Merit in a Society of Unequal Opportunities: Pre-Analysis Plan

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1 Introduction

Recent evidence shows that many individuals across societies hold meritocratic fairness views: they find inequalities fair when generated by effort and ability but unfair when determined by luck (Cappelen et al., 2007; Almås et al., 2020, 2021). However, a fundamental assumption in meritocratic reasoning is that people have equal opportunities (Daniels, 1978). Yet, this assumption is often violated in reality. For example, (Chetty et al., 2014) show that in the U.S., an individual's income, college attendance, and other life outcomes all heavily depend on their parents' income. Drawing on these observations, we ask how individuals, particularly meritocrats, redistribute income when the assumption of equality of opportunity fails.

Unequal opportunities pose an interesting dilemma for meritocrats. We define unequal opportunities as a type of luck, the *opportunity luck*, i.e., an *ex ante* randomness that affects an individual's marginal benefit from exerting effort. Unequal opportunities imply that individuals with superior opportunities are incentivized to work harder than those with inferior opportunities. Because opportunities are randomly assigned and outside of individuals' control, we would expect meritocrats to find inequality due to unequal opportunities unfair. However, they may simultaneously wish to reward effort per se, even though the high effort is an outcome of their unequal opportunities.

In our first research question, we ask whether or not individuals treat unequal opportunities the same as *outcome luck*, i.e., luck that directly affects individuals' outcomes without influencing their choices. Previous studies find that when inequality is driven by outcome luck, the majority of individuals in Norway and the U.S. are willing to redistribute to compensate for the unlucky worker (Almås et al., 2020). However, when luck is in the form of unequal opportunities, arguably as prevalent as outcome luck, if not more in modern societies, we do not yet have much evidence of how willing individuals are to redistribute.

We study this question experimentally in a Worker-Spectator framework. In our experiment, third-party individuals, so-called spectators, make real redistributive choices between two workers who have performed a real-effort task. We generate unequal opportunities by randomly assigning a high piece-rate or a

low piece-rate to workers for the real-effort task *ex ante*. We create the outcome luck by randomly giving an extra payment to some workers but not others *ex post*. We create a "twin" outcome-luck worker pair for each opportunity-luck worker pair to make the two pairs identical in income inequality through carefully calibrating the extra payment to the outcome-luck workers. We can study whether spectators are more reluctant to income redistribution when unequal opportunities drive inequality by keeping inequality the same.

Unequal opportunities may also affect an individual's redistributive decisions through the belief channel. When the incentive effect arising from unequal opportunities is not apparent, individuals may be subject to attribution bias. They may overly attribute the differences in outcomes between workers of superior opportunities and workers of inferior opportunities to differences in their productivity instead of differences in opportunities and thus overestimate the productivity of the superior-opportunities workers. This attribution bias implies that unequal opportunities further increase an individual's inequality acceptance because meritocrats also find inequalities generated by productivity fair.

In our second research question, we ask whether individuals over-estimate the productivity of the superior-opportunities workers and whether more biased beliefs lead to more inequality acceptance under unequal opportunities. Before the round in which we randomly assign different piece rates to different workers under unequal opportunities, workers in our experiment work on the same task for the same period with the same piece rate. Because workers are randomly assigned to the high and low piece-rate, the superior-opportunities workers and the inferior-opportunities workers' expected productions in this equal-opportunity round should be the same. We incentivize spectators to guess the equal-opportunity productions of the two workers she is matched with. The results are consistent with attribution bias if spectators believe that the superior-opportunities workers produce more.

To estimate the impact of the biased belief on inequality acceptance, we very the information that spectators receive regarding the productivity of the two workers. In our *full information* condition, we provide the within-group ranking information of the two workers. The *within-group ranking* is a worker's ranking among 100 workers with the same piece-rate in the unequal-opportunity round based on the number of problems solved. For each spectator, the corresponding two workers (one low piece-rate worker and one high piece-rate worker) always have the same within-group ranking, implying they have the same expected productivity. The only difference between the full information condition and the *limited information* condition is that this information is made available to the spectator in the full information condition but not in the limited information condition. We expected spectators to have more biased beliefs in favor of the superior opportunity worker and higher inequality acceptance in the limited information condition.

Lastly, we study the difference between Americans and Scandinavians when faced with unequal opportunities. Previous studies suggest that even though Americans and Scandinavians are pretty different in their fairness views, the most popular fairness view is the meritocratic view in both regions (Almås et al., 2020, 2021). However, as pointed out earlier, unequal opportunities create a novel trade-off for meritocrats between rewarding hard work and compensating

for bad luck. The interesting question is whether meritocrats in the U.S. and Scandinavia handle this trade-off similarly or in different ways? We recruit spectators from nationally representative samples in the U.S. and Scandinavia (Denmark, Norway, and Sweden) to conduct this comparison.

This pre-analysis plan outlines the research strategy, the design of our experiment(s), variables we plan to collect, hypotheses we plan to test, and the empirical strategy for the data analysis.

1.1 Main Research Questions

- Do individuals accept more inequality when inequality is driven by unequal opportunities than when it is driven by outcome luck?
- Do individuals over-estimate the abilities of those who have superior opportunities?
  - If so, do these biased beliefs lead to greater inequality acceptance?
- Heterogeneity:
  - **U.S. vs. Scandinavia**:
    - Are Americans more willing to accept inequality under unequal opportunities than Scandinavians?
    - Do unequal opportunities, compared to outcome luck, affect Americans more than Scandinavians in their redistribution decisions? Do biased beliefs under unequal opportunities affect Scandinavians more than Americans in their redistribution decisions?

2 Research Strategy

Our experiment consists of two types of participants, *workers* and *spectators*.

2.1 Recruitment of Workers

Workers are **recruited** using the international online data collection platform Prolific, a crowdsourcing web service which enables researchers to connect with participants across the globe. Subjects are recruited by posting a brief description on the website, including the time required and the estimated payment. Survey-takers on Prolific browse postings for which they are qualified and upon acceptance, subjects are redirected to our experiment platform where they participate in our study.

We impose two **requirements** for our subject pool: U.S. residents and 99%+ approval rating on previous assignments. We also recommend participating via a laptop or a tablet, and naturally only allow subjects to participate in the study once and only include subjects who complete the whole experiment.

We use a number of **exclusion criteria** and inform the workers of these before they accept the assignment:

- **Comprehension checks**: We ask four comprehension questions, wherefore the workers need to provide at least three correct answers. We also require the last question to be correct as it is of key importance.[1] All comprehension checks are placed at the start of the study.
- **Inactivity**: Subjects are directly excluded if they fail a total of two attention checks that are shown during the non-work activities of the experiment. Subjects are warned after the first failed attention check.
- **Repeat participation**: By requiring subjects to submit their unique "Prolific IDs" on our experiment platform, and matching these to the submissions on Prolific, we verify that workers only participate once in our study. For cases of suspected multiple participation, we reject and exclude all but the first participation.

2.2 Recruitment of Spectators

Spectators will be recruited from the general population, using a survey agency. In particular, we recruit individuals who are residents in either the United States or Scandinavia (Denmark, Norway or Sweden).

Naturally, spectators who do not finish the study or participate multiple times are excluded and not paid for their (second) participation. In addition, for our main sample we impose one **exclusion criteria**:

- **Quick decisions**: We exclude all spectators who complete either the redistribution decision or the performance guess screen in less than 10 seconds.

[1] The question is " True or False. If you click "STOP WORKING" and go to the non-work activity you will NOT receive any further earnings", with correct answer "False".

2.3 Sample Sizes

We plan to recruit 1000 spectators per spectator-worker treatment, for a total of 4000 spectators each from the U.S. and Scandinavia and an overall total of 8000.[2] In turn, for workers our final samples will contain 67 pairs of workers per spectator-worker treatment, for a total of 536 pairs and an overall total of 1072 workers. This implies a 1 in 15 probability of a spectator's choice being implemented in all treatments.

3 Design

The experiment consists of two phases. In the **Worker phase** workers perform simple real-effort tasks and accumulate income; in the **Spectator phase** spectators make real redistribution decisions over the income of a pair of workers. Additionally, we elicit the beliefs of spectators in the Spectator phase regarding the performance of the two workers in a counterfactual situation with equal opportunities.

We employ three treatments in total. First, we vary the type of luck workers face, outcome luck or unequal opportunities, and study spectators' redistribution decisions. Second, we vary the amount of information provided regarding worker performance under unequal opportunities, and test whether attribution bias exists and how it affects inequality acceptance.

3.1 Worker Phase

3.1.1 Experiment Flow

Workers perform a simple **real-effort task** based on the counting zeros task in Abeler et al. (2011). We ask subjects to count the number of ones in tables of zeros and ones where the table increases in size.[3] In return, workers are paid a show-up fee of \$10 as well as a piece-rate payment for each correctly completed problem, with the latter subject to redistribution decisions of spectators. Workers know that there is a third person who could decide how much they earn when they start working, but they do not know who is this third person or how this third person can affect their earrings.

The working phase is split into two **rounds**. In Round 1, all workers face equal opportunities and in Round 2 with inequality of opportunity. Each round lasts 30 minutes. There are two versions of the real-effort task: the *high-pay* version has a piece-rate of \$0.6 while the *low-pay* version only yields \$0.1 in earnings per correct problem. In the first round, all workers are paid the low piece-rate, \$0.1. In the second round, workers are randomized into one of two piece rates with equal probability.

[2]For each of the OppLimited and OutLuck 1000-spectator treatments, we match a 1000-worker OppFull treatment to it for comparison. Therefore, we have 2000 spectators in both OppLimited and OutLuck treatments and 4000 spectators in the OppFull treatment.

[3]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 matrix, they need to re-do the task. For each digit in a matrix there is an equal probability of it taking the value of zero and of one.

At any point during the work phase, workers can switch to a **non-work activity** which pays \$0.25 per minute. In this activity, workers simply have to indicate that they are still active in the study by responding to attention checks, i.e. by clicking a button which appears on screen roughly once every 60 seconds and remains on screen for 5 seconds. Once subjects switch to the non-work activity, they cannot go back to the counting task.

The idea behind the attention check is that we do not want workers to conduct other activities in the non-work mode. If we allowed them to browse the internet or read a magazine during the non-work activity, then different people would derive different amount of leisure utility from the non-work activity. By forcing them to look at the screen and click buttons the whole time, we make the leisure utility homogeneous. Also, because the leisure activity is not enjoyable, we avoid the concern that even though the high piece-rate worker earns more money than the low piece-rate worker, the overall utility the two obtain is similar.

By providing the non-work activity and making the counting task increasingly difficult, we vary the attractiveness of the leisure option over time. Workers will at some point earn more by switching to the non-work activity. Assuming that the randomly assigned piece-rate does not impact effort costs, we expect workers with the high piece-rate to work for longer and to complete more tasks than workers with the low piece-rate.

3.1.2 unequal opportunities and Outcome Luck

We vary the type of luck in the worker phase:

- In the Unequal Opportunity condition, all is as described above: lucky workers are randomly assigned the high piece-rate and unlucky workers are randomly assigned the low piece-rate in the second round.
- In the Outcome Luck condition, all is identical with one exception: the two workers face the same opportunities (the low piece-rate) in the second round. We then randomly assign an extra payment to half of the workers after the second round: lucky workers receive an increase in earnings and unlucky workers do not.

3.2 Spectator Phase

3.2.1 Experiment Flow

The Spectator phase consists of four parts: information and understanding questions, a real redistribution choice, an incentivized belief elicitation, and a non-incenitivized survey.

First, spectators are matched to a pair of workers and receive **information** on the general setup of the second working round, including piece-rates, time available, and the non-work activity. In addition, using a combination of text and simple graphical illustrations, as seen in the example in Appendix A Figure 1, spectators are informed about the workers' total earnings, number of tasks completed and time spent working. Depending on the treatment, the spectators may also receive further information. The spectators are <u>not</u> informed of the

existence of the first working round. To verify that the spectators have understood the information we ask a set of understanding questions, and provide the correct answer in case of mistakes.

Second, spectators decide on whether to **redistribute** income between the lucky worker and the unlucky worker in their matched pair. They are aware of the fact that workers also expect a third person to affect how much they earn when they start working. For each worker pair, the amount available to redistribute is the total income of the two workers in the second working round.[4] Spectators make one allocation decision and each spectator's choice is implemented with a probability of 1/15. The spectators are informed that they will remain fully anonymous to workers and are paid a fixed rate ($3) for their participation.

Third, we elicit spectators' **beliefs** regarding the earnings of the two workers in an equal opportunity scenario. Spectators again receive general information about the scenario and are asked to guess the number of tasks completed by the two workers when they had the same opportunities in the first round. We randomly select one of the two guesses for payment and spectators receive an additional $2 if their guess is at most two tasks higher or lower than the actual performance of the worker in the first round for the selected question.

Fourth, spectators respond to a non-incentivized survey regarding their educational and income-levels, age, gender, region of residence, general view on redistribution and political orientation.

3.2.2 Matching Workers to Spectators

Because we are interested in people's fairness views when inequality is purely driven by unequal opportunities, we would like to keep the productivity dimension fixed between the two workers. This is difficult when workers have different opportunities. To level the playing field so productivity comparison can take place, we construct a metric called *within-group ranking*. It is a worker's ranking among 100 workers who have *the same piece-rate* based on the number of problems she solved in the second round. Then, we match each lucky worker with an unlucky worker who has the same within-group ranking. Because they perform the same compared to other workers of the same piece-rate, we expect them to perform the same if they had the same piece-rate. The within-group ranking information is provided to the spectators in some treatments.

Similarly, when we match workers in the outcome luck condition, we find two workers of the same second-round production, i.e. the number of tasks completed. Since they have the same opportunity in the second round, the same production implies that they have the same within-group ranking.

3.2.3 Treatments

There are three treatments: *Unequal Opportunities Full Information* (OppFull), *Outcome Luck* (OutLuck), and *Unequal Opportunities Limited Information* (OppLimited). We vary the type of luck between the OppFull treatment

[4]Income in the first round and the show-up fee are not subject to redistribution and paid out in full to workers directly.

and the OutLuck treatment. And we vary the information the spectators receive between the OppFull treatment and the OppLimited treatment.

Unequal Opportunities Full Information

In the OppFull treatment, we expand the basic information (including the number of tasks completed, time worked and earnings) by providing the within-group ranking information. Concretely, we use a graphical illustration to display this information (Figure 2). We then accompany the graph with texts to explain that having the same within-group ranking implies that the two workers have the same expected productivity.

In addition, to be sure that the spectator trusts this interpretation we also provide evidence that when workers (in another study with the same set-up) are asked to perform two assignments, one with equal and one with unequal opportunities, we find exactly this (Figure 3).

Outcome Luck

In the OutLuck treatment, we also provide a "within-group ranking" to be consistent with the OppFull 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. Because spectators are already presented with an equal opportunity scenario in this treatment, we do not elicit their beliefs regarding performance in the first round.

We intend to compare spectators' redistribution decisions in the OppFull treatment to the OutLuck treatment. Two features of our design ensure that the only difference between the two treatments is the cause of inequality, unequal opportunities versus luck. First, by providing the within-group ranking information, we let spectators in both treatments know that the two workers have the same expected productivity. Second, we calibrate the random payments to the lucky workers in the OutLuck treatment to create the same income inequality prior to redistribution. Specifically, for each worker pair in the OppFull treatment, we search for two workers in the OutLuck 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 OutLuck treatment who both earned \$6. Next, we calibrate the random payment to replicate the inequality among the OppFull pair. In the above example, we will randomly assign \$16 to one of the two OutLuck 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 OppFull treatment.

Unequal Opportunities Limited Information

In the *Limited Information* treatment, we provide the basic information of the two workers (1) without informing the spectators of the within-group rankings of

the two workers. Therefore, it becomes unclear how productive the two workers are.

Note: We do not test the combination of limited information and outcome luck, as presenting the productions when there is only outcome luck naturally provides full information on the productivity of the two workers.

Another notable feature of our design is that we compare the OppFull treatment to both the OutLuck treatment and the OppLimited treatment. To ensure that differences between treatments are not driven by differences in the worker sample facing spectators, we let OppFull and OutLuck share the same worker sample and, at the same time, let OppFull and OppLimited share the same worker sample. Therefore, we have twice as many spectators in the OppFull treatment as in the OutLuck and OppLimited treatments.

4 Hypotheses & Predictions

4.1 Outcome Variables

Inequality Acceptance

1. Share of individuals who equalize the final income of the two workers.

   Since the two workers have the same expected productivity in all treatments, any inequality in income before redistribution is purely driven by luck (unequal opportunities or outcome luck). Meritocrats who have full information would equalize incomes of the two workers in both treatments if they treat the two forms of luck the same.

2. Income share of the unlucky worker after redistribution.

   With unequal opportunities, the unlucky worker is the worker who is randomly assigned the low piece-rate; with outcome luck, the unlucky worker is the worker who receives no extra random payment.

Belief Bias

1. Share of spectators who guess that one worker completed at least two tasks more or less than the other.[5]

   Similar to inequality acceptance we first focus on the **level** of biased beliefs, by using the share of individuals who have non-Bayesian beliefs about the performance of the two workers in a counterfactual scenario. Spectators with Bayesian beliefs would, on average, believe that the two workers have the same productivity even though the high piece-rate worker produced more than the low piece-rate worker when facing unequal opportunities. However, attribution-biased spectators would attribute the differences in production to differences in productivity, and thus overestimate the productivity of the high piece-rate worker relative to the low piece-rate worker.

[5]Note: We choose a margin of two tasks, because spectators' guesses are considered correct if they are at most two tasks away from the the true answer.

2. As our measure of the **degree** of belief bias, we use the absolute distance between the guessed performance of the lucky and the unlucky worker.

4.2 Classification of Fairness Views

First, we define a number of fairness types by comparing the level of implemented inequality across treatments:

Libertarians: *Individuals who find neither differences in merit, in outcome luck nor in opportunities unfair, never redistribute.*

Egalitarians: *Individuals who find differences in merit, in outcome luck and in opportunities all unfair, always equalize fully.*

Meritocrats: *Individuals who find differences in merit fair but differences in luck unfair, redistribute to compensate for luck.*

Unequal opportunities not only contains a luck component which impacts earnings directly and indirectly, but also changes the incentives individuals face and thereby their effort choices. It is thus unclear how meritocrats respond to inequality driven by unequal opportunities. In light of this we separate meritocrats into

- **Factual meritocrats**: *Individuals who consider all choices made by individuals morally relevant but all differences in outcome luck unfair, redistribute more when outcome luck generates inequality than when unequal opportunities does.*
- **Counterfactual meritocrats**: *Individuals who find both outcome luck and unequal opportunities unfair. When facing unequal opportunities, they construct the equality of opportunity counterfactual in their mind and redistribute based on workers' performances in that counterfactual.*

For *Counterfactual meritocrats* redistribution decisions will depend on whether they construct the correct equal-opportunity counterfactual. By comparing spectators' beliefs on the performance of the superior- and inferior-opportunity workers in a counterfactual scenario with equal opportunity, we can observe the counterfactual constructed in their mind. If they have biased beliefs favoring the worker with superior opportunity, then they will believe that the superior opportunity worker on average performs better than the inferior opportunity worker. Then they may also choose to redistribute less under unequal opportunities than under outcome luck.

4.3 Unequal Opportunities versus Outcome Luck

Hypothesis 1: *Individuals are not more or less willing to accept income inequality when it is a result of unequal opportunities than when it is due to outcome luck.*

To test H1, we focus on the *Unequal Opportunities Full Information* treatment and compare the inequality acceptance in it to the *Outcome Luck* treatment. As such, we keep both information and pre-redistribution inequality constant across treatments.

Egalitarians and libertarians do not distinguish between unequal opportunities and outcome luck. Therefore, we will not expect them to behave differently in the OppFull treatment and in the OutLuck treatment. Factual meritocrats will redistribute less in the OppFull treatment because they consider inequality driven by effort fair, even though the differences in effort are driven by differences in opportunities. Counterfactual meritocrats will also redistribute the same in the two treatments because the expected equal-opportunity productions of the two workers are the same in both treatments. To summarize, we expect to find lower level of redistribution in the OppFull treatment if there are factual meritocrats.

Prediction 1: We expect to find that spectators accept more inequality under unequal opportunities than under outcome luck.

4.4 Impact of Information on Biased Beliefs and Inequality Acceptance

To understand the impact of biased beliefs regarding productivity on inequality acceptance, we first test whether individuals have biased beliefs under unequal opportunities and whether belief biases exacerbate inequality acceptance. As such, we split our hypothesis in three parts:

- **Hypothesis 2a**: *Spectators are not biased in their beliefs on productivity of the workers, i.e. they do not predict the high piece-rate worker to produce more or less than the low piece-rate worker in a counterfactual scenario where opportunities are equal.*
- **Hypothesis 2b**: *Limited information does not increase or decrease inequality acceptance compared to limited information.*

To test H2a, we test whether beliefs on productivity are biased in the unequal opportunity treatment under *Limited Information*. To test H2b, we compare the inequality acceptance in the OppFull treatment to the OppLimited treatment. As such, we keep both the luck component and the pre-redistribution inequality constant.

When information on within-group relative performance is lacking, as is often the case in reality, bias in beliefs regarding the productivity of the two workers may kick in. Spectators in the limited information treatment must base their beliefs about the performance of the two workers under equal opportunities on performances under unequal opportunities. As high piece-rate workers have higher productions, attribution-biased spectators are more likely to have biased beliefs in the limited information treatment than in the full information treatment in which this uncertainty about the relative productivity of the two is reduced.

Prediction 2a: We predict that spectators have biased beliefs in favor of the superior opportunity workers. They believe that the high piece-rate worker completed more tasks than the low piece-rate worker when they faced the same piece-rate in Round 1.

We do not expect egalitarians and libertarians to react to the change in information. Importantly, the within-group ranking information is not relevant to

factual meritocrats either. For them, what happens in the equal-opportunity counterfactual does not matter. Only counterfactual meritocrats will react to this information. When the within-group ranking information is not available, we expect the counterfactual meritocrats to have a stronger bias in favor of the superior opportunity worker and be more accepting of inequality.

Prediction 2b: We predict that spectators acceptance more inequality under limited compared to under full information.

Exploratory analysis: Beyond the pre-specified hypotheses, we explore the correlation between the attribution bias in beliefs and the inequality acceptance. Here we expect to find that spectators who are more biased in beliefs are more likely to accept inequality under unequal opportunities.

4.4.1 Inequality acceptance with unbiased beliefs

In an ideal world, we would have a condition where bias in beliefs on the workers' productivity is wholly gone through sufficient information provision. We would then be able to identify the effect of unequal opportunities on people's fairness preference. However, we still expect some spectators to overestimate the productivity of the superior opportunity workers in the full information treatment due to attribution bias. To isolate the impact of preference change caused by unequal opportunities on inequality acceptance, we focus on a sub-sample: the group of spectators who are unbiased in their beliefs on the productivity of the two workers.

In particular, we define the unbiased sub-sample as spectators whose guesses of the productions of the two workers in the equal opportunity round differ by one task or less. This definition is clear enough in the unequal opportunities treatments. However, to enable the comparison between it and the Outcome Luck treatment on inequality acceptance, we will also need to identify the unbiased sub-sample in the OutLuck treatment. The problem is that Outcome Luck spectators already know that the two workers who faced the same opportunity completed the same number of tasks. To test whether their beliefs are biased or not, we instead ask them to guess two unequal opportunities workers' productions in Round 1. Therefore, Outcome Luck spectators first need to make a redistribution decision for two OutLuck workers and then guess productions of another two workers who participated in the unequal opportunities treatments.

By comparing the unbiased sub-sample in the two treatments, we can isolate the preference effect of unequal opportunities. However, there are potential downsides to this analysis. First, unbiased spectators may be different from those who are biased. What we conclude from this sub-sample may not generalize to the whole population. To partially alleviate this issue, we test whether the biased and unbiased spectators redistribute similarly or not in the Outcome Luck treatment. Suppose the two groups are similar in their fairness views. In that case, we should observe that their inequality acceptance levels are similar because beliefs about two unequal opportunities workers' productivity are not relevant to their redistribution decisions regarding two Outcome Luck workers. Second, the belief elicitation task is more demanding to Outcome Luck spectators. Whereas unequal opportunities spectators only need to state their beliefs about two workers upon whom they have decided, Outcome Luck spec-

tators need to learn about a new environment and two new workers when they make their guesses. The comparison between the two unbiased sub-samples only makes sense when the extra cognitive demand does not change the distribution of elicited beliefs. To summarize, the analysis of inequality acceptance with unbiased beliefs is exploratory. We will only conduct this analysis when the above two assumptions are supported by empirical evidence.

4.5 Heterogeneity

4.5.1 Heterogeneity across Countries

While previous research shows that there are similar shares of meritocrats in the U.S. and in Scandinavia (Almås et al., 2020), the way meritocrats treat the dilemma of rewarding effort and compensating for luck may depend on the type of luck. For example, meritocrats may redistribute similarly when inequality is driven by outcome luck but differently when it is driven by unequal opportunities.

To answer our research questions we formulate three hypotheses:

- **Hypothesis 3a:** *Individuals across the two groups accept the same level of inequality under unequal opportunities.*
- **Hypothesis 3b:** *The difference in inequality acceptance under unequal opportunities and outcome luck is not larger or smaller across the two groups.*
- **Hypothesis 3c:** *The difference in inequality acceptance with full and limited information under unequal opportunities is not larger or smaller across the two groups.*

To test H3a, we compare inequality acceptance across the two groups in the *unequal opportunities* treatment under *Full Information*. To test H3b, we focus on differences in treatment effects between the OppFull treatment and the Out-Luck treatment for the two groups. Finally, to test H3c, we focus on differences in treatment effects between the OppFull and the OppLimited treatment for the two groups.

Predictions 3a/3b/3c: We do not pre-specify any particular direction for these hypotheses, as depending on the composition of different types of fairness views we may find different results.

Exploratory analysis: Beyond the pre-speficied research questions and hypotheses which relate to heterogeneity in inequality acceptance, it may also be interesting to explore heterogeneity in level of and treatment effects on belief biases.

4.5.2 Heterogeneity depending on Political Orientation

To study heterogeneity in inequality acceptance depending on political orientation (right-wing versus others) we follow the same three hypotheses (Hypothesis 3a, 3b, and 3c) as for heterogeneity between the U.S. and Scandinavia and use the same comparisons to test the hypotheses.

Predictions: Also here we do not pre-specify any particular direction of our hypotheses, again due to potential compositional differences between the groups in terms of fairness views. Since we are likely under-powered to detect the the smallest difference of interest between the right-wing and the rest, this analysis is mainly exploratory.

4.5.3 Heterogeneity along the Performance Distribution

Moreover, we also study whether inequality acceptance under unequal opportunities affect all unlucky workers equally, or if workers at some part of the performance distribution are more impacted than others. For example, one can imagine that very high- or low-achieving workers are less affected by unequal opportunities as their lucky counterparts exert similar levels of effort. Our pilot results show that the largest difference in effort provision is among medium-achieving unlucky and lucky workers. Here our analysis is exploratory.

Specifically, we split the performance distribution, i.e. the ranking of workers based on the number of tasks they completed in the unequal opportunity round, into five equal groups, and compare the average level of inequality acceptance or belief biases in each of these groups between the treatments.

4.5.4 Heterogeneity across Personal Characteristics of Spectators

Finally, for our background characteristics we focus on gender, education and income. Here, we conduct exploratory analyses along the same dimensions as when comparing the United States and Scandinavia.

5 Empirical Strategy

Below we provide the key variable definitions, main analyses and OLS specifications we intend to use for our analysis.

5.1 Variable Definitions

Inequality Acceptance

1. Equalizing incomes of the two workers, i.e.:

$$e_{s,j} = I\left(\left|income_{U,j}^{post} - income_{L,j}^{post}\right| \leq 0.5\right)$$

   where e_s is an indicator taking value 1 if spectator s matched with worker pair j equalizes the incomes after redistribution ($income_{i,j}^{post}$) of the two workers $i \in \{L, U\}$, where L is the lucky worker and U the unlucky worker, and value 0 otherwise. We use a margin of \$0.5 since spectators can only redistribute in multiples of \$0.1 and thus cannot always exactly equalize. We will also check the robustness of our results to different margins ranging from \$0.1 to \$1.

2. Income share of the unlucky worker after redistribution, i.e.:

$$e_{s,j} = \frac{income_{U,j}^{post}}{income_{U,j}^{post} + income_{L,j}^{post}}$$

Belief Biases

1. Belief that one worker completed at least two tasks more or less than the other worker, i.e.:

$$b_{s,j} = I\left(|E_j(tasks_{L,j}) - E_j(tasks_{U,j})| \geq 2\right)$$

where b_s is an indicator taking value 1 if spectator s matched with worker pair j expects that the number of completed tasks ($E_j(tasks_{i,j})$) of the two workers $i \in \{L, U\}$, where L is the lucky worker and U the unlucky worker, are at most 1 task different, and value 0 otherwise.

2. Distance between the guessed performance of the lucky and the unlucky worker, i.e.:

$$b_{s,j} = |E_j(tasks_{L,j}) - E_j(tasks_{U,j})|$$

Survey Variables
For full variable definitions, see Appendix B.

- **Income**: Split sample in below and above median income in the respective country.
- **Education**: Split sample into two groups: With higher education (college or above) and without higher education.
- **Gender**: Split sample into male and female.
- **Age**: Split above and below median.
- **Political orientation**: Split sample into right- and not-right-wing.

5.2 Analysis Specifications

5.2.1 Inequality Acceptance

First, we test H1 by estimating the following regression equation:

$$e_{s \in FI,j} = \alpha + \alpha_{OP} OP_j + \epsilon_{s,j} \tag{1}$$

where $e_{s \in FI,j}$ is our inequality acceptance measure for spectator s in the OppFull treatment (FI) matched with worker pair j. OP_j is an indicator taking the value 1 if the worker pair is in the unequal opportunities treatments.

Second, to test hypothesis H2b, we keep the type of luck constant and let spectator treatments vary, by estimating:

$$e_{s,j \in OP} = \alpha + \alpha_{FI} FI_s + \epsilon_{s,j} \tag{2}$$

where $e_{s,j \in OP}$ is our inequality acceptance measure for spectator s matched with worker pair j in the unequal opportunities treatments (OP). FI_s is an indicator taking the value 1 if the spectator is in the full information treatment. In our exploratory analyses we expand this by including also the spectator's level of attribution bias ($b_{s,j}$) both directly in the regression as well as interaction with the treatment:

$$e_{s,j \in OP} = \alpha + \alpha_{FI} FI_s + \gamma b_{s,j} + \gamma_{FI} FI_s b_{s,j} + \epsilon_{s,j} \tag{3}$$

Third, to test H3a and H3c we expand Equation 2 by estimating:

$$e_{s,j\in OP} = \alpha + \alpha_{FI} FI_s + \beta G_s + \beta_{FI} G_s FI_s + \epsilon_{s,j} \tag{4}$$

where G_s is an indicator taking the value 1 if the spectator is in one of the two groups of interest for our heterogeneity analyses, i.e. if the spectator is from the U.S. or of a right-wing political orientation. The other group (Scandinavia or non-right-wing) is thereby our baseline. We test H3a by testing whether β is equal to 0 and we test H3c by testing whether β_{FI} is equal to 0. We test H3b by adding the interaction term $G_s OP_j$ to Equation (1).

5.2.2 Bias in Belief

To test the hypotheses for the level of belief biases (H2a), we perform a t-test for the average level of belief biases $\bar{b}_{s\in LI, j\in OP}$ in the OppLimited treatment, where LI stands for the OppLimited treatment. Specifically, we test if $\bar{b}_{s\in LI, j\in OP} = 0$.

5.3 Control Variables

To test the robustness of our results we include a set of control variables in our regressions: rank of the worker pair, gender, age, region of residence, educational attainment, political orientation.

5.4 Power Analysis

This sample size (1000 per treatment, 8000 in total) is sufficient to detect a 5 percentage point difference in share of spectators who redistribute fully between treatments with 80% power and significance level $p < 0.05$ for Hypotheses 1, Hypothesis 2a, 2b, and Hypotheses 3a. The standard deviations used in the power calculation are from the pilot results. The 5 percentage point difference is the smallest effect size of interest (SESOI).

More observations are needed to estimate the heterogeneous effects. To test whether there is an interaction effect between a spectator's region of residence (U.S. or Scandinavia) and unequal opportunities (H3b and H3c), our sample size (4000 per test) is only sufficient when the interaction effect is larger than the SESOI (for example, if the effect size is 0.8 instead of 0.5), or when the standard deviation of the interaction term is smaller than its standard deviation in the pilot (0.9). We provide the required sample size for different values of the effect size and the standard deviation in Table 2.

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A Appendix

Figure 1: Example of illustration shown to spectators

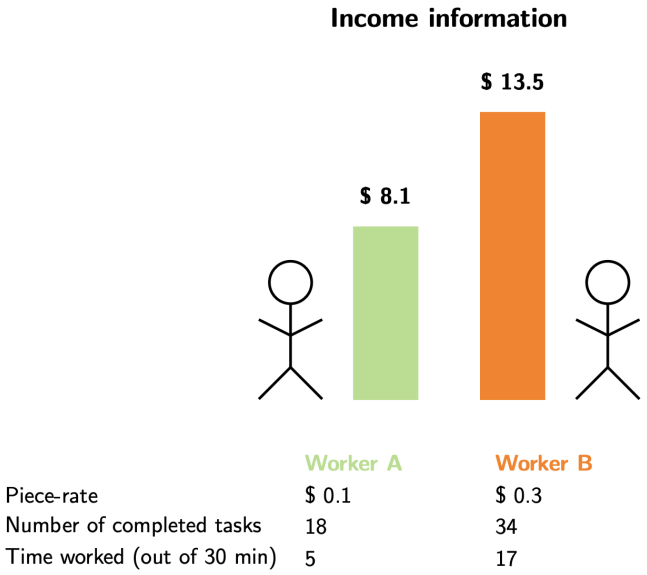

Figure 2: Example of within-group ranking information

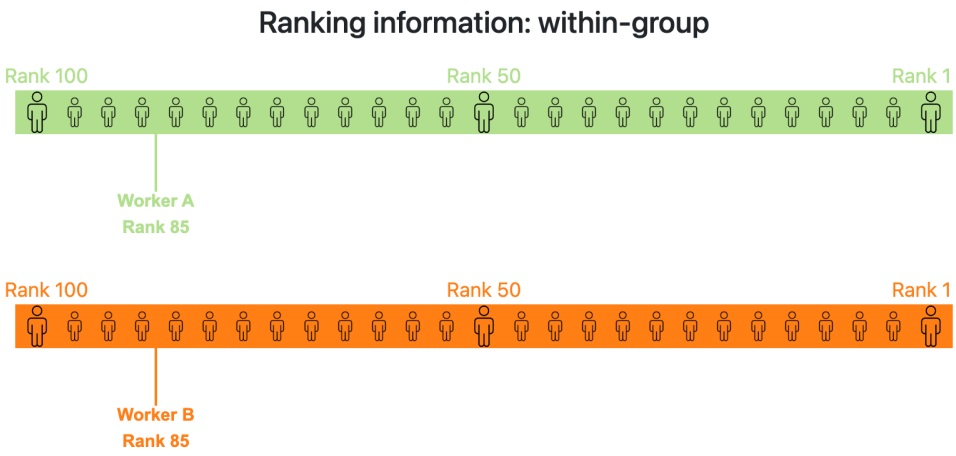

Figure 3: Example of within-group ranking explanation

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 85 among the 100 workers with the low piece-rate ($0.1). The orange, lower part of the figure shows that Worker B has Rank 85 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**.

B Appendix

Table 1: Definitions of variables collected in survey

Variable	United States	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		Högskole- eller universitetstudier, men ingen examen
	Associates degree	**Bacheloruddannelse**		**Kandidatexamen**
	College degree	**Masteruddannelse**		**Mastererexamen**
	Postgraduate degree	**Ph.D.-grad**		**Doktorsexamen**
	Doctorate	Andet		Annan
	Other			
	Prefer not to say	Foretrækker ikke at 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		Föredrar att inte svara
Political Affiliation (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)		Röstade inte
		Kristendemokraterne (K)		Föredrar att inte svara
		Annet		
		Har ikke stemmret		
		Stemte ikke		
		Fortrækker ikke at svare		

Table 2: Power Calculation

Alpha	Power	Totol N	Effect Size	Treatment Effect: U.S.	Treatment Effect: Scandinavia	sd
.05	.8	788	-.1	.44	.34	.5
.05	.8	1,134	-.1	.44	.34	.6
.05	.8	1,542	-.1	.44	.34	.7
.05	.8	2,012	-.1	.44	.34	.8
.05	.8	2,546	-.1	.44	.34	.9
.05	.8	972	-.09	.44	.35	.5
.05	.8	1,398	-.09	.44	.35	.6
.05	.8	1,902	-.09	.44	.35	.7
.05	.8	2,484	-.09	.44	.35	.8
.05	.8	3,142	-.09	.44	.35	.9
.05	.8	1,230	-.08	.44	.36	.5
.05	.8	1,768	-.08	.44	.36	.6
.05	.8	2,406	-.08	.44	.36	.7
.05	.8	3,142	-.08	.44	.36	.8
.05	.8	3,976	-.08	.44	.36	.9
.05	.8	1,604	-.07	.44	.37	.5
.05	.8	2,310	-.07	.44	.37	.6
.05	.8	3,142	-.07	.44	.37	.7
.05	.8	4,104	-.07	.44	.37	.8
.05	.8	5,192	-.07	.44	.37	.9
.05	.8	2,184	-.06	.44	.38	.5
.05	.8	3,142	-.06	.44	.38	.6
.05	.8	4,276	-.06	.44	.38	.7
.05	.8	5,584	-.06	.44	.38	.8
.05	.8	7,066	-.06	.44	.38	.9
.05	.8	3,142	-.05	.44	.39	.5
.05	.8	4,524	-.05	.44	.39	.6
.05	.8	6,156	-.05	.44	.39	.7
.05	.8	8,040	-.05	.44	.39	.8
.05	.8	10176	-.05	.44	.39	.9