Full information sample (Column 1) as well as the two parts, each matched with one other treatment. In Column 2 we find the means for the sample which contains the same set of worker pairs as the Outcome Luck treatment and in Column 3 we show the difference between (2) and the Outcome Luck treatment means. In Column 4 we find the means for the sample with the same worker pairs as Unequal Opportunities with Limited Information and Column 5 gives the differences between (4) and the Limited Information treatment. Standard deviations in parentheses in (1), (2) and (4). Standard errors in parentheses in (3) and (5). $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

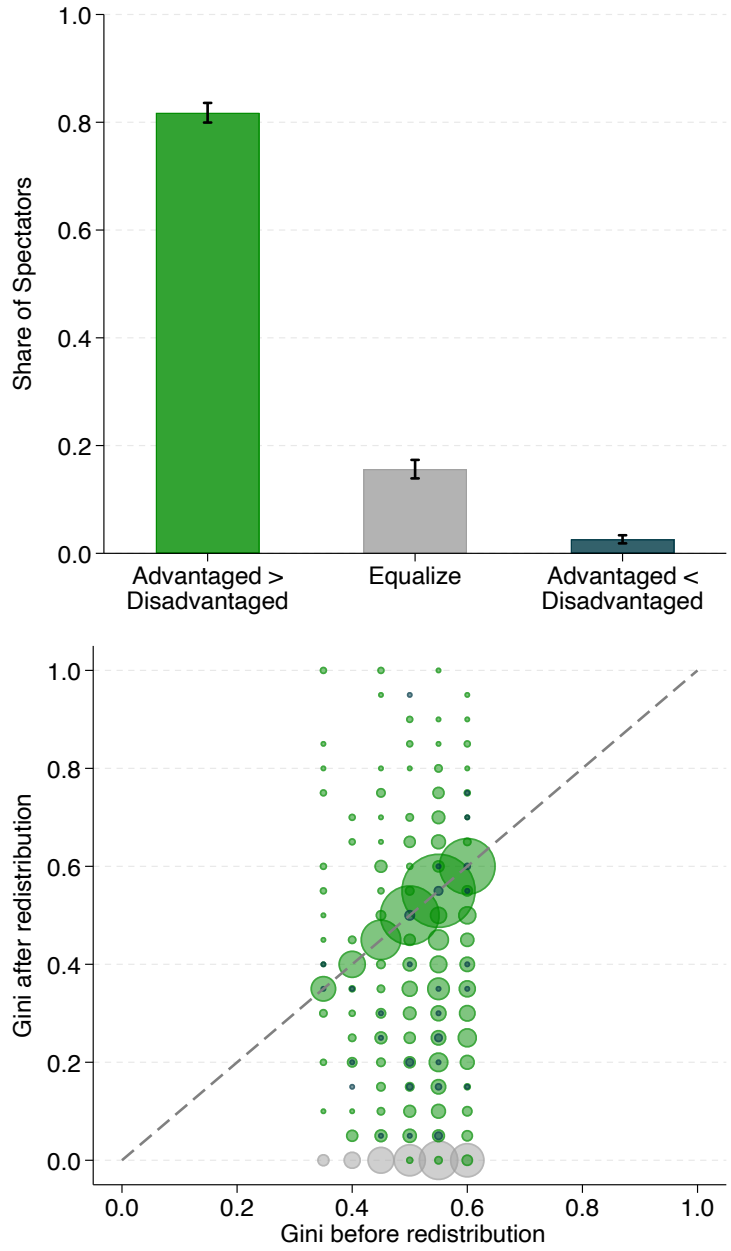

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Figure C.1: Inequality acceptance under unequal opportunities (Matched sample)

NOTE: See Figure 2.

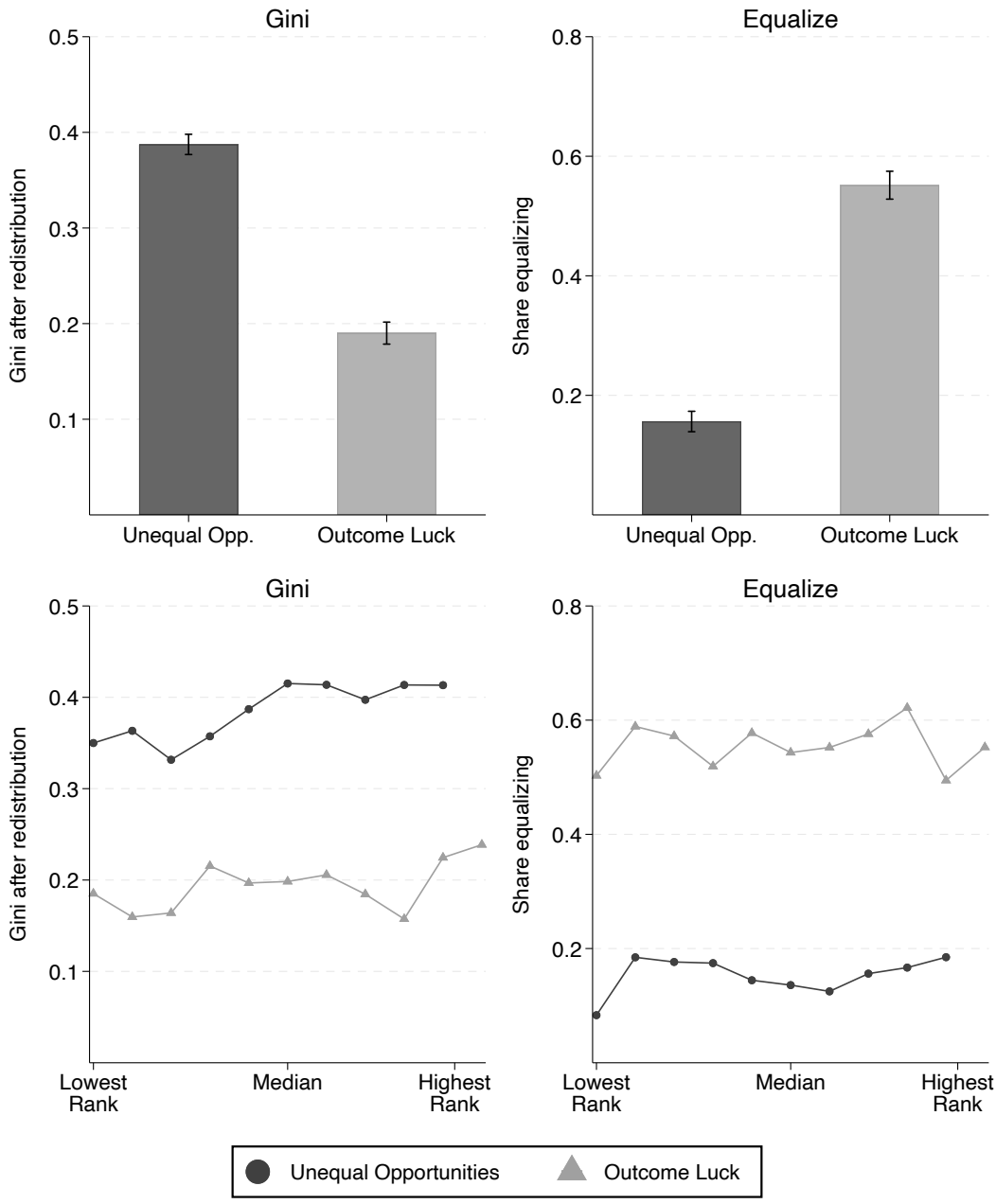


Figure C.2: Unequal Opportunities vs. Outcome Luck (Matched sample)

NOTE: See Figure 3.

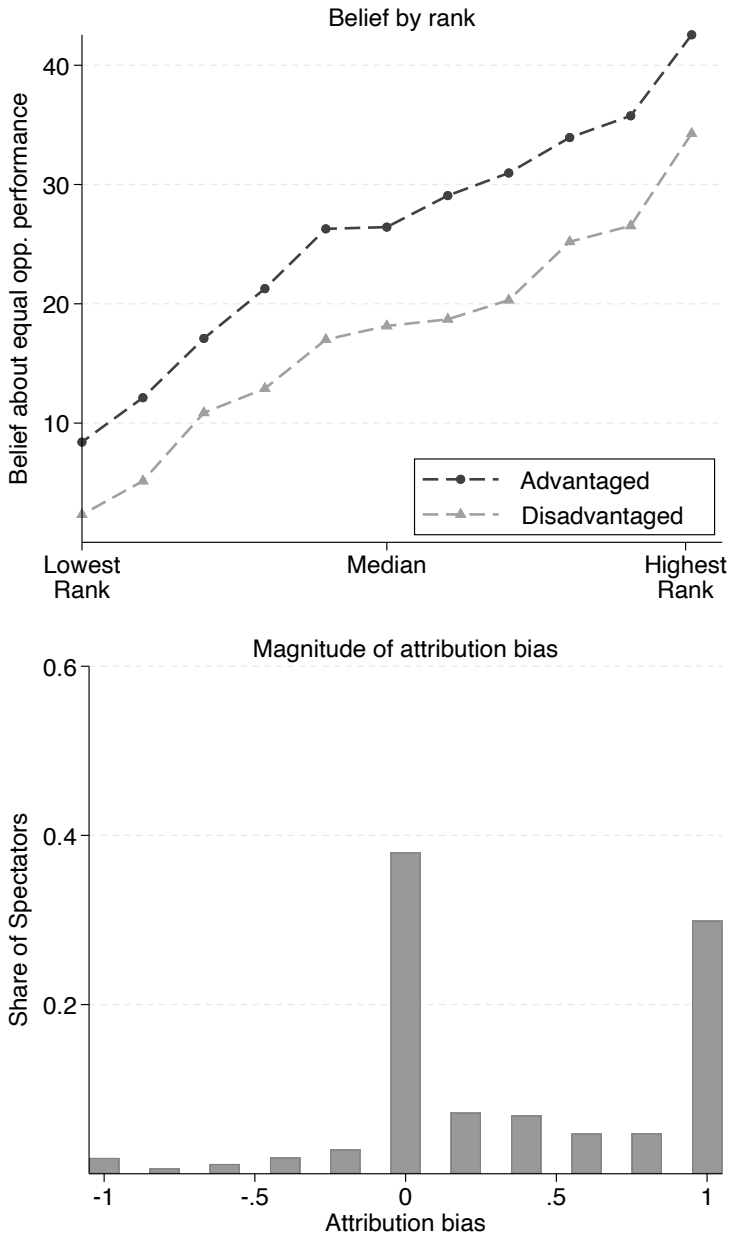


Figure C.3: Beliefs about worker productivity (Matched sample)

NOTE: See Figure 4.

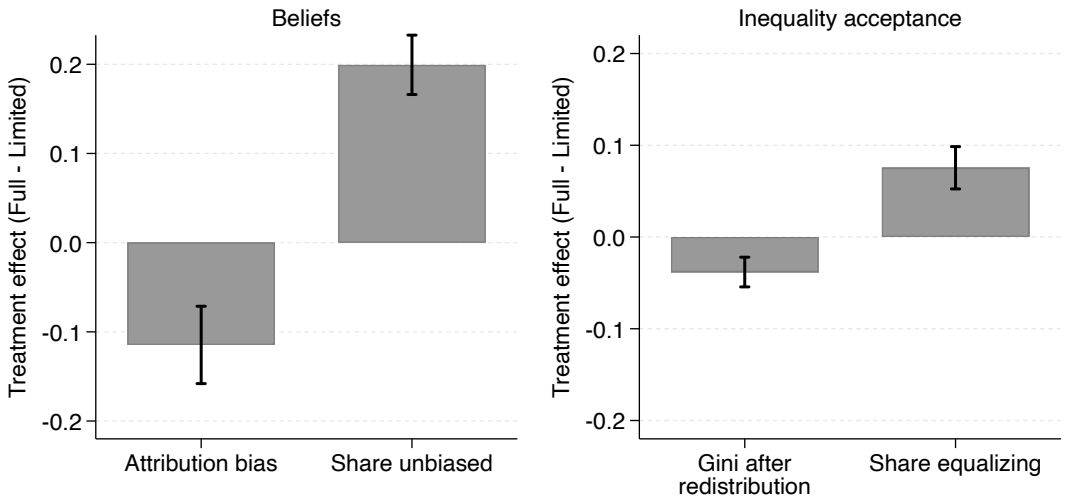


Figure C.4: Treatment effects: Limited vs. Full Information (Matched sample)

NOTE: See Figure 5

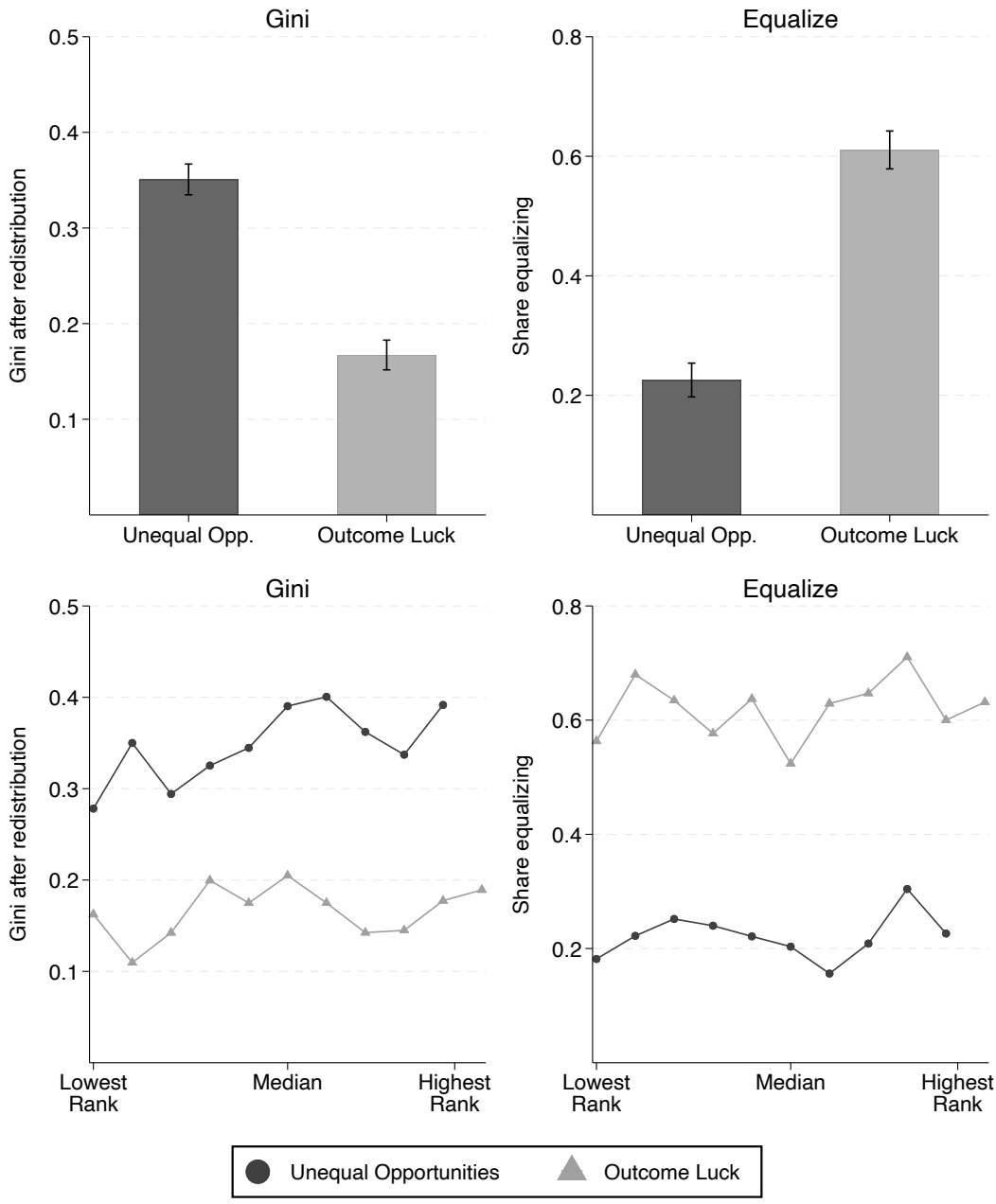


Figure C.5: Unbiased spectators: Unequal Opportunities vs. Outcome Luck (Matched sample)

NOTE: See Figure 6.

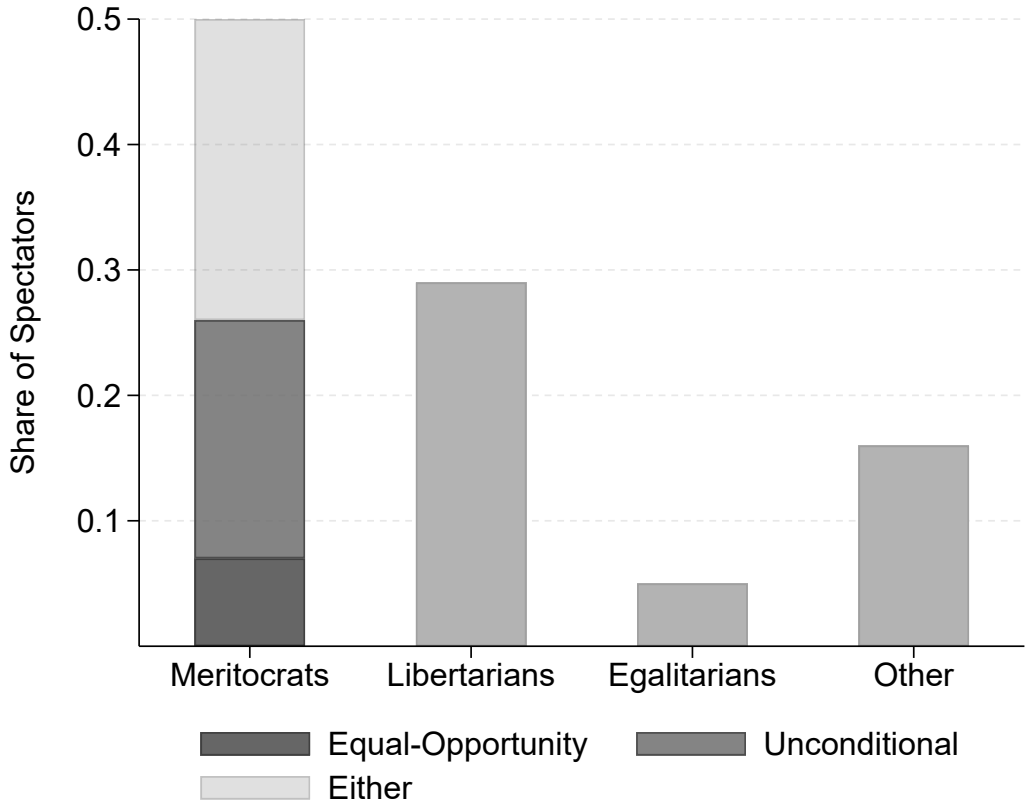


Figure C.6: Prevalence of fairness views (Matched sample)
NOTE: see Figure 7.

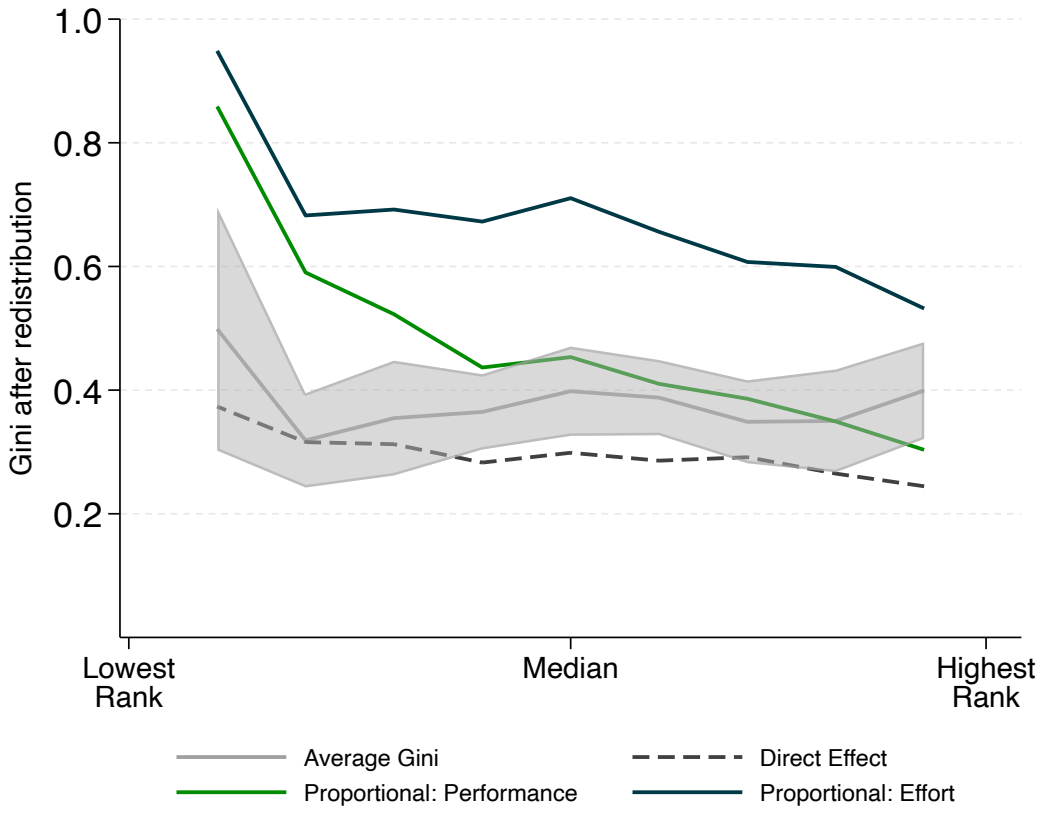


Figure C.7: Rules of redistribution: Predicted vs. actual Gini (Matched sample)

NOTE: See Figure 8.

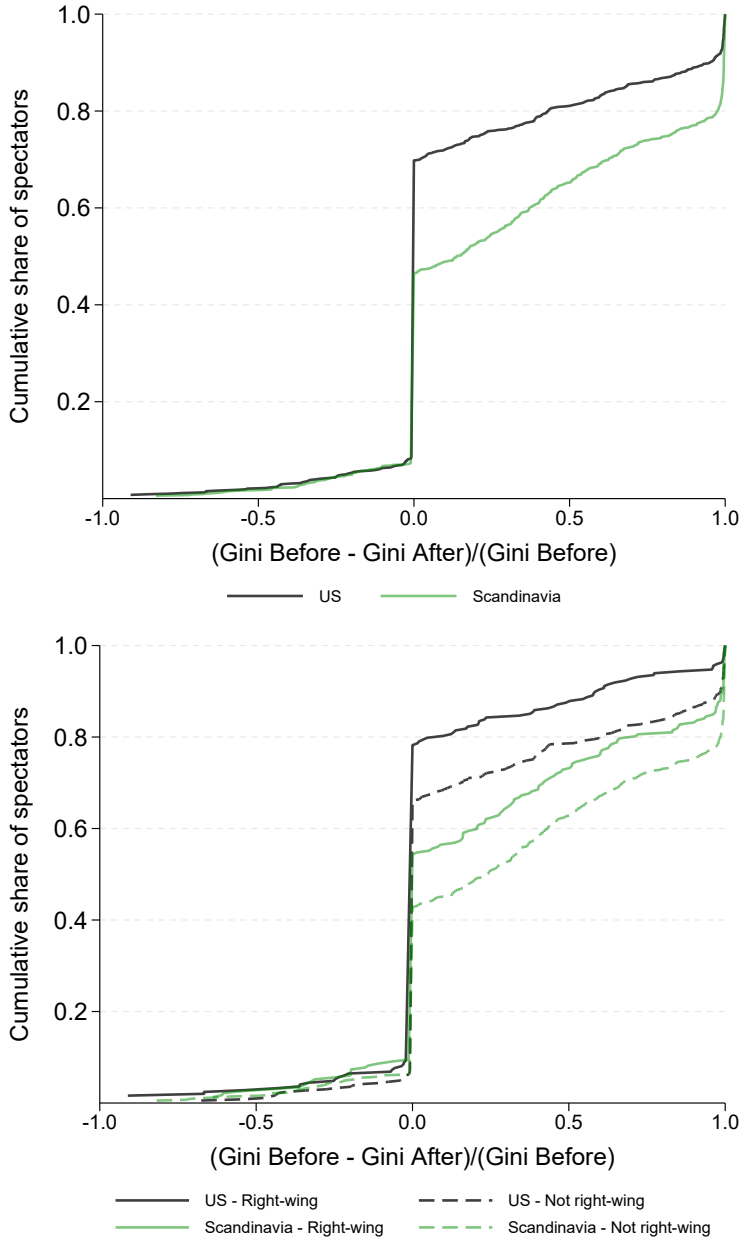


Figure C.8: Change in Gini across societies and political orientation (Matched sample)

NOTE: See Figure 9

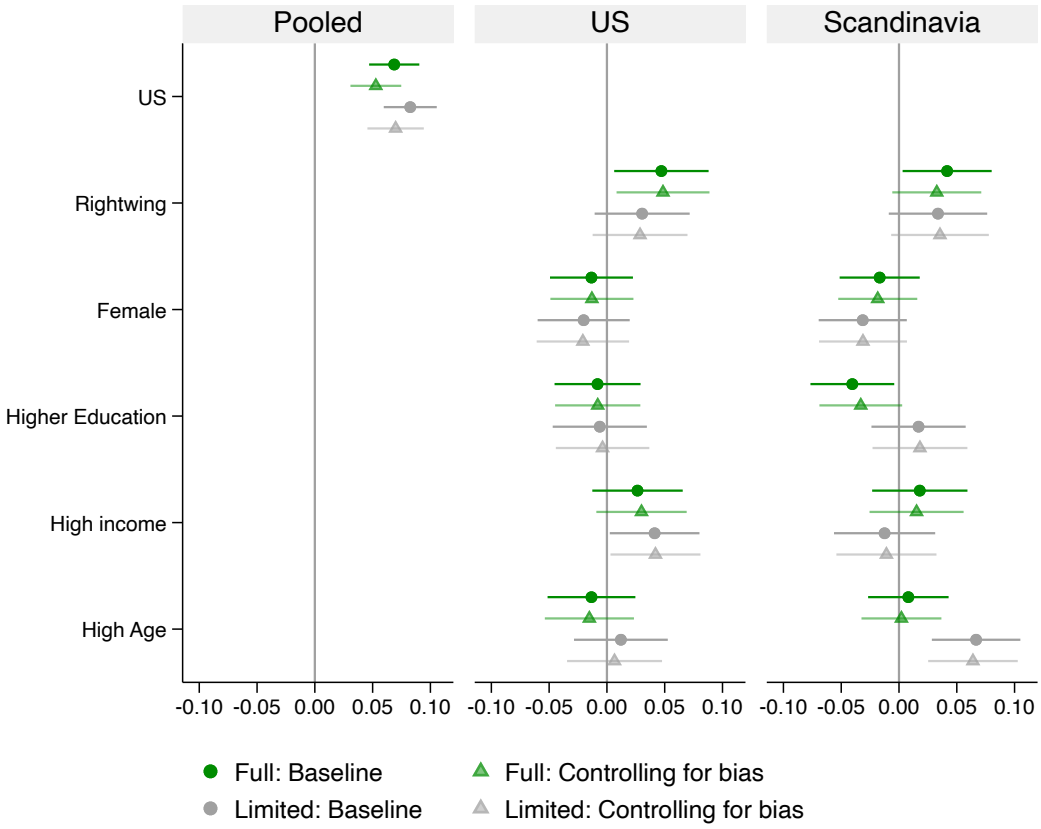


Figure C.9: Determinants of inequality acceptance under unequal opportunities (Matched sample)

NOTE: See Figure A.2.

C.2 Including excluded participants

In this section we show that our results are robust to including all 8,041 spectators who completed the study, without imposing any exclusion restrictions. As preregistered and outlined in Section 3 and Appendix B.1, we exclude any subject who is inattentive, defined as completing either the redistribution decision or the belief elicitation screen in less than 10 seconds (N=1,059). In addition, we exclude observations where the initial earnings of the two workers in a pair are identical, implying that we cannot distinguish between spectators choosing to equalize earnings or spectators choosing to not redistribute at all.

We reproduce all the main treatment comparisons using this non-exclusive sample and report the results in Table C.3. Next, we reproduce all main figures and tables using this sample.

Table C.3: Alternative Sample: Non-exclusive sample

(a) Panel A: Inequality Acceptance

	(1) Full UneqOppFull	(2) Full OutLuck	(3) Full UneqOppLim	(4) Difference (1) vs. (2)	(5) Difference (1) vs. (3)
Share Adv. > Disadv.	0.820	0.482	0.872	0.338***	0.052***
	(0.384)	(0.500)	(0.334)	(0.012)	(0.010)
Share equalizing	0.138	0.476	0.075	-0.338***	-0.063***
	(0.345)	(0.500)	(0.264)	(0.011)	(0.009)
Gini before	0.515	0.521	0.508	-0.006*	-0.007*
	(0.111)	(0.062)	(0.143)	(0.003)	(0.003)
Gini after	0.400	0.235	0.427	0.165***	0.028***
	(0.230)	(0.256)	(0.224)	(0.007)	(0.006)
Observations	3992	2028	2021	6020	6013

(b) Panel B: Attribution Bias

	(1) Full UneqOppFull	(2) Full OutLuck	(3) Full UneqOppLim	(4) Difference (1) vs. (2)	(5) Difference (1) vs. (3)
Performance Adv. vs. Disadv.	9.203	9.292	12.328	-0.089	3.125***
	(11.674)	(12.164)	(12.448)	(0.323)	(0.326)
Magnitude attribution bias.	0.273	0.298	0.432	-0.024	0.158***
	(1.702)	(0.554)	(1.234)	(0.039)	(0.043)
Share unbiased	0.490	0.521	0.316	-0.031*	-0.174***
	(0.500)	(0.500)	(0.465)	(0.014)	(0.013)
Share fully biased	0.199	0.258	0.258	-0.059***	0.059***
	(0.399)	(0.438)	(0.438)	(0.011)	(0.011)
Observations	3992	2028	2021	6020	6013

NOTE: The table shows the means for our main outcome variables for the full sample, separated by treatment, in columns (1)-(3). In columns (4) and (5) we show the main treatment differences for the full sample. Standard deviations in parentheses in (1)-(3). Standard errors in parentheses in (4) and (5). *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

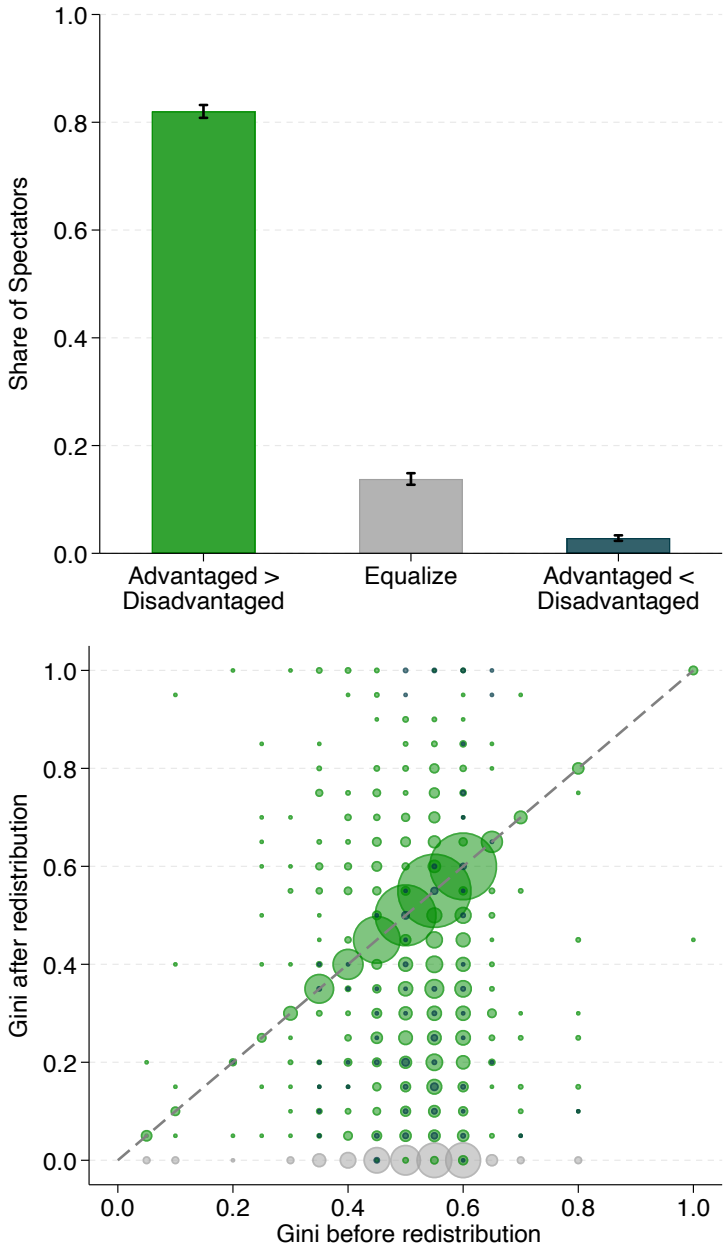


Figure C.10: Inequality acceptance under unequal opportunities (Non-exclusive sample)

NOTE: See Figure 2.

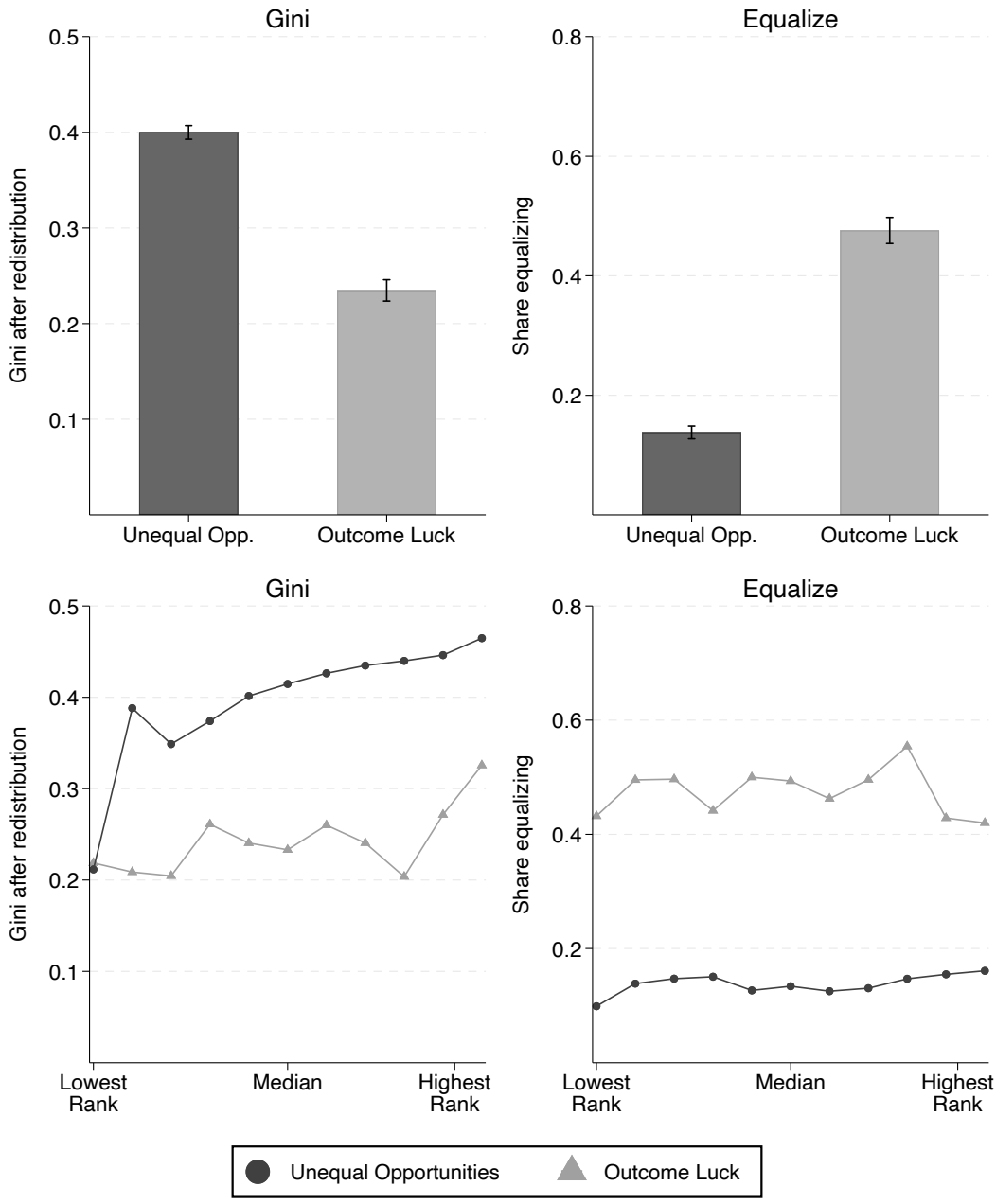


Figure C.11: Unequal Opportunities vs. Outcome Luck (Non-exclusive sample)

NOTE: See Figure 3.

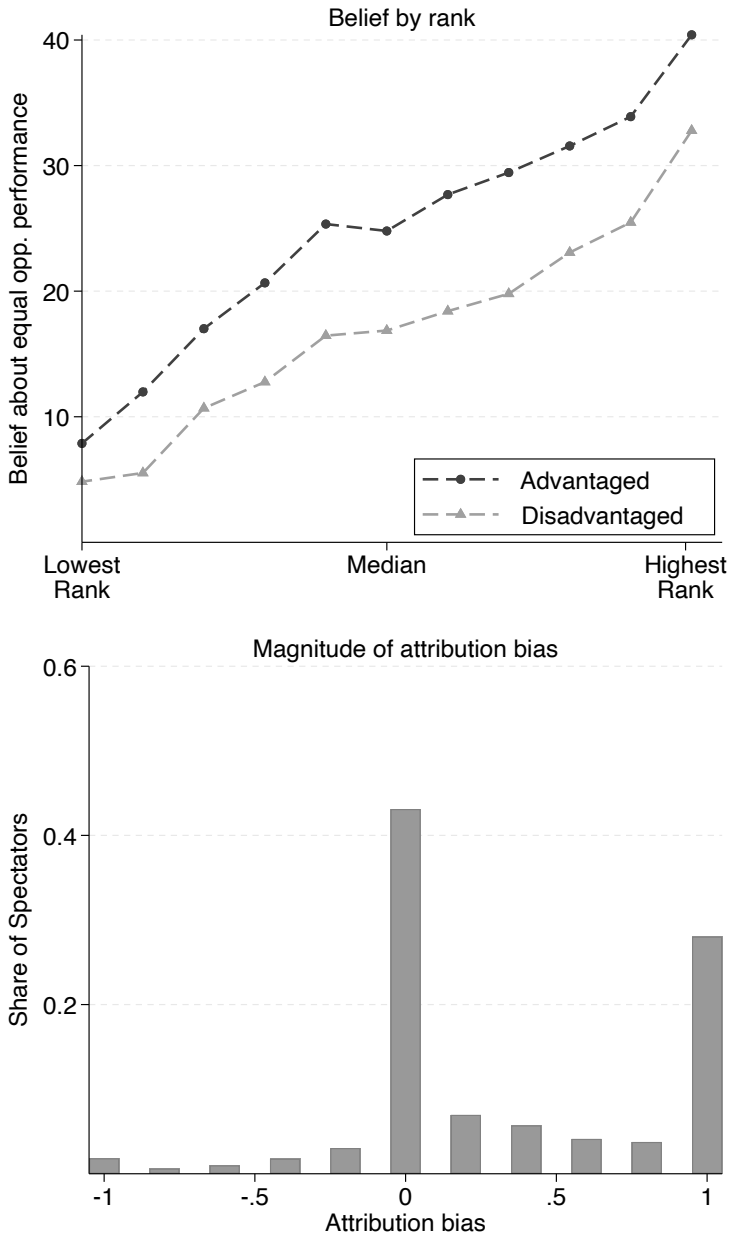


Figure C.12: Beliefs about worker productivity (Non-exclusive sample)

NOTE: See Figure 4.

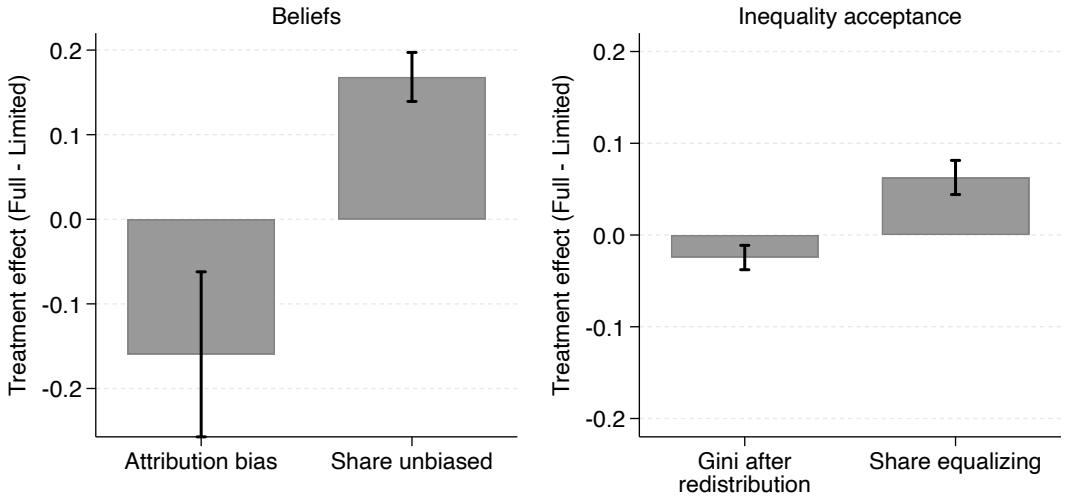


Figure C.13: Treatment effects: Limited vs. Full Information (Non-exclusive sample)

NOTE: See Figure 5

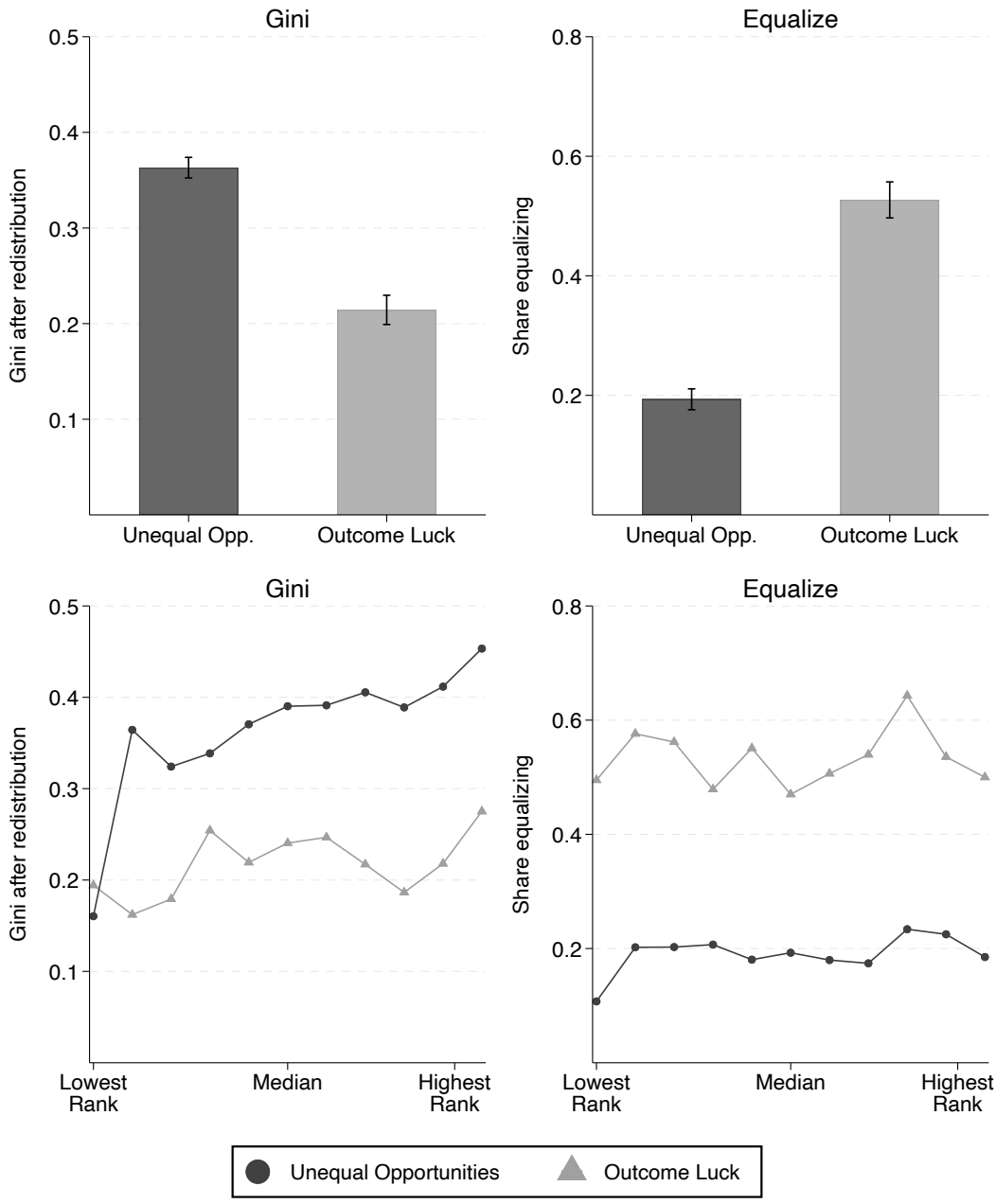


Figure C.14: Unbiased spectators: Unequal Opportunities vs. Outcome Luck (Non-exclusive sample)

NOTE: See Figure 6.

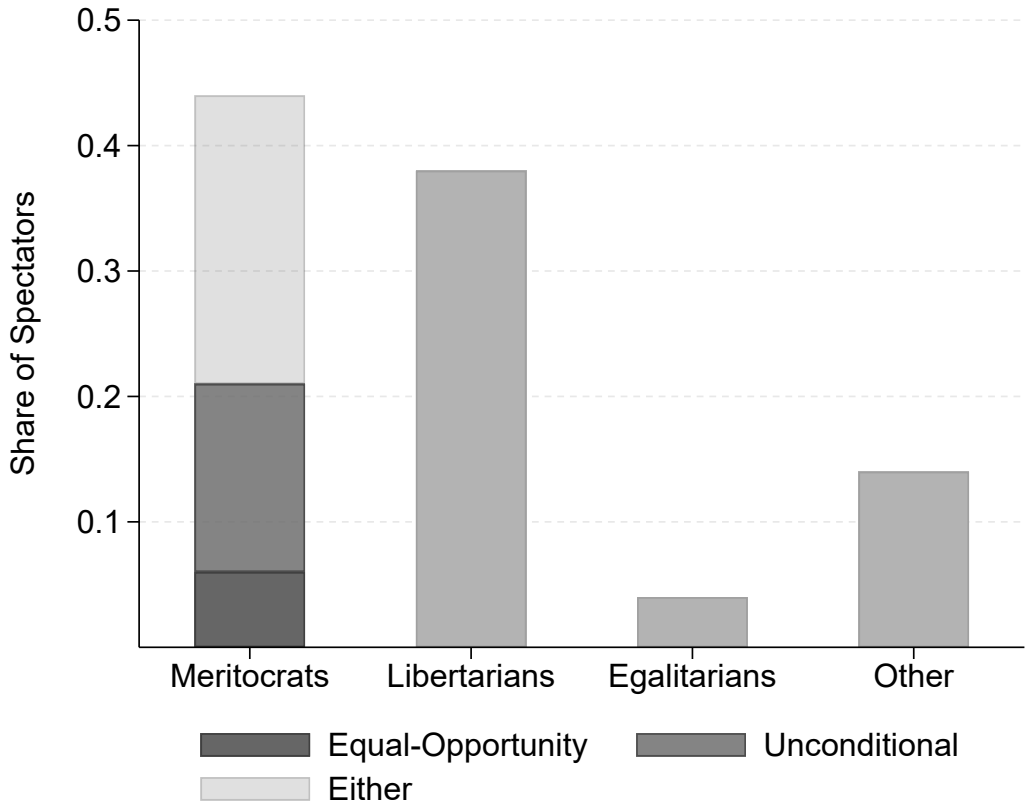


Figure C.15: Prevalence of fairness views (Non-exclusive sample)
NOTE: see Figure 7.

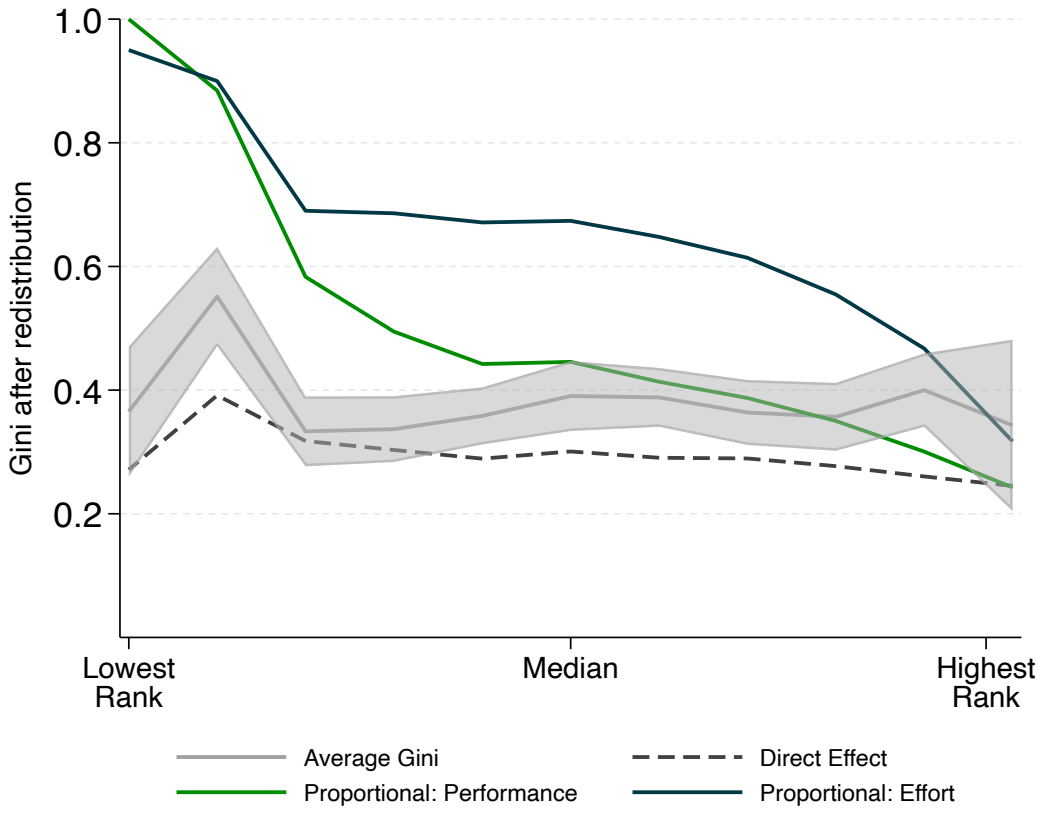


Figure C.16: Rules of redistribution: Predicted vs. actual Gini (Non-exclusive sample)

NOTE: See Figure 8.

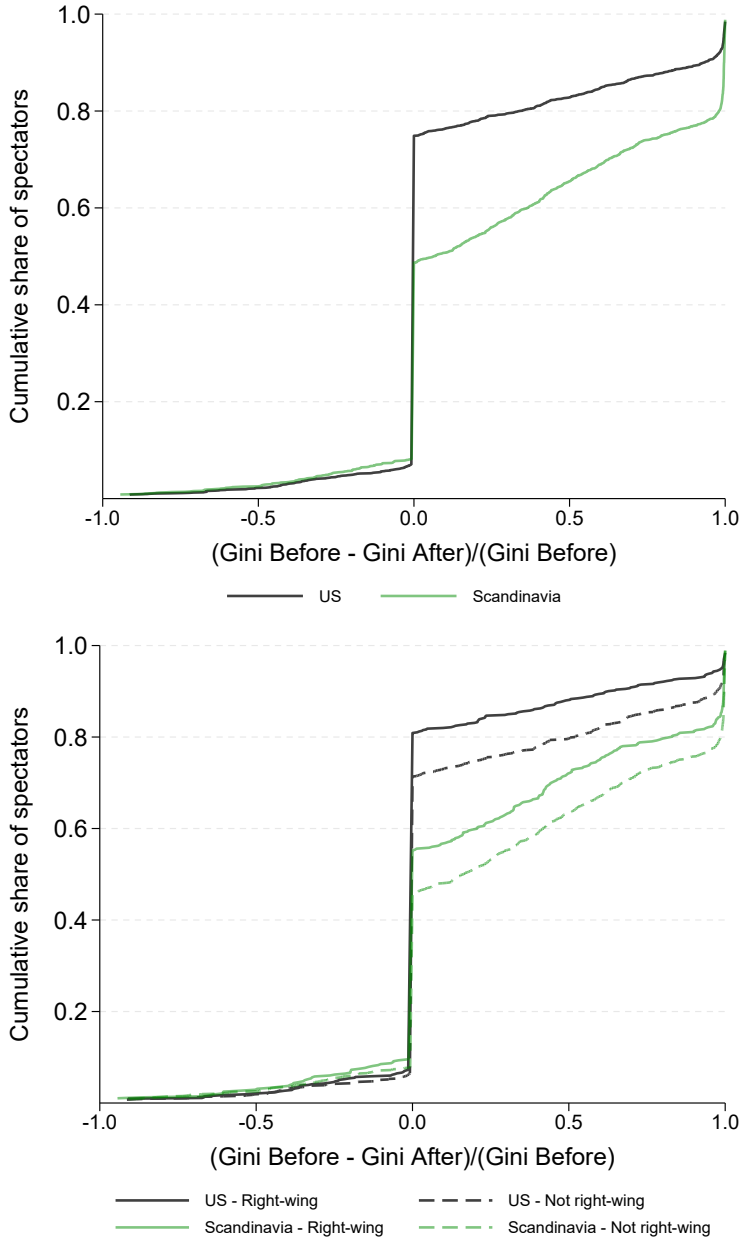


Figure C.17: Change in Gini across societies and political orientation (Non-exclusive sample)

NOTE: See Figure 9

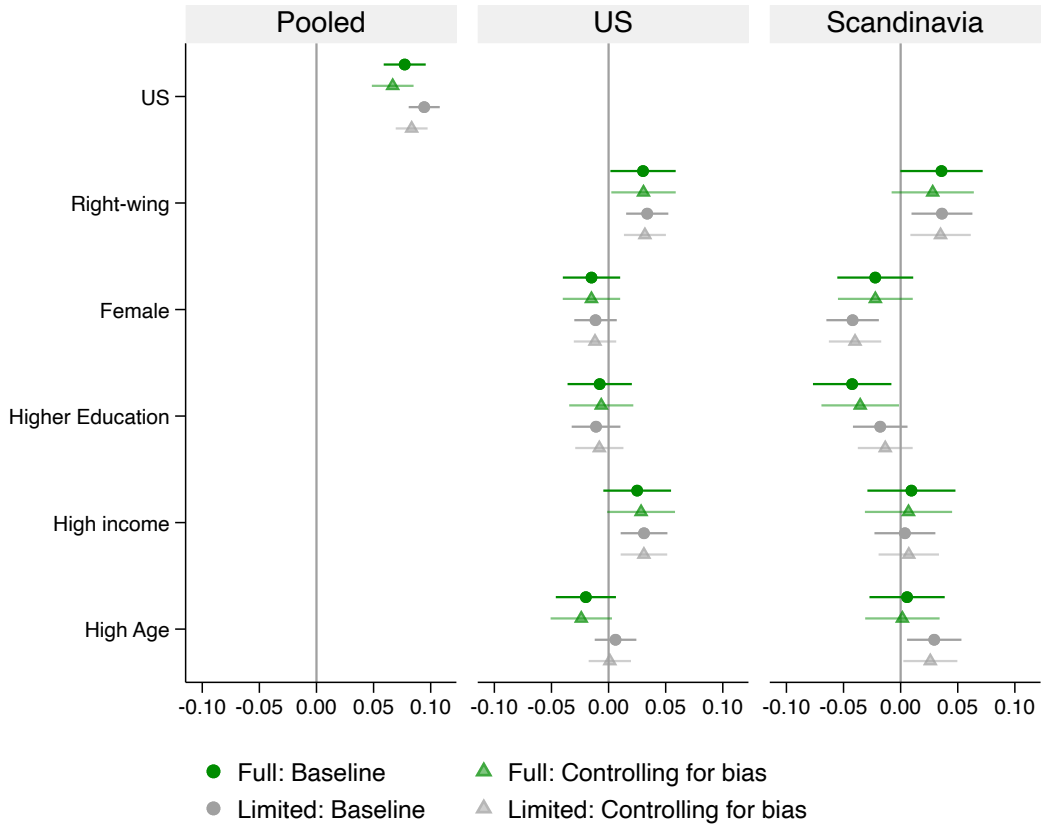


Figure C.18: Determinants of inequality acceptance under unequal opportunities (Non-exclusive sample)

NOTE: See Figure A.2.

C.3 Appendix: Pre-Plan

In this section we present the regression results outlined in our pre-registration document.

Table C.8 presents regressions using the share of spectators equalizing the income of the two workers as dependent variables (as in our main results). Table C.5 presents regressions using the income share of the disadvantaged worker after redistribution as the dependent variable. Like the Gini coefficient that we used in our main results, this outcome variable captures the degree of inequality between the two workers in a pair after redistribution. However, it has the shortcoming that it does not standardize this measure across worker pairs. Nevertheless, as can be seen, our main results remain significant when using the share of the disadvantaged worker as our outcome variable.

In Figure C.19, we report the treatment differences for both treatment comparisons (Outcome Luck versus Unequal Opportunities; Full versus Limited information) using three different margins of equalization. As can be seen, our treatment differences are robustly different from zero, regardless of whether we use a narrower or wider definition of what it means to equalize the earnings of the two workers. We conclude that our results are not sensitive to the exact margins of what we consider equalizing.

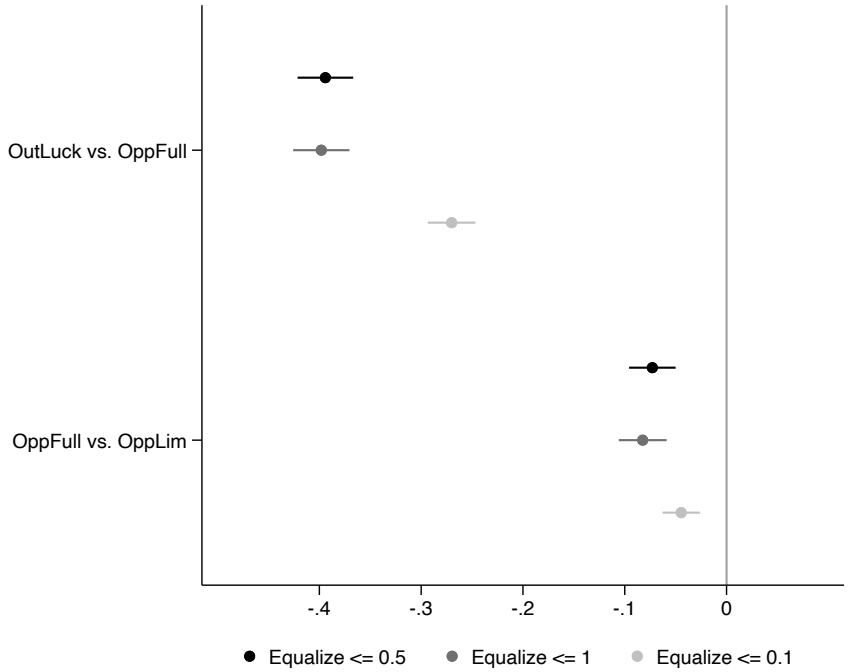


Figure C.19: Robustness: Treatment effect across definitions of equalizing

NOTE: This figure plots the treatment differences for three different definitions of variable "Equalizing", as pre-registered. In all regressions we include the treatment, keeping Outcome Luck as the baseline in the comparison with Uneq. Opp with Full Info, and keeping Limited Info the baseline when comparing with Full Info, as well as a constant. We use worker-pair fixed effects across all comparisons.

Table C.5: Inequaltiy Acceptance: Income share of disadvantaged

(a) Panel A: OutLuck vs. OppFull

	(1)	(2)	(3)	(4)
	Sh. Disadv.	Sh. Disadv.	Sh. Disadv.	Sh. Disadv.
OppFull	-0.100***	-0.106***	-0.096***	-0.100***
	(0.004)	(0.006)	(0.006)	(0.008)
US		-0.050***		-0.038***
		(0.006)		(0.004)
OppFull x US		0.014		
		(0.008)		
Right-wing			-0.017*	-0.015**
			(0.007)	(0.005)
OppFull x Right-wing			-0.011	
			(0.009)	
Constant	0.417***	0.440***	0.422***	0.426***
	(0.003)	(0.004)	(0.005)	(0.037)
Demographic controls				Yes
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	5154	5154	4218	3957

(b) Panel B: OppFull vs. OppLim

	(1)	(2)	(3)
	Sh. Disadv.	Sh. Disadv.	Sh. Disadv.
OppFull	0.019***	0.015**	0.019*
	(0.004)	(0.006)	(0.008)
Size of attrib. bias		-0.001***	
		(0.000)	
OppFull x Size of attrib. bias		-0.000	
		(0.000)	
Attrib. bias			-0.021**
			(0.008)
OppFull x Attrib. bias			-0.006
			(0.009)
Constant	0.297***	0.314***	0.313***
	(0.004)	(0.005)	(0.007)
Worker-pair fixed effects	Yes	Yes	Yes
Observations	5078	5078	5078

(c) Panel C: OppFull vs. OppLim (cont.)

	(1)	(2)	(3)
	Sh. Disadv.	Sh. Disadv.	Sh. Disadv.
OppFull	0.025***	0.023***	0.017***
	(0.006)	(0.006)	(0.005)
US	-0.023***		-0.027***
	(0.007)		(0.004)
OppFull x US	-0.012		
	(0.008)		
Right-wing		-0.018*	-0.021***
		(0.008)	(0.005)
OppFull x Right-wing		-0.011	
		(0.010)	
Constant	0.307***	0.303***	0.317***
	(0.005)	(0.005)	(0.062)
Demographic controls			Yes
Worker-pair fixed effects	Yes	Yes	Yes
Observations	5078	4133	3867

OTE: In Panel A we use Outcome Luck as our baseline treatment and test for the difference to the Unequal Opportunities with Full Information treatment. In Panel B and C we instead use the Unequal Opportunities with Limited information as our baseline and test for differences to the Unequal Opportunities with Full Information treatment. When controlling for the size of the attribution bias we control for the absolute difference in the beliefs regarding the number of tasks completed by the two workers under equal opportunity. When controlling for the attribution bias we follow the pre-plan and deviate from the definition in the main text, and consider spectators as attribution biased when the absolute difference in beliefs is larger than 1 (as compared 2 in our main results). The demographic controls included in Panel A column (4) and Panel C column (3) include dummies for US, right-wing, above median age, female gender, higher education as well as controls for the rank of the worker pair. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses.

Table C.8: Inequality Acceptance: Equalizing

(a) Panel A: OutLuck vs. OppFull

	(1) Equalize	(2) Equalize	(3) Equalize	(4) Equalize
OppFull	-0.394***	-0.439***	-0.386***	-0.389***
	(0.014)	(0.018)	(0.018)	(0.025)
US		-0.217***		-0.148***
		(0.020)		(0.014)
OppFull x US		0.102***		
		(0.025)		
Right-wing			-0.041	-0.029*
			(0.023)	(0.014)
OppFull x Right-wing			-0.027	
			(0.029)	
Constant	0.552***	0.649***	0.563***	0.526***
	(0.011)	(0.014)	(0.015)	(0.113)
Demographic controls				Yes
Worker-pair fixed effects	Yes	Yes	Yes	Yes
Observations	5154	5154	4218	3957

(a) Panel B: OppFull vs. OppLim

	(1)	(2)	(3)
	Equalize	Equalize	Equalize
OppFull	0.073***	0.084***	0.083***
	(0.012)	(0.015)	(0.021)
Size of attrib. bias		-0.003***	
		(0.001)	
OppFull x Size of attrib. bias		-0.002*	
		(0.001)	
Attrib. bias			-0.079***
			(0.020)
OppFull x Attrib. bias			-0.043
			(0.024)
OppFull	0.101***	0.074***	0.057***
	(0.015)	(0.015)	(0.013)
US	-0.057***		-0.089***
	(0.017)		(0.011)
OppFull x US	-0.058**		
	(0.021)		
Right-wing		-0.040	-0.048***
		(0.020)	(0.012)
OppFull x Right-wing		-0.031	Constant
0.086***	0.121***	0.146***	
	(0.009)	(0.013)	(0.018)
Worker-pair fixed effects	Yes	Yes	Yes
Observations	5078	5078	5078

NOTE: In Panel A we use Outcome Luck as our baseline treatment and test for the difference to the Unequal Opportunities with Full Information treatment. In Panel B and C we instead use the Unequal Opportunities with Limited information as our baseline and test for differences to the Unequal Opportunities with Full Information treatment. When controlling for the size of the attribution bias we control for the absolute difference in the beliefs regarding the number of tasks completed by the two workers under equal opportunities. When controlling for the attribution bias we follow the pre-plan and deviate from the definition in the main text, and consider spectators as attribution biased when the absolute difference in beliefs is larger than 1 (as compared 2 in our main results). The demographic controls included in Panel A column (4) and Panel C column (3) include dummies for US, right-wing, above median age, female gender, higher education as well as controls for the rank of the worker pair. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses.

D Appendix: Instructions

D.1 Worker Instructions

[Screen 1]

INTRODUCTION

- The results from this experiment will be used in a research project. It is therefore important that you carefully read and follow all instructions. Note that you will remain anonymous throughout the experiment. We will only use your Prolific ID to assign final payments and check that you have not participated in this experiment before.
- You will be paid a fixed participation fee of **$10** upon successfully completing the experiment. You may, depending on the actions you and others take, receive additional income. You are only allowed to participate in this study once.
- The tasks in this experiment require manual completion and it is, therefore, important you do NOT take part in other studies at the same time. There will be attention checks, and you will not be able to continue with the study if you fail these checks.
- The experiment has three parts. You will be given detailed instructions on your screen before each part of the experiment. Please read all instructions carefully.
- If you have any questions regarding this experiment, you may contact nielsenh@hu-berlin.de.

I have read and understood the above and want to participate in this study: [Yes]/[No]

I have understood that the study requires continuous participation over the next 65 minutes and I am available to participate: [Yes/No]

I have understood that it is important I do not work on other studies while participating in this study: [Yes/No]

To begin the first part, click "Next". [Next]

[Screen 2]

Part 1

In this part, you are asked to work on a counting task or to do a non-work activity

for **30** minutes. You decide how to divide your time between the work task and the non-work activity.

Counting task

- In the counting task, you are to count how many 1s there are in a series of tables with 1s and 0s. For example, if the table has digits 1 0 1 0 0 then the correct answer is 2; if instead, it has 1 1 1 0 0 1 then the correct answer is 4.
- You earn **$0.1 for each correct answer**. If your answer is incorrect, you will need to re-do the task.
- The size of the tables increases over time. For the first table, you need to count how many 1s there are among 24 digits. If you give the correct answer, the next table will have three more digits. This means that in the second table you need to count how many 1s there are among 27 digits and in the third table among 30 digits, and so on.

Non-work activity

- At any point during the **30** minutes, you can stop working on the counting task and go to the non-work activity instead for the remainder of the 30 minutes.
- You earn **$0.25 for every minute** in the non-work activity as long as you show you are still active in the assignment by completing attention checks.
- Please note that once you decide to stop working, you will NOT be able to return to the counting task.

[Screen 3]

Before you start the **30** minute period, you can now test both the work task and the non-work activity. You will not earn any money during this practice.

To practice, we ask you to complete two counting tasks and spend one minute on the non-work activity. After finishing the two counting tasks, click "STOP WORKING" to go to the non-work activity.

How many 1s are there in the table? [Answer]

"STOP WORKING" will be available once you successfully complete two counting tasks. [Tables]

[Screen 4]

If you have chosen to stop working you earn **$0.25 for every remaining minute** as long as you show you are still active in the assignment.

To do this, you need to click a button that pops up roughly once every 60 seconds and remains on the screen for 5 seconds. If you fail to click a button before it disappears, you will receive a warning message.

[Screen 5]

Please answer three questions to test that you have understood the instructions.

Question 1: If you correctly count 10 tables in one minute and you earn **$ 0.1** for each correct answer, then what are your total earnings for that minute?

- $ 10
- $ 1
- $ 0.5
- $ 0.1

[If incorrect answer →"**Incorrect**: the correct answer is B. Your earnings for that minute is equal to the earnings per table times the number of correctly completed tables in that minute, namely **0.1*10=1**."]

Question 2: True or False: If you think that you will earn **$ 0.1** in the next minute by working on the counting task and you earn **$ 0.25** per minute in the non-work activity, then your earnings in the next minute will be higher if you stop working.

- True
- False

[If incorrect answer →"**Incorrect**: the correct answer is True. **$ 0.25** per minute is more than **$ 0.1** per minute."]

Question 3: True or False: If you choose to stop working, then you cannot go back to the counting task.

- True
- False

[If incorrect answer →"**Incorrect**: the correct answer is True. You cannot go back to the counting task if you have gone to the non-work activity."]

Question 4: True or False: If you click "STOP WORKING" and go to the non-work activity you will **NOT** receive any further earnings.

- True

- False

[If incorrect answer →"**Incorrect**: the correct answer is False. You earn a per minute payment after you click "STOP WORKING" and start the non-work activity."]

[Screen 6]

[On screen: Timer + Table with Tasks/Earnings per Task/Total/Earnings in the last minute/Earnings per minute in the non-work activity]

[Table]

How many 1s are there in the table? [Answer]

[Button: STOP WORKING]

[If stop working →New screen with button appearing + Timer + Table with Earnings per Minute/Total]

[Screen 7]

You have now completed the first part of the experiment.

Click "NEXT" to move to the second part of the experiment.

[NEXT]

[Screen 8 - Unequal Opportunities treatment with high piece-rate]

Part 2

In this part, you are again asked to work on a counting task or to do non-work activity for **30** minutes. You decide how to divide the time between the work task and the non-work activity.

Counting task

- The work task is the same as in Part 1.
- The only difference in this part is the earnings for the counting task. You now earn **$ 0.6 for each correct answer**. The counting task and the difficulty level also remain the same.

Non-work activity

- The non-work activity is the same as in Part 1, and, as before, you earn **$ 0.25 for every minute** if you pass simple attention checks.

[START PART 2]

[Screen 8 - Unequal Opportunities treatment with low piece-rate]

Part 2

In this part, you are again asked to work on a counting task or to do non-work activity for **30** minutes. You decide how to divide the time between the work task and the non-work activity.

Counting task

- The work task is the same as in Part 1.
- You now earn **$ 0.1 for each correct answer.**. The counting task and the difficulty level also remain the same.

Non-work activity

- The non-work activity is the same as in Part 1, and, as before, you earn **$ 0.25 for every minute** if you pass simple attention checks.

[START PART 2]

[Screen 8 - Outcome Luck treatment]

Part 2

In this part, you are again asked to work on a counting task or to do non-work activity for **30** minutes. You decide how to divide the time between the work task and the non-work activity.

Counting task

- The work task is the same as in Part 1.
- You now earn **$ 0.1 for each correct answer.**. The counting task and the difficulty level also remain the same.

Non-work activity

- The non-work activity is the same as in Part 1, and, as before, you earn **$ 0.25 for every minute** if you pass simple attention checks.

[START PART 2]

[Screen 9]

[On screen: Timer + Table with Tasks/Earnings per Task/Total/Earnings in the last minute/Earnings per minute in the non-work activity]

[Table]

How many 1s are there in the table? [Answer]

[Button: STOP WORKING]

[If stop working →New screen with button appearing + Timer + Table with Earnings per Minute/Total]

[Screen 10 - Unequal Opportunities treatment]

Determination of payments

- You have now completed your work on both parts. We will now explain how you will be paid for each part.
- **Part 1**: You will be paid what you earned in this part.
- **Part 2**: After you have completed this study, we will match you with another participant in the experiment. The payment to you and the other participant from this part will depend on the choices of a randomly selected third person.
- This third person will be informed about how much you and the other participant have earned in the second part and given the opportunity to redistribute the earnings between the two of you. They will not know your identity or that of the other participant. If the third person chooses not to redistribute, each of you will be paid your earnings from this part.
- Your fixed participation fee of **$ 10** and the payment from the first part will be paid separately from your payment from the second part.

Thank you for your participation. Below you find the link to auto-redirect you back to Prolific.

[Screen 10 - Outcome Luck treatment]

Determination of payments

- You have now completed your work on both parts. We will now explain how you will be paid for each part.
- **Part 1**: You will be paid what you earned in this part.
- **Part 2**: After you have completed this study, we will match you with another participant in the experiment. The payment to you and the other participant from this part will be affected by a random draw, and the choices of a randomly selected third person.
- This third person will be informed about how much you and the other participant have earned in the second part and the outcome of the random draw.

Then they will be given the opportunity to redistribute the earnings between the two of you. They will not know your identity or that of the other participant. If the third person chooses not to redistribute, each of you will be paid your earnings from this part.

- Your fixed participation fee of **$ 10** and the payment from the first part will be paid separately from your payment from the second part.

Thank you for your participation. Below you find the link to auto-redirect you back to Prolific.

D.2 Spectator Instructions (English)

Sections highlighted in yellow differ between treatments. The translated instructions in Danish, Norwegian and Swedish are available upon request.

D.2.1 Unequal Opportunities (with Full Information) treatment

[Screen 0]

The results from this study will be used in a research project. It is therefore important that you carefully read and follow all instructions. Note that you will remain anonymous throughout the experiment.

You will receive a participation fee for your participation in this research project. Depending on your answers to some questions, you may receive an additional payment equivalent to 2 USD, which will be provided to you by your panel provider once the study is closed. Please note that it might take 4-6 weeks after the study completion for these additional payments to arrive in your account.

We will ask you to make decisions that may have real consequences for the payment other individuals receive.

If you have any questions regarding this study, you may contact your panel provider.

I have read and understood the above and wish to participate in the study: [YES/NO]

[Screen 1]

Recently, our research team recruited two individuals from a job portal to take part in a study. Let us call them Worker A and Worker B. Both workers were paid a participation fee, but could also receive payments from an assignment.

The two workers were asked to work on an assignment that consisted of completing small tasks on a website. A worker earned a **piece-rate** of either $0.6 or $0.1 for

each completed task. A worker could **work for a period of 30 minutes**, but could at any time choose to **stop working** and just stay on the study website for the remaining time. If they chose to stop working, they would receive a compensation of $0.25 per minute of the remaining time.

The tasks became increasingly difficult over time, and workers typically stopped working when they earned less per minute from completing tasks than they received in compensation for just staying on the website.

At the end of the study, the workers were informed that they would be matched with a third party. They were told that this third party would be given the opportunity to redistribute income and that the decision of this third party would determine how much each worker is paid for the study.

[Screen 2]

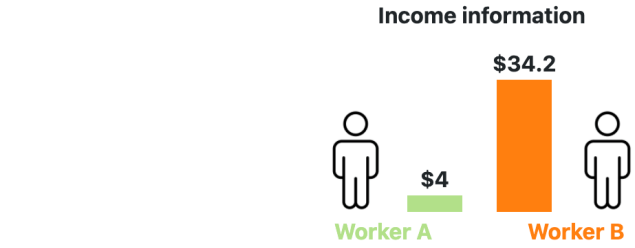
Worker A was **randomly** assigned a **low piece-rate** per task ($0.1) and Worker B was **randomly** assigned a **high piece-rate** per task ($0.6). The two workers were informed of their own piece-rate **before** they started working, but were not aware of how much other workers earned per task.

We will give you the **opportunity to redistribute income between Worker A and Worker B**. Your decision is completely anonymous. You and 14 other third-party participants will each make a decision for these two workers. We will randomly select one of the 15 decisions and implement it. This means that there is a 1/15 probability that Worker A and Worker B will be paid according to **your decision**. The workers will be paid within the coming days, and will not receive any further information.

Before you make your decision, you will be provided with information about the income of the two workers.

[Screen 3]

Worker A had an income of [Worker A earnings] and Worker B had an income of [Worker B earnings]. Each worker's **income** is the sum of earnings for completing tasks and compensation for the time they just stayed on the study website without working. Earnings and compensation are rounded to the nearest whole number.



	Worker A	Worker B
Piece-rate	$0.1	$0.6
Number of completed tasks	35	57
Time worked (out of 30 min)	28	30

Figure D.1: Example of basic worker information

Explanation of the figure: the figure shows that Worker A received a piece-rate of $0.1 and Worker B received a piece-rate of $0.6. Further, it shows that Worker A completed [Worker A completed tasks] tasks and Worker B completed [Worker B completed tasks] tasks; and that Worker A worked for [Worker A minutes worked] minutes and Worker B worked for [Worker B minutes worked] minutes. As a result: Worker A earned $[Worker A earnings from tasks] for completing tasks and received a compensation $[Worker A compensation for not working] for just staying on the website; Worker B earned $[Worker B earnings from tasks] for completing tasks and received a compensation $[Worker B compensation or not working] for just staying on the website.

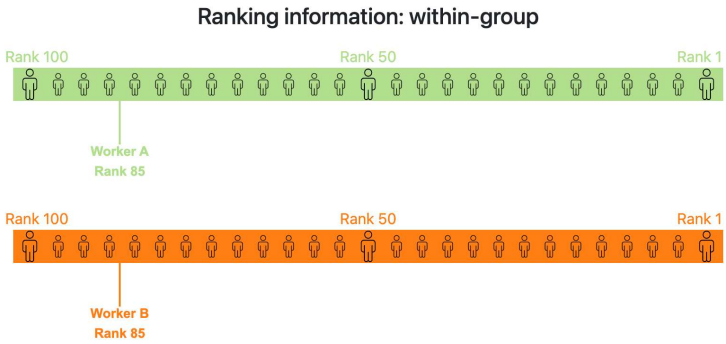


Figure D.2: Example of within-group ranking information

Explanation of the figure: the figure shows the two workers' **rankings compared to other workers with the same piece-rate** in terms of completed tasks. The green, upper part of the figure shows that Worker A has Rank [Worker A ranking] among the 100 workers with the low piece-rate ($0.1). The orange, lower part

of the figure shows that Worker B has Rank [Worker B ranking] among the 100 workers with the high piece-rate ($0.6). In other words, Worker A and Worker B have the same rank within the group of workers with the same piece-rate as themselves.

Interpretation: since Worker A and Worker B performed equally well compared to workers with the same piece-rate as themselves, ***we would expect Worker B to have completed the same number of tasks as Worker A if Worker B also had the low piece-rate.***

Evidence: this interpretation is in line with what we found in another study where we recruited the same type of workers for two assignments. In one of the assignments, we also randomly assigned workers a low ($0.1) or a high ($0.6) piece-rate. The piece-rate affected the number of tasks the workers completed. On average, the group of workers with the high piece-rate completed more tasks than the group of workers with the low piece-rate. However, in the study's other assignment we assigned **the same low piece-rate** to all workers, and then found that **the two groups performed equally well on average**.

Understanding questions

Please answer the following questions to check your understanding of the information regarding Worker A and Worker B.

1. How were the piece-rates assigned?
   (a) The piece-rates were assigned relative to the workers' productivity.
   (b) The piece-rates were randomly assigned.
2. When were workers informed of the piece-rate they were assigned to?
   (a) Workers were informed of the piece-rate before they started working.
   (b) Workers were informed of the piece-rate after they finished working.
3. How much did Worker B earn per task?
   (a) Worker B earned $0.1 per task.
   (b) Worker B earned $0.6 per task.
4. How much did Worker A earn for completing tasks?
   (a) Worker A earned $[Worker A earnings from tasks] for completing tasks.
   (b) Worker A earned $[Worker B earnings from tasks] for completing tasks.
5. How many minutes did Worker B <u>not</u> work (out of 30 minutes)?

(a) Worker B did not work for [Worker B minutes not worked] minutes.

(b) Worker B did not work for [Worker B minutes worked] minutes.

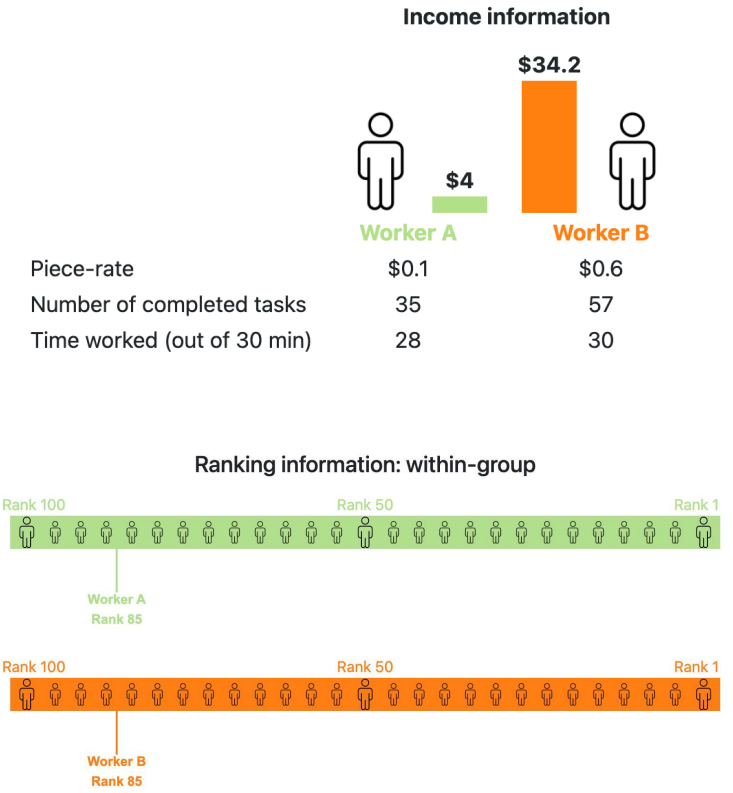
6. Do Worker A and Worker B rank in terms of completed tasks, the same or differently when compared to other workers with the same piece-rate?

   (a) Worker A and Worker B rank the same.

   (b) Worker A and Worker B rank differently.

[Screen 4]

You are now going to make your decision about how much Worker A and Worker B are paid. **Below we repeat the information about the two workers. Please review the information carefully before you make your decision.**



Interpretation: since Worker A and Worker B performed equally well compared to workers with the same piece-rate as themselves, ***we would expect Worker B to have completed the same number of tasks as Worker A if Worker B also had the low piece-rate.***

Evidence: this interpretation is in line with what we found in another study where we recruited the same type of workers for two assignments. In one of the assignments, we also randomly assigned workers a low ($0.1) or a high ($0.6) piece-rate. The piece-rate affected the number of tasks the workers completed. On average, the group of workers with the high piece-rate completed more tasks than the group of workers with the low piece-rate. However, in the study's other assignment we assigned **the same low piece-rate** to all workers, and then found that **the two groups performed equally well on average**.

Your decision

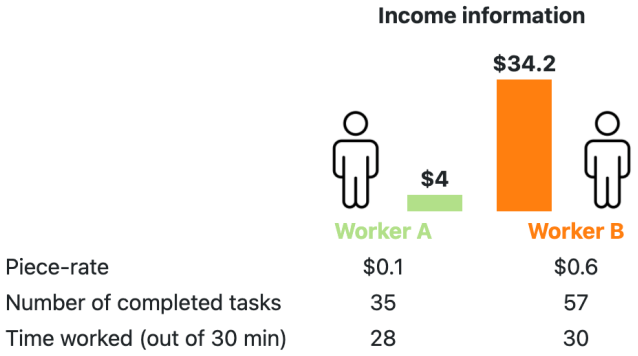
Remember that your decision **may determine how much Worker A and Worker B are paid.**

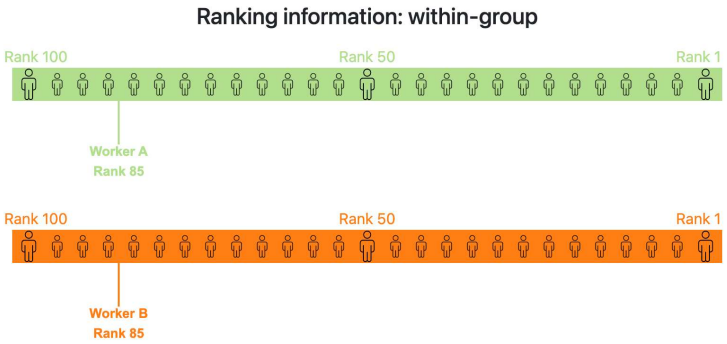
If you want to redistribute between Worker A and Worker B, move the slider below left or right. The **ends of the bar** show how much Worker A and Worker B are paid. Leave the slider at the position where it shows what you think Worker A and Worker B should be paid. If you do not want to redistribute, leave the slider at "Income".

To confirm your choice of **payments** ($X to Worker A and $Y to Workers B), click "CONFIRM".

[Screen 5]

We would now like to ask you to guess the number of tasks completed by Worker A and Worker B in another assignment. First, we remind you again of the income information.





Interpretation: since Worker A and Worker B performed equally well compared to workers with the same piece-rate as themselves, ***we would expect Worker B to have completed the same number of tasks as Worker A if Worker B also had the low piece-rate.***

Evidence: this interpretation is in line with what we found in another study where we recruited the same type of workers for two assignments. In one of the assignments, we also randomly assigned workers a low ($0.1) or a high ($0.6) piece-rate. The piece-rate affected the number of tasks the workers completed. On average, the group of workers with the high piece-rate completed more tasks than the group of workers with the low piece-rate. However, in the study's other assignment we assigned **the same low piece-rate** to all workers, and then found that **the two groups performed equally well on average**.

Guess completed tasks in another assignment

Before the assignment we described to you above, the two workers **participated in another assignment** for the same length of time (30 minutes), with the same type of tasks, and the same option to stop working ($0.25 per minute for the time they did not work and just stayed on the study website).

However, in this previous assignment, **Worker A and Worker B** were assigned the same piece-rate: they were both informed, before they started working, that they would earn the low piece-rate of $0.1 per completed task. The workers did not know about the assignment for which you made a decision when they worked on this previous assignment.

We now ask you to **guess the number of tasks completed** by Worker A and by Worker B in this previous assignment where **they earned the same piece-rate**. One of your two guesses will be randomly selected for payment. You will receive an additional payment of $2 if your guess on the selected question is not more than two tasks higher or lower than the worker's actual number of completed tasks.

Worker A completed [ ] tasks in the previous assignment.

Worker B completed [ ] tasks in the previous assignment.

D.2.2 Outcome Luck treatment

[Screen 0]

... as above ...

[Screen 1]

Recently, our research team recruited two individuals from a job portal to take part in a study. Let us call them Worker A and Worker B. Both workers were paid a participation fee, but could also receive payments from an assignment.

The two workers were asked to work on an assignment that consisted of completing small tasks on a website. Each worker earned a **piece-rate** of \$0.1 for **each completed task**. The workers were informed of the piece-rate **before** they started working. A worker could **work for a period of 30 minutes**, but could at any time choose to **stop working** and just stay on the study website for the remaining time. If they chose to stop working, they would receive a compensation of \$0.25 per minute of the remaining time.

The tasks became increasingly difficult over time, and workers typically stopped working when they earned less per minute from completing tasks than they received in compensation for just staying on the website.

At the end of the study, the workers were told that their income from the assignment would be affected by a random draw. The random draw could give each worker an increase in income. They were also informed that they would be matched with a third party. They were told that this third party would be given the opportunity to redistribute income and that the decision of this third party would determine how much each worker is paid for the study.

[Screen 2]

From the random draw, Worker A was **randomly** assigned **no increase in income**, and Worker B was **randomly** assigned **an increase in income** by \$[Worker B lottery payment]. The workers were informed of the random increase in income **after** they had finished the assignment.

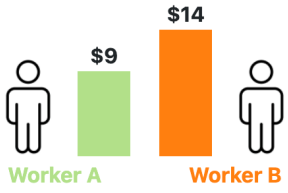
We will give you the **opportunity to redistribute income between Worker A and Worker B**. Your decision is completely anonymous. You and 14 other third-party participants will each make a decision for these two workers. We will randomly select one of the 15 decisions and implement it. This means that there

is a 1/15 probability that Worker A and Worker B will be paid according to **your decision**. The workers will be paid within the coming days, and will not receive any further information.

Before you make your decision, you will be provided with information about the income of the two workers.

[Screen 3]

Worker A had an income of [Worker A earnings] and Worker B had an income of [Worker B earnings]. Each worker's **income** is the sum of earnings for completing tasks and compensation for the time they just stayed on the study website without working. Earnings and compensation are rounded to the nearest whole number.



	Worker A	Worker B
Piece-rate	$0.1	$0.1
Number of completed tasks	26	26
Time worker (out of 30 min)	7	7
Income without random income	$9	$9
Random Income	$0	$5

Figure D.3: Example of basic worker information

Explanation of the figure: the figure shows that Worker A received a piece-rate of $0.1 and Worker B received a piece-rate of $0.6. Further, it shows that Worker A completed [Worker A completed tasks] tasks and Worker B completed [Worker B completed tasks] tasks; and that Worker A worked for [Worker A minutes worked] minutes and Worker B worked for [Worker B minutes worked] minutes. As a result: Worker A earned $[Worker A earnings from tasks] for completing tasks, received a compensation $[Worker A compensation for not working] for just staying on the website, and received $0 from the random draw; Worker B earned $[Worker B earnings from tasks] for completing tasks, received a compensation $[Worker B compensation for not working] for just staying on the website, and received $[Worker B lottery payment] from the random draw.

[Image 2]

Explanation of the figure: the figure shows the two workers' **rankings among the 200 workers who participated** in the study in terms of completed tasks.

The figure shows that Worker A has Rank [Worker A] and Worker B has Rank [Worker B].

Understanding questions

Please answer the following questions to check your understanding of the information regarding Worker A and Worker B.

1. How were the incomes from the random draw assigned?
   (a) The incomes from the random draw were assigned relative to the workers' productivity.
   (b) The incomes from the random draw were randomly assigned.
2. What was the income Worker B received from the random draw?
   (a) Worker B received $[Worker A lottery payment].
   (b) Worker B received $[Worker B lottery payment].
3. How much did Worker A earn for completing tasks?
   (a) Worker A earned $[Worker A earnings from tasks] for completing tasks.
   (b) Worker A earned $[Worker A total earnings] for completing tasks.
4. How many minutes did Worker B <u>not</u> work (out of 30 minutes)?
   (a) Worker B did not work for [Worker B minutes not worked] minutes.
   (b) Worker B did not work for [Worker B minutes worked] minutes.
5. Does Worker A and Worker B have the same ranking in terms of completed tasks, compared to all workers?
   (a) Worker A and Worker B rank the same.
   (b) Worker A and Worker B rank differently.

[Screen 4]

You are now going to make your decision about how much Worker A and Worker B are paid. **Below we repeat the information about the two workers. Please review the information carefully before you make your decision.**

Piece-rate	$0.1	$0.1
Number of completed tasks	26	26
Time worker (out of 30 min)	7	7
Income without random income	$9	$9
Random Income	$0	$5

[Image 2]

Your decision

Remember that your decision **may determine how much Worker A and Worker B are paid.**

If you want to redistribute between Worker A and Worker B, move the slider below left or right. The **ends of the bar** show how much Worker A and Worker B are paid. Leave the slider at the position where it shows what you think Worker A and Worker B should be paid. If you do not want to redistribute, leave the slider at "Income".

To confirm your choice of **payments** ($X to Worker A and $Y to Workers B), click "CONFIRM".

[Screen 5]

Like Worker A and Worker B, **two other individuals** also took part in our study. Let us call them **Worker C** and **Worker D**.

Worker C and Worker D completed the same assignment with the same tasks and were paid the same participation fee as Worker A and Worker B. Each worker could work for a period of 30 minutes and if they chose to stop working they would receive a compensation of $0.25 per minute for the remaining time.

However, unlike Worker A and Worker B, **Worker C and Worker D earned different piece-rates**. Worker C was **randomly** assigned a **low-piece rate** per task ($0.1) and Worker D was **randomly** assigned a **high-piece rate** per task ($0.6). Please see below for the two workers' income information.

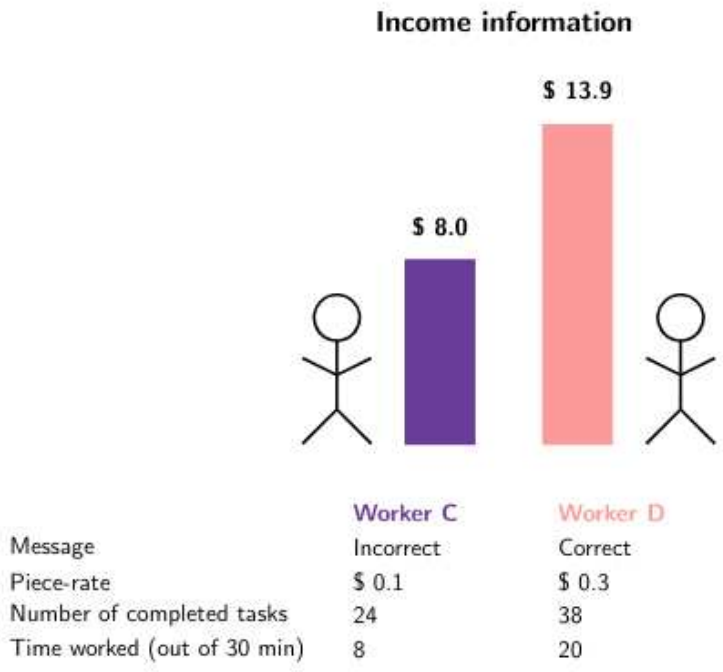


Figure D.4: Example of basic worker information: Worker C and D

Explanation of the figure: the figure shows that Worker C received a piece-rate of $0.1 and Worker D received a piece-rate of $0.6. Further, it shows that Worker C completed [Worker C completed tasks] tasks and Worker D completed [Worker D completed tasks] tasks; and that Worker C worked for [Worker C minutes worked] minutes and Worker D worked for [Worker D minutes worked] minutes. As a result: Worker C earned $[Worker C earnings from tasks] for completing tasks and received a compensation $[Worker C compensation for not working] for just staying on the website; Worker D earned $[Worker D earnings from tasks] for completing tasks and received a compensation $[Worker D compensation or not working] for just staying on the website.

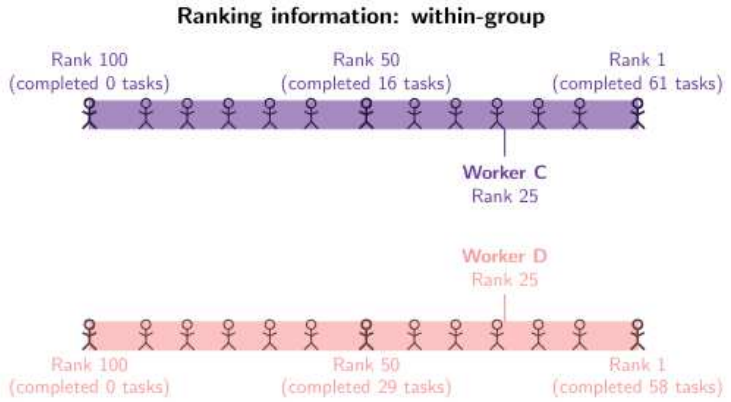


Figure D.5: Example of within-group ranking information: Worker C and D

Explanation of the figure: the figure shows the two workers' **rankings compared to other workers with the same piece-rate (\$0.1 or \$0.6)** in terms of completed tasks. The blue, upper part of the figure shows that Worker C has Rank [Worker C ranking] among the 100 workers with the low piece-rate (\$0.1). The yellow, lower part of the figure shows that Worker D has Rank [Worker D ranking] among the 100 workers with the high piece-rate (\$0.6). In other words, Worker C and Worker D have the same rank within the group of workers with the same piece-rate as themselves.

Interpretation: since Worker C and Worker D performed equally well compared to workers with the same piece-rate as themselves, ***we would expect Worker D to have completed the same number of tasks as Worker C if Worker D also had the low piece-rate.***

Evidence: this interpretation is in line with what we found in another study where we recruited the same type of workers for two assignments. In one of the assignments, we also randomly assigned workers a low (\$0.1) or a high (\$0.6) piece-rate. The piece-rate affected the number of tasks the workers completed. On average, the group of workers with the high piece-rate completed more tasks than the group of workers with the low piece-rate. However, in the study's other assignment we assigned **the same low piece-rate** to all workers, and then found that **the two groups performed equally well on average**.

Guess completed tasks in another assignment

Before the assignment we described to you above, Worker C and Worker D **partic-**

ipated in another assignment for the same length of time (30 minutes), with the same type of tasks, and the same option to stop working ($0.25 per minute for the time they did not work and just stayed on the study website).

However, in this previous assignment, **Worker C and Worker D** were assigned the same piece-rate: they were both informed, before they started working, that they would earn the low piece-rate of $0.1 per completed task. The workers did not know about the assignment for which you made a decision when they worked on this previous assignment.

We now ask you to **guess the number of tasks completed** by Worker C and by Worker D in this previous assignment where **they earned the same piece-rate**. One of your two guesses will be randomly selected for payment. You will receive an additional payment of $2 if your guess on the selected question is not more than two tasks higher or lower than the worker's actual number of completed tasks.

Worker C completed [ ] tasks in the previous assignment.

Worker D completed [ ] tasks in the previous assignment.

D.2.3 Unequal Opportunities (with Limited Information) treatment

[Screen 0]

... as above ...

[Screen 1]

Recently, our research team recruited two individuals from a job portal to take part in a study. Let us call them Worker A and Worker B. Both workers were paid a participation fee, but could also receive payments from an assignment.

The two workers were asked to work on an assignment that consisted of completing small tasks on a website. A worker earned a **piece-rate** of either $0.6 or $0.1 for **each completed task**. A worker could **work for a period of 30 minutes**, but could at any time choose to **stop working** and just stay on the study website for the remaining time. If they chose to stop working, they would receive a compensation of $0.25 per minute of the remaining time.

The tasks became increasingly difficult over time, and workers typically stopped working when they earned less per minute from completing tasks than they received in compensation for just staying on the website.

At the end of the study, the workers were informed that they would be matched with a third party. They were told that this third party would be given the

opportunity to redistribute income and that the decision of this third party would determine how much each worker is paid for the study.

[Screen 2]

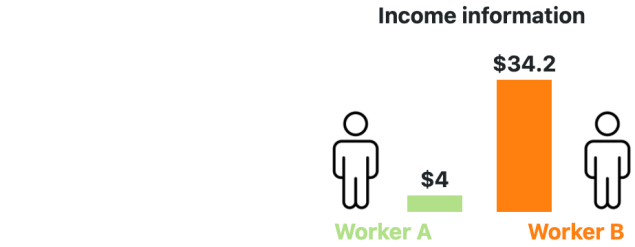
Worker A was **randomly** assigned a **low piece-rate** per task ($0.1) and Worker B was **randomly** assigned a **high piece-rate** per task ($0.6). The two workers were informed of their own piece-rate **before** they started working, but were not aware of how much other workers earned per task.

We will give you the **opportunity to redistribute income between Worker A and Worker B**. Your decision is completely anonymous. You and 14 other third-party participants will each make a decision for these two workers. We will randomly select one of the 15 decisions and implement it. This means that there is a 1/15 probability that Worker A and Worker B will be paid according to **your decision**. The workers will be paid within the coming days, and will not receive any further information.

Before you make your decision, you will be provided with information about the income of the two workers.

[Screen 3]

Worker A had an income of [Worker A earnings] and Worker B had an income of [Worker B earnings]. Each worker's **income** is the sum of earnings for completing tasks and compensation for the time they just stayed on the study website without working. Earnings and compensation are rounded to the nearest whole number.



	Worker A	Worker B
Piece-rate	$0.1	$0.6
Number of completed tasks	35	57
Time worked (out of 30 min)	28	30

Figure D.6: Example of basic worker information

Explanation of the figure: the figure shows that Worker A received a piece-rate of $0.1 and Worker B received a piece-rate of $0.6. Further, it shows that Worker A

completed [Worker A completed tasks] tasks and Worker B completed [Worker B completed tasks] tasks; and that Worker A worked for [Worker A minutes worked] minutes and Worker B worked for [Worker B minutes worked] minutes. As a result: Worker A earned $[Worker A earnings from tasks] for completing tasks and received a compensation $[Worker A compensation for not working] for just staying on the website; Worker B earned $[Worker B earnings from tasks] for completing tasks and received a compensation $[Worker B compensation or not working] for just staying on the website.

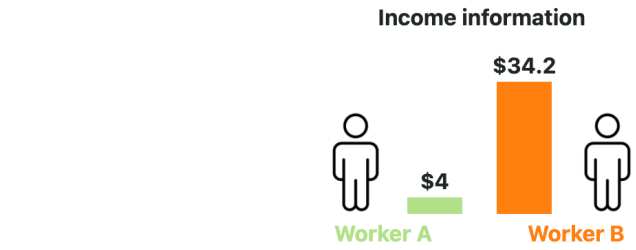
Understanding questions

Please answer the following questions to check your understanding of the information regarding Worker A and Worker B.

1. How were the piece-rates assigned?
   (a) The piece-rates were assigned relative to the workers' productivity.
   (b) The piece-rates were randomly assigned.
2. When were workers informed of the piece-rate they were assigned to?
   (a) Workers were informed of the piece-rate before they started working.
   (b) Workers were informed of the piece-rate after they finished working.
3. How much did Worker B earn per task?
   (a) Worker B earned $0.1 per task.
   (b) Worker B earned $0.6 per task.
4. How much did Worker A earn for completing tasks?
   (a) Worker A earned $[Worker A earnings from tasks] for completing tasks.
   (b) Worker A earned $[Worker B earnings from tasks] for completing tasks.
5. How many minutes did Worker B <u>not</u> work (out of 30 minutes)?
   (a) Worker B did not work for [Worker B minutes not worked] minutes.
   (b) Worker B did not work for [Worker B minutes worked] minutes.

[Screen 4]

You are now going to make your decision about how much Worker A and Worker B are paid. **Below we repeat the information about the two workers. Please review the information carefully before you make your decision.**



	Worker A	Worker B
Piece-rate	$0.1	$0.6
Number of completed tasks	35	57
Time worked (out of 30 min)	28	30

Figure D.7: Example of basic worker information

Your decision

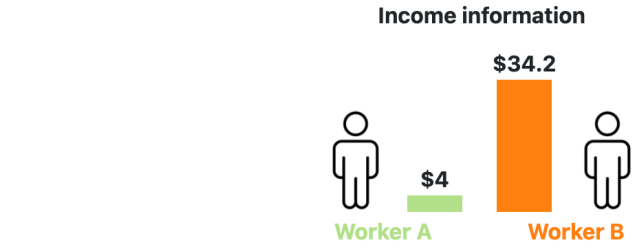
Remember that your decision **may determine how much Worker A and Worker B are paid.**

If you want to redistribute between Worker A and Worker B, move the slider below left or right. The **ends of the bar** show how much Worker A and Worker B are paid. Leave the slider at the position where it shows what you think Worker A and Worker B should be paid. If you do not want to redistribute, leave the slider at "Income".

To confirm your choice of **payments** ($X to Worker A and $Y to Workers B), click "CONFIRM".

[Screen 5]

We would now like to ask you to guess the number of tasks completed by Worker A and Worker B in another assignment. First, we remind you again of the income information.



	Worker A	Worker B
Piece-rate	$0.1	$0.6
Number of completed tasks	35	57
Time worked (out of 30 min)	28	30

Figure D.8: Example of basic worker information

Guess completed tasks in another assignment

Before the assignment we described to you above, the two workers **participated in another assignment** for the same length of time (30 minutes), with the same type of tasks, and the same option to stop working ($0.25 per minute for the time they did not work and just stayed on the study website).

However, in this previous assignment, **Worker A and Worker B** were assigned the same piece-rate: they were both informed, before they started working, that they would earn the low piece-rate of $0.1 per completed task. The workers did not know about the assignment for which you made a decision when they worked on this previous assignment.

We now ask you to **guess the number of tasks completed** by Worker A and by Worker B in this previous assignment where **they earned the same piece-rate**. One of your two guesses will be randomly selected for payment. You will receive an additional payment of $2 if your guess on the selected question is not more than two tasks higher or lower than the worker's actual number of completed tasks.

Worker A completed [ ] tasks in the previous assignment.

Worker B completed [ ] tasks in the previous assignment.

D.2.4 Questionnaire

[Shown to all spectators at the end, with some variation in option targeted to the specific country (see Table A.1). None of the questions are forced-response.]

1. What is the highest level of education you have completed?
    - Some high school

- High school diploma or equivalent
- Some college credits, but no degree
- Associate degree
- Bachelor's degree
- Master's degree
- Doctorate
- Other
- Prefer not to say

2. What was your income in the previous year before taxes?
    - $0 - $10,000
    - $10,001 - $20,000
    - $20,001 - $30,000
    - $30,001 - $40,000
    - $40,001 - $50,000
    - $50,001 - $60,000
    - $60,001 - $70,000
    - $70,001 - $80,000
    - $80,001 - $90,000
    - $90,001 - $100,000
    - $100,001 - $110,000
    - $110,001 - $120,000
    - $120,001 - $130,000
    - $130,001 - $140,000
    - $140,001 - $150,000
    - $150,001 or above
    - Prefer not to say
3. Which political party did you vote for in the last election?

- The Republican Party
- The Democratic Party
- Another party
- I do not have the right to vote
- I did not vote
- Prefer not to say

4. How old are you? _____
5. What is your gender?
   - Male
   - Female
   - Transgender
   - Other
   - Prefer not to say
6. In which state do you live? [Options: Drop down of states in the U.S. + Prefer not to say]
7. Please indicate to what extent you agree with the following statements:
   1 means that you agree completely with the statement, 'A society should aim to equalize incomes,' 10 means that you agree completely with the statement, 'A society should not aim to equalize incomes,' and the numbers in between indicate the extent to which you agree or disagree with the statements. [Options: 1 to 10]