1. Main Experiment

1.1 Design Overview

We use the paradigmatic spectator-worker design (ex. Almås, Cappelen, and Tungodden 2020) to create experimentally controlled situations of inequality between two workers. In our pre-registered design, each worker's task was to study a brief reading material passage for up to 5 minutes, then answer multiple-choice questions in a knowledge evaluation within another 5 minutes.[1] Workers were then randomly paired ex-post and assigned initial rewards in addition to a fixed base payment of $2. Across all treatments, the initial reward allocation is the same: one of the workers received $6, while the other worker received $0. The treatments differ in terms of the underlying source of the workers' reward and/or performance differential.

Another set of participants playing the role of the impartial third parties, which we refer to as spectators, was informed about the workers' scenario and initial reward allocation, and then given an opportunity to reallocate the rewards from the higher earning worker to the lower earning worker. One-third of spectators' decisions were randomly chosen to be implemented on the participants playing the role of workers. The primary variable of interest is the spectator's redistributive decision, which is interpreted as reflecting decision-makers' views about fairness concepts and preferences over redistribution.

1.2. Treatments

We implement four between-subjects treatments, which vary in terms of the source of inequality and/or performance differential between workers. We discuss the treatments in the following sections, organised by whether there was an opportunity differential imposed between workers. We also implement two additional treatments addressing spectators' desire to seek information about inequality, but we will defer discussion of them until Section 5.1.

1.2.1. (Benchmark) treatments with equal opportunities

In the two treatments, *Luck* and *Merit*, both workers in a pair first studied identical reading materials, then completed the same set of multiple-choice knowledge questions pertaining to the topic addressed in the reading materials. In the Luck treatment, the initial assignment of rewards between the two workers was fully determined by a lottery (i.e., one of them was randomly selected

---

[1] Prior worker tasks considered in the literature include word unscrambling (Almås, Cappelen, and Tungodden 2020) and other tasks of a routine manual nature (Konow 2000). For our research question of interest, it is essential that our worker task is knowledge or training-based, so that the concepts of opportunity we are interested in can be accurately represented to spectators.

to receive the entire initial reward, $6), independently of workers' performances on the knowledge questions. In the Merit treatment, the initial assignment was determined by the workers' performances on the knowledge questions (i.e., the worker who answered more questions correctly received the entire initial reward, $6).

To avoid spectators' decisions being affected by workers' expectations, workers were not told the lottery result (in the Luck treatment) or the result about their relative performance (in the Merit treatment).[2] Spectators were informed of the exact procedure of the assignment of initial rewards to workers. These two treatments have been previously implemented in Almås, Cappelen, and Tungodden (2020), and we replicate them here in the context of our worker task for the purpose of straightforward comparability with our novel unequal opportunity treatments.

1.2.2. Treatments with unequal opportunities

The Luck and Merit treatments, which serve as benchmarks for comparison to our other treatments, do not involve any unequal opportunities. However, in our main treatments, *Random-Education* and *Random-Employment*, we introduce the potential for unequal opportunities to affect workers' performance outcomes.

The Random-Education treatment differs from the Merit treatment in that workers randomly received different reading materials: one set of reading materials was highly relevant to the subsequent knowledge questions, containing all the information needed for the worker to answer all the knowledge questions correctly. By contrast, the other set of reading materials, about the exact same topic and of similar length, lacked several pieces (exactly 11 out of 15 pieces) of vital information for successfully completing the knowledge questions. Workers were not explicitly informed that there were two different versions of reading materials. The spectators, however, were informed about which of the two workers received the highly relevant reading materials, and that the worker with the higher number of correct answers in the evaluation received the entire initial reward, $6, while the other worker received $0. Since the answers to the knowledge evaluation were embedded in the relevant reading materials, and we informed workers that answers should be based on these materials, the worker who received the highly relevant reading materials was substantially advantaged. Therefore, we expect that workers who performed better on the evaluation should most likely be the one who received the highly relevant materials. We ex-post match each pair of workers, such that one worker who received the highly relevant materials and performed better, is paired with a worker who received the less relevant materials

---

[2] However, as tested in a robustness study by Almås, Cappelen, and Tungodden (2020), spectators' behaviour in these two treatments is not affected even when spectators are told that the workers have been informed about their initial earnings.

and performed worse.[3] In practice, our reading materials successfully generated the higher expected performance among workers who received the highly relevant materials (average score: 12.5 out of 15) while workers receiving the less relevant materials performed worse (average score: 7.9 out of 15). The Random-Education treatment is designed to partially represent unequal opportunities originating in access to varying qualities of education for individuals, which often arise from circumstances beyond an individual's personal control.[4]

The Random-Employment treatment represents another type of unequal opportunity. In this treatment, a pair of workers received the identical highly relevant reading materials. One randomly determined worker had access to the full set of 15 knowledge questions, while the other worker only had access to a truncated set of the knowledge questions, specifically a subset of 4 questions from the full set. Workers were not informed that there were two different versions of knowledge questions. The spectators, however, were informed about which worker had received the full set of questions, and that the worker with the higher number of correct answers received the entire initial reward of $6 (we expect this worker should almost always be the one who received the full set of questions), while the other worker received $0. Again, we ex-post match each pair to include one worker who received the full set of questions and performed better, and a worker who received the truncated set of questions and performed worse. The Random-Employment treatment is intended to partially emulate circumstances in the labour market that might be unevenly imposed upon different workers.[5]

---

[3] In this study, we applied ex-post matching to ensure that within each pair, workers with disadvantaged conditions were always the lower performers. This approach, while precluding the chance for disadvantaged workers to outperform their advantaged counterparts, was not designed to deceive or mislead participants. All workers were informed about the pairing and the potential for earning bonuses only after they completed their task. Furthermore, our spectator instructions described a specific example of a scenario between two unnamed but specific workers (in which a disadvantaged worker performed worse and earned less initial payment than an advantaged counterpart) but made no broader statements about how prevalent or absolute this scenario was in our experiment or beyond. This matching method is to maximize the efficiency of the observations. It is, however, possible that spectators may behave differently if they knew the empirical impossibility for the lower-opportunity workers to win under this implementation. We leave this as a topic for future research.

[4] Unequal educational access is prevalent and near universal in most societies around the world. For example, in the United States, two equally able and hard-working children could receive vastly different education qualities due to differences in the quality of local public education in the school districts they reside in. In addition, gaps in educational experiences vary widely based on parental investments made towards children's education. While families with economic means can enhance their children's education through supplementary courses outside of the formal required schooling, families with lesser economic means usually cannot afford to do so. At an even more fundamental level, students vary in terms of the family background endowments such as family-specific values and norms, which could affect parental support and prioritisation of educational pursuits. Our Random-Education treatment can be interpreted as implementing this inequality in educational opportunity, but isolated from the other potentially confounding factors mentioned, so that spectators' attitudes towards educational opportunity can be observed while isolated from any preconceived notions they may have about geographic and socio-economic differences between workers.

[5] For example, in the United States, due to circumstances beyond individual workers' personal control, some geographic regions have accelerated economically, providing local residents with ample job opportunities. On the other hand, some regions have stagnated economically, leaving residents with very limited opportunities for gainful

In both of the treatments with unequal opportunities, spectators were told that the worker who scored higher in the knowledge evaluation was assigned $6 while the other worker was assigned $0. We highlighted to the spectator that the worker who studied the highly relevant reading materials also scored higher in the evaluation in the Random-Education treatment and that the worker who had access to the full set of the evaluation questions scored higher in the Random-Employment treatment. Spectators are only informed about the nature of the unequal opportunities but not their degree. (How varying degrees of unequal opportunities impact redistributive decisions will be our subject of study in the follow-up experiment introduced in Section 4.) Spectators were also not informed of the absolute performance difference between the paired workers. Importantly, spectators were informed that the workers were not aware of their own performance result as compared to that of their paired worker, and furthermore that workers were not aware of any potential differences between the reading materials or number of knowledge evaluation questions provided, between the paired workers. This specific set of information provided to spectators and workers serves to help isolate the effects of unequal opportunity on spectators' redistributive decisions, from potential influences of perceptions of workers' expectations, and further helps mitigate the possibility that spectators would attribute performance differences between workers as driven by differing effort levels due to knowledge of their unequal opportunities.

In summary, across these four treatments, the spectators faced the same redistributive decisions with identical levels of initial income inequality, and a pair of workers with identical incentives to learn and perform. The only difference is the source of inequality in initial rewards, by treatment. By comparing spectators' redistributive decisions in the Random-Education treatment to the joint benchmark provided by the Luck and Merit treatments, we are able to causally identify the effect of the unequal learning opportunities on the level of redistribution preferred by spectators. Similarly, by comparing spectators' redistributive decisions in the Random-Employment treatment to the same joint benchmark, we are able to causally identify the effect of the unequal employment opportunities on the spectators' choice of redistribution. Table 1 summarises the main features of these treatments.

---

employment. For personal or economic reasons, some residents may not be able to migrate to other regions, and may then be limited to the local labour market conditions. Another potential domain for interpreting the Random-Employment treatment is in terms of workers' established professions of employment. Workers may have trained or studied in their profession, and sudden economic shocks may alter the employment opportunities available to different professions in the economy. For workers whose professions are suddenly in low demand, the overall work opportunities are fewer than for other professions, which tends to result in lower income earned. While removing real world contexts and potential other confounding factors which could affect spectators' attitudes, our Random-Employment treatment aims to gauge third party attitudes on deservingness based on the differing exogenous work opportunity levels available to individuals.

Table 1: Main treatments in Main Experiment

Treatment	Reading materials	Performance evaluation	Initial assignment
Luck	Same	Same	Random worker gets 6 USD
Merit	Same	Same	Better performer gets 6 USD
Random-Education	Different	Same	
Random-Employment	Same	Different	

2. Follow-up experiment

In this section, we present the results from a follow-up experiment, which extends our main experiment in two significant ways. First, whereas in the main experiment's unequal opportunity treatments, spectators were aware of workers having unequal opportunities ex-ante (in the Random-Education treatment) or ex-post (in the Random-Employment treatment) without knowing the degree of inequality, our follow-up experiment provides explicit information about the degree of unequal opportunity. This enables us to test the robustness of our findings from the main experiment.

Second, in the main experiment, spectators made redistributive decisions under a single unequal opportunity scenario. The follow-up experiment, however, requests each spectator to redistribute rewards among seven different pairs of workers, each under a distinct scenario with varying degrees of unequal opportunity, with the spectators informed of the exact degrees. To provide an equal opportunity benchmark, we create seven corresponding scenarios in which the initial reward is probabilistically determined by workers' performance and luck, and these exact probabilities are disclosed to spectators (Cappelen et al. 2022). This new experimental design allows us to examine how spectators' redistributive preferences react to their perceptions of varying degrees of unequal opportunity and how these reactions contrast with the patterns observed in the equal opportunity benchmark.

2.1 Treatments

We implement three between-subject treatments: 1) Varying degrees of unequal educational opportunity (the *Vary-Education* treatment); 2) Varying degrees of unequal employment opportunity (the *Vary-Employment* treatment); 3) Varying combinations of probabilistic weighting of performance and luck (the *Vary-Probability* treatment).

2.1.1. The Vary-Education and Vary-Employment treatments

Each unequal opportunity treatment includes seven scenarios with varying degrees of unequal opportunity, characterised by the values 15 vs 0, 15 vs 1, 15 vs 4, 15 vs 7, 15 vs 11, 15 vs 14, and 15 vs 15. In the Vary-Education treatment, the first number indicates that one worker in the pair has access to all 15 relevant pieces of information necessary for correct answers in their reading materials, while the second number indicates the amount of relevant information received by the other worker.[6] For instance, in the 15 vs 4 scenario, the disadvantaged worker has access to only 4 specific information pieces for the knowledge questions, while the other 11 pieces are transformed into vague phrases without information content. Likewise, in the Vary-Employment treatment, the first number represents one worker's access to all 15 knowledge questions, and the second number indicates the number of questions accessible to the other worker. Both workers study the same fully relevant reading materials before attempting the knowledge questions.

Spectators were explicitly aware of the exact degree of unequal opportunity under each scenario, a detail not provided in the main experiment. Other features, however, were maintained to be consistent with the main experiment. Specifically, the workers were not informed that different versions of reading materials or knowledge questions were being used, although spectators were made aware of this. The spectators were also informed about which worker in each pair received the fully relevant reading materials, or had access to the complete set of 15 knowledge questions, and that the worker with the higher number of correct answers would receive the entire initial reward of $6. After the evaluation, workers were paired ex-post under each scenario. A worker receiving the fully relevant materials or the complete set of questions and performing better was matched with another worker who had access to less relevant materials or a reduced number of questions and performed worse.[7]

2.1.2. The Vary-Probability treatment

The Vary-Probability treatment incorporates seven scenarios, each with distinct probabilistic combinations of luck and performance determining the initial assignment of rewards. In this treatment, both workers have equal opportunities, accessing the same fully relevant reading

---

[6] To implement this design, we create seven different versions of reading materials in which the number of pieces of information relevant to the knowledge questions is 0, 1, 4, 7, 11, 14 and 15 respectively. In particular, the version with 4 pieces of relevant information is the same one used in the Random-Education treatment of the main experiment. We also create seven different versions of knowledge questions in which the total number of questions is 0, 1, 4, 7, 11, 14 and 15, respectively. In particular, the version with 4 questions is the same one used in the Random-Employment treatment of the main experiment.

[7] Similar to the main experiment, this ex-post matching procedure precludes the chance for disadvantaged workers to outperform their advantaged counterparts. Therefore, the experiment specifically varies spectators' perceptions of the degree of unequal opportunity and examine their reactions to these perceptions.

materials and answering the same full set of knowledge questions. Designed to align with the experiment reported in Cappelen et al. (2022), this treatment allows us to use the same structural model to estimate and compare how different combinations of probabilistic weighting between luck and performance, as well as perceptions of varying degrees of unequal opportunity, affect spectators' redistributive preferences.

In each scenario, the probability assigned to luck takes on the values of 0%, 1%, 10%, 50%, 90%, 99%, and 100%. For example, in the 10% luck scenario, spectators were informed that the initial reward was determined by luck with a 10% probability and by performance with a 90% probability. In the luck scenario, the worker in the pair who was fortunate based on a computerised fair coin toss earned $6, while in the performance scenario, the worker with more correct answers earned $6 (with ties resolved randomly by the computer). Spectators were provided with the exact probability under each scenario. However, the workers were not given specific details, being informed only that their initial reward would be determined by either luck or performance, with some undefined probability.