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"They Never Had a Chance": Unequal Opportunities and Fair Redistributions

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"They Never Had a Chance": Unequal Opportunities and Fair Redistributions

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Abstract: A meritocratic fairness ideal is generally believed to regard income inequality as fair if it stems from performance differentials rather than luck. In this study, we present experimental evidence showing that merit judgments are shaped by *the source* of performance differentials while holding fixed the underlying impact on willingness to perform. Inspired by real-world phenomena which generate inequality, we investigate two types of unequal opportunities that impact performance: educational quality and employment opportunity. Contrary to some previous findings that merit judgements are often insensitive to unequal circumstances, we find that individuals are more inclined to split resources equally when the performance differential involves either type of unequal opportunity. We also find that when participants were given the option to expend personal effort to reveal information about the presence of unequal opportunity, a substantial number of them declined to do so, but held optimistic beliefs about the social norm of seeking such information. These findings enrich our understanding of the factors that lead individuals to support income redistribution, while also obtaining an assessment regarding to what degree redistributing third-party decision-makers are vested in these choices.

Keywords: Meritocracy, fairness, redistribution, socio-economic inequality, unequal opportunity, procedural fairness attitude

JEL: C91, D63, H23, I24

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> The worker said to himself: "Here I am, a workman. Why am I a workman? Am I fit for nothing else? Of course not. Had I had a proper chance I would have shown the world. A doctor? A brewer? A minister? I could have done anything. I never had a chance. And so I am a worker. But don't think that at bottom I am any worse than anyone else. I'm better." (Young, 1958, p. 106) [1]

1. Introduction

Inequality attracts immense concerns among both scholars (Piketty and Saez 2014) and the general public. The top one percent of the world's population owns nearly half of all the world's wealth, while the bottom half of the population altogether only accounts for less than one percent of total wealth (Shorrocks, Davies and Lluberas 2021), a striking statistic which has led to widespread outrage. On a practical level, people are concerned about the underlying sources of inequality, that is, whether or not inequality is the result of fair origins (Starmans, Sheskin and Bloom 2017). Although views on what is considered fair can differ within the population, many ordinary citizens as well as political leaders endorse a meritocratic view of fairness. [2] In this view, instead of luck, heritage or other factors beyond their control, individuals should be rewarded based on their merit, that is, their effort or choice, ideally in environments with a level playing field (Konow 2000; Cappelen et al. 2007, see a recent review in Cappelen, Falch and Tungodden 2020).

In practice, however, the presence of unequal opportunities posits a challenge to making meritocratic judgements. To the extent that unequal opportunities blur the boundary between luck and merit, what then constitutes a reasonable meritocratic judgement in situations with unequal opportunities? The objective of this paper is to uncover people's fairness view and redistribution preferences in such situations. In our study, we focus on two important forms of unequal opportunities. One is about inequality in educational opportunities, and the other is about inequality in career opportunities. While most people believe that a good education is the key component to climbing the social ladder,[3] educational quality, measured either by school quality (such as rankings and performance measures) or parental investment, is often substantially

[1] In his book "The Rise of the Meritocracy," Young (1958) coined the term "meritocracy." In this dystopian political fiction, he explored the potential pitfalls of a society which relies primarily on meritocracy.

[2] That meritocracy and equal opportunity are core American values is reflected in political discourse. For example, in President Barack Obama's inauguration speech, he asserted, "We are true to our creed when a little girl born into the bleakest poverty knows that she has the same chance to succeed as anybody else…" A similar idea was also reflected in the inauguration speech of President George W. Bush, in which he said, "The ambitions of some Americans are limited by failing schools and hidden prejudice and the circumstances of their birth… I will work to build a single nation of justice and opportunity."

[3] A global median of sixty percent rates it as ten, "very important," on a scale from zero to ten on the importance of education, see Pew Research Center 2014).

unequal.[4] If merit is measured by exam performance or productivity at work, a key input into these performances is largely unaccounted for, which is the opportunity an individual was provided with to generate that performance.

Besides inequality in educational opportunities, which reflects the ex-ante investment that individuals received in their human capital cultivation and training, the other potential source of inequality in performance we investigate here occurs *at the time* of the performance evaluation. Individuals may be given differential opportunities to exhibit their willingness to work hard and their ability to perform well. A series of prominent examples are in the domain of labor market discrimination based on gender, race, ethnicity, age, sexual orientation, and other factors (see a review in Bertrand and Duflo 2017). Another example of unequal employment involves unequal and unexpected macroeconomic conditions or shocks. Perhaps through no fault of their own, a worker has built up their human capital in a particular industry, only to find that due to unforeseen macro-level factors, that industry is no longer growing, while other workers are well prepared to work in the currently high growth industries. Alternatively, a worker may be constrained to living and working in specific geographic areas, which may differ vastly in terms of their opportunity for employment and job growth.

In this paper, we conduct an online experiment with over 2000 participants to test individuals' fairness preferences under these two types of unequal opportunities. We are interested in individuals' redistributive preferences when they encounter a pair of workers, who have the same incentives to perform well, experienced unequal opportunities. We implement two treatments. The first treatment is called *random education*; it aims to capture different quality levels of education that individuals may receive. The second treatment is called *random employment*; it intends to represent the different career opportunities individuals may have fully beyond their own control. Under each of these opportunity-based treatments, there are two competing hypotheses on what a meritocrat would do. On the one hand, since unequal outcomes between the two workers are not due to any merit-related factor, they may fully equalize income between the two workers motivated by the underlying fairness considerations. On the other hand, they would judge the workers only

[4] Inequalities in education and job opportunities can be the key drivers of unequal outcomes (Goldin and Katz 2009; Autor 2014; Alves et al. 2015). In early childhood, parents invest much more money and time on child-care than less educated parents (Lareau 2002; Doepke and Zilibotti 2017; Blanden, Doepke and Stuhler 2022). Children from advantageous socioeconomic backgrounds are healthier (Currie 2009), score higher on IQ tests (Falk et al. 2021), and are more likely to pursue schooling on an academic rather than vocational track (Falk, Kosse and Pinger 2020); they also have access to better information on college application and job opportunities (Hoxby and Turner 2015; Hällsten and Thaning 2018; Jackson 2021). Chetty et al (2020) found that children from families in the top one percent of the income spectrum are 77 times more likely to attend an Ivy-Plus college compared to children from families in the bottom income quintile.

by their productivity result regardless of the opportunity condition workers were randomly assigned to, and therefore not redistribute the income.

The two opportunity treatments were implemented as follows: In the random education treatment, participants in our intervention were assigned different learning materials within each pair of workers. While workers' motivation to learn and perform well in the subsequent evaluation task were identical, one worker's learning materials were highly relevant to the final knowledge evaluation, while the other worker's materials were of similar topic and length, but lacked critically relevant information with respect to the eventual performance evaluation task. After participants each read their respective materials, they were asked to complete the same knowledge evaluation. In the random employment treatment, the two workers read fully identical learning materials. However, in the knowledge evaluation which followed, one worker was asked to complete the full set of knowledge questions, while the other worker only had access to a truncated subset of the questions. The knowledge evaluation task mirrors many performance metrics present in labor market settings, including job interviews, promotion qualification procedures, as well as performance in the job itself, which can require workers to draw upon specific previously gained knowledge.

Under both unequal opportunity scenarios, the default initial allocation is that the worker who answers more correct answers in the knowledge evaluation is assigned 6 dollars while the other worker is assigned zero dollars, following the setup of Almås, Cappelen and Tungodden (2020). This extreme inequality serves as the status quo. We then examine a third-party individual's redistributive preferences through their redistribution decisions when they are well-informed about the implementation of the unequal opportunities described above. Thus, the third-party spectators faced the same redistribution decisions with identical initial allocation rules, identical levels of initial income inequality, and a pair of workers with identical incentives to learn and perform. The only difference by treatment is the source of inequality in the initial payment allocation.

Our conjecture is that providing unequal opportunities to different workers will be perceived as unfair to third-party decision-makers, resulting in redistribution towards the participant with the lower initial income. Our results confirm this conjecture: compared to the benchmark treatment where opportunities for learning and performance are equal, spectators made more equalizing redistributions when opportunities were unequal. However, the redistributed amount is not as large as in luck treatment in which pure luck determines the initial income inequality. Thus, inequality stemming from unequal opportunities is considered mostly unfair but are not functionally equivalent to pure luck. We also find that the impact of unequal opportunities varies across the different opportunity scenarios: the redistributed amount is higher under unequal educational

opportunities than under unequal employment opportunities, implying that the former is perceived as closer to pure luck than the latter. Furthermore, spectators' stated attitudes about procedural fairness closely mirror their redistribution decisions, suggesting that their procedural fairness attitudes play a significant role in determining their meritocratic judgments.

These findings from the treatments in which the source of inequality is transparent suggest that availability of information about the origins of unequal opportunities is a key for different redistributive behavior. However, lack of information is precisely one of the greatest challenges when dealing with inequality in the real world, that is, we typically cannot observe precisely how a person's performance advantage is gained. Studies find that people's perception about how wealth is created strongly correlated to their attitudes toward redistribution: if they believe wealth is created by effort, they choose low taxes; if they believe wealth is created by luck, birth or corruption, they prefer heavy taxes (Alesina, Glaeser, and Sacerdote 2001; Alesina and La Ferrara 2005). One of the possible reasons that individuals hold vastly different views on the redistributive characteristics of tax policy, is an imperfect knowledge and assessment of the factors that produce inequality.

Therefore, the natural next question to ask is whether people would be willing to investigate the underlying source of performance inequality, even if it costs them personal effort and time. In the corresponding treatments of our study, we informed the third player about the *possibility* of unequal opportunities and offered them a chance to obtain information about the true source of inequality before making their redistributive decision. Specifically, they only learn for sure about the nature of opportunities the workers receive if they are willing to complete a tedious number-checking task. Alternatively, they can opt out of the effort task and thus obtain no information about the true state of workers' opportunities. We incorporate this same information-seeking procedure to both the random education and random employment treatments. By comparing the random education (employment) treatment to its corresponding information-seeking treatment, we can discern whether people are willing to expend effort to obtain information about sources of inequality and whether their redistribution decisions are correlated with their information-seeking decisions. This question is important because if people are not willing to go through the trouble to uncover the source of inequality, then any preferences expressed regarding unequal opportunities through the allocation decision may be relatively shallow in nature, and could disintegrate once decision-makers are asked to have a personal stake in the implementation of fair allocations.

We find that in both of the information-seeking treatments, about half of the participants do not appear to sufficiently care about the source of performance differential, and thus decline to complete the number-checking task to reveal such information. The redistribution levels of those who declined to seek information are indistinguishable from those who did seek information,

indicating that a substantial fraction of individuals feel comfortable redistributing without actually knowing whether or not the original inequality was due to unequal opportunities. This raises some concern about whether people truly care about equalizing outcomes based on the actual sources of inequality in society. Treating a pair of workers faced with equal opportunity as if they were presented with unequal opportunities is in principle, equally unfair as treating a pair of workers faced with unequal opportunities as though they enjoyed equal opportunity, although it is the latter that our society is often most concerned about from a policy perspective.

Finally, we test for the external validity of our main experiment using hypothetical real-world scenarios in which work supervisors make a judgement about fair compensation decisions analogous to the ones in our main experiment, but with more detailed real-world contexts than that of online workers. The quantitative results in these hypothetical scenarios are remarkably similar to our main experimental findings. Moreover, we find that although only about half of the individuals are willing to seek information about unequal opportunities at their personal cost in the main experiment, they hold a relatively optimistic belief about the social norm of most supervisors making fair and informed compensation decisions. However, this belief still falls short of the ideal one in which all decision-making individuals would take necessary steps to learn about the true situation involving unequal opportunities.

Our study contributes to the broad literature that seeks to understand people's preferences for redistribution. A number of survey experiments explore the general pattern and structure of distributional preferences without varying the source of inequality (Ashok, Kuziemko, and Washington 2015; Kuziemko et al. 2015; Karadja, Mollerstrom, and Seim 2017; Fisman, Kuziemko, and Vannutelli 2021). Also using survey experimental methods, Alesina and Angeletos (2005) and Alesina, Stantcheva, and Teso (2018) document that people's beliefs about the sources of inequality such as effort, luck, birth, or other factors related to equality of opportunities are critical determinants of their preferences for redistribution. In contrast to survey methods, a growing literature aims to reveal preferences in redistributive decisions that have real payoff consequences (Konow 2000; Cappelen et al. 2007; Cappelen, Sørensen, and Tungodden 2010; Cappelen et al. 2013; Akbaş, Ariely, and Yuksel 2019; Almås, Cappelen, and Tungodden, 2020; Cappelen et al. 2020). Typically, the decision-making environment involves a production phase where a pair of workers complete some tasks and a redistribution phase where an impartial spectator is asked to redistribute money between the workers. Experiments generally reveal that people hold different fairness ideals even in the same redistribution situation and these fairness ideals interact with the source of inequality to significantly impact their redistributive decisions.

Within this broad literature, our study is mostly related to Almås, Cappelen, and Tungodden (2020) who study how the source of inequality affects redistributive decisions. Serving as our

control treatments, we replicate two of their main treatments in which either luck or merit determines the initial earning assignment to a pair of workers. Their results show that spectators are more likely to equalize the total income when the initial earning assignment is determined by luck rather than merit. Cappelen et al. (2022) further demonstrates that even when luck plays a very small role in determining performance (as small as 1 percent), people are more likely to equalize the total income relative to when luck plays no role. In their studies, luck is viewed as a direct determinant of inequality. In many real-life scenarios, however, luck often plays a more indirect and subtler role in determining the different amount of resources and opportunities that people can employ in their academic or job performance. It is this indirect role that is arguably more fundamental and, if internalized as merit or simply ignored, can cause as much socio-economic inequality as when luck plays absolutely no role. Our study is one of the first to investigate how people's redistributive preferences are influenced by this indirect role of luck manifested as different forms of unequal opportunities, relative to the control environments involving no unequal opportunities.

The previous literature has also shed some light on how redistributive preferences are shaped by unequal circumstances. In these experimental studies, circumstantial differences are typically represented by exogenously determined and differing piece-rates for completing the same task (Cappelen et al. 2007; Cappelen, Sørensen, and Tungodden 2010; Andre 2022). Although different circumstances can potentially alter workers' incentives to work, results in these studies suggest that most spectators respect their final performance as though luck is irrelevant. For example, in a study that is contemporaneous to ours, Andre (2022) finds that even if spectators correctly predict hampered effort from the worker with a lower piece rate, they still reward workers according to their final productivity, irrespective of the circumstantial differences. Unlike the above-mentioned studies where workers' incentives to exert effort are purposefully altered by different circumstances, we keep the workers' incentives unchanged. We are mainly interested in environments in which everyone is equally willing to work hard, irrespective of unequal access to resources or opportunities. By doing so, a counterfactual in which an unequal opportunity could have just as easily benefited the other worker and produced an exact opposite outcome is also carefully implied in the descriptions. This environment may be more closely appropriate for observing the sensitivity of meritocratic fairness under unequal opportunities; however, if people's merit judgements are still insensitive to unequal opportunities by only respecting the final outcome, it would imply that our society may face a more fundamental obstacle in reducing socio-economic inequality than the literature has currently realized.

2. Experimental Design

2.1 Overview and Implementation

We use the paradigmatic spectator-worker design (ex. Almås, Cappelen, and Tungodden 2020) to create an experimentally controlled situation of inequality between two workers. In the spectator-worker designs, two subjects ("workers") engage in a task and are given initial rewards based on various specified criteria such as relative performance. A third subject ("spectator") is then informed about the workers' scenario and initial reward allocation, then given an opportunity to reallocate the rewards between the two workers. The primary variable of interest is the spectator's reallocation decision, which is interpreted as reflecting decision-makers' views about fairness concepts and preferences over redistribution. 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 realistically represented to spectators.

In our pre-registered design, a worker's task was to study some brief reading materials provided, then answer questions in a multiple-choice knowledge evaluation. Workers were then randomly paired ex-post and assigned initial earnings. Across all treatments, the initial earnings allocation is the same: one of the workers received $6, while the other worker received $0. The treatments differ in terms of the source of the initial inequality and/or the underlying source of workers' performance differential as will be described below. In separate experimental sessions, another set of subjects playing the role of the impartial third parties, which we refer to as spectators, had the opportunity to redistribute the workers' earnings after reading a description of the workers' scenarios. One-third of spectators' decisions were randomly chosen to be implemented on our subjects playing the role of workers.

Each worker earned a base fixed payment of $2 for completing the task. Based on the criteria of each specific treatment, they were informed that their task performance would determine their reward *temporarily*, but that another participant in our study would be tasked with making the *final* reward allocation decision between them and their paired worker. Note that workers *only* knew the rule determining the initial allocation of reward, but they were not told whether or not they were actually assigned the initial $6, and spectators were informed of this fact. This feature helps to rule out the influence of workers' expectations on spectators' decisions.

Spectators were randomly assigned to one of the six treatments and earned a flat payment of $3 for completing their task. They were provided with instructions describing the situation faced by the workers and the initial payment reward result between "Worker A" and "Worker B." After reading the instructions, they must pass a comprehension quiz in order to proceed. This helps ensure that spectators comprehended the situation of the workers correctly, and understood the reality of their own role in workers' outcomes. Spectators had to determine whether and how much

to redistribute the earnings from the worker initially awarded \$6 to the worker initially awarded \$0. They could choose to redistribute the workers' earnings in increments of \$1 or alternatively, decline to redistribute. They were informed that one-third of all participants doing the same task as them would have their choice implemented on real workers in our study, and therefore, they should make their reallocation decision as though it would be implemented.

A total of 2100 participants were recruited for our study via Amazon Mechanical Turk, a crowdsourcing web service specializing in recruiting anonymous workers to complete small tasks online. For each of the six treatments, we recruited 210 spectators and 140 workers (70 pairs) to implement the above-described procedure. The sample was targeted to be approximately representative of the general population in the United States with respect to gender, age and income by using the Cloud Research Panel of Mechanical Turk workers.[5] Experimental instructions and procedures for both the spectators and workers are provided in Appendix B.

2.2. Treatments

We implement six between-subjects treatments, which vary in terms of the source of inequality and/or performance differential between workers. Since the primary objective of our study is to address how opportunity affects redistribution preferences, we discuss the treatments in the following sections, organized by whether there was an opportunity differential imposed between workers.

2.2.1. Treatments with equal opportunities

In the two treatments, *Luck* and *Merit*, both workers first studied identical reading materials for up to 5 minutes, then completed the same set of multiple-choice knowledge questions pertaining to the topic addressed in the reading material.

In the Luck treatment, the initial assignment of earnings between the two workers was fully determined by a lottery (i.e. one of them was randomly selected to receive the entire initial reward, \$6). In the Merit treatment, the initial assignment of earnings was determined by performance on the knowledge questions (i.e. the worker who answered more questions correctly received the entire initial reward, \$6).

[5] More than 1260 spectators have tried to do the spectators' sessions. As explained in the pre-analysis plan, we excluded participants if they (1) failed the Captcha; (2) did not pass the comprehension quiz; (3) have already participated in the study; (4) spent too little time on reading the experimental instructions, making decisions, and completing the questionnaire (i.e., participants who completed the entire task in less than 2 minutes). All of the 1260 spectators passed all the criteria and are included for data analysis.

To avoid spectators' decisions being affected by workers' expectations, workers were not told the lottery result (in the Luck treatment) or the result about relative performance (in the Merit treatment).[6] Spectators were informed of the exact procedure of the assignment of initial earnings to workers in the spectator task instructions. These two treatments have been previously implemented in Almås, Cappelen, and Tungodden (2020), and we replicate them here for the purpose of straightforward comparability with our novel treatments.

2.2.2. 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 performance outcomes.

The Random-Education treatment differs from the Merit treatment in that workers, unbeknownst to themselves, randomly received different reading materials: one set of reading material was highly relevant to the subsequent knowledge questions, containing all the information needed for the worker to answer all the questions correctly. By contrast, the other set of reading material, about the exact same topic and of similar length, lacked several pieces of vital information for successfully completing the knowledge questions. The spectators were informed about which worker 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. 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 relevant reading materials was substantially advantaged. Therefore, we expect that workers who had a better performance 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 receiving the highly relevant materials and performing better is paired with a worker who received the less-relevant materials and performed worse. 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 material performed worse (average score: 7.9 out of 15).

The Random-Education treatment is designed to reflect unequal opportunities in terms of access to different qualities of education by individuals, due to circumstances beyond their own

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

control. This situation is prevalent and near universal in most societies around the world, despite some policy efforts to equalize educational access. 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 prioritization of educational pursuits. Our Random-Education treatment implements this inequality in educational opportunity, but isolated from other potentially confounding factors mentioned, so that spectators' attitudes towards educational opportunity can be observed isolated from any preconceived notions they may have about geographic and socio-economic differences of workers.

The Random-Employment treatment represents another type of unequal opportunity. In this treatment, a pair of workers received the same set of highly relevant reading material. 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. The spectators 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). 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.

The Random-Employment treatment is designed to reflect circumstances such as labor market circumstances which are inflicted unevenly on different workers. 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 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 labor 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 work opportunity levels available to individuals.

In both of the treatments with unequal opportunity, 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.[7] Spectators were not told about the absolute performance difference of 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 serves to help isolate the potential effects of unequal opportunity from potential influences of perceptions of workers' expectations on spectators' reallocation decisions based solely on the discrepancy between the reading materials or the number of questions, and further helps to mitigate the possibility that spectators would attribute performance differences between workers as driven by differing effort levels.

These two types of unequal opportunities are commonly observed in our daily lives. In their essence, unequal opportunities might matter differently to people's fairness views depending on how much they attribute the initial earning distribution to luck or merit. On the one hand, unequal opportunities might be considered as merely luck, since they are completely out of workers' own control in the context of our experiment. On the other hand, the difference in initial earnings is directly a product of workers' different performances. Therefore, it is also not completely unreasonable to dismiss the impact of unequal opportunities and simply attribute final performance to merit. Hence, the Luck and Merit treatments provided a joint benchmark which would allow us to position the degree to which the spectators would attribute performance differentials compromised by unequal opportunities to pure luck or pure effort.

By comparing spectators' redistributive decisions in this treatment to the joint benchmark provided by the Luck and Merit treatments, we are able to causally identify the effect of the presence of unequal learning opportunities on the level of redistribution preferred by spectators. Similarly, by comparing spectators' redistributive decisions in this treatment to the joint

[7] This was indeed true both on average in the treatments which validates our treatment interventions, and in our implemented matchings of workers. In addition, workers were told that in the event of a tie in performance between themselves and their paired worker, another worker match would be assigned to them such that the other worker's performance would be either strictly better than or strictly worse than their own.

benchmark provided by the Luck and Merit treatments, we are able to causally identify the effect of the presence of unequal performance opportunities on the spectators' choice of redistribution.

It is worth noting that in the Random-Education treatment, the experimental design also helps to rule out the possibility that performance differential in the knowledge evaluation can be attributed to difference in effort: *ex ante*, both workers are expected to exert a similar amount of effort both in studying reading materials and in completing the knowledge evaluation. In the Random-Employment treatment, the unequal performance opportunities in the knowledge evaluation inherently cause a difference in the effort *able to be* exerted in the evaluation, but not the incentive to exert effort. However, *ex ante*, both workers are assumed to exert a similar amount of effort in studying reading materials.

2.2.3. Treatments with information-seeking

In the previously discussed treatments, spectators knew the exact information about the source of inequality (luck or merit) or the source of performance differential (unequal opportunities). However, in many real-world situations, the sources of performance differentials are not immediately observable or could be easily overlooked. For example, school admission officers might only see the scores of prospective students without knowing or paying much attention to students' family backgrounds which influence their pre-application educational opportunities; human resource administrators may only see the previous track records of two job candidates while dismissing their prior career opportunities.

Therefore, we design two treatments to discern whether spectators, knowing about the *possibility* of unequal opportunities, would proactively choose to search for information about the source of the performance differential. This question is important because it allows us to distinguish between decision-makers who are willing to incur a personal cost in their efforts to implement fairness from those who might merely be "conveniently" fair, meaning that they would not take personally costly actions to implement fairness that is unrelated to their own welfare. In both information treatments, before making redistributive decisions, spectators had the option to exert effort in order to find out whether the pair of workers faced unequal opportunities.

In the *Info-rEducation* treatment, some pairs of workers received the same set of reading materials as in the Merit treatment, while other pairs received different sets of reading materials as in the Random-Education treatment. Spectators knew that the assignment of earnings had been determined by the number of correct answers in the knowledge evaluation, but they needed to complete a real-effort number-checking task to unveil whether the two workers actually received the same reading materials, or reading material of different relevance levels.

Similarly, in the *Info-rEmployment* treatment, some pairs of workers received the same full set of the knowledge evaluation as in the Merit treatment, while other pairs received different versions of knowledge questions as in the Random-Employment treatment. Spectators knew the assignment of earnings had been determined by the number of correct answers in the knowledge evaluation, but they needed to complete the same number-checking task to reveal whether the two workers had access to the same or different numbers of knowledge questions.

Just as in the previous treatments, spectators were accurately informed that the workers were unaware of their relative performances and similarly unaware that the reading materials (Info-rEducation) or number of knowledge questions (Info-rEmployment) might differ between workers.

The number-checking task offered to spectators in the information treatments is to identify a specific number at least 20 times in a 17x17 matrix of three-digit numbers. If the spectators chose to skip this option, they proceeded directly to the redistribution phase, remaining unaware of whether unequal opportunities between the pair of workers was present. On the other hand, if they chose to participate in and passed the number-checking task, they would be shown whether the worker pair had unequal opportunities.[8]

Table 1 summarizes the main features of all six treatments.

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	
Info-rEducation	Different	Same	
Info-rEmployment	Same	Different	

3. Theoretical Framework and Hypotheses

[8] Spectators who passed the number-checking task were in practice always shown that the pair had unequal opportunities. This design maximizes observations regarding how spectators would allocate between pairs of workers with unequal opportunities. It involves no deception because spectators were informed that "there is some chance" that workers have access to different opportunities, instead of a specific percentage. To ensure we have a sufficient number of pairs of workers with unequal opportunities in case spectators choose to seek this information, 80% of pairs were implemented with unequal opportunities while the other 20% of pairs were implemented with equal opportunity as described in the Merit treatment.

3.1. Theoretical Framework

Following the framework proposed in Cappelen et al. (2013) and Almås, Cappelen, and Tungodden (2020), we present the following simple theoretical model to provide an intuitive framework for understanding our results.

In each treatment j, the spectator chooses the distribution between the two workers, $(y, 1-y)$ where y denotes the share of total earnings redistributed to the worker who received no initial earnings. We assume that the spectator cares about a "fair allocation" between the two workers according to the quadratic loss utility function

$$U(y) = -\big(y - m(j)\big)^2,$$

where $m(j)$ denotes the spectator's perceived *fair* share allocated to the worker who did not receive any initial earnings. Thus, the optimal interior solution has the spectator selecting their perceived fair distribution as that to be allocated in the experiment

$$y(j) = m(j).$$

It follows immediately from the model that any differences in y across treatments must come from the difference in spectators' fairness views regarding workers' predicaments in the different treatments. Following the literature, we can tentatively conceptualize the spectators' attitudes towards the luck and merit treatments as being the endpoints in the range of possible fairness attitudes that spectators could have. The idea being that true merit, if fully accurately represented, by definition, reflects the perceived appropriate and fair initial allocation. On the other hand, pure luck, being fully independent of any of the notions of deservingness discussed in this paper, is the other possible endpoint at least among the treatments we consider here. Spectators' fairness views about the source of inequality due to merit as compared to pure luck can be identified by the following difference in their redistributive decisions across the two treatments:

$$y(Luck) - y(Merit) = m(Luck) - m(Merit).$$

An intuitive way of conceptualizing unequal opportunities is that it is equivalent to an initial round of (random) luck being imposed on workers prior to their chance to demonstrate their work performance. Under this interpretation, we would expect that the level of redistribution in treatments with unequal opportunities is positioned between those of the Luck and Merit treatments. Therefore, to represent the degree to which spectators attribute income inequality under

unequal opportunities to merit or luck components, we describe the relationship between the redistributive decisions in the three conditions as:

$$m(Unequal\ Opportunity) = \alpha \cdot m(Merit) + (1-\alpha) \cdot m(Luck)$$

$$\rightarrow y(Unequal\ Opportunity) = \alpha \cdot y(Merit) + (1-\alpha) \cdot y(Luck),$$

where the parameter α measures how closely inequality with the presence of unequal opportunities is perceived as merit, and correspondingly, how close the chosen redistribution between workers is to the merit condition.

The literature on fairness perceptions has typically categorized people as holding one of the three fairness views: egalitarian, libertarian and meritocratic (Cappelen et al. 2007; see Cappelen, Falch, and Tungodden (2020) for a review). Spectators with an egalitarian fairness view will completely equalize the total income, while those with a libertarian fairness view will never redistribute the income at all. Only the meritocratic fairness view distinguishes between the source of income inequality; inequality due to luck is considered unfair while inequality due to merit is regarded as fair. Therefore, any difference in behavior between the Luck and Merit treatments must be driven by spectators holding meritocratic fairness views.

However, this categorization of fairness views is silent about the potential effects of unequal opportunities. Although it is clear that the source of inequality is due to performance, the extent to which performance can be attributed to merit is unclear, since merit can be compromised by the presence of unequal opportunities. It follows that it is not clear how much importance spectators with a meritocratic fairness ideal would assign to the underlying opportunity-based source of performance differential. Our experiment can help shed light on this inherently empirical issue.

3.2. Hypotheses

Here, we present our pre-registered hypotheses. Our Luck and Merit treatments are direct replications of the treatments with the same labels as reported in Almås, Cappelen, and Tungodden (2020). They find that merit generally increases the acceptance of inequality. Following on their finding, we propose the following hypothesis in our study:

Hypothesis 1 (Merit vs. Luck): The amount that spectators redistribute is higher in the Luck treatment than in the Merit treatment.

For hypotheses about the relative redistribution tendencies between the other treatments, we follow related studies (Almås, Cappelen, and Tungodden 2020) in remaining agnostic ex-ante

about the direction of effects found. This is also reasonable for the objectives of our study because the question of to what degree spectators attribute performance differentials to merit is fundamentally an empirical one. Thus, we frame the comparison between our other treatments as null hypotheses of no statistical differences between the other treatments and Luck or Merit, while it is understood that our study is fundamentally interested in which treatments invoke the greatest redistributive tendencies by spectators.

In the two unequal opportunity treatments, based on our simple theoretical framework, spectators' views of fair allocations depend on the degree to which they perceive the random assignment of learning opportunities or performance opportunities as mere luck or whether they perceive it as merit. Therefore, on the one hand, if they perceive the random assignment of learning opportunities or performance opportunities as mere luck, we propose the following hypotheses.

Hypothesis 2a (Luck vs. Random-Education): The amount redistributed by spectators does not differ between the Luck and Random-Education treatments.

Hypothesis 3a (Luck vs. Random-Employment): The amount redistributed by spectators does not differ between the Luck and Random-Employment treatments.

On the other hand, if performance differentials due to random assignment of learning or performance opportunities are interpreted as differences in true merit, the following hypotheses reflect the predictions.

Hypothesis 2b (Merit vs. Random-Education): The amount redistributed by spectators does not differ between the Merit and Random-Education treatments.

Hypothesis 3b (Merit vs. Random-Employment): The amount redistributed by spectators does not differ between the Merit and Random-Employment treatments.

In our two information-seeking treatments we test whether spectators are willing to complete the number-ticking task which personally costs them extra effort. The following null hypothesis simply adopts the standard assumption that spectators are mostly selfish.

Hypothesis 4 (Information-seeking): Given that it is personally costly, spectators decide to skip the number-ticking task in the Info-rEducation and Info-rEmployment treatments.

Finally, we compare redistributive decisions between unequal opportunities treatments and their information-seeking counterpart treatments. We test whether the introduction of uncertain source of performance differential ultimately affects redistributive behavior. Without further

evidence regarding Hypothesis 4, we remain agnostic about any treatment differences and propose the following null hypotheses about the effect of information seeking in the opportunity treatments.

Hypothesis 5 (Random-Education vs. Info-rEducation): The amount redistributed by spectators does not differ between the Random-Opportunity and Info-rEducation treatments.

Hypothesis 6 (Random-Employment vs. Info-rEmployment): The amount redistributed by spectators does not differ between the Random-Employment and Info-rEmployment treatments.

4. Results

Before presenting our main results, in Table 2 we show an overview of spectators' characteristics across all treatments. We observe that characteristics are almost balanced across all treatments and close to the general U.S. population data, except that the share of highly educated spectators is overrepresented in our data. To provide a balance test for all characteristics, we first run a multinomial logit regression of treatments on all characteristics and then conduct a joint orthogonality test (chi-squared test). The p-value for this joint significance test is 0.453, indicating that an overall balance is achieved.

Table 2: Descriptive statistics about spectators' characteristics

	Treatment						U.S. Population (ACS, 2020)
	Luck	Merit	Random-Education	Random-Employment	Info-rEducation	Info-rEmployment	
Female (%)	54.8	56.7	50.5	54.8	57.6	52.9	50.8
Age (years)	44.1	44.7	43.3	43.5	43.6	46.1	38.2
High education (%)	49.5	58.6	53.8	57.6	56.7	53.3	32.9
Individual yearly income (USD)	58210	59134	57105	58854	57789	58799	68764
Conservative (%)	24.8	28.1	30.0	21.0	28.1	24.8	27.0
Obs.	210	210	210	210	210	210	

Note: The table reports descriptive statistics for spectators' characteristics in the experiment as well as the population data (from the American Community Survey (ACS) 2020 for sex, age, education and income, and Gallup for the party affiliation since 2021). A subject is categorized as "high education" if he or she has completed at least 4-year college education. Conservative is defined as having selected either Republican or Libertarian as their political party/stance most typically supported. Individual yearly income indicates subjects' self-reported pre-tax income, while in the population data it refers to mean earnings for full-time, year-round workers in the past 12 months.

4.1. Spectators' decisions

Our main outcome variable of interest is the share of the "better performing" worker's initial reward redistributed by spectators to the other worker in different treatments, $r_i \in [0, 1]$. The different redistributed amounts will lead to different implemented final inequality levels, that is, the absolute value of the difference in payment between the two workers divided by total payment across both workers, $e_i = |1 - 2r_i| \in [0, 1]$.

Figure 1 shows the average share of the original \$6 redistributed in each of the treatments. Comparing the Luck and Merit treatments, we replicate the stylized finding in the literature that people are significantly more willing to redistribute when the source of inequality in earnings is due to luck rather than performance ($p < 0.001$, Wilcoxon rank-sum test). Figure A1 in Appendix A further shows the distribution of spectators' decisions across all treatments. Complete equalization is the modal behavior when luck is the source of inequality: in the Luck treatment 70.5% of the spectators equalize completely between the two workers, whereas only 9.1% do not redistribute at all. By contrast, in the Merit treatment where performance is the source of inequality, only 11.4% completely equalize the total income, while 33.3% do not redistribute at all.

Turning to the two treatments with unequal opportunities, we find that opportunities matter differently to spectators' fairness considerations depending on the source of the performance differential. First, we observe that the average share redistributed in the Random-Education treatment (34.1%) is relatively closer to that in the Luck treatment (42.1%) than the Merit treatment (22.2%), although the difference is significant ($p < 0.001$) in the comparisons with both benchmark treatments. As shown in Figure A1, complete equalization, albeit to a lesser extent, is also the modal behavior in the Random-Education treatment (46.7%). On the other hand, 18.1% do not redistribute at all. This pattern is more similar to the Luck treatment than the Merit treatment. Thus, the overall pattern suggests that when the source of performance differential is due to random assignment of high-quality or low-quality learning materials, spectators tend to attribute it more to luck than to merit.

Second, the average share redistributed in the Random-Employment treatment (29.0%) tend to be closer to that in the Merit treatment than the Luck treatment, and again in both comparisons with the two benchmark treatments the difference is significant ($p < 0.001$). As shown in Figure A1, complete equalization is no longer the modal behavior in the Random-Employment treatment (30.0%). On the other hand, 23.8% do not redistribute at all and 35.2% redistribute 40% of the total income. This pattern is more similar to the Merit treatment than the Luck treatment. Thus, the overall pattern suggests that when the source of performance differential is due to random assignment of workload, spectators tend to attribute pay inequalities to merit more so than to luck.

Third, we verify that spectators redistribute significantly more in the Random-Education treatment than in the Random-Employment treatment ($p = 0.002$). Fourth, in the two treatments with information-seeking, we observe that the average shares redistributed are not significantly different between the Random-Education and Info-rEducation treatments (34.0% vs. 37.1% respectively, $p = 0.100$) as well as between the Random-Employment and Info-rEmployment treatments (29.0% vs. 31.9% respectively, $p = 0.135$). We will explore the two information treatments in more detail in the next subsection.

Finally, Figure A2 in Appendix A further shows that the pattern of inequality implemented reversely mirrors the pattern of share redistributed by spectators. That is, inequality is much higher in the Merit treatment than in the Luck treatment. Inequality in the treatments with unequal opportunities is somewhere in between: inequality in the Random-Education is closer to that in the Luck treatment while inequality in the Random-Employment is closer to that in the Merit treatment. We also find little difference in the implemented inequality between the treatments with unequal opportunities and their corresponding information treatments.

Summarizing the overall pattern across the two benchmark treatments and the two treatments with unequal opportunities, we find that while spectators did attribute at least part of unequal opportunities to luck, the tendency is stronger when unequal opportunities are about learning prior to the performance evaluation than when they are about workload in the performance evaluation. In other words, people are less tolerant of inequality arising from the deprivation of learning opportunities. Intuitively, the treatment difference may arise because in the situation with unequal educational opportunities despite having exerted a similar amount of effort in studying their learning materials and finishing the knowledge evaluation, some workers simply arrived at the test ill prepared to complete it very successfully, through no fault of their own. In the situation with unequal employment opportunities, however, some workers did exert less effort in the knowledge evaluation despite in the counterfactual situation they might have abled to exert a similar amount of effort. Spectators seemed to hold them personally responsible for completing less work, even though it was also through no fault of their own.

This finding may appear surprising given that in the Random-Employment treatment workers who receive the truncated set of questions have almost no chance of performing better than the other worker, whereas in the Random-Education treatment such a chance is much better for those who receive low-quality learning materials.[9] In this counter-argument we should expect

[9] In the Random-Employment treatment, only one out of 70 advantaged workers answered no more than 4 questions correctly, which is the maximum number of correct answers that disadvantaged workers could obtain. In the Random-Education treatment, 11 out of 70 advantaged workers answered no more than 10 questions correctly, which is the best performance among all disadvantaged workers.

spectators to feel more sympathetic toward the disadvantaged worker when unequal opportunities about workload in the performance evaluation. However, our finding suggests that spectators assign more weight to the actual effort exerted in the knowledge evaluation than the willingness to work hard, which is ex-ante similar for both workers.

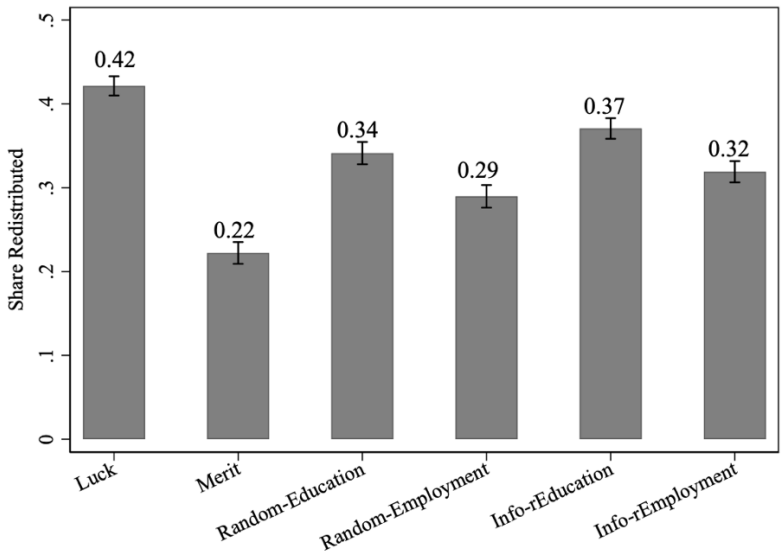

Figure 1: Share redistributed by spectators

Note: The figure shows the average share redistributed by the spectators in each of the six treatments. Standard errors are indicated by the bars.

Table 3 reports the corresponding regressions of share redistributed on the treatment indicators and on characteristics variables collected from the post-experimental questionnaire, with the Luck treatment serving as the reference category. In column (1) we observe that consistent with the non-parametric tests, performance as the source of inequality reduces the share redistributed by 19.9% compared to luck. The shares redistributed in the two treatments with unequal opportunities are significantly different from the joint benchmark provided by the Luck and Merit treatments. However, the reduction in share redistributed is smaller when the source of performance differential is due to educational opportunities than when it is due to employment opportunities (8.0% vs. 13.2%, $p = 0.007$ from the post-estimation F-test). Finally, the pattern is the same regardless of the opportunity to seek information about whether unequal opportunities are present. In Table A1 of Appendix A, we show that these results are robust to multiple hypothesis testing adjustments.

Table 3: Regression results on share redistributed

	(1)	(2)	(3)
Merit	-0.199***	-0.118***	-0.199***
	(0.017)	(0.017)	(0.017)
Random-Education	-0.080***	-0.073***	-0.078***
	(0.018)	(0.015)	(0.018)
Random-Employment	-0.132***	-0.092***	-0.134***
	(0.018)	(0.016)	(0.018)
Info-rEducation	-0.051***	-0.043***	-0.050***
	(0.017)	(0.015)	(0.017)
Info-rEmployment	-0.102***	-0.063***	-0.102***
	(0.017)	(0.016)	(0.017)
Procedural fairness attitude		-0.077***	
		(0.004)	
Female			0.012
			(0.010)
Age			0.000
			(0.000)
High education			0.015
			(0.011)
High income			-0.022**
			(0.011)
Conservative			-0.033**
			(0.013)
Constant	0.421***	0.593***	0.412***
	(0.012)	(0.012)	(0.020)
Observation	1260	1260	1260
R^2	0.103	0.347	0.114
H0: Merit = Random-Education	$p < 0.001$	$p = 0.005$	$p < 0.001$
H0: Merit = Random-Employment	$p < 0.001$	$p = 0.094$	$p < 0.001$
H0: Random-Education = Random-Employment	$p = 0.007$	$p = 0.231$	$p = 0.003$
H0: Random-Education = Info-rEducation	$p = 0.106$	$p = 0.047$	$p = 0.118$
H0: Random-Employment = Info-rEmployment	$p = 0.114$	$p = 0.070$	$p = 0.089$

Note: The table reports OLS regression results on share redistributed by spectators. The Luck treatment serves as the reference category. "Procedural fairness attitude" is spectators' self-reported attitude about the allocation rule in their treatment. "High income" is an indicator variable for having yearly income higher than $50,000. "High education" is an indicator variable for having 4-year college education or higher. "Conservative" is an indicator variable for having selected either Republican or Libertarian as their political party/stance most typically supported. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In columns (2) and (3) we additionally control for a measure about procedural fairness attitudes and individual spectator characteristics. The measure about procedural fairness is spectators' self-reported attitude about the initial allocation rule in their treatment. The specific question they were asked is the following: "For the initial distribution of payments between worker A and worker B, how fair did you think it was?" The possible answers range from 1 to 5 with 1

indicating "very unfair" and 5 indicating "very fair". The procedural fairness attitude question is intended to provide a hint regarding how closely inequality in the presence of unequal opportunities is perceived as merit as opposed to luck—that is, this attitude can be considered as a noisy measure of the parameter α in our theoretical framework. Therefore, if this measure can fully capture α, we should expect to observe no treatment difference between the Merit treatment and treatments with unequal opportunities after controlling for this variable. Figure A3 in Appendix A shows the average level of procedural fairness attitudes across all treatments. We find the overall pattern in procedural fairness attitudes closely tracks the actual share redistributed, indicating that the more unfair spectators rated the situation, the more they tended to redistribute. Indeed, estimates from column (2) show that including the procedural fairness attitudes in the regression substantially improves the overall fit of the model from an R-squared of 0.103 to 0.347. Notably, in this specification, the estimated differences across treatments are smaller, and the difference in the redistributed share between the Random-Education and Random-Employment treatments is no longer significant (bottom of Table). Moreover, the difference between Merit and Random-Employment is very close to being insignificant ($p = 0.094$). These findings incorporating the procedural fairness attitudes indicate that spectators' attitudes or beliefs play a significant explanatory role in their merit judgments. However, they still fall short of fully explaining the observed treatment differences. Finally, compared to column (1), the estimates are virtually unaffected when we control for individual characteristics as shown in column (3).

4.2. Spectators' information-seeking decisions

In the two treatments with information-seeking, the other outcome variable of interest is whether spectators spend effort in the number-checking task to reveal information about unequal opportunities. In the Info-rEducation treatment, we observe that 54.3% of spectators chose to work on the number-checking task and 48.6% of spectators successfully completed the task and were thus informed about whether unequal opportunity is present. Similarly, the corresponding percentages are 51.4% and 46.2% in the Info-rEmployment treatment.[10] Thus, around 50% of spectators did sufficiently care about fairness and the possibility that merit might be compromised by unequal opportunities to exert real effort to discover this information.

It is worth noting that spectators holding egalitarian or libertarian fairness views in principle lack incentives to seek the information since they would either completely equalize the income or

[10] To explore factors contributing to this information-seeking decision, in Table A2 of Appendix A we report a regression analysis of this decision on spectators' self-reported procedural fairness and characteristics variables. The estimates show that in the Info-rEducation treatment spectators who expressed more negative views about procedural fairness were more likely to work on the number-checking task. However, this effect is not observed in the Info-rEmployment treatment.

not redistribute at all, regardless of the opportunity condition. The share of egalitarians can be inferred from the share of spectators dividing equally in the Merit treatment; the share of libertarians can be inferred from the share of spectators allocating everything to the lucky worker in the Luck treatment. In our data the share of each type is 11.4% and 9.1% respectively. Therefore, assuming the distribution of fairness types is constant across treatments, even if every egalitarian and libertarian selected to skip the number-checking task, the remaining 30% who did not seek information could only be meritocrats who were expected to make their merit judgments based on the true state of the opportunity condition.

Next, we test whether being informed about the presence of unequal opportunities affects spectators' redistributive decisions. Table 4 reports the regression results on the share redistributed conditional on whether spectators were informed of workers' opportunities obtained or not. In columns (1) and (2), we test for the information effect in the Info-rEducation treatment by using the Random-Education treatment as the reference category. The estimates in column (1) indicate that spectators who became aware of unequal opportunities by choosing to learn about this information, redistributed 4.3% more than those who directly learned about this information by experimental design ($p = 0.053$). Surprisingly, spectators who remain uncertain about the presence of unequal opportunities did not redistribute less than those in the Random-Education treatment ($p = 0.291$). Column (2) shows that by additionally controlling for procedural fairness perceptions and individual characteristics, spectators' redistribution decisions do not significantly predict whether they learned about the presence of unequal opportunities, or whether the information was actively or passively learned.

Similarly, in columns (3) and (4) we test for the information effect in the Info-rEmployment treatment by using the Random-Employment treatment as the reference category. The estimates in both columns suggest that spectators' redistributive decisions do not depend on whether they learned about the presence of unequal opportunities or whether the information was actively or passively learned.

In Table A3 of Appendix A, we show that these results are largely robust to multiple hypothesis testing adjustments. The one exception is that the small difference in behavior between informed spectators in the Info-rEducation treatment and those in the Random-Education treatment is no longer significant even without other control variables.

Table 4: Regression results on share redistributed for informed and uninformed spectators

	(1)	(2)	(3)	(4)
Info-rEducation	0.043*	0.021		
	(0.022)	(0.018)		
Info-rEmployment			0.032	0.027
			(0.024)	(0.021)
Uninformed	-0.026	0.014	-0.004	0.004
	(0.025)	(0.021)	(0.026)	(0.022)
Procedural fairness		-0.090***		-0.080***
		(0.007)		(0.007)
Female		0.016		0.006
		(0.015)		(0.016)
Age		0.001		-0.000
		(0.000)		(0.005)
High education		0.005		-0.001
		(0.017)		(0.017)
High income		-0.007		0.003
		(0.017)		(0.016)
Conservative		-0.028		-0.006
		(0.019)		(0.022)
Constant	0.341***	0.255***	0.290***	0.283***
	(0.013)	(0.026)	(0.014)	(0.031)
Observation	420	420	420	420
R^2	0.009	0.343	0.006	0.301

Note: The table reports OLS regression results on share redistributed by spectators. In columns (1) and (2) the Random-Education treatment serves as the reference category. In columns (3) and (4) the Random-Employment treatment serves as the reference category. "Procedural fairness attitude" is spectators' self-reported attitude about the allocation rule in their treatment. "High income" is an indicator variable for having yearly income higher than $50,000. "High education" is an indicator variable for having 4-year college education or higher. "Conservative" is an indicator variable for having selected either Republican or Libertarian as their political party/stance most typically supported. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To summarize, despite the fact that only around 50% of the spectators were willing to expend effort to learn about the presence of unequal opportunities, in practice when making redistribution choices, they behaved as if they knew for sure about its presence. These results suggest both good and bad news. The good news is that people tend to assume the "worse" scenario and take the presence of unequal opportunities into account when making their redistributive decisions. However, we caution that this result might be due to the salience of the possibility of unequal opportunities in our experimental design. In real-world scenarios, the fact about whether unequal opportunities are present or not is more hideous such that its possibility may not easily come into people's mind. Furthermore, the fact that some pairs of workers faced with equal opportunity were treated as if they were presented with unequal opportunities could be just another incidence of unfairness. On the other hand, the bad news is that a substantial proportion of spectators did not

appear to truly care about finding information about potential unequal opportunities. Given that the redistribution levels are indistinguishable among those who sought information compared to those who declined to do so, it suggests a substantial fraction of individuals simply feel comfortable redistributing without knowing whether the original inequality was due to unequal opportunity or other reasons. This could raise concerns about whether individuals truly care about equalizing outcomes based on the actual sources of inequality in society.

4.3. Heterogeneity analysis

As specified in our pre-analysis plan, we conduct an analysis of the redistributive choices of different subgroups of spectators along the following demographic dimensions: gender, education, income and political identity.

We first discuss the previously observed subgroup differences in average share redistributed. Column (3) in Table 3 shows that conservatives tend to redistribute significantly less than non-conservatives (30.2% vs. 33.6%, $p = 0.012$). Figure A4 in Appendix A shows that the political party difference is noticeable in all treatments except for the Luck treatment. We also observe some weaker evidence that spectators with high income tend to redistribute less than those with relative lower income (31.7% vs. 33.8%, $p = 0.050$), which is consistent with a self-serving bias in people's fairness perceptions. By contrast, we find no evidence of association between gender and share redistributed. Education level also does not appear to matter in redistributive decisions. In Table A4 of Appendix A, we show that these results are largely robust to multiple hypothesis testing adjustments, with the exception that the small difference in behavior associated with income level is no longer statistically significant.

Next, we test subgroup differences across all treatments based on gender, education, income and political affiliation. Table 5, which incorporates dummy variables for subgroups based on these factors, shows that the treatment effects are generally consistent across subgroups. In almost all subgroups, merit as the source of inequality, whether it is in its purest form (in the Merit treatment) or partly compromised by unequal opportunities, results in a significantly lower redistributed share than luck. The exceptions are that redistributed shares by males or spectators with relatively low income level do not significantly differ between the Info-rEducation and Luck treatments. Table A5 in Appendix A (column showing unadjusted p-values) further shows that for other treatment comparisons, the effects are also generally consistent across subgroups.[11]

[11] In Table A5 of Appendix A, we show that these results are largely robust to multiple hypothesis testing adjustments but with important exceptions. For all subgroups except for non-conservatives, the difference between the Random-Education and Random-Employment treatments is not statistically significant. The difference between the Random-

Table 5: Heterogeneity analysis on share redistributed

	(1) Gender (D = 1 if Female)	(2) Education (D = 1 if High)	(3) Income (D = 1 if High)	(4) Political (D = 1 if Conservative)
Merit	-0.180***	-0.200***	-0.174***	-0.192***
	(0.028)	(0.024)	(0.023)	(0.019)
Random-Education	-0.068***	-0.085***	-0.066***	-0.064***
	(0.026)	(0.025)	(0.024)	(0.019)
Random-Employment	-0.111***	-0.145***	-0.111***	-0.127***
	(0.028)	(0.027)	(0.025)	(0.019)
Info-rEducation	-0.026	-0.055**	-0.036	-0.041**
	(0.027)	(0.025)	(0.025)	(0.018)
Info-rEmployment	-0.068***	-0.103***	-0.090***	-0.096***
	(0.026)	(0.024)	(0.023)	(0.018)
Merit × D	-0.035	0.000	-0.047	-0.024
	(0.035)	(0.035)	(0.035)	(0.044)
Random-Education × D	-0.020	0.008	-0.029	-0.054
	(0.036)	(0.035)	(0.035)	(0.046)
Random-Employment × D	-0.039	0.022	-0.042	-0.025
	(0.036)	(0.036)	(0.035)	(0.046)
Info-rEducation × D	-0.046	0.008	-0.028	-0.034
	(0.034)	(0.034)	(0.034)	(0.042)
Info-rEmployment × D	-0.063*	0.002	-0.024	-0.026
	(0.035)	(0.034)	(0.034)	(0.046)
D	0.046*	0.003	0.010	-0.006
	(0.023)	(0.023)	(0.023)	(0.032)
Constant	0.396***	0.420***	0.417***	0.423***
	(0.019)	(0.016)	(0.016)	(0.012)
Observation	1260	1260	1260	1260
R^2	0.107	0.104	0.107	0.111
Merit (D = 1)	-0.215***	-0.199***	-0.221***	-0.216***
	(0.022)	(0.025)	(0.026)	(0.040)
Random-Education (D = 1)	-0.088***	-0.076***	-0.095***	-0.118***
	(0.024)	(0.025)	(0.026)	(0.042)
Random-Employment (D = 1)	-0.149***	-0.123***	-0.154***	-0.152***
	(0.023)	(0.024)	(0.025)	(0.042)
Info-rEducation (D = 1)	-0.072***	-0.048**	-0.064***	-0.075**
	(0.021)	(0.023)	(0.023)	(0.038)
Info-rEmployment (D = 1)	-0.131***	-0.102***	-0.114***	-0.122***
	(0.023)	(0.024)	(0.025)	(0.043)

Note: The table reports OLS regression results on share redistributed by adding interactions with subgroups of spectators. The Luck treatment serves as the reference category. “High income” is an indicator variable for having yearly income higher than $50,000. “High education” is an indicator variable for having 4-year college education or

Education and Luck treatments and the difference between the Random-Employment and Merit treatments are also not significant for some subgroups such as males.

higher. "Conservative" is an indicator variable for having selected either Republican or Libertarian as their political party/stance most typically supported. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.4. Supporting evidence about external validity from workers' questionnaire

We now turn to the survey responses and data generated by workers. We note that due to the features of our experiment design, workers' performance on the knowledge evaluation itself is of limited interest and insight. While workers were informed that a third-party spectator would potentially redistribute the earnings based on information they received about workers' conditions and performances, due to the limited and one-directional interaction between workers and spectator, workers still found it in their best interest to perform as best they could on the knowledge evaluation.

Before workers started to work on their main task, they were asked to answer a question about a brief hypothetical scenario, which is designed to shed light on the external validity of our main experiment. In other words, were our findings on spectators' redistribution choices specific to our worker-spectator setup and the performance task at hand, or does the experiment successfully detect underlying perceptions and preferences about redistribution under unequal opportunity? Workers were randomly assigned to one of the six hypothetical scenarios described as follows, independently of the treatment scenario they were assigned to as workers in the main experiment.[12] The exact text of each scenario is presented in the experimental protocol for workers in Appendix B. We deliberately placed the hypothetical scenarios chronologically before the performance task for the main experiment, so that workers' answers to the hypothetical scenarios would not be potentially affected by their experiences in the knowledge assessment task. Meanwhile, our experimental findings, which focus on spectators' decisions, are essentially unaffected by workers performances in the knowledge evaluation.

Merit-Training Scenario: Two employees in a company are requested to take a certification test after participating in the same training program. Both employees pass the test but one obtains a higher score. The company then awards a bonus of $600 to the employee for his high score obtained on the certification test.

Random-Training Scenario: The background story is similar to the Merit-Training scenario. However, one employee is randomly selected to participate in a new and improved training program while the other is still enrolled in the ordinary training program. Both of them

[12] The workers' characteristics across all six scenarios are summarized in Table A6. A balance test produces a p-value of 0.476, indicating that the overall balance is achieved.

pass the test but the former employee obtains a higher score and is awarded a bonus of $600 by his company.

Merit-Department Scenario: Two employees work in the same department of a company with the same working conditions and client base. One employee completes a higher number of reports than the other. The company then awards a bonus of $600 to the employee for his superior job performance.

Random-Department Scenario: The background story is similar to the Merit-Department scenario 3. However, in their initial employment assignments, one employee is randomly placed into a department which serves a large client base. The other is randomly placed into a department which serves a moderate-sized client base. The former employee completes a higher number of reports and is awarded a bonus of $600 by his company.

In the first four scenarios, workers were asked to indicate whether and how they would reallocate the $600 bonus with the two employees if they were hypothetically able to do so. The Merit-Training and Random-Training scenarios are designed to provide a more realistic real-world scenario mirroring the learning opportunities in the Merit and Random-Education treatments, while the Merit-Department and Random-Department scenarios are designed to embed the feature about performance or job opportunities in the Merit and Random-Employment in a real-world scenario beyond our implemented experiment. Note that we conduct two scenarios (Merit-Training and Merit-Department) that mimic the Merit treatment in order to provide a benchmark for the Random-Training and Random-Department scenarios with unequal opportunities, respectively. This is important for comparison purposes because the background story is different in these two scenarios.[13]

We have two additional scenarios mimicking the two treatments with information-seeking in our main experiment:

Info-Training Scenario: The background story is similar to the Random-Training scenario. However, the manager who is in the position to award the $600 bonus to higher performer on the certification test does not actually know whether one of the employees in fact attended the new and improved training program while the other attended the ordinary program.

Info-Department Scenario: The background story is similar to the Random-Department scenario. However, the manager who is in the position to award the $600 bonus to higher performer

[13] We did not include a scenario that mimics the Luck treatment because it seems difficult to come up with a realistic scenario in which pure luck determines the initial allocation of the bonus.

on their job does not actually know whether one of the employees was in fact assigned to a different department than the other.

In the Info-Training and Info-Department scenarios, workers were asked to indicate their belief or opinion about what percentage of managers would check the relevant information (i.e., the training program history or department assignment) before deciding about how to award the bonus. Since we are mainly interested in subjects' perceptions about the information-seeking, we do not additionally ask them to reallocate the bonus. Also note that the question we asked in the hypothetical scenarios can be interpreted as the *perceived* social norm in information-seeking in such scenarios, while in our main experiment, spectators made an individual information-seeking decision.

We first discuss the results from the first four scenarios. Figure 2 shows the average share redistributed by workers across these hypothetical scenarios. We observe remarkably quantitatively similar results compared to the actual decisions in our main experiment. In the Merit-Training and Random-Training scenarios, the average redistributed share is 20.9% and 34.9% respectively. The difference is statistically significant ($p < 0.001$, Wilcoxon ranksum test).[14] Similarly, in the Merit-Department and Random-Department scenarios, the average redistributed share is 18.8% and 35.0%, respectively ($p < 0.001$).[15]

It is however worth noting that, somewhat differently from the findings in our main experiment, the redistributed shares are very close in the two scenarios with different types of unequal opportunities. One possibility is the time compensation for MTurk workers in our main experiment. Spectators may assume that the disadvantaged worker who got fewer knowledge questions to answer was able to complete the task faster, and thus needs less compensation. Presumably, this argument is less relevant in the Random-Department scenario in which a smaller client base does not necessarily imply less time or effort spent on the work.

[14] Figure A5 in Appendix A shows the distribution of the redistributed share in these four scenarios. Complete equalization is the modal behavior in the Random-Training scenario, representing for 47.9% of all observations. However, only 13.4% equalize the total income in the Merit-Training scenario. These numbers are remarkably similar to what we observe in the spectators' decisions.

[15] As shown in Figure A5, complete equalization accounts for 35.4% of all observations in the Random-Department scenario, while merely 6.7% equalize the total income in the Merit-Department scenario.

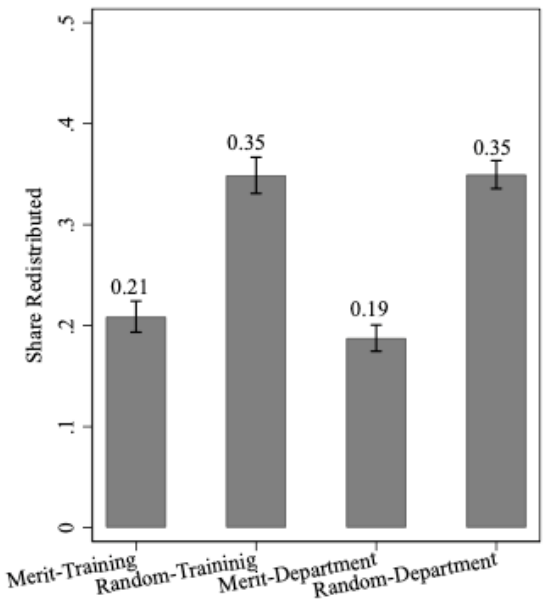

Figure 2: Share redistributed (hypothetical scenarios)

Note: The figure shows the average share redistributed by the workers in each of the four hypothetical scenarios in their questionnaire. Standard errors are indicated by the bars.

Next, we turn to the Info-Training and Info-Department scenarios to assess the perceived social norm regarding information-seeking about unequal opportunities. The workers reported that on average 61.3% and 71.5% of managers would check the information of potential unequal opportunities in the Info-Training and Info-Department scenarios, respectively. These numbers are higher than the actual rate of information-seeking in our experiment (around 50%). We can draw a few observations from the results of these two scenarios. Firstly, while the percentage of managers that workers thought would check the unequal opportunity information is quite high, workers did not generally believe that all managers would check the information.[16] In addition, given that only (50%) of spectators actually checked the information in the main experiment, workers' impressions about managers' due diligence may be overly optimistic. On the other hand, the discrepancy could reflect the potential difference between real world managers and the spectators in our experiment. In any case, the relatively high percentage quoted by workers in these information seeking scenarios indicates that individuals seem to trust most supervisors to make fair and informed compensation decisions.

[16] Figure A6 in Appendix A shows the distribution of the social norm regrading information-seeking in these two scenarios. 24.5% and 30.3% of subjects indicated that at least 80% of managers will check the information in the Info-Training and Info-Department scenarios, respectively.

Overall, the workers' answers across the hypothetical scenarios help to confirm the external validity of the findings in our main experiment. Specifically, people do consider the impact of unequal opportunities when deciding upon the fair allocation of total earnings, and the allocation result lies in between that of merit and luck alone. However, in the main experiment a large proportion of our subjects do not seem to care enough about the information of unequal opportunities when the information can only be obtained at some cost. When evaluating the hypothetical scenarios, workers also do not believe everyone would choose to go through the trouble to obtain such information, while they do seem to maintain an optimistic belief about this possibility.

5. Discussion and Conclusion

Unequal opportunities permeate almost every aspect of society, and inspire intense intellectual debates regarding the morality of such situations and their often negative impact on our society. Some of them, such as inherited wealth, even have a direct impact on income inequality (Bastani and Waldenström 2021; Lekfuangfu, Powdthavee, and Riyanto 2022). Compared to more obvious potential disadvantages that individuals may face in society, unequal opportunities play a more subtle but profound role in influencing their earnings prospects through, for example, early-life education (Falk et al. 2021) and homophilic job-related networks (Jackson, 2021).

To see how much importance individuals attach to some of the commonly-discussed sources of inequality, in our post-experiment survey for workers in our experiment, we asked participants to indicate their beliefs about the importance of each of five factors in causing socio-economic inequality. These factors were education ("some people have better educational opportunities than others"), job opportunities ("some people have access to better job opportunities than others"), hard work ("some people work harder than others"), luck ("some people have better luck than others"), and inheritance ("some people have inherited money from their family, giving them a head start compared to others"). For each factor, subjects rated its perceived importance from 1 to 10, with 1 indicating "not important at all" and 10 indicating "extremely important". Figure 3 shows the average rating for each factor. Interestingly, and consistently with the overall research question of our study, we observe that unequal educational and job opportunities are considered to be more important than either hard work or luck (for each pairwise comparison, $p < 0.001$, Wilcoxon signed-rank test). Meanwhile, inheritance is considered slightly less important than educational opportunities ($p = 0.009$), but more important than hard work ($p < 0.001$). This emphasis on education agrees with a global survey by Pew Research Center, in which people rated education as the most important factor for getting ahead in life (Pew Research Center 2014).

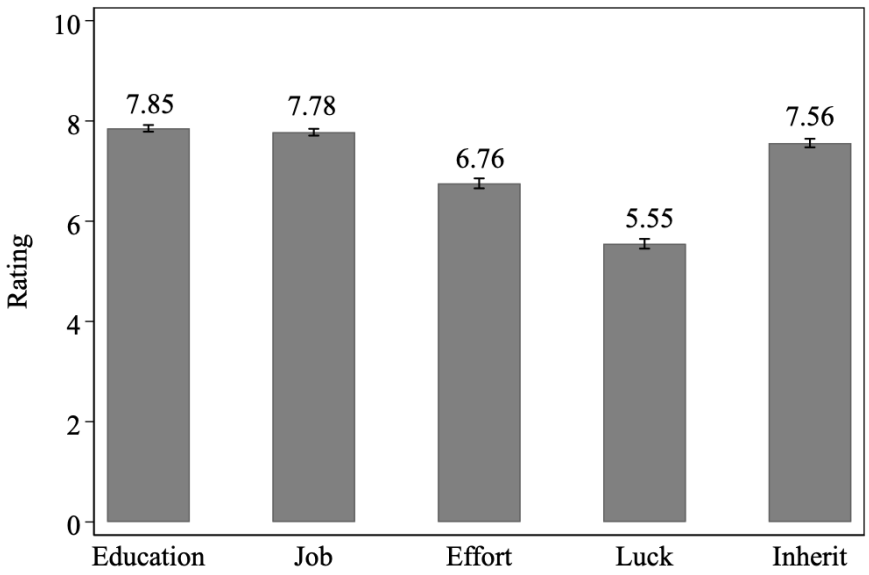

Figure 3: Rating of factors contributing to socio-economic inequality

Note: The figure shows the average rating of the importance of each factor in causing socio-economic inequality. Standard errors are indicated by the bars.

Despite the perceived importance of unequal opportunities for our society at large, thus far there has been little scientific assessment of individuals' fairness perceptions and redistribution preferences toward circumstances involving unequal opportunities. This line of investigation is important because we must first understand and anticipate the reaction of the public toward any potential corrective policy that aims to address social problems caused by unequal opportunities before putting it into practice. Our study helps to fill this gap. Conceptually, unequal opportunity can be understood as a form of underlying luck, which occurs prior to the actual performance evaluation. In our treatment of unequal educational opportunities, participants with the relevant learning materials represent the luck of individuals born into wealthier families that can afford high-quality education. In our treatment of unequal employment opportunities, participants with better job opportunities emulate the luck of facing better labor market conditions. To what extent people perceive inequality resulting from unequal opportunities as unfair is ultimately an empirical question. Our experiment helps shed light on this issue.

In our experiment, which utilizes participants representative of the U.S. population, we exogenously manipulate the educational or employment opportunities presented to one of the paired workers, without altering their incentives to perform in their task. In all but one treatment (the luck treatment), subjects' relative task performances, which are affected by the unequal opportunities, determines the initial income distribution. Therefore, the third-party spectators face the same redistribution decision with this identical initial allocation rule, an identical level of initial

income inequality, and a pair of workers with identical incentives to learn and work. A distinction between our experiment and others in the literature, is that we phrase the description of the initial allocation procedure and the third-party potential reallocation in such a way in order to preserve the incentives for hard work across workers (we also test spectators' understanding of this feature through the comprehension quiz), regardless of whether they are randomly assigned the high or low opportunity condition.

We find that both unequal educational opportunities and unequal employment opportunities lead spectators to reallocate in favor of more equal payment distributions compared to the benchmark Merit treatment (equal opportunity). However, the amount redistributed is not as large as in the benchmark Luck treatment, in which pure luck determines the initial income inequality. Based on the average amount redistributed by treatment, inequality stemming from unequal opportunities is considered unfair but not fully equivalent to pure luck. In fact, based on redistribution outcomes, we find that unequal education opportunities are perceived as closer to pure luck in terms of deservingness of redistribution than unequal employment opportunities. We also find that spectators' redistribution decisions largely ordinally match their stated procedural fairness attitudes towards the initial allocation rule in each treatment.

These findings present a challenge to the original definition of meritocratic fairness views (e.g., Cappelen et al. 2007; Almås, Cappelen, and Tungodden 2020), which thus far has not incorporated the impacts of different sources of performance differentials. An enhanced definition needs to accommodate the fact that people appear to only partially equate unequal opportunities with pure luck and the degree of equivalence also depends on the finer details of each situation. Andre (2022) takes a first step in this direction by distinguishing between two different types of meritocrats: actual choice meritocrats and comparable choice meritocrats. The former type holds the disadvantaged workers accountable for their low performance, because relative performance is the only verifiable information. By contrast, the latter type engages in counterfactual reasoning by considering what the disadvantaged worker would achieve had she been in the role of the advantaged worker. In our main experiment as well as in our hypothetical real-world scenarios, although such a counterfactual is carefully implied in the descriptions, some participants may still fail to engage in counterfactual reasoning or simply prefer to base merit judgments on verifiable information, which in the context of our study is workers' relative performances. However, this more detailed classification of meritocrats cannot fully explain why merit judgments are still significantly different between Random-Education and Random-Employment in our experiment.

Alternatively, we may try to use spectators' attitudes or beliefs to account for our findings without introducing additional fairness types. Our results regarding spectators' procedural fairness attitudes provide a hint at this possibility. We do observe that their procedural fairness attitudes

closely mirror their redistribution decisions. As a result, they explain a significant portion of variation in redistribution decisions across treatments. More importantly, once these attitudes are controlled for, the estimated treatment difference between Random-Education and Random-Employment is no longer significant. These findings imply that, perhaps in addition to inherent redistributive preferences displayed by individuals with different social preferences, procedural fairness attitudes also play a significant role in merit judgments. More fundamentally, any type classification of meritocrats may need to be combined with beliefs or attitudes about what constitutes fairness in order to more fully explain behavior. In future research, it would be valuable to further explore the role of attitudes and/or beliefs in merit judgments under situations involving unequal opportunities. For example, we could perhaps specifically measure individuals' beliefs about how unequal opportunities affect income inequality in the general population (not just for a specific pair of workers), and their views about the extent to which unequal opportunities could be attributed to pure luck. However, to design such an extension of our current work which can potentially more deeply explain the differences in redistribution across treatments, requires careful consideration of how to properly incentivize belief elicitations in this context while simultaneously avoiding false consensus effects.

In real life, the presence of unequal opportunities is often not immediately observable due to either their indirect role in influencing income as mentioned earlier or perhaps their socially uncomfortable nature that often makes advantaged people purposefully hide this fact from outsiders. In our treatments where spectators needed to expend effort to reveal information about the presence of unequal opportunities, we find that a substantial proportion of them do not appear to sufficiently care about the source of performance differential and thus decline to reveal such information. Even though their redistribution decision does not appear to be affected by the information, it still raises concerns about whether people truly care about equalizing outcomes based on the actual sources of inequality in society. More research, however, is needed to gain a better understanding of the effect of information on people's redistribution preferences beyond our relatively abstract environments.

Recall that in Figure 3 we show that people believe unequal educational and job opportunities are perhaps the most important causes of socio-economic inequality. It is, however, somewhat contradictory that spectators (drawn from an identical subject pool as workers) only investigated the source of inequality in the main experiment with the same likelihood as a coin toss, when they had to expend effort to do so. Furthermore, in real-world scenarios, workers assumed that a relatively high fraction of supervisors would investigate the sources of workers' unequal opportunities. These contradictions are worrisome in that they suggest a passive role of decision-makers in getting to the bottom of the sources of inequality, while they simultaneously

think inequality in opportunity is very important and believe others will do the work to bring fairness to unequal situations.

One of the practical implications of the findings in this paper pertains to the implementation of technology and automation in making business decisions, such as deciding which workers to hire. Given that worker performance is path dependent upon past performance, initial advantages such as being born into a better neighborhood with better primary schools (which in turn may depend on one's parents' performance or luck in their careers), can be amplified. As another example, an algorithm could recommend candidates to employers by matching their characteristics with past successful candidates with similar characteristics (who are known as "Doppelgangers"). However, since this algorithm often ignores factors related to unequal opportunities that contribute to the past success of those Doppelgangers, decisions made based on this algorithm alone would be incapable of guaranteed fair assessments of the opportunity factors we consider in this study.[17] This unending cycle of path-dependent job seeking and matching could severely constrain social mobility, while potentially contributing to more polarized attitudes toward redistributive policies due to heterogeneous knowledge by citizens about the true nature of such algorithms, particularly under potentially inaccurate assumptions that automated processes are objective and thus fair. This also highlights the importance of cultivating a public understanding of the content of decision algorithms, and ensuring that citizens do not automatically adopt the assumption that computerized decisions are by nature fair and just. An important avenue for future research is to better understand people's fairness attitudes in distributive contexts which involve AI-influenced unequal opportunities.

Finally, while our current study has focused on redistributive attitudes in the context of one worker being distinctly *disadvantaged* through lower opportunity than the other worker, a potential future direction for further research is to examine attitudes towards one worker being distinctly *advantaged* over the other worker through different possible sources of better and 'unfair' opportunities. Some possible sources of advantage which could be simulated in an experimental setting include bribery/corruption, cheating and nepotism, which similarly to our cases of educational and employment opportunity examined here, represent common phenomena in the real world that contribute to generating inequality through altering the opportunities of an individual. Such a direction can help extend the objective of our line of inquiry, which seeks to better understand preferences for redistribution under heterogeneity in underlying opportunity.

[17] https://www.brookings.edu/research/fairness-in-algorithmic-decision-making/

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Online Appendix

A. Additional Figures

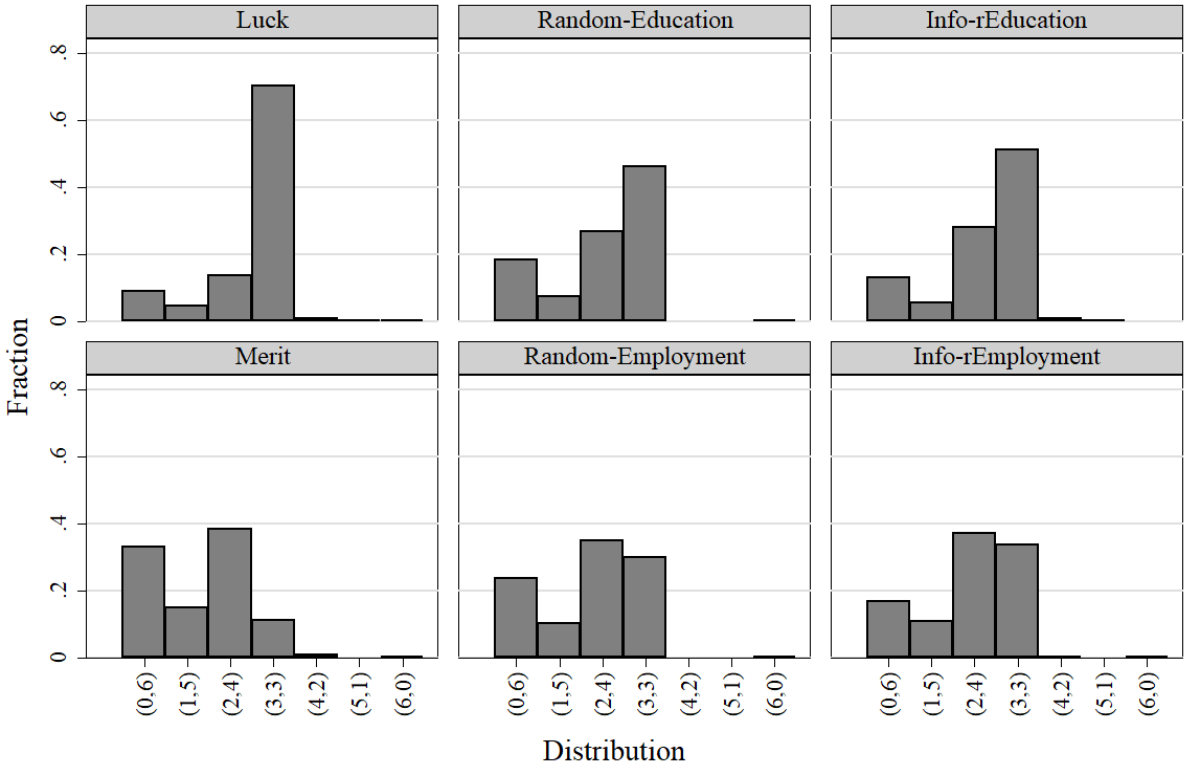


Figure A1: Distribution of the spectators' decisions

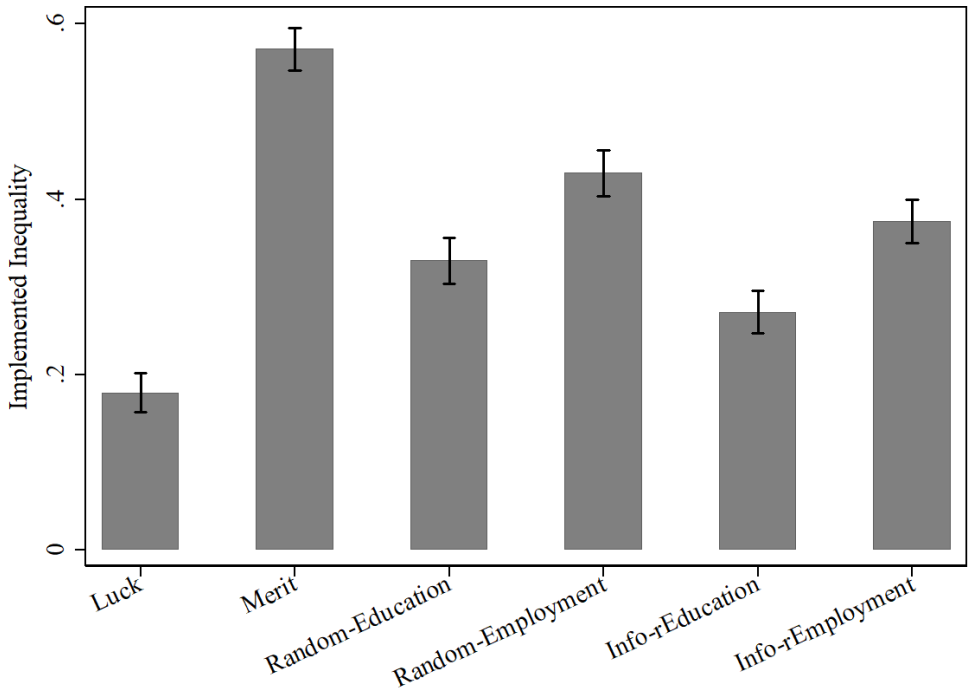


Figure A2: Implemented inequality

Note: The figure shows the average level of implemented inequality in each of the six treatments. Standard errors are indicated by the bars.

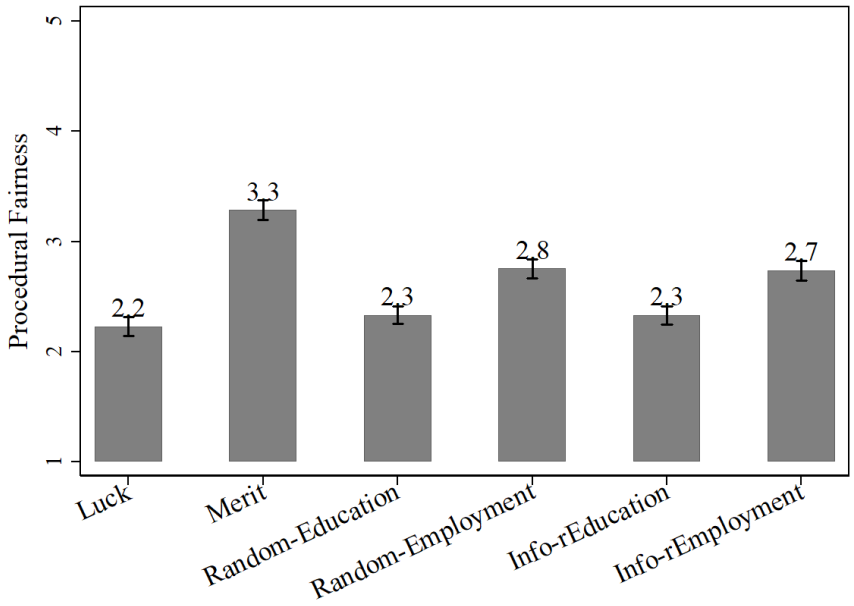


Figure A3: Self-reported procedural fairness attitude

Note: The figure shows the average level of self-reported procedural fairness attitude in each of the six treatments. The data is collected from the spectators' post-experimental questionnaire item: "for the initial distribution of payments between worker A and worker B, how fair did you think it was?" 1 indicates "very unfair" while 5 indicates "very fair". Standard errors are indicated by the bars.

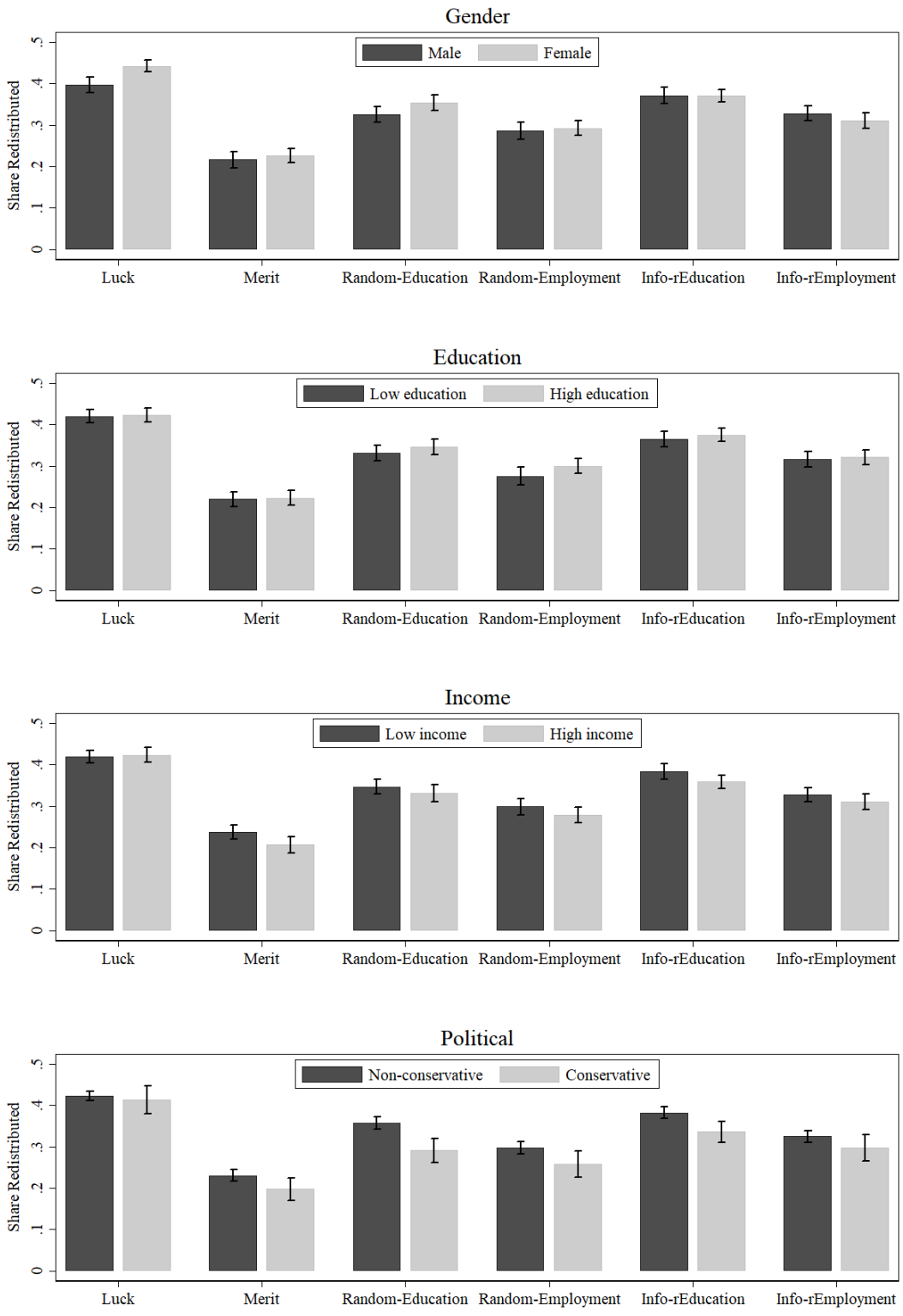


Figure A4: Share redistributed in subgroups

Note: The figure shows the average share redistributed by the spectators for each subgroup in each of the six treatments. Standard errors are indicated by the bars.

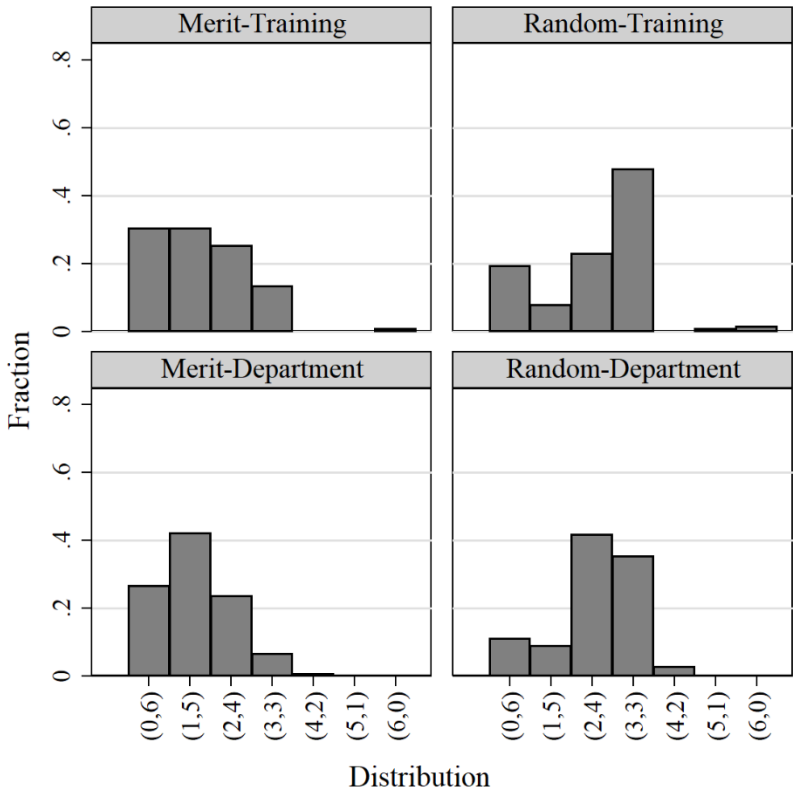


Figure A5: Distribution of the workers' redistributive decisions in the hypothetical scenarios

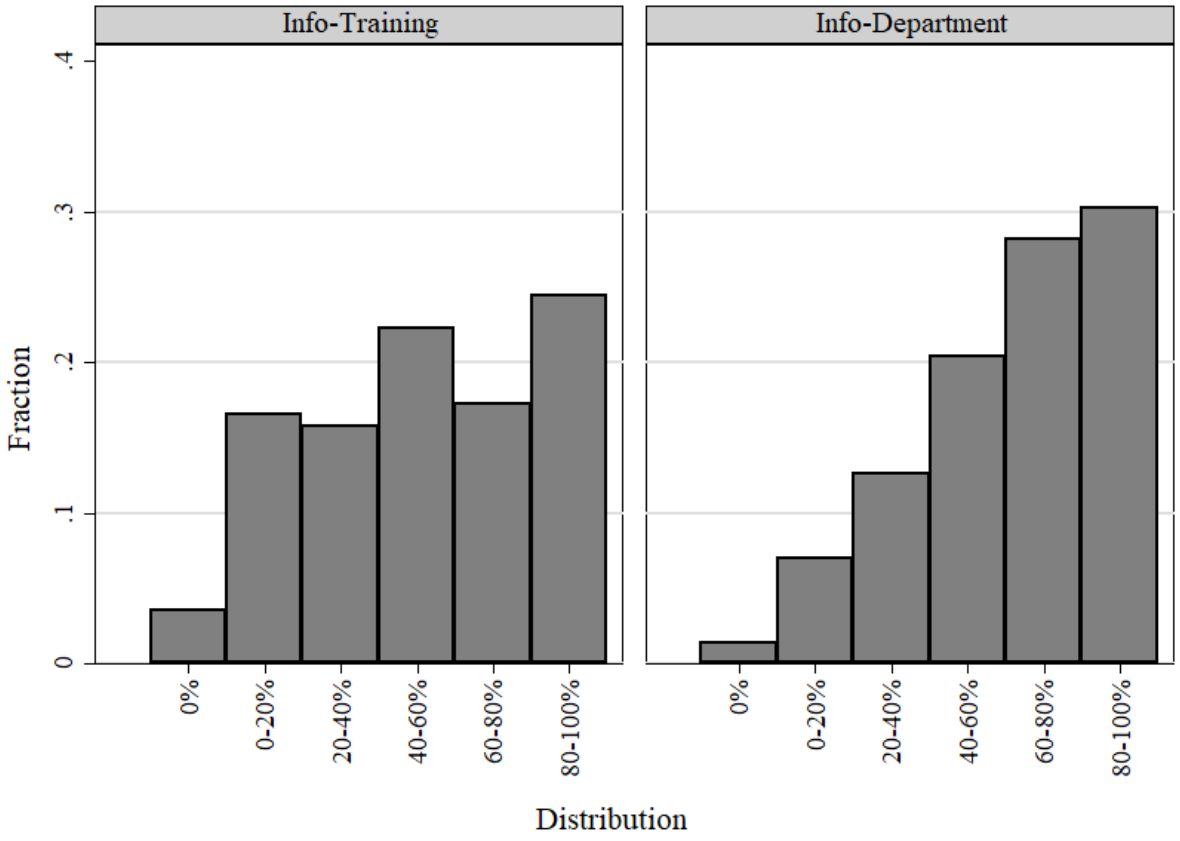


Figure A6: Distribution of the workers' perceived social norm in information seeking in the hypothetical scenarios

Table A1: Treatment effects: *p*-values with multiple hypothesis testing adjustments

	Difference	Multiple testing adjustment			
		Unadjusted p-values	Bonferroni p-values	Holm p-values	List et al. p-values
Merit vs. Luck	-0.199	0.000	0.003	0.002	0.000
Random-Education vs. Luck	-0.080	0.000	0.003	0.003	0.000
Random-Employment vs. Luck	-0.132	0.000	0.003	0.002	0.000
Random-Education vs. Merit	0.119	0.000	0.003	0.002	0.000
Random-Employment vs. Merit	0.067	0.001	0.008	0.004	0.003
Random-Education vs. Random-Employment	0.052	0.007	0.053	0.020	0.019
Random-Education vs. Info-rEducation	-0.029	0.109	0.875	0.219	0.208
Random-Employment vs. Info-rEmployment	-0.029	0.115	0.920	0.115	0.115

Note: List et al. (2019) p-values are produced using Stata command "mhtreg", which allows the testing procedure to be used in multivariate regressions (Steinmayr 2020). The underlying regressions are estimated using OLS with robust standard errors in which "Difference" refers to the coefficient estimate of each comparison.

Table A2: Determinants of information-seeking behavior

	Info-rEducation	Info-rEmployment
Procedural fairness attitude	-0.078**	0.005
	(0.030)	(0.027)
Female	0.060	-0.109
	(0.071)	(0.070)
Age	-0.001	-0.002
	(0.002)	(0.002)
High education	0.075	0.124
	(0.072)	(0.076)
High income	0.021	0.104
	(0.071)	(0.076)
Conservative	0.087	0.028
	(0.077)	(0.081)
Constant	0.436***	0.526***
	(0.113)	(0.124)
Observation	210	210
R^2	0.046	0.051

Note: The table reports OLS regression results on determinants of information-seeking behavior in the two treatments with information-seeking. The binary dependent variable is 1 if a spectator chose to do the number-checking task. "Procedural fairness attitude" is spectators' self-reported attitude about the allocation rule in their treatment. "High income" is an indicator variable for having yearly income higher than $50,000. "High education" is an indicator variable for having 4-year college education or higher. "Conservative" is an indicator variable for having selected either Republican or Libertarian as their political party/stance most typically supported. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Information effects: *p*-values with multiple hypothesis testing adjustments

	Difference	Multiple testing adjustment			
		Unadjusted p-values	Bonferroni p-values	Holm p-values	List et al. p-values
Informed vs. Random-Education	0.043	0.057	0.228	0.228	0.205
Uninformed vs. Random-Education	0.017	0.455	1.000	0.455	0.454
Informed vs. Random-Employment	0.032	0.179	0.715	0.536	0.435
Uninformed vs. Random-Employment	0.027	0.200	0.800	0.400	0.360

Note: List et al. (2019) p-values are produced using Stata command "mhtreg", which allows the testing procedure to be used in multivariate regressions (Steinmayr 2020). The underlying regressions are estimated using OLS with robust standard errors in which "Difference" refers to the coefficient estimate of each comparison.

Table A4: Share redistributed in subgroups: *p*-values with multiple hypothesis testing adjustments

	Difference	Multiple testing adjustment			
		Unadjusted p-values	Bonferroni p-values	Holm p-values	List et al. p-values
Female vs. Male	0.012	0.271	1.000	0.271	0.271
High vs. Low education	0.015	0.172	0.687	0.343	0.312
High vs. Low income	-0.022	0.047	0.187	0.140	0.130
Conservative vs. non-conservative	-0.033	0.010	0.039	0.039	0.036

Note: List et al. (2019) p-values are produced using Stata command "mhtreg", which allows the testing procedure to be used in multivariate regressions (Steinmayr 2020). The underlying regressions are estimated using OLS with robust standard errors in which "Difference" refers to the coefficient estimate of each comparison.

Table A5: Treatment effects for subgroups: *p*-values with multiple hypothesis testing adjustments

	Difference	Multiple testing adjustment			
		Unadjusted p-values	Bonferroni p-values	Holm p-values	List et al. p-values
Female					
Merit vs. Luck	-0.215	0.000	0.021	0.015	0.000
Random-Education vs. Luck	-0.088	0.000	0.021	0.021	0.000
Random-Employment vs. Luck	-0.149	0.000	0.021	0.019	0.000
Random-Education vs. Merit	0.127	0.000	0.021	0.014	0.000
Random-Employment vs. Merit	0.066	0.008	0.512	0.248	0.155
Random-Education vs. Random-Employment	0.061	0.016	1.000	0.425	0.261
Random-Education vs. Info-rEducation	-0.017	0.518	1.000	0.518	0.518
Random-Employment vs. Info-rEmployment	-0.018	0.451	1.000	1.000	0.817
Male					
Merit vs. Luck	-0.180	0.000	0.021	0.016	0.000
Random-Education vs. Luck	-0.068	0.010	0.640	0.300	0.188
Random-Employment vs. Luck	-0.111	0.001	0.043	0.026	0.012
Random-Education vs. Merit	0.112	0.000	0.021	0.015	0.000
Random-Employment vs. Merit	0.070	0.016	1.000	0.432	0.263
Random-Education vs. Random-Employment	0.043	0.123	1.000	1.000	0.768
Random-Education vs. Info-rEducation	-0.042	0.122	1.000	1.000	0.778
Random-Employment vs. Info-rEmployment	-0.042	0.130	1.000	1.000	0.758
High education					
Merit vs. Luck	-0.199	0.000	0.021	0.017	0.000
Random-Education vs. Luck	-0.076	0.004	0.277	0.147	0.099
Random-Employment vs. Luck	-0.123	0.000	0.021	0.015	0.000
Random-Education vs. Merit	0.123	0.000	0.021	0.020	0.000
Random-Employment vs. Merit	0.077	0.004	0.235	0.128	0.086
Random-Education vs. Random-Employment	0.046	0.071	1.000	1.000	0.645
Random-Education vs. Info-rEducation	-0.029	0.256	1.000	1.000	0.897
Random-Employment vs. Info-rEmployment	-0.021	0.401	1.000	1.000	0.935
Low education					
Merit vs. Luck	-0.200	0.000	0.021	0.018	0.000
Random-Education vs. Luck	-0.085	0.001	0.085	0.051	0.032
Random-Employment vs. Luck	-0.145	0.000	0.021	0.020	0.000
Random-Education vs. Merit	0.115	0.000	0.021	0.019	0.000
Random-Employment vs. Merit	0.055	0.058	1.000	1.000	0.592
Random-Education vs. Random-Employment	0.060	0.037	1.000	0.843	0.446
Random-Education vs. Info-rEducation	-0.029	0.272	1.000	1.000	0.875
Random-Employment vs. Info-rEmployment	-0.041	0.161	1.000	1.000	0.801

High income					
Merit vs. Luck	-0.221	0.000	0.021	0.016	0.000
Random-Education vs. Luck	-0.095	0.000	0.021	0.018	0.000
Random-Employment vs. Luck	-0.154	0.000	0.021	0.014	0.000
Random-Education vs. Merit	0.127	0.000	0.021	0.020	0.000
Random-Employment vs. Merit	0.068	0.014	0.875	0.396	0.242
Random-Education vs. Random-Employment	0.059	0.031	1.000	0.736	0.399
Random-Education vs. Info-rEducation	-0.031	0.228	1.000	1.000	0.891
Random-Employment vs. Info-rEmployment	-0.039	0.137	1.000	1.000	0.757
Low income					
Merit vs. Luck	-0.174	0.000	0.021	0.017	0.000
Random-Education vs. Luck	-0.066	0.007	0.448	0.224	0.141
Random-Employment vs. Luck	-0.111	0.000	0.021	0.013	0.000
Random-Education vs. Merit	0.109	0.000	0.021	0.018	0.000
Random-Employment vs. Merit	0.063	0.020	1.000	0.500	0.296
Random-Education vs. Random-Employment	0.046	0.083	1.000	1.000	0.689
Random-Education vs. Info-rEducation	-0.030	0.271	1.000	1.000	0.896
Random-Employment vs. Info-rEmployment	-0.021	0.417	1.000	1.000	0.860
Conservative					
Merit vs. Luck	-0.216	0.000	0.021	0.021	0.000
Random-Education vs. Luck	-0.128	0.006	0.405	0.209	0.134
Random-Employment vs. Luck	-0.152	0.000	0.021	0.017	0.000
Random-Education vs. Merit	0.098	0.015	0.981	0.429	0.261
Random-Employment vs. Merit	0.065	0.094	1.000	1.000	0.711
Random-Education vs. Random-Employment	0.034	0.409	1.000	1.000	0.904
Random-Education vs. Info-rEducation	-0.043	0.247	1.000	1.000	0.906
Random-Employment vs. Info-rEmployment	-0.030	0.466	1.000	0.932	0.714
Non-conservative					
Merit vs. Luck	-0.192	0.000	0.021	0.016	0.000
Random-Education vs. Luck	-0.064	0.000	0.021	0.019	0.000
Random-Employment vs. Luck	-0.127	0.000	0.021	0.021	0.000
Random-Education vs. Merit	0.129	0.000	0.021	0.014	0.000
Random-Employment vs. Merit	0.066	0.003	0.213	0.123	0.081
Random-Education vs. Random-Employment	0.063	0.003	0.213	0.120	0.081
Random-Education vs. Info-rEducation	-0.022	0.290	1.000	1.000	0.868
Random-Employment vs. Info-rEmployment	-0.031	0.128	1.000	1.000	0.764

Note: List et al. (2019) p-values are produced using Stata command "mhtreg", which allows the testing procedure to be used in multivariate regressions (Steinmayr 2020). The underlying regressions are estimated using OLS with robust standard errors in which "Difference" refers to the coefficient estimate of each comparison.

Table A6: Descriptive statistics about workers' characteristics

	Hypothetical scenarios						U.S. Population
	Merit-Training	Random-Training	Merit-Department	Random-Department	Info-Training	Info-Department	
Female (%)	52.8	52.9	55.6	58.3	49.6	59.2	50.8
Age (years)	40.7	40.1	40.9	38.9	41.4	41.0	38.2
High education (%)	52.8	53.6	51.1	48.6	61.9	57.0	32.9
Individual yearly income (USD)	57659	50000	58825	61482	55638	49708	68764
Conservative (%)	27.5	24.3	28.9	22.2	25.2	27.5	27.0
Obs.	142	140	135	142	139	142	

Note: The table reports descriptive statistics for spectators' characteristics in the experiment as well as the population data (from the American Community Survey (ACS) 2020 for sex, age, education and income, and the Gallup for the party affiliation since 2021). A person is categorized as "high education" if he or she has completed at least 4-year college education. Individual yearly income is the pre-tax income; in the population data it refers to mean earnings for full-time, year-round workers in the past 12 months.

B. Experimental protocol

B.1. Spectators

General information

Thank you for your interest in our academic study! Please read the instructions on the following pages carefully.

Participation in this study is entirely voluntary. Should you wish to stop your participation at any time, you can simply close your browser window. Your information and survey responses collected in this study are for research purposes only. We will only use your Worker ID to assign payments and check that you have not participated in this experiment before. Any identifying information associated with your responses will remain anonymous and confidential in the reporting of the study's results.

Note that we can only offer rewards to participants who successfully complete the task by providing their MTurk ID at the end of the questionnaire. You will be paid a fixed participation fee of $3 USD for completing the entire assignment.

The assignment is expected to take about 10 minutes of your time, however you will have up to 20 minutes to finish it.

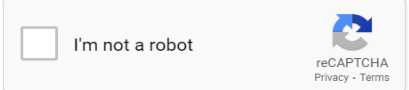
Should any question or concern arise, you can contact the investigators at espel.mturk@gmail.com Further instructions will be provided on the next page. If you agree to participate, please select 'I agree'.

I have read and understood the above and I consent to participate in this study:

- ○ I agree
- ○ I do not agree

Captcha verification

Before proceeding to the task, please complete the Captcha below to help ensure you are a real participant.



[Protocol for spectators in the Luck treatment]

*Please read the following information carefully. There will be a comprehension quiz on the next page, and **only participants who answer the quiz correctly can proceed to complete this task**.*

Unlike traditional survey questions that are about hypothetical situations, we now ask you to make a choice that has actual consequences for a real-life situation. Out of the participants in our study who are doing the same task as you are now, 1 out of every 3 will be randomly selected to have their decision actually implemented. What this means is that you may very well be one of those selected, and so you should make your choice carefully, as though it will actually be implemented.

Two individuals, let us call them worker A and worker B, were recruited via an online marketplace to complete an assignment. They were first each paid a participation compensation of 2 USD regardless of their performance on the assignment.

In the assignment, worker A and worker B were each given 5 minutes to read the exact same learning materials. After that, they each worked on the same set of multiple-choice questions related to the learning materials.

After completing the assignment, they were told that **their earnings from the assignment would be determined by a random lottery drawing**.

The worker winning this lottery drawing would earn 6 USD for the assignment and the other worker would earn nothing for the assignment. **They were not informed about the outcome of the lottery.**

However, worker A and worker B were told that a third person would be informed about the assignment and the outcome of the lottery, and that the third person would be given the opportunity to redistribute the earnings and thus determine how much they were paid for the assignment.

You are the third person and we now want you to choose whether to redistribute the earnings for the assignment between worker A and worker B. Your decision is completely anonymous. The workers will receive the payment that you choose for the assignment within a few days.

Now, you learn that **worker A won the lottery procedure and earned 6 USD. Thus, worker B earned nothing for the assignment.**

○ I am ready for the comprehension quiz.

Next page:

Worker A and worker B each read the same learning materials and worked on the same multiple-choice questions.

- ◯ True
- ◯ False

A lottery randomly determines which worker receives $6 USD for the assignment while the other worker receives $0 USD for the assignment.

- ◯ False
- ◯ True

The workers do not know the lottery result.

- ◯ True
- ◯ False

Next page:

You have passed the comprehension quiz. *For your reference, below are the same exact information you read on the first page (before the comprehension quiz), along with the opportunity to make your decision.*

{Same copy of the instructions. Omitted here.}

Now, you learn that **worker A won the lottery procedure and earned 6 USD. Thus, worker B earned nothing for the assignment.**

Please state which of the following alternatives you choose:

I do not redistribute

- ◯ Worker A is paid 6 USD and worker B is paid 0 USD.

I do redistribute

- ◯ Worker A is paid 5 USD and worker B is paid 1 USD.
- ◯ Worker A is paid 4 USD and worker B is paid 2 USD.
- ◯ Worker A is paid 3 USD and worker B is paid 3 USD.
- ◯ Worker A is paid 2 USD and worker B is paid 4 USD.
- ◯ Worker A is paid 1 USD and worker B is paid 5 USD.
- ◯ Worker A is paid 0 USD and worker B is paid 6 USD.

[Protocol for spectators in the Merit treatment]

Please read the following information carefully. There will be a comprehension quiz on the next page, and ***only participants who answer the quiz correctly can proceed to complete this task.***

Unlike traditional survey questions that are about hypothetical situations, we now ask you to make a choice that has actual consequences for a real-life situation. Out of the participants in our study who are doing the same task as you are now, 1 out of every 3 will be randomly selected to have their decision actually implemented. What this means is that you may very well be one of those selected, and so you should make your choice carefully, as though it will actually be implemented.

Two individuals, let us call them worker A and worker B, were recruited via an online marketplace to complete an assignment. They were first each paid a participation compensation of 2 USD regardless of their performance on the assignment.

In the assignment, worker A and worker B were given 5 minutes to read the exact same learning materials. After that, they each worked on the same set of multiple-choice questions related to the learning materials.

After completing the assignment, they were told that **their earnings from the assignment would be determined by the number of questions they answered correctly**. Specifically, the worker that answered more questions correctly would earn 6 USD for the assignment, and the other worker would earn nothing for the assignment. **The workers were not informed about which of them answered more questions correctly.**

However, worker A and worker B were told that a third person would be informed about the assignment and which worker answered more questions correctly, and that the third person would be given the opportunity to redistribute the earnings and thus determine how much they were paid for the assignment.

You are the third person and we now want you to choose whether to redistribute the earnings for the assignment between worker A and worker B. Your decision is completely anonymous. The workers will receive the payment that you choose for the assignment within a few days.

Now, you learn that **worker A answered more questions correctly than worker B and earned 6 USD. Thus, worker B earned nothing for the assignment**.

◯ I am ready for the comprehension quiz.

Next page:

Worker A and worker B each read the same learning materials and worked on the same multiple-choice questions.

- ◯ True
- ◯ False

The worker who answers more multiple-choice questions correctly receives $6 USD for the assignment while the other worker receives $0 USD for the assignment.

- ◯ True
- ◯ False

The workers do not know whether they or the other worker answered more multiple-choice questions correctly.

- ◯ True
- ◯ False

Next page:

You have passed the comprehension quiz. *For your reference, below are the same exact information you read on the first page (before the comprehension quiz), along with the opportunity to make your decision.*

{Same copy of the instructions. Omitted here.}

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

Please state which of the following alternatives you choose:

I do not redistribute

- ◯ Worker A is paid 6 USD and worker B is paid 0 USD.

I do redistribute

- ◯ Worker A is paid 5 USD and worker B is paid 1 USD.
- ◯ Worker A is paid 4 USD and worker B is paid 2 USD.
- ◯ Worker A is paid 3 USD and worker B is paid 3 USD.
- ◯ Worker A is paid 2 USD and worker B is paid 4 USD.
- ◯ Worker A is paid 1 USD and worker B is paid 5 USD.
- ◯ Worker A is paid 0 USD and worker B is paid 6 USD.

[Protocol for spectators in the Random-Education treatment]

Please read the following information carefully. There will be a comprehension quiz on the next page, and ***only participants who answer the quiz correctly can proceed to complete this task****.*

Unlike traditional survey questions that are about hypothetical situations, we now ask you to make a choice that has actual consequences for a real-life situation. Out of the participants in our study who are doing the same task as you are now, 1 out of every 3 will be randomly selected to have their decision actually implemented. What this means is that you may very well be one of those selected, and so you should make your choice carefully, as though it will actually be implemented.

Two individuals, let us call them worker A and worker B, were recruited via an online marketplace to complete an assignment. They were first each paid a participation compensation of 2 USD regardless of their performance on the assignment.

In the assignment, worker A and worker B were given 5 minutes to read some learning materials. After that, they each worked on the same set of multiple-choice questions related to the learning materials. **The learning materials that worker A and worker B had read were randomly assigned and their contents were different**.

The learning materials that **worker A had read were highly relevant** to the multiple-choice questions that he/she later worked on, while the learning materials that **worker B had read had low relevance** to the multiple-choice questions. Worker A and B were not informed specifically about whether or not the learning materials were different for the other worker.

After completing the assignment, they were told that **their earnings from the assignment would be determined by the number of questions they answered correctly**. Specifically, the worker that answered more questions correctly would earn 6 USD for the assignment, and the other worker would earn nothing for the assignment. **The workers were not informed about which of them answered more questions correctly.**

However, worker A and worker B were told that a third person would be informed about the assignment and which worker answered more questions correctly, and that the third person would be given the opportunity to redistribute the earnings and thus determine how much they were paid for the assignment.

You are the third person and we now want you to choose whether to redistribute the earnings for the assignment between worker A and worker B. Your decision is completely anonymous. The workers will receive the payment that you choose for the assignment within a few days.

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

◯ I am ready for the comprehension quiz.

Next page:

One worker receives the highly relevant learning materials, while the other worker receives the less relevant learning materials. The assignment of the learning materials is random between the two workers.

- ◯ True
- ◯ False

The worker who answers more multiple-choice questions correctly receives $6 USD for the assignment while the other worker receives $0 USD for the assignment.

- ◯ False
- ◯ True

The workers do not know whether they or the other worker answered more multiple-choice questions correctly.

- ◯ True
- ◯ False

Next page:

You have passed the comprehension quiz. *For your reference, below are the same exact information you read on the first page (before the comprehension quiz), along with the opportunity to make your decision.*

{Same copy of the instructions. Omitted here.}

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

Please state which of the following alternatives you choose:

I do not redistribute

- ◯ Worker A is paid 6 USD and worker B is paid 0 USD.

I do redistribute

- ◯ Worker A is paid 5 USD and worker B is paid 1 USD.
- ◯ Worker A is paid 4 USD and worker B is paid 2 USD.
- ◯ Worker A is paid 3 USD and worker B is paid 3 USD.
- ◯ Worker A is paid 2 USD and worker B is paid 4 USD.
- ◯ Worker A is paid 1 USD and worker B is paid 5 USD.
- ◯ Worker A is paid 0 USD and worker B is paid 6 USD.

[Protocol for spectators in the Random- Employment treatment]

Please read the following information carefully. There will be a comprehension quiz on the next page, and ***only participants who answer the quiz correctly can proceed to complete this task****.*

Unlike traditional survey questions that are about hypothetical situations, we now ask you to make a choice that has actual consequences for a real-life situation. Out of the participants in our study who are doing the same task as you are now, 1 out of every 3 will be randomly selected to have their decision actually implemented. What this means is that you may very well be one of those selected, and so you should make your choice carefully, as though it will actually be implemented.

Two individuals, let us call them worker A and worker B, were recruited via an online marketplace to complete an assignment. They were first each paid a participation compensation of 2 USD regardless of their performance on the assignment.

In the assignment, worker A and worker B were given 5 minutes to read the exact same learning materials. After that, they each worked on multiple-choice questions related to the learning materials.

The **sets of multiple-choice questions that worker A and worker B worked on were different and randomly assigned**. Under this situation, **worker A had the opportunity to work on all the multiple-choice questions worker B had access to, plus additional multiple-choice questions**. Worker A and B were not specifically informed that their set of multiple-choice questions were different.

After completing the assignment, they were told that **their earnings from the assignment would be determined by the number of questions they answered correctly**. Specifically, the worker that answered more questions correctly would earn 6 USD for the assignment, and the other worker would earn nothing for the assignment. **The workers were not informed about which of them answered more questions correctly.**

However, worker A and worker B were told that a third person would be informed about the assignment and which worker answered more questions correctly, and that the third person would be given the opportunity to redistribute the earnings and thus determine how much they were paid for the assignment.

You are the third person and we now want you to choose whether to redistribute the earnings for the assignment between worker A and worker B. Your decision is completely anonymous. The workers will receive the payment that you choose for the assignment within a few days.

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

◯ I am ready for the comprehension quiz.

Next page:

One worker has the opportunity to work on all the multiple-choice questions that the other worker has access to, plus additional multiple-choice questions. The assignment of the sets of multiple-choice questions is random between the two workers.

- ◯ True
- ◯ False

The worker who answers more multiple-choice questions correctly receives $6 USD for the assignment while the other worker receives $0 USD for the assignment.

- ◯ False
- ◯ True

The workers do not know whether they or the other worker answered more multiple-choice questions correctly.

- ◯ False
- ◯ True

Next page:

You have passed the comprehension quiz. *For your reference, below are the same exact information you read on the first page (before the comprehension quiz), along with the opportunity to make your decision.*

{Same copy of the instructions. Omitted here.}

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

Please state which of the following alternatives you choose:

I do not redistribute

- ◯ Worker A is paid 6 USD and worker B is paid 0 USD.

I do redistribute

- ◯ Worker A is paid 5 USD and worker B is paid 1 USD.
- ◯ Worker A is paid 4 USD and worker B is paid 2 USD.
- ◯ Worker A is paid 3 USD and worker B is paid 3 USD.
- ◯ Worker A is paid 2 USD and worker B is paid 4 USD.
- ◯ Worker A is paid 1 USD and worker B is paid 5 USD.
- ◯ Worker A is paid 0 USD and worker B is paid 6 USD.

[Protocol for spectators in the Info-rEducation treatment]

Please read the following information carefully. There will be a comprehension quiz on the next page, and ***only participants who answer the quiz correctly can proceed to complete this task.***

Unlike traditional survey questions that are about hypothetical situations, we now ask you to make a choice that has actual consequences for a real-life situation. Out of the participants in our study who are doing the same task as you are now, 1 out of every 3 will be randomly selected to have their decision actually implemented. What this means is that you may very well be one of those selected, and so you should make your choice carefully, as though it will actually be implemented.

Two individuals, let us call them worker A and worker B, were recruited via an online marketplace to complete an assignment. They were first each paid a participation compensation of 2 USD regardless of their performance on the assignment.

In the assignment, worker A and worker B were given 5 minutes to read some learning materials. After that, they each worked on the same set of multiple-choice questions related to the learning materials.

The learning materials that worker A and worker B were given to read could have been different. With **some chance**, the learning materials were exactly the same for the two workers.

However, with **some chance**, their contents were different and randomly assigned. Under this situation, the learning materials that **worker A had read were highly relevant** to the multiple-choice questions that he/she later worked on, while the learning materials that **worker B had read had low relevance** to the multiple-choice questions. Worker A and B were not informed specifically about whether or not the learning materials were different for the other worker.

After completing the assignment, they were told that **their earnings from the assignment would be determined by the number of questions they answered correctly**. Specifically, the worker that answered more questions correctly would earn 6 USD for the assignment, and the other worker would earn nothing for the assignment. **The workers were not informed about which of them answered more questions correctly.**

However, worker A and worker B were told that a third person would be informed about the assignment and which worker answered more questions correctly, and that the third person would be given the opportunity to redistribute the earnings and thus determine how much they were paid for the assignment.

You are the third person and we now want you to choose whether to redistribute the earnings for the assignment between worker A and worker B. Your decision is completely anonymous. The workers will receive the payment that you choose for the assignment within a few days.

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

○ I am ready for the comprehension quiz.

Next page:

The learning materials that worker A and worker B received may or may not be the same. If they are different, one worker receives the highly relevant learning material, while the other worker receives the less relevant learning material. The assignment of the learning materials is random between the two workers.

- ◯ False
- ◯ True

The worker who answers more multiple-choice questions correctly receives $6 USD for the assignment while the other worker receives $0 USD for the assignment.

- ◯ True
- ◯ False

The workers do not know whether they or the other worker answered more multiple-choice questions correctly.

- ◯ False
- ◯ True

Next page:

You have passed the comprehension quiz.

Before making your decision, you have the chance to learn whether the learning materials were different for the two workers. But you have to work on another assignment in order to obtain this information.

If you decide to work on that assignment, on the next page your task will be to find a 3-digit code correctly among a matrix (of a total of 289 numbers) of 3-digit codes in random order. The assigned code will appear multiple times in the same matrix and you will score 1 point for each correct marking. You will lose 1 point if you check off a wrong code. **If your accumulated points are 20 or higher (the maximum possible score is 33), you will learn whether the learning materials were different for the two workers.**

On the other hand, you can skip this 3-digit code task and go straight to making your redistribution decision. Now please choose one option:

- ◯ Go directly to the page about the redistribution decision
- ◯ Go to the 3-digit code task to try to learn the information

Next page (if they choose to do the number-checking task; skip this page if not):

The code you must check off is: 241. Tick the box to the left of the number.

☐ 407	☐ 221	☐ 622	☐ 314	☐ 883	☐ 603	☐ 574	☐ 989	☐ 205	☐ 234
☐ 743	☐ 365	☐ 891	☐ 410	☐ 879	☐ 241	☐ 340	☐ 954	☐ 641	☐ 241
☐ 602	☐ 882	☐ 180	☐ 873	☐ 965	☐ 446	☐ 876	☐ 308	☐ 172	☐ 241
☐ 537	☐ 347	☐ 833	☐ 674	☐ 241	☐ 566	☐ 701	☐ 354	☐ 661	☐ 268
☐ 144	☐ 375	☐ 173	☐ 241	☐ 678	☐ 241	☐ 966	☐ 606	☐ 527	☐ 170
☐ 435	☐ 917	☐ 938	☐ 508	☐ 850	☐ 241	☐ 405	☐ 695	☐ 840	☐ 654
☐ 265	☐ 330	☐ 926	☐ 634	☐ 674	☐ 358	☐ 843	☐ 784	☐ 637	☐ 431
☐ 689	☐ 602	☐ 108	☐ 187	☐ 474	☐ 540	☐ 280	☐ 882	☐ 464	☐ 884
☐ 617	☐ 241	☐ 537	☐ 205	☐ 749	☐ 388	☐ 495	☐ 160	☐ 258	☐ 317
☐ 809	☐ 723	☐ 801	☐ 273	☐ 218	☐ 241	☐ 191	☐ 372	☐ 783	☐ 702
☐ 350	☐ 618	☐ 459	☐ 241	☐ 602	☐ 518	☐ 545	☐ 730	☐ 241	☐ 809
☐ 948	☐ 943	☐ 816	☐ 395	☐ 975	☐ 711	☐ 121	☐ 389	☐ 912	☐ 583
☐ 408	☐ 416	☐ 241	☐ 919	☐ 891	☐ 241	☐ 477	☐ 546	☐ 925	☐ 495
☐ 764	☐ 572	☐ 241	☐ 809	☐ 567	☐ 380	☐ 234	☐ 334	☐ 124	☐ 777
☐ 220	☐ 874	☐ 241	☐ 241	☐ 533	☐ 604	☐ 241	☐ 360	☐ 900	☐ 674
☐ 881	☐ 806	☐ 980	☐ 743	☐ 554	☐ 589	☐ 494	☐ 527	☐ 130	☐ 926
☐ 149	☐ 274	☐ 843	☐ 998	☐ 674	☐ 241	☐ 290	☐ 241	☐ 796	☐ 707
☐ 559	☐ 818	☐ 233	☐ 241	☐ 354	☐ 881	☐ 615	☐ 596	☐ 307	☐ 235
☐ 538	☐ 241	☐ 524	☐ 274	☐ 334	☐ 341	☐ 859	☐ 807	☐ 543	☐ 377
☐ 241	☐ 979	☐ 241	☐ 337	☐ 628	☐ 590	☐ 757	☐ 233	☐ 140	☐ 241
☐ 914	☐ 609	☐ 585	☐ 391	☐ 778	☐ 300	☐ 393	☐ 247	☐ 968	☐ 739
☐ 942	☐ 858	☐ 361	☐ 924	☐ 269	☐ 197	☐ 729	☐ 265	☐ 241	☐ 842
☐ 146	☐ 522	☐ 997	☐ 648	☐ 144	☐ 744	☐ 241	☐ 388	☐ 494	☐ 447
☐ 241	☐ 265	☐ 542	☐ 456	☐ 751	☐ 213	☐ 560	☐ 210	☐ 454	☐ 549
☐ 795	☐ 121	☐ 932	☐ 241	☐ 630	☐ 163	☐ 759	☐ 427	☐ 749	☐ 225
☐ 705	☐ 340	☐ 241	☐ 623	☐ 795	☐ 303	☐ 472	☐ 942	☐ 957	☐ 355
☐ 763	☐ 922	☐ 550	☐ 241	☐ 241	☐ 578	☐ 653	☐ 216	☐ 775	☐ 906
☐ 241	☐ 219	☐ 293	☐ 216	☐ 223	☐ 241	☐ 809	☐ 510	☐ 174	☐ 605
☐ 303	☐ 723	☐ 661	☐ 355	☐ 874	☐ 971	☐ 555	☐ 810	☐ 703	

Next page (if they succeed in passing the number-checking task, they will see the information):

Your score is 23, which is no less than 20.

Now you learn the learning materials that worker A and worker B had read ***were indeed different.***

The learning materials that ***worker A had read were highly relevant*** *to the multiple-choice questions that he/she later worked on, while the learning materials that* ***worker B had read has low relevance*** *to the multiple-choice questions. Worker A and B were not informed specifically about whether or not the learning materials were different for the other worker.*

For your reference, below are the same exact information you read on the first page (before the comprehension quiz), along with the opportunity to make your decision.

{Same copy of the instructions. Omitted here.}

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

Please state which of the following alternatives you choose:

I do not redistribute

- ◯ Worker A is paid 6 USD and worker B is paid 0 USD.

I do redistribute

- ◯ Worker A is paid 5 USD and worker B is paid 1 USD.
- ◯ Worker A is paid 4 USD and worker B is paid 2 USD.
- ◯ Worker A is paid 3 USD and worker B is paid 3 USD.
- ◯ Worker A is paid 2 USD and worker B is paid 4 USD.
- ◯ Worker A is paid 1 USD and worker B is paid 5 USD.
- ◯ Worker A is paid 0 USD and worker B is paid 6 USD.

[Protocol for spectators in the Info-rEmployment treatment]

Please read the following information carefully. There will be a comprehension quiz on the next page, and ***only participants who answer the quiz correctly can proceed to complete this task.***

Unlike traditional survey questions that are about hypothetical situations, we now ask you to make a choice that has actual consequences for a real-life situation. Out of the participants in our study who are doing the same task as you are now, 1 out of every 3 will be randomly selected to have their decision actually implemented. What this means is that you may very well be one of those selected, and so you should make your choice carefully, as though it will actually be implemented.

Two individuals, let us call them worker A and worker B, were recruited via an online marketplace to complete an assignment. They were first each paid a participation compensation of 2 USD regardless of their performance on the assignment.

In the assignment, worker A and worker B were given 5 minutes to read the exact same learning materials. After that, they each worked on multiple-choice questions related to the learning materials.

The **sets of multiple-choice questions that worker A and worker B worked on could have been different**. With **some chance**, the set of multiple-choice questions was exactly the same for the two workers.

However, with **some chance,** the sets of multiple-choice questions were different and randomly assigned. Under this situation, worker A had the opportunity to work on all the multiple-choice questions worker B had access to, **plus additional multiple-choice questions**. Worker A and B were not specifically informed that their sets of multiple-choice questions were different.

After completing the assignment, they were told that **their earnings from the assignment would be determined by the number of questions they answered correctly**. Specifically, the worker that answered more questions correctly would earn 6 USD for the assignment, and the other worker would earn nothing for the assignment. **The workers were not informed about which of them answered more questions correctly.**

However, worker A and worker B were told that a third person would be informed about the assignment and which worker answered more questions correctly, and that the third person would be given the opportunity to redistribute the earnings and thus determine how much they were paid for the assignment.

You are the third person and we now want you to choose whether to redistribute the earnings for the assignment between worker A and worker B. Your decision is completely anonymous. The workers will receive the payment that you choose for the assignment within a few days.

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

◯ I am ready for the comprehension quiz.

Next page:

The sets of multiple-choice questions that worker A and worker B received may or may not be the same. If they are different, one worker has the opportunity to work on all the multiple-choice questions that the other worker has access to, plus additional multiple-choice questions. The assignment of the sets of multiple-choice questions is random between the two workers.

- ◯ False
- ◯ True

The worker who answers more multiple-choice questions correctly receives $6 USD for the assignment while the other worker receives $0 USD for the assignment.

- ◯ False
- ◯ True

The workers do not know whether they or the other worker answered more multiple-choice questions correctly.

- ◯ False
- ◯ True

Next page:

You have passed the comprehension quiz.

Before making your decision, **you have the chance to learn whether the sets of multiple-choice questions were different for the two workers.** But you have to work on another assignment in order to obtain this information.

If you decide to work on that assignment, on the next page your task will be to find a 3-digit code correctly among a matrix (of a total of 289 numbers) of 3-digit codes in random order. The assigned code will appear multiple times in the same matrix and you will score 1 point for each correct marking. You will lose 1 point if you check off a wrong code. **If your accumulated points are 20 or higher (the maximum possible score is 33), you will learn whether the number of available multiple-choice questions were different for the two workers**.

On the other hand, you can skip this 3-digit code task and go straight to making your redistribution decision. Now please choose one option.

- ◯ Go directly to the page about the redistribution decision
- ◯ Go to the 3-digit code task to try to learn the information

Next page (if they choose to do the number-checking task; skip this page if not):

The code you must check off is: 241. Tick the box to the left of the number.

☐ 407 ☐ 221 ☐ 622 ☐ 314 ☐ 883 ☐ 603 ☐ 574 ☐ 989 ☐ 205 ☐ 234
☐ 743 ☐ 365 ☐ 891 ☐ 410 ☐ 879 ☐ 241 ☐ 340 ☐ 954 ☐ 641 ☐ 241
☐ 602 ☐ 882 ☐ 180 ☐ 873 ☐ 965 ☐ 446 ☐ 876 ☐ 308 ☐ 172 ☐ 241
☐ 537 ☐ 347 ☐ 833 ☐ 674 ☐ 241 ☐ 566 ☐ 701 ☐ 354 ☐ 661 ☐ 268
☐ 144 ☐ 375 ☐ 173 ☐ 241 ☐ 678 ☐ 241 ☐ 966 ☐ 606 ☐ 527 ☐ 170
☐ 435 ☐ 917 ☐ 938 ☐ 508 ☐ 850 ☐ 241 ☐ 405 ☐ 695 ☐ 840 ☐ 654
☐ 265 ☐ 330 ☐ 926 ☐ 634 ☐ 674 ☐ 358 ☐ 843 ☐ 784 ☐ 637 ☐ 431
☐ 689 ☐ 602 ☐ 108 ☐ 187 ☐ 474 ☐ 540 ☐ 280 ☐ 882 ☐ 464 ☐ 884
☐ 617 ☐ 241 ☐ 537 ☐ 205 ☐ 749 ☐ 388 ☐ 495 ☐ 160 ☐ 258 ☐ 317
☐ 809 ☐ 723 ☐ 801 ☐ 273 ☐ 218 ☐ 241 ☐ 191 ☐ 372 ☐ 783 ☐ 702
☐ 350 ☐ 618 ☐ 459 ☐ 241 ☐ 602 ☐ 518 ☐ 545 ☐ 730 ☐ 241 ☐ 809
☐ 948 ☐ 943 ☐ 816 ☐ 395 ☐ 975 ☐ 711 ☐ 121 ☐ 389 ☐ 912 ☐ 583
☐ 408 ☐ 416 ☐ 241 ☐ 919 ☐ 891 ☐ 241 ☐ 477 ☐ 546 ☐ 925 ☐ 495
☐ 764 ☐ 572 ☐ 241 ☐ 809 ☐ 567 ☐ 380 ☐ 234 ☐ 334 ☐ 124 ☐ 777
☐ 220 ☐ 874 ☐ 241 ☐ 241 ☐ 533 ☐ 604 ☐ 241 ☐ 360 ☐ 900 ☐ 674
☐ 881 ☐ 806 ☐ 980 ☐ 743 ☐ 554 ☐ 589 ☐ 494 ☐ 527 ☐ 130 ☐ 926
☐ 149 ☐ 274 ☐ 843 ☐ 998 ☐ 674 ☐ 241 ☐ 290 ☐ 241 ☐ 796 ☐ 707
☐ 559 ☐ 818 ☐ 233 ☐ 241 ☐ 354 ☐ 881 ☐ 615 ☐ 596 ☐ 307 ☐ 235
☐ 538 ☐ 241 ☐ 524 ☐ 274 ☐ 334 ☐ 341 ☐ 859 ☐ 807 ☐ 543 ☐ 377
☐ 241 ☐ 979 ☐ 241 ☐ 337 ☐ 628 ☐ 590 ☐ 757 ☐ 233 ☐ 140 ☐ 241
☐ 914 ☐ 609 ☐ 585 ☐ 391 ☐ 778 ☐ 300 ☐ 393 ☐ 247 ☐ 968 ☐ 739
☐ 942 ☐ 858 ☐ 361 ☐ 924 ☐ 269 ☐ 197 ☐ 729 ☐ 265 ☐ 241 ☐ 842
☐ 146 ☐ 522 ☐ 997 ☐ 648 ☐ 144 ☐ 744 ☐ 241 ☐ 388 ☐ 494 ☐ 447
☐ 241 ☐ 265 ☐ 542 ☐ 456 ☐ 751 ☐ 213 ☐ 560 ☐ 210 ☐ 454 ☐ 549
☐ 795 ☐ 121 ☐ 932 ☐ 241 ☐ 630 ☐ 163 ☐ 759 ☐ 427 ☐ 749 ☐ 225
☐ 705 ☐ 340 ☐ 241 ☐ 623 ☐ 795 ☐ 303 ☐ 472 ☐ 942 ☐ 957 ☐ 355
☐ 763 ☐ 922 ☐ 550 ☐ 241 ☐ 241 ☐ 578 ☐ 653 ☐ 216 ☐ 775 ☐ 906
☐ 241 ☐ 219 ☐ 293 ☐ 216 ☐ 223 ☐ 241 ☐ 809 ☐ 510 ☐ 174 ☐ 605
☐ 303 ☐ 723 ☐ 661 ☐ 355 ☐ 874 ☐ 971 ☐ 555 ☐ 810 ☐ 703

Next page (if they succeed in passing the number-checking task, they will see the information):

Your score is 21, which is no less than 20.

Now you learn the sets of multiple-choice questions that worker A and worker B worked on ***were indeed different.***

Worker A had the opportunity to work on all the multiple-choice questions worker B had access to, plus additional multiple-choice questions*. Worker A and B were not specifically informed that their sets of multiple-choice questions were different.*

For your reference, below are the same exact information you read on the first page (before the comprehension quiz), along with the opportunity to make your decision.

{Same copy of the instructions. Omitted here.}

Now, you learn that **worker A answered more questions correctly and earned 6 USD. Thus, worker B earned nothing for the assignment.**

Please state which of the following alternatives you choose:

I do not redistribute

- ◯ Worker A is paid 6 USD and worker B is paid 0 USD.

I do redistribute

- ◯ Worker A is paid 5 USD and worker B is paid 1 USD.
- ◯ Worker A is paid 4 USD and worker B is paid 2 USD.
- ◯ Worker A is paid 3 USD and worker B is paid 3 USD.
- ◯ Worker A is paid 2 USD and worker B is paid 4 USD.
- ◯ Worker A is paid 1 USD and worker B is paid 5 USD.
- ◯ Worker A is paid 0 USD and worker B is paid 6 USD.

A.2. Workers

General information

Thank you for your interest in our academic study! Please read the instructions on the following pages carefully.

Participation in this study is entirely voluntary. Should you wish to stop your participation at any time, you can simply close your browser window. Your information and survey responses collected in this study are for research purposes only. We will only use your Worker ID to assign payments and check that you have not participated in this experiment before. Any identifying information associated with your responses will remain anonymous and confidential in the reporting of the study's results.

Note that we can only offer rewards to participants who successfully complete the task by providing their MTurk ID at the end of the questionnaire. You will be paid a fixed participation fee of 2 USD, and depending on the choices made by you and other participants, you might earn additional money. You will be given detailed instructions on the screen before each part of the experiment.

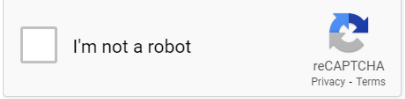
The assignment is expected to take a little over 10 minutes of your time, however you will have up to 20 minutes to complete it and enter your completion code in the MTurk window. Should any question or concern arise, you can contact the investigators at espel.mturk@gmail.com

Further instructions will be provided on the next page. If you agree to participate, please select 'I agree'. I have read and understood the above and I consent to participate in this study:

- ◯ I agree
- ◯ I do not agree

Captcha verification

Before proceeding to the task, please complete the Captcha below to help ensure you are a real participant.



Next page:

Before we begin our main task, on the next page you will be asked to read a short story about a potential real world scenario, and select the option which best represents your opinion about it.

Next page (each worker sees one of the following six scenarios):

{Merit-Training Scenario}

Two workers named Jim and Bill, at a company called Generic Inc., participate in the exact same training program prior to taking their certification test.

Jim obtained a high score on the test, while Bill obtained a moderately passing score. Note that both workers passed the certification test.

Generic Inc. awards a bonus of $600 USD to Jim for his high score obtained on the certification test, while Bill does not receive any bonus.

Suppose you were in a position to potentially reallocate the $600 bonus between the two workers.

Would you reallocate, and if so, how?

I would not reallocate:

- ◯ Jim is awarded a bonus of $600 and Bill is awarded a bonus of $0

I would reallocate, and my preferred reallocation can be best approximated as:

- ◯ Jim is awarded a bonus of $500 and Bill is awarded a bonus of $100
- ◯ Jim is awarded a bonus of $400 and Bill is awarded a bonus of $200
- ◯ Jim is awarded a bonus of $300 and Bill is awarded a bonus of $300
- ◯ Jim is awarded a bonus of $200 and Bill is awarded a bonus of $400
- ◯ Jim is awarded a bonus of $100 and Bill is awarded a bonus of $500
- ◯ Jim is awarded a bonus of $0 and Bill is awarded a bonus of $600

{Merit-Department Scenario}

Two workers named Jim and Bill, are of identical skill and training levels, at the same company, Generic Inc. They work in the same department, and have the exact same working conditions and client base.

Jim completed a high number of reports, while Bill completed a moderate number of reports.

Due to Jim' s high number of reports completed, Generic Inc. awarded a bonus of $600 to Jim, while Bill did not receive any bonus.

Suppose you were in a position to potentially reallocate the $600 bonus between the two workers.

Would you reallocate, and if so, how?

I would not reallocate:

- ◯ Jim is awarded a bonus of $600 and Bill is awarded a bonus of $0

I would reallocate, and my preferred reallocation can be best approximated as:

- ◯ Jim is awarded a bonus of $500 and Bill is awarded a bonus of $100
- ◯ Jim is awarded a bonus of $400 and Bill is awarded a bonus of $200
- ◯ Jim is awarded a bonus of $300 and Bill is awarded a bonus of $300
- ◯ Jim is awarded a bonus of $200 and Bill is awarded a bonus of $400
- ◯ Jim is awarded a bonus of $100 and Bill is awarded a bonus of $500
- ◯ Jim is awarded a bonus of $0 and Bill is awarded a bonus of $600

{Random-Training Scenario}

Two workers named Jim and Bill at a company called Generic Inc., participate in training programs prior to taking their certification test.

An external training company offered a small number of slots for their *new and improved* training program to several local companies, including Generic Inc.

Generic Inc. decided to give out their limited slots for the new and improved program to their own employees randomly, based on employees' birthdates.

Jim was randomly selected to attend the new and improved training program, and thus obtained a high score on the certification test. Bill was not randomly selected for the improved training program, and only attended the ordinary training program. He obtained a moderately passing score on the certification test.

Note that both workers worked equally hard in their respective training programs and both workers did pass the certification test.

Generic Inc. awards a bonus of $600 USD to Jim for his high score obtained on the certification test, while Bill does not receive any bonus.

Suppose you were in a position to potentially reallocate the $600 bonus between the two workers.

Would you reallocate, and if so, how?

I would not reallocate:

- ○ Jim is awarded a bonus of $600 and Bill is awarded a bonus of $0

I would reallocate, and my preferred reallocation can be best approximated as:

- ○ Jim is awarded a bonus of $500 and Bill is awarded a bonus of $100
- ○ Jim is awarded a bonus of $400 and Bill is awarded a bonus of $200
- ○ Jim is awarded a bonus of $300 and Bill is awarded a bonus of $300
- ○ Jim is awarded a bonus of $200 and Bill is awarded a bonus of $400
- ○ Jim is awarded a bonus of $100 and Bill is awarded a bonus of $500
- ○ Jim is awarded a bonus of $0 and Bill is awarded a bonus of $600

{Random-Department Scenario}

Two workers, Jim and Bill, are of identical skill and training levels at the same company, Generic Inc.

In their employment assignments, Jim and Bill have been randomly placed into different departments based on their employee ID numbers. Jim was placed into the Acquisitions Department, which serves an especially large client base. Jim was placed into the Regional Department, which serves only a moderate-sized client base.

Due to the large client base in his department, Jim was able to complete a high number of reports. Given the moderate-sized client base in his department, Bill was able to complete a moderate number of reports. However, both Jim and Bill were equally willing to do the work of completing reports.

Due to Jim' s high number of reports completed, the company awarded a bonus of $600 to Jim, while Bill did not receive any bonus.

Suppose you were in a position to potentially reallocate the $600 bonus between the two workers.

Would you reallocate, and if so, how?

I would not reallocate:

- ◯ Jim is awarded a bonus of $600 and Bill is awarded a bonus of $0

I would reallocate, and my preferred reallocation can be best approximated as:

- ◯ Jim is awarded a bonus of $500 and Bill is awarded a bonus of $100
- ◯ Jim is awarded a bonus of $400 and Bill is awarded a bonus of $200
- ◯ Jim is awarded a bonus of $300 and Bill is awarded a bonus of $300
- ◯ Jim is awarded a bonus of $200 and Bill is awarded a bonus of $400
- ◯ Jim is awarded a bonus of $100 and Bill is awarded a bonus of $500
- ◯ Jim is awarded a bonus of $0 and Bill is awarded a bonus of $600

{Info-Training Scenario}

Two workers named Jim and Bill, at a company Generic Inc., participate in a training program prior to taking their certification test.

An external training company offered a small number of slots for their *new and improved* training program to several local companies, including Generic Inc.

Generic Inc. decided to give out their limited slots for the new and improved program to their own employees randomly, based on employees' birthdates.

There is a possibility that Jim and Bill attended different training programs. In particular, it is possible that one of them attended the new and improved training program, while the other could only attend the ordinary training program.

It turns out that Jim obtained a high score on the test, while Bill obtained a moderately passing score. Note that both workers did pass the certification test.

However, their manager does not actually know whether one of the workers in fact attended the new and improved training program.

The company has a fund available to award a bonus of $600 USD to workers who score high on the certification test, such as Jim. However, the decision about whether and how much bonus to award to workers is up to the manager.

When you think about this scenario as applied to most workplaces, what percent of managers do you think will check the training program history of the workers before deciding about how to award the bonus?

- ◯ 0 percent of managers
- ◯ Above 0 percent but below 20% of managers
- ◯ At least 20% but below 40% of managers
- ◯ At least 40% but below 60% of managers
- ◯ At least 60% but below 80% of managers
- ◯ At least 80% of managers

{Info-Department Scenario}

Two workers named Jim and Bill, at a company Generic Inc., are of identical skill and training levels at the same company.

In their employment assignment, they have been randomly placed into departments within the company based on their employee ID numbers. It turns out that Jim has completed a large number of reports, while Bill has completed a moderate number of reports.

There is a possibility that Jim and Bill were assigned to different departments, which have different sized client bases.

The size of a department's client base has the potential to affect each worker's number of reports that they have the opportunity to complete, even if Jim and Bill are both equally willing to do the work.

The company has a fund available to award a bonus of $600 USD to workers who complete a high number of reports, such as Jim. However, the decision about whether and how much bonus to award to workers is up to the manager.

When you think about this scenario as applied to most workplaces, what percent of managers do you think will check the department assignments of the workers before deciding about how to award the bonus?

- ◯ 0 percent of managers
- ◯ Above 0 percent but below 20% of managers
- ◯ At least 20% but below 40% of managers
- ◯ At least 40% but below 60% of managers
- ◯ At least 60% but below 80% of managers
- ◯ At least 80% of managers

Next page:

Instructions - Part 1

On the next page, you will be asked to read a short passage about earthworms, for up to 5 minutes.

You will then be asked to answer a series of multiple-choice questions, followed by a short survey. You will be informed about the number of questions you answered correctly before the end of the task.

[Protocol for workers in the Luck treatment]

Next page (all workers received highly-relevant learning materials):

Please read the following passage carefully. You have up to 5 minutes.

Earthworms are night creatures that eat dirt and build tunnels in the soil. In one night, an earthworm can eat up to 1/3 of its own body weight. Earthworms deposit nutrients to the soil from their waste, called castings, which are effective fertilizer.

Earthworms are invertebrates and can live up to 6 years. An earthworm's body is made up of more than 100 ring shaped parts that bend and stretch as they move. Their bodies have small bristles to help them grip the ground. As earthworms move or dig, they leave a mucus trail behind them which hardens, serving as the walls of the tunnels they dig.

Earthworms breathe directly through their skin. They cannot survive fully underwater, or in overly dry conditions. Earthworms do not have ears or eyes, but can sense movements in the ground and light via their skin.

Baby worms stay in their cocoon for weeks to months, and come out when the soil is warm and wet enough for survival. Moles, rats, fish and toads are among the animals that eat earthworms.

Next page (all workers received the full set of test questions):

QUIZ: Please choose the answer to each multiple choice question to the best of your ability based on the earlier passage you have read. You will have 5 minutes to complete the multiple choice questions.

1. Which of the following are activities of earthworms?

- ◯ Building tunnels in the soil
- ◯ Trapping their prey
- ◯ Slithering in the rain
- ◯ Underwater swimming

2. What purpose do the bristles attached to earthworms' bodies serve?

- ◯ Help them sense danger
- ◯ Help them grip the ground
- ◯ Help them seek mates
- ◯ Help them digest food

3. What can earthworm waste be used for?

- ◯ Birdfeed
- ◯ Insect repellent
- ◯ Fertilizer
- ◯ A type of traditional medicine

4. What are earthworms' bodies made of?

- ◯ Ring shaped parts that bend and stretch
- ◯ Tiny scales similar to those of a snake
- ◯ Elastic stripes
- ◯ A rubber-like substance

5. Earthworms mainly eat:

- ◯ Dirt
- ◯ Small insects
- ◯ Moss or other plants
- ◯ Micro-organisms

6. Approximately how many ring-shaped parts is an earthworm's body made of?

- ◯ More than 20
- ◯ More than 50
- ◯ More than 100
- ◯ More than 1000

7. An earthworm can live up to

- ◯ 60 days
- ◯ 9 months
- ◯ 2 years
- ◯ 6 years

8. The mucus trail that earthworms leave behind when they move serves which purpose?

- ◯ To mark their territory
- ◯ To attract a mate
- ◯ To create walls for the tunnels they dig
- ◯ To confuse their predators

9. How long do baby worms stay in their cocoons?

- ◯ A few days at most
- ◯ Weeks to months
- ◯ The better part of a year
- ◯ Over one year

10. How do earthworms breathe?

- ◯ Through their skin
- ◯ Through their nose
- ◯ Through their mouth
- ◯ Through their bristles

11. Which of the following are conditions in which earthworms can survive best?

- ◯ Warm and wet
- ◯ Cool and dry
- ◯ Underwater
- ◯ Warm and dry

12. How do earthworms typically sense light?

- ◯ Through their bristles
- ◯ Through their skin
- ◯ Via infrared
- ◯ Earthworms do not sense light but rely on sonar

13. What is earthworm waste typically called?

- ◯ Earthworm feces
- ◯ Castings
- ◯ Casings
- ◯ Pellets

14. Which of the following are earthworm predators?

- ◯ Moles, rats, fish and toads
- ◯ Rats, fish, hawks and spiders
- ◯ Rats, toads, humans and gophers
- ◯ Toads, gophers, spiders and humans

15. How much can an earthworm eat in one night?

- ◯ Twice its own body weight
- ◯ 1/3 its own body weight
- ◯ 1/10 its own body weight
- ◯ A quarter of a pound

[Protocol for workers in the Merit treatment]

Next page (all workers received highly-relevant learning materials):

Please read the following passage carefully. You have up to 5 minutes.

Earthworms are night creatures that eat dirt and build tunnels in the soil. In one night, an earthworm can eat up to 1/3 of its own body weight. Earthworms deposit nutrients to the soil from their waste, called castings, which are effective fertilizer.

Earthworms are invertebrates and can live up to 6 years. An earthworm's body is made up of more than 100 ring shaped parts that bend and stretch as they move. Their bodies have small bristles to help them grip the ground. As earthworms move or dig, they leave a mucus trail behind them which hardens, serving as the walls of the tunnels they dig.

Earthworms breathe directly through their skin. They cannot survive fully underwater, or in overly dry conditions. Earthworms do not have ears or eyes, but can sense movements in the ground and light via their skin.

Baby worms stay in their cocoon for weeks to months, and come out when the soil is warm and wet enough for survival. Moles, rats, fish and toads are among the animals that eat earthworms.

Next page (all workers received the full set of test questions):

QUIZ: Please choose the answer to each multiple choice question to the best of your ability based on the earlier passage you have read. You will have 5 minutes to complete the multiple choice questions.

1. Which of the following are activities of earthworms?

- ◯ Building tunnels in the soil
- ◯ Trapping their prey
- ◯ Slithering in the rain
- ◯ Underwater swimming

2. What purpose do the bristles attached to earthworms' bodies serve?

- ◯ Help them sense danger
- ◯ Help them grip the ground
- ◯ Help them seek mates
- ◯ Help them digest food

3. What can earthworm waste be used for?

- ◯ Birdfeed
- ◯ Insect repellent
- ◯ Fertilizer
- ◯ A type of traditional medicine

4. What are earthworms' bodies made of?

- ◯ Ring shaped parts that bend and stretch
- ◯ Tiny scales similar to those of a snake
- ◯ Elastic stripes
- ◯ A rubber-like substance

5. Earthworms mainly eat:

- ◯ Dirt
- ◯ Small insects
- ◯ Moss or other plants
- ◯ Micro-organisms

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- ◯ More than 20
- ◯ More than 50
- ◯ More than 100
- ◯ More than 1000

7. An earthworm can live up to

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- ◯ 9 months
- ◯ 2 years
- ◯ 6 years

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- ◯ To mark their territory
- ◯ To attract a mate
- ◯ To create walls for the tunnels they dig
- ◯ To confuse their predators

9. How long do baby worms stay in their cocoons?

- ◯ A few days at most
- ◯ Weeks to months
- ◯ The better part of a year
- ◯ Over one year

10. How do earthworms breathe?

- ◯ Through their skin
- ◯ Through their nose
- ◯ Through their mouth
- ◯ Through their bristles

11. Which of the following are conditions in which earthworms can survive best?

- ◯ Warm and wet
- ◯ Cool and dry
- ◯ Underwater
- ◯ Warm and dry

12. How do earthworms typically sense light?

- ◯ Through their bristles
- ◯ Through their skin
- ◯ Via infrared
- ◯ Earthworms do not sense light but rely on sonar

13. What is earthworm waste typically called?

- ◯ Earthworm feces
- ◯ Castings
- ◯ Casings
- ◯ Pellets

14. Which of the following are earthworm predators?

- ◯ Moles, rats, fish and toads
- ◯ Rats, fish, hawks and spiders
- ◯ Rats, toads, humans and gophers
- ◯ Toads, gophers, spiders and humans

15. How much can an earthworm eat in one night?

- ◯ Twice its own body weight
- ◯ 1/3 its own body weight
- ◯ 1/10 its own body weight
- ◯ A quarter of a pound

Next page:

Instructions - Part 2

We will now explain how you will get paid for your work completed in Part 1. We will pair you with another participant who has completed our task. The payment to you and your paired participant is determined by a two-stage procedure:

First step:
Your bonus earnings are determined by how many questions you answered correctly compared to your paired participant. The participant who has answered more questions correctly earns 6 USD while the other earns 0 USD. If both of you have answered the same number of questions correctly, you will be matched with another participant who has answered either a greater number or lesser number of questions.

Second step:

- A randomly selected third person (also a participant in this study) will be given an opportunity to redistribute the temporary earnings between you and your paired participant. This third person will not know the identity of either you or the other participant, but they will be informed about the nature of the work and your **First step** temporary earnings for this work.
  - If the third person chooses not to redistribute, each of you will be paid your final earnings according to the **First step** temporary earnings.
  - If the third person chooses to redistribute, they can readjust your temporary earnings and that of your paired participant, with 1 USD as the smallest unit of adjustment. Your final earnings will be determined by their readjustment.

You will receive your fixed participation fee of 2 USD within three days, as well as any additional payment you gained based on the above described procedure within seven days.

[Protocol for workers in the Random-Education treatment]

Next page (50% workers received highly-relevant learning materials):

Please read the following passage carefully. You have up to 5 minutes.

Earthworms are night creatures that eat dirt and build tunnels in the soil. In one night, an earthworm can eat up to 1/3 of its own body weight. Earthworms deposit nutrients to the soil from their waste, called castings, which are effective fertilizer.

Earthworms are invertebrates and can live up to 6 years. An earthworm's body is made up of more than 100 ring shaped parts that bend and stretch as they move. Their bodies have small bristles to help them grip the ground. As earthworms move or dig, they leave a mucus trail behind them which hardens, serving as the walls of the tunnels they dig.

Earthworms breathe directly through their skin. They cannot survive fully underwater, or in overly dry conditions. Earthworms do not have ears or eyes, but can sense movements in the ground and light via their skin.

Baby worms stay in their cocoon for weeks to months, and come out when the soil is warm and wet enough for survival. Moles, rats, fish and toads are among the animals that eat earthworms.

Next page (50% workers received lowly-relevant learning materials):

Please read the following passages carefully. You have up to 5 minutes.

Earthworms are creatures that build tunnels in the soil. These small creatures are a whole lot more interesting than they appear. Their bodies are made of ring shaped parts that bend and stretch as they move. This is why earthworms seem to be so flexible.

Earthworms are invertebrates, and have small bristles attached to their bodies that help them grip the ground. As they move, they leave a mucus trail behind them. This is why many people may have the impression that earthworms are slimy, as we watch them wriggle on the ground.

Earthworms cannot survive in overly dry conditions, or their bodies will tend to dry out. Although they do not have eyes or ears, they can sense movement in the ground, which is important for their survival. The waste of earthworms are effective fertilizer for soil. So we can give thanks to earthworms for helping farmers to grow the vegetables we eat each day.

Earthworms do have predators, which include rats and toads. These animals will consider earthworms to be a very satisfying and tasty meal.

Next page (all workers received the full set of test questions):

QUIZ: Please choose the answer to each multiple choice question to the best of your ability based on the earlier passage you have read. You will have 5 minutes to complete the multiple choice questions.

1. Which of the following are activities of earthworms?

- ◯ Building tunnels in the soil
- ◯ Trapping their prey
- ◯ Slithering in the rain
- ◯ Underwater swimming

2. What purpose do the bristles attached to earthworms' bodies serve?

- ◯ Help them sense danger
- ◯ Help them grip the ground
- ◯ Help them seek mates
- ◯ Help them digest food

3. What can earthworm waste be used for?

- ◯ Birdfeed
- ◯ Insect repellent
- ◯ Fertilizer
- ◯ A type of traditional medicine

4. What are earthworms' bodies made of?

- ◯ Ring shaped parts that bend and stretch
- ◯ Tiny scales similar to those of a snake
- ◯ Elastic stripes
- ◯ A rubber-like substance

5. Earthworms mainly eat:

- ◯ Dirt
- ◯ Small insects
- ◯ Moss or other plants
- ◯ Micro-organisms

6. Approximately how many ring-shaped parts is an earthworm's body made of?

- ◯ More than 20
- ◯ More than 50
- ◯ More than 100
- ◯ More than 1000

7. An earthworm can live up to

- ◯ 60 days
- ◯ 9 months
- ◯ 2 years
- ◯ 6 years

8. The mucus trail that earthworms leave behind when they move serves which purpose?

- ◯ To mark their territory
- ◯ To attract a mate
- ◯ To create walls for the tunnels they dig
- ◯ To confuse their predators

9. How long do baby worms stay in their cocoons?

- ◯ A few days at most
- ◯ Weeks to months
- ◯ The better part of a year
- ◯ Over one year

10. How do earthworms breathe?

- ◯ Through their skin
- ◯ Through their nose
- ◯ Through their mouth
- ◯ Through their bristles

11. Which of the following are conditions in which earthworms can survive best?

- ◯ Warm and wet
- ◯ Cool and dry
- ◯ Underwater
- ◯ Warm and dry

12. How do earthworms typically sense light?

- ◯ Through their bristles
- ◯ Through their skin
- ◯ Via infrared
- ◯ Earthworms do not sense light but rely on sonar

13. What is earthworm waste typically called?

- ◯ Earthworm feces
- ◯ Castings
- ◯ Casings
- ◯ Pellets

14. Which of the following are earthworm predators?

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- ◯ Rats, fish, hawks and spiders
- ◯ Rats, toads, humans and gophers
- ◯ Toads, gophers, spiders and humans

15. How much can an earthworm eat in one night?

- ◯ Twice its own body weight
- ◯ 1/3 its own body weight
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You will receive your fixed participation fee of 2 USD within three days, as well as any additional payment you gained based on the above described procedure within seven days.

[Protocol for workers in the Random-Employment treatment]

Next page (all workers received highly-relevant learning materials):

Please read the following passage carefully. You have up to 5 minutes.

Earthworms are night creatures that eat dirt and build tunnels in the soil. In one night, an earthworm can eat up to 1/3 of its own body weight. Earthworms deposit nutrients to the soil from their waste, called castings, which are effective fertilizer.

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Baby worms stay in their cocoon for weeks to months, and come out when the soil is warm and wet enough for survival. Moles, rats, fish and toads are among the animals that eat earthworms.

Next page (50% workers received the full set of test questions):

QUIZ: Please choose the answer to each multiple choice question to the best of your ability based on the earlier passage you have read. You will have 5 minutes to complete the multiple choice questions.

1. Which of the following are activities of earthworms?

- ◯ Building tunnels in the soil
- ◯ Trapping their prey
- ◯ Slithering in the rain
- ◯ Underwater swimming

2. What purpose do the bristles attached to earthworms' bodies serve?

- ◯ Help them sense danger
- ◯ Help them grip the ground
- ◯ Help them seek mates
- ◯ Help them digest food

3. What can earthworm waste be used for?

- ◯ Birdfeed
- ◯ Insect repellent
- ◯ Fertilizer
- ◯ A type of traditional medicine

4. What are earthworms' bodies made of?

- ◯ Ring shaped parts that bend and stretch
- ◯ Tiny scales similar to those of a snake
- ◯ Elastic stripes
- ◯ A rubber-like substance

5. Earthworms mainly eat:

- ◯ Dirt
- ◯ Small insects
- ◯ Moss or other plants
- ◯ Micro-organisms

6. Approximately how many ring-shaped parts is an earthworm's body made of?

- ◯ More than 20
- ◯ More than 50
- ◯ More than 100
- ◯ More than 1000

7. An earthworm can live up to

- ◯ 60 days
- ◯ 9 months
- ◯ 2 years
- ◯ 6 years

8. The mucus trail that earthworms leave behind when they move serves which purpose?

- ◯ To mark their territory
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- ◯ To create walls for the tunnels they dig
- ◯ To confuse their predators

9. How long do baby worms stay in their cocoons?

- ◯ A few days at most
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- ◯ The better part of a year
- ◯ Over one year

10. How do earthworms breathe?

- ◯ Through their skin
- ◯ Through their nose
- ◯ Through their mouth
- ◯ Through their bristles

11. Which of the following are conditions in which earthworms can survive best?

- ◯ Warm and wet
- ◯ Cool and dry
- ◯ Underwater
- ◯ Warm and dry

12. How do earthworms typically sense light?

- ◯ Through their bristles
- ◯ Through their skin
- ◯ Via infrared
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13. What is earthworm waste typically called?

- ◯ Earthworm feces
- ◯ Castings
- ◯ Casings
- ◯ Pellets

14. Which of the following are earthworm predators?

- ◯ Moles, rats, fish and toads
- ◯ Rats, fish, hawks and spiders
- ◯ Rats, toads, humans and gophers
- ◯ Toads, gophers, spiders and humans

15. How much can an earthworm eat in one night?

- ◯ Twice its own body weight
- ◯ 1/3 its own body weight
- ◯ 1/10 its own body weight
- ◯ A quarter of a pound

Next page (50% workers received the truncated set of test questions):

QUIZ: Please choose the answer to each multiple choice question to the best of your ability based on the earlier passage you have read. You will have 5 minutes to complete the multiple choice questions.

1. Which of the following are activities of earthworms?

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- ◯ A type of traditional medicine

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  - If the third person chooses to redistribute, they can readjust your temporary earnings and that of your paired participant, with 1 USD as the smallest unit of adjustment. Your final earnings will be determined by their readjustment.

You will receive your fixed participation fee of 2 USD within three days, as well as any additional payment you gained based on the above described procedure within seven days.

[Protocol for workers in the Info-rEducation treatment]

80% of workers followed the same protocol as the Random-Education treatment; 20% of workers followed the same protocol as the Merit treatment. So details are omitted here.

[Protocol for workers in the Info-rEmployment treatment]

80% of workers followed the same protocol as the Random-Employment treatment; 20% of workers followed the same protocol as the Merit treatment. So details are omitted here.