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Myopic Meritocracy? An Experimental Survey Approach

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Abstract

In my master's thesis, I present a hypothetical survey experiment (n=95) examining the impact of two types of inequality on participants' fairness perceptions. The experimental design introduces a novel approach by providing impartial spectators with precise information about workers' performance in two consecutive effort-rounds and the underlying sources of inequality. Participants possess complete certainty about the determining factors of the outcome. The thesis investigates whether, on average, spectators exhibit more sensitivity towards direct sources of inequality compared to indirect sources. Drawing on a theoretical framework, a specific type of spectator, termed *Myopic Meritocrat*, exhibits varying distributive behavior based on the type of source affecting workers' initial payoff. Alongside this novel investigation, the data is also utilized to conceptually replicate previous experimental findings on decision-making in a distributive settings. To ensure transparency, I preregistered all hypothesis tests before collecting the data. I do not find any statistically significant findings in the predicted direction of the hypotheses.

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1 Introduction

According to Roemer and Trannoy (2015), the modern formulation of inequality of opportunity begins with Rawls's Theory of Justice from 1971. It suggests that inequalities that emerged due to unequal initial circumstances, meaning circumstances in which a person, for example, is born into and therefore has no control upon, should be considered unfair (Rawls, 2020). Starmans et al. (2017) state that people are generally concerned about the underlying causes of inequality and whether it stems from fair or unfair origins. In their literature review, they argue that people do not dislike inequality itself, but rather economic unfairness that is confounded with inequality. Corak (2013) gives a global picture of the relationship between the inequality of opportunity and income inequality. Overall, there also is a clear positive correlation between income inequality and intergenerational income elasticity.

Therefore, social mobility seems to be negatively correlated with the income inequality of a country. There is sound evidence that in most Western democracies, public support for redistribution is a critical factor in shaping social policies, including tax laws and government subsidies (Acemoglu et al., 2015). Despite different opinions on what is considered fair, many citizens and political leaders endorse a meritocratic view of fairness. This perspective of fairness rewards individuals based on their effort and choice rather than factors beyond their control, such as luck, heritage, or environments with unequal opportunities (Cappelen et al., 2020). In recent years, a number of studies can show that individual behavior is fundamentally shaped by the social preferences that the individual holds (Fehr and Schmidt 1999, Charness and Rabin 2002, Bolton and Ockenfels 2000, Falk and Fischbacher 2006, Ellingsen and Johannesson 2004). Besides behavioral economists, the topic of fairness is also widely investigated by social and moral psychologists who try to understand the foundations of moral judgment. The Moral Foundation Theory by Graham et al. (2013) proposes five moral foundations upon which people make their moral reasoning. Haidt (2012) proposes to extend this theory by a sixth foundation describing to what extent people endorse liberty in their moral judgment. By incorporating the liberty foundation, the theory could better explain different concepts of fairness between political groups. A crucial insight of this theory is the possibility that different moral foundations can lead to different fairness ideals. Haidt, suggests that both conservatives and liberals[1] value fairness, but while liberals tend to prioritize equality, meaning that they view fairness in terms of equal treatment and outcomes for all individuals, conservatives prioritize proportionality, meaning that they view fairness in terms of individuals getting what they deserve based on their efforts and contributions. These differences in perception of fairness are rooted in different moral foundations. Liberals tend to focus more heavily on the care/harm and fairness/cheating foundations, while conservatives place more emphasis on the loyalty/betrayal, authority/subversion, and sanctity/degradation foundations. This eventually results in the circumstance, that conservatives on average are more likely to accept economic inequalities (Haidt, 2012). Furthermore, people's perception of fairness can depend on how their culture weights aspects of morality such as loyalty, equity, or proportionality. An overview is given by Rai and Fiske (2011). With the research done in Neuropsychology and the novel area of Neuroeconomics, moral foundations and elements of economic decision-making can even be related to human biology (see for example Greene et al. 2001, Sanfey et al. 2003, Ruff and Fehr 2014). By utilizing brain imaging methods, studies such as Greene et al. (2001) give insight into how emotions can influence the direction of a person's moral decision-making on a neurological level.

In more recent years, several experimental studies test for differences in the perception of fairness. Cappelen et al. (2010) test what aspects people hold other people responsible for in case of differences in economic outcomes. The authors also test what impact institutions such as the labor market or the educational system could have on people's fairness preferences. The study allows us to distinguish between different fairness ideals and the weight people put on fairness considerations in their decision-making. One aspect, the paper suggests is that people do prefer to redistribute resources that are due to aspects for which people cannot be held responsible for. Since then, the perception of fairness has been investigated in different settings, often comparing samples from different countries (e.g. Almås et al. 2020).

While these studies shed light on several important aspects of the perception of fairness on distributive decision-making, one aspect has not been investigated rigorously - the *Inequality of Opportunity*.

[1]Liberals (in the American political context) are defined as individuals who prioritize values such as care, fairness, and liberty. Liberals tend to emphasize the importance of social justice, equality, and individual rights (Haidt, 2012).

With this thesis, I wish to investigate whether more indirect sources of inequality such as inequality of opportunity impact a person's perception of fairness differently compared to more direct inequalities. Two contemporary working papers by Preuss et al. (2022) and Dong et al. (2022) also analyze this question by simulating the inequality of opportunity in an experimental game and comparing it to a more direct source of inequality.

My experimental design uses a novel experimental survey approach for comparing a spectator's fairness perception in the setting of an experimental labor market. For this, my project combines elements from several experimental designs of studies using real-effort tasks (Cappelen et al. 2010, Preuss et al. 2022). The experimental survey game was initially distributed via Qualtrics on the 13th and 14th of April 2023 to all Bachelor and Master students of the Stockholm School of Economics (SSE). I preregistered the analysis plan of the experiment before the survey was distributed and any data was gathered. The pre-analysis plan can be found here: osf.io/8n3tk/
Compared to other studies in this area, this survey experiment is hypothetical and there is only one type of real participant, namely the spectators who are asked to redistribute money from hypothetical[2] "workers". This aspect resembles an important restriction of the experiment since it makes the decision-making more abstract compared to studies in which real money has to be distributed between workers who actually conduct real-effort tasks.

The research design is inspired by a combination of previous studies in this field and my reasoning that the circumstances we are born into do not directly interact with the effort we show in our working live. This means that are big temporal differences between two adults competing for a job and their birth, time in kindergarten, or time in school during which they are all affected by their socioeconomic background. At the same time, a person can achieve the same level and quality of education despite a disadvantageous background. This however suggests that she on average must have either exhibited more effort or had more innate talent (or a combination of both) than a person with the same level and quality of education but a higher socioeconomic background. A meritocratic

[2]Although it is common practice in Behavioral Economics to work with monetary incentives, I was not able to offer these to students because of GDPR requirements. These do not allow students at SSE to gather personal data such as an email-address, making it unfeasible to pay participants individually (depending on their performance). However, there are published studies such as Faravelli (2007) who conduct economic experiments by letting participants decide for hypothetical distributions of resources.

fairness ideal would assess a person's merit in a lifetime and not only in the present.

Based on the findings of Preuss et al. (2022) and Dong et al. (2022), I believe that performance will be less relevant for a spectator's redistribution decision if interacting with luck in opportunity that influences a form of experimental skill accumulation. The experimental design tests this hypothesis in an RCT setting with two treatment conditions. Here, the *Inequality of Opportunity* treatment is compared to a direct *Price* treatment. The experiment is designed to be as easy to understand as possible. This includes for example that different scenarios within treatments are designed in a computationally mild way. Also, it is ensured that both treatments are equal in their impact on the initial outcome of the game.

Additionally, compared to most other related studies with spectator[3] design, this experiment does not result in a "winner-takes-it-all" outcome, but instead in an initial allocation of money that is based on a combination of workers' efforts and exogenous factors. Overall, the impact of both treatments on the assigned allocation should be easy to understand for the spectator and is not impacted by uncertainty.

The study is based on a theoretical framework that defines four types of spectators based on their underlying fairness ideals. Besides Egalitarians, Meritocrats, and Libertarians, who have been rigorously studied in several papers (e.g. Cappelen et al. 2010, Cappelen et al. 2020, Dong et al. (2022)) I define a "new" type of fairness ideal called *Myopic Meritocrat*. This type of spectator can be seen as a hybrid between Meritocrat and Libertarian. They are meritocratic because they consider luck in the earning-stage of the game and thus do not hold workers responsible for different prices during the second round. In the first round, however, Myopic Meritocrats will neglect exogenous luck and thus hold workers responsible for their accumulated skills in round 2.

Due to a low response rate and high attrition rate, the analysis lacks sufficient statistical power to reliably detect a medium effect size. With 95 observations used, no statistically significant results in the predicted direction, as stated in the preregistered hypotheses, were found.

[3]"Spectator" describes a third-party decision-maker who is not economically affected by the distributive choice she is doing.

2 Background

Experimental research in the lab allows to investigate how fairness is perceived on an individual level and how different perceptions can result in unequal economic outcomes. Fehr and Schmidt (1999) provide a simple model that alters the utility of a person based on two types of inequity aversion. By assuming that people receive disutility from having "more" or "less" payoff than others, the model is better suited to predict the outcomes of dictator and ultimatum games[4] compared to a "rational" utility model that assumes agents will simply maximize their payoff and therefore offer and accept the Nash-equilibrium of the game. Other prominent micro-models highlight the importance of fairness attitudes in shaping individuals' redistribution decisions (e.g. Benabou and Tirole 2006, Alesina and Angeletos 2005).

Many lab studies tested the ultimatum game in several variants, for example, with different levels of anonymity, rounds, groups of respondents, or frames. One interesting study by Falk et al. (2003) which is also based on an ultimatum game, tests the impact of proposer's alternatives on the likelihood that a responder is going to accept an unequal offer. The authors find that the responder is less likely to accept an uneven offer if the proposer had a more equitable option to choose. The important implication is that it might not be sufficient to measure people's perception of fairness solely based on the monetary distribution of an action but also the actor's intention. This relates to studies that utilize a third-party spectator design which is highly relevant to this thesis. In these designs, the distributive decision is not influenced by direct egoistic incentives and focuses on the spectator's fairness ideal. In a very recent study, Cappelen et al. (2022a) find that a third-party spectator with the possibility to distribute an allocation between two agents decides for more unequal distributions in case an agent is in a worse economic position because she made a nominal or forced choice. This finding reveals that the act of making a choice might create an agency, even if the agent had no meaningful alternative option available. In another paper, Cappelen et al. (2023) show that a spectator is less willing to

[4]In a dictator game, one individual is the "dictator" who possesses a certain amount of money and unilaterally determines the allocation to be transferred to another participant. The other participant has no influence over the decision-making process. In the ultimatum game, one participant, the "proposer", offers a certain division of money, while the other participant, the "responder", has the power to either accept or reject this proposal. A rejection would result in no monetary gain for both.

redistribute from a high to a low earner, in case the difference in earnings was determined by a combination of merit and luck. While spectators are likely to redistribute in case luck is the source of inequality, the interaction of luck and more merit leads to a much higher inequality acceptance. They conclude that the merit of a person seems to justify other (non-merit) reasons for inequality. The authors call this pattern *Merit Primacy Effect*. The distributive behavior of spectators is also affected by uncertainty regarding the source of inequality. In a study in which either luck or performance determines the outcome of a game between two agents, Cappelen et al. (2022b) find that uncertainty causes a strong egalitarian pull among a majority of meritocratic individuals. Here, the degree of uncertainty varies with the probability that luck instead of performance determines the outcome of the game.

In a contemporary working paper, Preuss et al. (2022) examine whether the distribution decisions of spectators are differently affected in case a worker's effort is magnified by luck compared to if it is directly determined by it. By "magnified" luck they refer to inequality of opportunity which they simulate by endogenously varying a worker's so-called Multiplier. This Multiplier will be multiplied by the number of correct tasks which equals a score. The player with the highest score then receives a fixed price in a "winner takes it all" fashion. Thus, a higher multiplier (better opportunities) therefore increases the probability of success. Redistribution is significantly lower in cases of luck in opportunity compared to pure luck, although luck in opportunity has a very strong (non-linear) impact on the chance of winning the game. Similar to the findings of Cappelen et al. (2023) the authors suggest that when luck and effort interact with each other, a spectator has difficulties assessing the source of inequality. Additionally, they find evidence that spectators might find it difficult to correctly asses the non-linear impact of the treatment on a worker's chance of winning, eventually resulting in low redistributions.

Another paper that examines the inequality of opportunity in an experimental labor market is given by Dong et al. (2022). Their paper aims to investigate individuals' fairness preferences when presented with two types of unequal opportunities. Here, two treatments were implemented to represent different types of opportunities: a random education treatment and a random employment treatment. Additionally, the authors compare spectators' behaviors to two control treatments similar to Almås et al. (2020) in which either luck or merit determines the outcome of the game. An important finding of this study is, that spectators do consider the influence of different opportunities in their distributive decisions. Here, both inequality of opportunity treatments show aggregated distributive behavior that is between the two control treatments. In both studies, spectators were presented with a pair of workers who had the same incentives to perform well but experienced unequal opportunities. Then, the spectators were asked to make redistribution decisions, motivated by underlying fairness considerations. Each paper sheds light on important aspects of what drives spectators' behavior when deciding whether to redistribute and how much to redistribute.

My thesis aims to delve deeper into the mechanisms that may influence people's perception of fairness in a distributive setting. In light of the existing literature, numerous aspects merit closer investigation. In particular, inequality of opportunity is an element that should be further investigated. In the following, I explain my motivation to extend the research on this area and why I believe that the previous literature in this area does not optimally capture perceptional aspects of (real) inequality of opportunity outside the lab.

First, I reason that inequality of opportunity should be implemented as a factor with a temporal difference to actual earnings. Second, it should affect a worker's earnings only indirectly by interacting with workers' efforts outside the earning stage.

The economic circumstances we are born into, the quality of schooling we receive as a child, and the support our parents can and want to give us certainly determine our opportunities. Within a country, different circumstances can lead to the same skill set among adults with different opportunities, but those with worse opportunities need more merit either by being more talented or by exerting more effort than those with better opportunities. In Preuss et al. (2022), the multiplier that simulates unequal opportunities is exogenously given and impacts the opportunity to win the game. The mechanism is very similar, however, there is no temporal difference between the Multiplier affecting the production. Thus, merit and Multiplier directly influence the performance of the workers which I do not believe to adequately simulate inequality of opportunity but rather different skill levels (which might come due to inequality of opportunity). This is why I designed inequality of opportunity as a factor that interacts with a worker's effort in an early round that is not directly linked to earnings, and which influences a worker's

Multiplier in an endogenous way.

Then, in a second round that is directly linked to the earnings of the game (and also is temporally closer), the earned multiplier will, together with a direct factor, be multiplied by a worker's performance in the second round. Here, one can think of the multiplier as representing skills or education that an agent brings to the earning stage of the game. These skills had to be earned in the previous round, similar to students spending several years of their lives in school or university. Effort can be seen as a universal currency that one invests for directly and indirectly achieving monetary and non-monetary goals. Inequality of opportunity leads to differences in how easy it is for a worker to acquire skills which indirectly affects his future income. A meritocratic fairness ideal would consider both sources of inequality and how they impact the economic outcome.

To my knowledge, this study is the only experimental approach that compares luck based on inequality of opportunity to luck based on a more direct source in a clear way. "Clear" in the sense that both sources of inequality have the same impact on the final outcome of a game. Therefore, this study design allows me to test whether spectators have more difficulty to incorporate luck in opportunity compared to direct luck. The experimental design shall be used to test whether spectators are more likely to hold participants responsible for exogenous inequalities in round 1 compared to inequalities in the earning round. Additionally, I utilize the data gathered to conceptually replicate previous findings of Cappelen et al. (2023) and Cappelen et al. (2022b).

The experimental design is inspired by related literature in Behavioral Economics as well as theory from Cognitive Psychology - in particular theories that related human problem-solving abilities to so-called mental models. Mental models are internal representations of external reality that people utilize to solve (complex) problems (Jones et al., 2011) such as determining a fair distribution of resources.

3 Experimental Design

The experiment was conducted using a Qualtrics survey that allowed me to create an anonymous survey with randomization, both by randomizing the treatment between respondents and varying the order of questions within a treatment. The experiment consists of participants deciding to (re)distribute money between two hypothetical workers. The experiment is described and framed as having three rounds in total. Two rounds in which individuals, referred to as "workers", work on a real-effort task and one round in which the participants of the survey experiment, referred to as "spectators", observe different scenarios. Based on these scenarios, spectators are asked to decide on a (re)distribution of money between the two workers. The first two rounds are hypothetical, so from my perspective, the experiment only exists out of the spectator round - which again might be separated into five scenarios.

The structure of the experiment and the function of the three rounds are explained to the participants in detail. Participants are informed that the experiment is hypothetical but that they should imagine it to be real. In order to ensure that participants carefully read the experimental instructions, the survey includes three control questions that need to be answered correctly. This is done by enabling the forced response option in Qualtrics for these three questions. It is important to note that wrong answers do not automatically result in an exclusion of the observations. In case a participant selects a wrong answer, a solution will pop up and explain the specific mechanism of the experiment again. The participant is then allowed to switch to the correct answer and continue with the survey. I present the exact experimental instructions and displayed scenarios in Appendix B.

3.1 Real-Effort Rounds

These two rounds do not take place in reality. But since participants of the survey experiment are asked to imagine that the real-effort rounds and pairs of workers that they observe are real, it is important to explain the mechanisms and essence of the real-effort tasks[5].

[5] It might cause some confusion for the reader that these tasks are called "real-effort" rounds, despite them being hypothetical. For better readability, I decided to keep referring to "real-effort task". The reader of this thesis can imagine it to be called "hypothetical real-effort task".

In both rounds, the workers have similar types of hypothetical real-effort tasks. Namely, solving math problems that require adding three 3-digit numbers. This type of real-effort task is similar to the one in a famous study by Niederle and Vesterlund (2007) in which participants had to add five 2-digit numbers. I chose this type of real-effort task, as it is easy to understand for survey participants. However, a potential disadvantage is that spectators could identify this type of real-effort task as "unfair" since some workers might like to solve math problems while others might have math anxiety (Charness et al. 2018). This could potentially result in participants, who observe scenarios in which one worker solved more problems than the other, relating this performance difference to luck due to talent. However, assuming that those participants will be equally assigned to the two treatment conditions, this form of luck should not impact the direction of the study results, it could however decrease the effect size due to fewer people adjusting their decision based on meritocratic reasoning.

The two rounds of real-effort tasks differ in their purpose for the worker. The first round can be seen as a pre-production phase in which workers work towards increasing their productivity. The second round is a production phase in which workers earn an experimental currency based on their effort in round two, their assigned *Price*, and their *Multiplier*. The Multiplier itself is a product of workers' effort in round 1 and an assigned *Factor*. Spectators are informed that workers have the following knowledge: Workers know that they have a chance to earn up to 100 SEK and that there will be two rounds lasting two minutes each in which they will have to work on a real-effort task. In these rounds, they have to solve as many tasks as possible which will positively influence their monetary outcome. They are informed that the maximum earning is 100 SEK and that participants on average earn 50 SEK.

Spectators also learn about three aspects that workers are not informed of. First, workers do not know that they compete with another randomly matched worker for the share of 100 SEK. Second, workers are not aware of the two factors *Factor* and *Price* that are randomly assigned and affect their performance. Finally, workers do not know that the final distribution of money will be decided by a spectator.

This information is given to participants of the survey to ensure that spectators can interpret workers' performances. The information given to the (hypothetical) workers makes it reasonable to believe that they will have the incentive to exert maximum effort in the real-effort tasks since the monetary stakes are high relative to the amount of time spent on the experiment. It is also important for spectators to know that workers are unaware of the competitive aspect of the game as well as the third-party distributor. Without this piece of information, spectators could think that workers might become less motivated to exert effort. Potential reasons could be that workers know that they are not in control of their earnings which would decrease the expected utility of exerting more effort. Another reason could be that workers might receive disutility since they know that their benefit is somebody else's loss. Furthermore, the spectators know that workers are unaware of the exogenous factors that impact their performance and therefore should have no influence on the number of tasks they solve.

3.1.1 Pre-production phase

The pre-production phase is the stage in which the workers exert effort in order to increase their *Multiplier*. Spectators learn that the *Multiplier* is a product of the following equation:

$$Multiplier = \textit{Number correctly solved tasks (Round 1)} \times Factor \tag{1}$$

After two minutes, each worker will have earned a Multiplier which will be transferred to the second round of the game.

3.1.2 Production phase

For workers, this round will not feel different compared to the pre-production round. But spectators know that this round determines the amount of Token the workers earn. Here, each worker is assigned an endogenously given *Price* that impacts their earnings.

The amount of Token a worker received, is determined by the number of tasks he or she correctly solves in the production phase times the *Price* times the *Multiplier* that was obtained in the previous round. Thus, the total amount of Token that a worker obtains can be expressed by the following equation:

$$\textit{Total Amount of Token} = \textit{Number correctly solved tasks (Round 2)} \times \textit{Price} \times \textit{Multiplier} \quad (2)$$

By the end of the production phase, each worker will have earned a certain amount of Token. Spectators learn that the amount of SEK that a worker initially gets assigned is determined by their relative share of the sum of Token of both Workers. The amount of SEK that Worker X is initially assigned, can be expressed by the following equation:

$$\textit{SEK assigned to Worker } X = \frac{\textit{Amount of Token Worker } X}{\textit{Amount of Token Worker } X + \textit{Amount of Token Worker } Y} \quad (3)$$

Note that this formula is equal to equation (4) from the Theoretical Framework, in case of an underlying libertarian fairness ideal.

3.1.3 Distribution Round

This round is when spectators of the survey experiment make decisions that are based on hypothetical scenarios of workers who finished the two real-effort rounds. In total, each spectator will observe five different scenarios of randomly matched workers. Each scenario is different in regard to the workers' performances measured in the number of solved tasks in either round 1 or round 2. Here, is an overview of the five different scenarios:

Table 1. Experiment Scenarios

Scenario	**A1**	**B1**	**A2**	**B2**
Scenario 1	10	10	10	10
Scenario 2	10	15	10	10
Scenario 3	15	10	10	10
Scenario 4	10	10	10	15
Scenario 5	10	10	15	10

A1 indicates the number of tasks Worker A solved in round 1, B1 indicates the number of tasks Worker B solved in round 1, and A2 and B2 indicate the number of tasks Worker A and B respectively solved in round 2.

Scenario 1 can be seen as a "Base Case" scenario in which both workers show an equal performance in the two rounds. Scenarios 2-5 deviate from this by having one of the players show increased performance in one of the two rounds. The scenarios were designed in order to give the possibility of measuring potential differences in spectators' decisions due to workers' performance differences while at the same time making the scenarios as computationally simple and easy to understand as possible. Due to the artificial character of these scenarios, it was not possibility generate a natural variation in performance differences. Thus, to enable later analysis, I decided to hold the extent to which a worker shows superior performance in a round fixed by with 15 solved tasks compared to 10 solved tasks. The drawback of this design is that spectators might be remembered of the hypothetical character of this study which could influence their decision making. Additionally, spectators might be puzzled why a worker shows such performance differences in two essentially equal rounds. A potential solution for this problem would have been to change the type of real-effort task in the two rounds (e.g. summing matrices (Corgnet et al., 2011) in round 1 and adding 3-digit numbers in round 2). For the sake of more simplicity in the experimental instruction, I decided to remain with only one type of real-effort task.

Each scenario was presented in form of a table that gives spectators an overview of each worker's *Factor*, *Multiplier*, *Price*, amount of *Token*, and the initially assigned *Amount of SEK*. The way information was displayed is inspired by several economic lab experiments that utilized the software z-Tree (Fischbacher, 2007). One important aspect is that the exact performances of the workers in each round are not explicitly stated, the information displayed, however, allows to easily calculate them. One way for a spectator interested in a worker's performance in round 1 is to solve the following equation:

$$\textit{Number correctly solved tasks (Round 1)} = \frac{\textit{Multiplier}}{\textit{Factor}} \tag{4}$$

In a similar way, a worker's performance in round 2 can be calculated in the following way:

$$\textit{Number correctly solved tasks (Round 2)} = \frac{\textit{Amount Token}}{\textit{Price} + \textit{Multiplier}} \tag{5}$$

Under each scenario, the spectator is given the option to adjust the scenario's distribution of the 100 SEK between the two workers. This can be done by adjusting the position of a cursor displaying Worker A's share of 100 SEK on a slider. The transfer of money to Worker A follows a zero-sum logic, meaning that each SEK of the fixed 100 SEK that Worker A receives will not be given to Worker B. Spectators are explicitly made aware of the zero-sum property twice in the instructions. In order to ensure that differences in transfer do not arise due to learning or fatigue effects, the scenarios (within a treatment) will be displayed to participants in random order. After deciding the position of the cursor, a spectator can proceed to the next scenario. In case a spectator does not adjust the position of the cursor, the initially assigned distribution will be recorded as a decision. After observing all five scenarios and deciding how to distribution the 100 SEK in each, the survey experiment ends.

3.2 Experimental Conditions

In total, the experiment will have two treatment conditions that participants are not aware of. Participants are randomly assigned to one of two experimental conditions, which are referred to as *Price* treatment and *Inequality of Opportunity* treatment. Random treatment assignment occurs when a participant starts the survey. The participants are equally likely to be assigned to the *Price* and *Inequality of Opportunity* treatment. The difference between these two treatments is a change in the scenarios displayed to the participants which represent different kinds of inequality between the two (hypothetical) workers. In both conditions, Worker B has an exogenous advantage over Worker A. As mentioned before, the spectator knows that both workers are not aware of these external factors and therefore should not let them influence their level of effort. In the *Price* treatment, Worker B is assigned a *Factor* of 1 in the pre-production phase and a *Price* of 2 in the production phase. In the *Inequality of Opportunity* treatment, Worker B's *Factor* is 2, while the assigned *Price* will be 1. In both treatment conditions, Worker A has a *Price* and *Factor* of 1.

It is easy to see, that Worker B has a significant advantage over Worker A. The combination of scenarios and treatments is designed to ensure that the exogenous factors outweigh Worker A's increased performance in Scenarios 3 and 5, resulting in more money initially allocated to Worker B in each scenario. At the same time, the decision to vary *Factor* and *Price* only by the values 1 and 2 is intended to make the hypothetical scenarios as computationally simple as possible.

4 Theoretical Framework

I assume that third-party spectators can be separated into four different types of fairness ideals. The framework of these definitions is similar to the one in Cappelen et al. (2010) and implies that different fairness ideals incorporate different factors for which the spectator holds workers responsible. Cappelen et al. (2010) refer to this as *responsibility cut* which defines the set of factors that a spectator holds a worker responsible for when deciding what she believes to be a fair share of the common money pool. The responsibility cut can be formalized by introducing the following responsibility set: $R^k \subset [a,\ b,\ m,\ p]$. This set presents the factors that a spectator with fairness ideal k will hold a worker responsible for. In total, there are four factors that need to be defined:

Factor a represents a worker's performance in round 1 which will be defined as the number of correctly solved tasks in round 1. Performance equals merit because the number of correctly solved tasks can be connected to a combination of effort and talent. Factor b stands for the performance in round 2 which again is defined as the number of correctly solved tasks (in round 2). The factor m is defined as the factor by which the so-called *Multiplier* increases with each correctly solved task in round 1. And finally, there is factor p which is defined as the *Price* in Token that a participant receives for each correctly solved task in round 2.

Having defined the different factors, I can formalize the different fairness ideals in the following way:

First, there are *Egalitarians* who do not hold individuals responsible for any factor and will therefore always choose an equal split. The responsibility set of an Egalitarian is defined as $R^E \subset [\emptyset]$.

Next, there are *Meritocrats.* These "true" Meritocrats are not holding people responsible for the inequality factor m in round 1 as well as potential price inequalities p in round 2. They do, however, hold workers responsible for their productivity in round 1 and round 2. A *Meritocrat*'s responsibility set is defined as $R^M \subset [a, b]$.

Next, there is the novel type of *Myopic Meritocrats.* This type of Meritocrat does not hold workers responsible for *direct* inequalities such as different prices that result in different earnings in Token. They do however, hold workers responsible for different inequalities in round 1 in the form of a different *Factor* that results in different opportunities for workers to increase their *Multipliers*. Thus, one could also say that a Myopic Meritocrat will take the Multiplier as a measure of a worker's performance in round 1. Accordingly, the responsibility set will be defined as $R^{MM} \subset [a, b, m]$.

Finally, one has to consider spectators that can be defined as *Libertarians*. Libertarians hold people responsible for all four factors. Therefore, a Libertarian will not decide to change any initially assigned distribution. The formalized responsibility set is given by $R^L \subset [a, b, m, p]$.

Summarized, the four fairness ideals can be captured by the following equation:

$$r(R^k, a_i, b_i, m_i, p_i) = \begin{cases} \frac{1}{2} & \text{if } k = E \\ a_i b_i & \text{if } k = M \\ a_i b_i m_i & \text{if } k = MM \\ a_i b_i m_i p_i & \text{if } k = L \end{cases} \tag{6}$$

Here, the function $r(R^k, a_i, b_i, m_i, p_i)$ presents the "production" of Token that the spectator considers a worker i responsible for. The production which worker i is held responsible for depends on the value of the factors as well as the spectator's responsibility set R^k that is based on his fairness ideal k. The fairness ideal of a spectator is crucial in order to interpret the different distributive decisions, spectators take during the experiment. As the scenarios differ both between and within the two treatments, there is a certain extent in variation of the factors. However, compared to "real" studies with actual real-effort tasks, such as Cappelen et al. (2010), these variations are essentially binary. While this does not change the theoretical framework in case of the factors m and p, it represents an important restriction in case of performance. Here, previous studies can offer a broad range of continuous variation regarding the level of effort/productivity which could result in a more realistic and comprehensive assessment of the relation between performance and distributive decision. Another crucial difference between this experiment and several previous studies is that the distributive decisions are made by impartial spectators who neither participated in the real-effort tasks nor have egoistical incentives when making a decision since any available decision would result in the same financial outcome for the spectator. Thus, the framework will be similar in several aspects to papers such as Preuss et al. (2022), Dong et al. (2022), and Cappelen et al. (2023) which all use an impartial spectator setting when analyzing distributive fairness.

Having defined the different fairness ideals, I implement them into the utility function of a spectator. Since spectators in this study are not monetarily involved, I assume that they maximize the following utility function:

$$U(s^A, k^A) = -(s^A - k^A)^2 \tag{7}$$

This utility function is the same as in Preuss et al. (2022) and Dong et al. (2022). Here, k^A represents the spectator's perceived fair share for Worker A, according to the spectator's underlying fairness ideal k, which determines which factors she holds workers responsible for. s^A stands for the actual share of money that will be allocated to Worker A. Worker A is the Worker who, in this hypothetical experiment, will be disadvantaged either by having a lower *Price* or a lower *Factor*. It follows that the interior solution of the equation is given by:

$$s^A = k^A \tag{8}$$

Thus, a spectator maximizes her utility (or better: minimizes her dis-utility) by assigning Worker A the amount of money she perceives as the fair share according to her fairness ideal k. The experimental design offers the possibility to further define k^A by incorporating the different fairness ideals from equation (1). Formalized, k^A will be based on the following equation:

$$k^A = \frac{r(R^k, a_A, b_A, m_A, p_A)}{r(R^k, a_A, b_A, m_A, p_A) + r(R^k, a_B, b_B, m_B, p_B)} * \ Y \tag{9}$$

Due to the fixed money pool of 100 SEK, Y equals 100 SEK. Therefore, k^A will be equal to the relative share of Tokens, the spectator holds the two workers responsible for multiplied by 100 SEK.

Finally, putting together equations (2), (3), and (4), the framework predicts that a spectator, in order to optimize her utility function, will decide to assign Worker A an amount of SEK s^A that is equal to A's relative share of earnings that the spectator holds A and B responsible for.

Table 2 summarizes the expected distributive decisions, depending on the fairness ideal of a spectator, for each of the scenarios. One can note, that the expected decision of a Myopic Meritocrat is equal to a Meritocrat in the *Price* treatment and equal to that of a Libertarian in the *Inequality of Opportunity* treatment. Additionally, I included columns that specify intervals for decisions that do not fit into any of the fairness ideals. Here, I define Soft Meritocrats as spectators who take a distributive decision between the Libertarian and Meritocratic outcome, and Soft Egalitarian as those who decide on a distribution between Libertarian and Egalitarian outcome. Both intervals vastly overlap across scenarios and are essentially equal in case of the Base Scenarios. I added the intervals to comprehend the spectrum of spectators' decisions that lay within the spectrum of fairness ideals. When establishing a hypothesis, the direction of an effect might be driven not only by clear fairness ideals but also by the position within an interval. Decisions outside Table 2's spectrum, for example, a decision to distribute all money to Worker B, are not possible to assign a fairness ideal to. Any recorded decision, whose response was not excluded according to the Outliers and Exclusions section, will still be incorporated in the analysis.

Table 2. Classification of the Fairness-ideal

SEK assigned to Worker A

Scenario	Libertarian	Egalitarian	Meritocrat	Myopic Meritocrat	Soft Meritocrat	Soft Egalitarian
Base (Price)	33	50	50	50	[34,49]	[34,49]
B Superior Round 1 (Price)	25	50	40	40	[26,39]	[26,49]
B Superior Round 2 (Price)	25	50	40	40	[26,39]	[26,49]
A Superior Round 1 (Price)	43	50	60	60	[44,59]	[44,49]
A Superior Round 2 (Price)	43	50	60	60	[44,59]	[44,49]
Base (Ineq. of Opp)	33	50	50	33	[34,49]	[34,49]
B Superior Round 1 (Ineq. of Opp)	25	50	40	25	[26,39]	[26,49]
B Superior Round 2 (Ineq. of Opp)	25	50	40	25	[26,39]	[26,49]
A Superior Round 1 (Ineq. of Opp)	43	50	60	43	[44,59]	[44,49]
A Superior Round 2 (Ineq. of Opp)	43	50	60	43	[44,59]	[44,49]

5 Data

5.1 Power Calculation

Due to the novel approach of this thesis, there were no studies that aim to detect a similar effect. Conceptually similar studies are those of Dong et al. (2022) and Preuss et al. (2022) which both use a spectator design and investigate distributive behavior based on different sources of inequality. However, none of the papers has two rounds of real-effort tasks, and in both studies, luck can determine the outcome entirely. Additionally, in the two studies, a spectator has to redistribute from a "Winner-takes-it-all" scenario in which the worker with better performance or luck is assigned the whole pool of money. In this study, the default distribution is equal to a libertarian outcome. Since there are no similar studies available, I assuming a Cohen's d of 0.5[6] in order to calculate the necessary sample size. This assumed effect size is equal to a medium-size effect. As recommended by Benjamin et al. (2018), I will use a statistical significance threshold of 0.005 with a power of 90%. A p-value between 0.005 and 0.05 will be interpreted as suggestive evidence. The main statistical tests used will be two-sided t-tests between two conditions. This would imply a target sample size of 136 participants in each of the treatment groups. Since the study has two treatments and no control, the minimum sample size of this study is 272[7]. In recent years, several meta-studies indicate that many experimental studies fail to replicate (Open Science Collaboration, 2015, Camerer et al. 2016, Camerer et al. 2018). The stricter definition of statistical significance, aim for enough statistical power, and pre-publication of an analysis plan shall ensure more reliable results. A pre-analysis plan provides a systematic approach to reduce a researcher degrees of freedom and thus promotes the reproducibility of research. By pre-specifying the analysis in a precise way,

[6]In Preuss et al. (2022) and Dong et al. (2022) I only find very small effect sizes with a Cohen's d smaller than 0.05. Both studies have a very high number of observations, Preuss et al. (2022) for examples has a degree of freedom of more than 9000 when testing between two treatments. In case of assuming this effect size with my aimed statistical significance threshold and power, I would require a sample size of 26752. Due to the mentioned differences in my experimental design, I decided to assume a medium size effect for my study.

[7]As the minimum desired sample size of 272 was not reached, I readjusted the power calculation. In order to find significant results under a two-sided t-test with a significance threshold of 0.005 and power of 90%, the effect size would need to be equal to a Cohen's d of 0.87. Alternatively, if relaxing the specifications one would need a Cohen's d of 0.6 while conduction a two-sided t-test with a significance threshold of 0.05 and power of 80%.

a pre-analysis plan can mitigate the risk of selection bias and reporting bias caused by researchers selectively reporting significant results(Open Science Collaboration, 2015). When analyzing the data, I followed the pre-analysis plan unless explicitly noted. Any deviation from the plan is indicated in this study.

5.2 Recruiting

The observations in this experiment were collected by using a Qualtrics survey. The survey was initially sent out to Bachelor and Master students at SSE on the 13th and 14th of April 2023. Prior to distributing the link to the experiment, my research plan was preregistered at the Open Science Framework (OSF). The pre-registration can be found under the following link: https://osf.io/8n3tk/. In addition, the full pre-analysis plan is displayed in Appendix C. After ten days I evaluated whether a sample size of 272 was reached. On the 24th of April, I had 115 responses and therefore sent out a reminder to SSE students. I also extended the sample to PhD-Students from SSE, an option the pre-analysis plan allowed for and which I choose to follow after consultation with my supervisor. Five days after the reminder was sent out, the total number of responses was 199. Since the desired sample size was not reached, I extended the survey (specified in the pre-analysis plan) to students from neighboring universities, namely KTH, Karolinska Institute(KI), and Stockholm University(SU).

In order to maximize the sample size, I let the survey run until the 7th of May. Data analysis began on the 7th of May and was conducted with the statistic software R. As specified in the pre-analysis plan, there were no further observations considered after this date. On the 7th of May, data collection was stopped with 222 recorded responses. With nearly 3000[8] emails initially sent out, the approximate response rate is around 7.5%. Such a response rate can be considered low but is not surprising due to the length and cognitive demand of the experiment. Due to randomization of conditions within the study, I do not expect a non-response bias to influence the direction of between-treatment analysis. In case of within-treatment analysis, the low response rate could impact the size and

[8] I contacted every SSE student whose email I could find twice. PhD students were contacted one day after the reminder was sent out. As I had no similar access to email addresses from students from other universites in Stockholm, I submitted the link to the experiment to friends from these universities who forwarded it to friends from their program. I estimate that approximately 200 students from KI, KTH and SU got access to the survey. The response rate was very low with 3% being the upper bound.

direction of effects.

5.3 Outliers and Exclusions

Only complete surveys were included in the sample. Due to the complex nature of the experiment, it is likely that many participants started the survey but did not finish it. In case the control variable *Age* is included in a regression, I excluded all answers[9] that were under 15 and above 60 years. Since the survey was only sent out to students, numbers outside this range are unlikely to be true and therefore would only bias the regression results.

5.4 Data Overview

Table 3 gives a brief overview of the sample's demographic characteristics.

Table 3. Sample Data

	Price	Opportunity	Total
Number of Responses	41	54	95
Average Age (in years)	25.1	24.5	24.8
Experience[10]	19	31	50
No Experience	22	23	45
Male	28	33	61
Female	13	21	34

The Table shows that from 222 observations, only 95 responses could be used for analysis. This means that 57.21% of the participants who initially enrolled in the survey dropped out before completing it completely. Besides the small sample size, which makes a reliable analysis of the data very challenging, this high attrition rate might be an additional concern as it could also bias the results. This could be especially the case if the attrition rate is higher in one of the treatment conditions. Here, table 1 shows that the number of completed responses is higher in the *Inequality of Opportunity* treatment[11]. Since Qualtrics is automatically trying to balance responses between the two survey conditions,

[9] No complete response indicated an age outside the range.

[11] From 169 participants who answered the control questions, 11 did not answer any distributive question. From the 158 that can be assigned to a condition, 82 were in the *Inequality of Opportunity*, and 76 in the *Price* treatment. The attrition rate of the *Price* treatment equals 46.05%, the attrition rate of the *Inequality of Opportunity* treatment 34.15%. When conducting a chi-square test, I receive a values of 2.73 which suggests that the difference between the two attrition rates is statistically not significant.

it would be possible that later responses were mainly assigned to the *Price* treatment which could lead to biased data, especially when considering that the survey was extended to students outside of SSE, not before the 29th of April. A closer look shows, that after the 29th of April, only six responses made it to the distribution phase of the survey of which three were assigned to each treatment group[12]. Further, from 61 responses that answer at least one but not all five distributive decisions, and therefore had to be excluded from the data, 29 were within the *Inequality of Opportunity* and 32 within the *Price* treatment, suggesting an attrition rate of 35% and 43% respectively. Taken together, I do not expect a large bias due to these differences.

[12]From these six responses, four answered all five distributive questions - two in each treatment.

6 Hypotheses

In the following section, I will explain and define the statistical tests conducted in this study. In total, there are 6 Hypothesis tests and 9 Regressions. The 6 Hypothesis tests are intended to answer four research questions, which is why I will label them Hypothesis 1, Hypothesis 2, Hypothesis 3.1, Hypothesis 3.2, Hypothesis 4.1, and Hypothesis 4.2. The high number of hypotheses and regressions indicate that the thesis is object to a multiple testing problem. This circumstance is a bit softened, however, since out of the 9 regressions, 4 are stated to be entirely exploratory and therefore do not state a specific outcome. In addition, the regressions are conducted to further investigate the mechanisms of hypothesis tests.

Based on findings in related literature, I designed hypotheses to conceptually replicate certain findings of other papers and explore evidence for mechanisms that drive these previous findings. All tests have been preregistered and their results are reported in the thesis. This is done to make results more credible. Additionally, the p-value threshold for significant findings is set to 0.005 as recommended by Benjamin et al. (2018). Each deviation from the pre-analysis plan is declared in the result section.

All hypothesis tests are conducted as two-sided t-tests, comparing if means are statistically different between two conditions. Even though I state clear expectations regarding the directions of an effect, I decided to test hypotheses as two-sided tests to further increase the robustness of the findings by default. Several of the hypotheses are designed to compare proportions instead of means. Here, a z-test might be the more conservative choice. Additionally, one could argue that additional robustness checks could be conducted, for example by running Mann-Whitney U-tests that do not assume a normal distribution of the data and therefore can be applied to both, normally and non-normally distributed data. As the sample size is large enough to invoke the central limit theorem Kwak and Kim (2017), I assume means and proportions in the hypotheses to follow an approximated normal distribution. Further, by invoking the fact that population standard deviations are unknown, I decided to conduct t-tests instead of z-tests when testing the Hypotheses. It is unlikely, however, that there would be more than a small difference between the two tests. Standard errors are clustered on spectator level to account for correlated observations of a participant. As a robustness check, all hypotheses and regressions are conducted a second time with robust standard errors[13]. Additionally, I am including three control variables (Age, Gender, Experience) in the pooled OLS regressions as a further robustness check. This is done to account for covariates that might influence the treatment effect. Based on prior literature[14], I would like to include this additional robustness test to increase the statistical power of the study.

In the following, I lay out the hypotheses of the thesis.

6.1 Descriptive Analysis

Hypothesis 1:

The *Price* treatment will lead more spectators to adjust the payout compared to the *Inequality of Opportunity* treatment.

This hypothesis is tested by comparing the share of active decisions between the two treatment conditions. Every decision in which the payout is unequal to the initially assigned distribution will count as an active decision. Here, the null hypothesis is that the sum of active decisions is going to be the same between the two treatments:

$$H0 : \%\text{of active decisions (Price)} = \%\text{of active decisions (IneqOp)}$$

$$H1 : \%\text{ of active decisions (Price)} > \%\text{ of active decisions (IneqOp)}$$

Here, (Price) refers to all five scenarios within the Price treatment, and (IneqOp) refers

[13]This is a deviation from the pre-analysis plan in which I specified to do this robustness check with "robust standard errors clustered on individual level". As clustered standard errors already account for potential correlation within a cluster, I decided to apply conventional robust standard errors (Eicker–Huber–White standard errors) instead. By doing this, I test for heteroskedasticity present in the data which might not be captured by clustering standard errors on individual level.

[14]Women on average are shown to be more inequity averse than men (Carlsson et al., 2005) while age might be positively correlated with a higher probability of holding a meritocratic fairness ideal (Cappelen et al., 2010). Finally, I was interested whether being familiar with economic experiments might influence spectators' decisions.

to all five scenarios within the Inequality of Opportunity treatment.

Hypothesis 2:

The *Price* treatment will lead to a higher share of SEK transferred to Worker A. Hypothesis 2 is tested by comparing the average amount SEK transferred to Worker A between the two experimental conditions:

$$H_0\text{: Avg. amount SEK transferred to A (Price)} = \\ \text{Avg. amount SEK transferred to A (IneqOp)}$$

$$H_1\text{: Avg. amount SEK transferred to A (Price)} > \\ \text{Avg. amount SEK transferred to A (IneqOp)}$$

Hypothesis 1 and Hypothesis 2 are designed to directly test whether spectators might process different types of inequality differently. The expectations here are conceptually in line with the findings of Preuss et al. (2022) and Dong et al. (2022) in which participants reacted less sensitively to inequality of opportunity conditions compared to for example more direct inequality in luck conditions.

One could argue, that one should test hypothesis 1 by defining a range (e.g. +/- 5 SEK) around the initial outcome, where all values within this range are not considered active decisions. My decision not to do so was based on the motivation that I am particularly interested in a different share of people who wish to alter a libertarian distribution. Based on the theoretical framework, the two hypotheses could be driven by participants, who can be defined as Myopic Meritocrats. Myopic Meritocrats in the *Price* treatment would be motivated to change the initial outcomes of the scenarios, behaving like "pure" Meritocrats. Myopic Meritocrats in the *Inequality of Opportunity* treatment, however, neglect different *Factors* and therefore would maximize their utility by not changing the outcome, essentially behaving like Libertarians. In that way, Hypothesis 1 is complementing the more general Hypothesis 2. I decided to test both aspects, to be able to identify whether higher average transfers might come with a higher share of active decisions - which would be evidence for that an average higher transfer is partly driven by more spectators deciding to adjust the initially assigned outcome. Here, the hypothetical character of the survey might become a problem due to potential "Action Bias" (Patt and Zeckhauser, 2000), describing the tendency of people to prefer action over inaction. In specific, people essentially agreeing with an initial distribution might still prefer to change it due to this bias. Thus, this bias would alter the utility function of the spectators in a sense that the act of performing an active decision yields extra utility that would lead to spectators adjusting the initial distribution even if it perfectly aligns with their fairness ideal.

6.2 Merit Primacy Effect

Hypotheses 3.1 and 3.2 are designed to test whether the Merit Primacy Effect - a situation that Cappelen et al. (2023) refer to as a positive complementary between luck and merit in the reward of the high earner – can be identified in this experiment. "Luck" in my study never fully determines the outcome after the working stage, but in each treatment, Worker B has "Luck" on his side since *Factor* or *Price* are in his favor. Similar to the descriptive analysis, the two hypotheses are intended to complement each other to identify whether the Merit Primacy Effect might be partly driven by spectators who perceive a libertarian outcome as fair in case the worker with exogenous luck also performed better. Both tests are a within-subject design.

Hypothesis 3.1:

Comparing the four scenarios (two in each treatment group) in which Worker B performs better than Worker A, to the remaining six scenarios in which Worker B has a worse or equal performance, the average **adjusted** transfer to Worker A will be less in case Worker B performed better. Average adjusted transfer to Worker A is defined as the difference between the actual transfer to Worker A and the initially assigned amount which would be transferred in case a spectator decides not to adjust the distribution. The reason for comparing average adjusted transfers instead of average transfers is that the scenarios have different initial distributions. In particular, in the scenarios in which Worker B performed better, either in round 1 or round 2, the initial transfer to Worker A is lower than in the other three scenarios. An important limitation of this test is that spectators who can be defined as Egalitarians (and similarly Soft Egalitarians) are not affected by the treatments and also will show higher adjusted transfers in scenarios in which Worker A receives less initial money – which are the scenarios in which Worker B performed better. Therefore, Egalitarians directly oppose the Merit Primacy Effect in this test. So, in case the share of (Soft) Egalitarians in the population is high enough, the Hypothesis could be reversed, as these spectators would adjust the distribution more in scenarios in which Worker B performed better. Not because of exogenously given factors or differences in performance of the workers, but rather due to the less equal distribution. Hypothesis 3.1 is the primary test of the Merit Primacy Effect and consists of a comparison between the averages of these two groups:

H_0: Avg. adjusted transfer to A in SEK (B performs better) =
Avg. adjusted transfer to A in SEK (B does not perform better)

H_1: Avg. adjusted transfer to A in SEK (B performs better) <
Avg. adjusted transfer to A in SEK (B does not perform better)

"B performs better" includes the scenarios 2 and 4 of each treatment group, whereas "B does not perform better" includes the scenarios 1,3, and 5 of both treatments.

Hypothesis 3.2:

Additionally, the Merit Primacy Effect is tested by comparing the share of active decisions made by spectators. Here, the four scenarios in which Worker B performs better are expected to have a lower share of active decisions. The necessity of this test arises partly due to the mentioned limitation regarding egalitarian spectators. A lower share of active decisions would indicate evidence that spectators would be more likely to accept a given distribution in case Worker B performed better, even though an exogenous factor plays an important role as well. However, as in the previous test, it could be possible that spectators instead of acting according to the Merit Primacy Effect, could be more focused on the different initial distribution itself. Here, the scenarios in which Worker B performed better and therefore, Worker A got a lower initial share of money compared to the other three (six) scenarios, could also result in more spectators making active decisions. The Hypothesis can be formalized by:

H_0: % of active decisions (B performs better) = % of active decisions (B does not perform better)

H_1: % of active decisions (B performs better) < % of active decisions (B does not perform better)

A better way to test these hypotheses would be to have more variation in the scenarios.

Especially regarding the strength of inequality while holding the initially assigned distributions constant. Such additional scenarios would allow to compare situations in which workers are assigned the same initial outcome distribution but with variation in the source of these outcomes. The Merit Primacy Effect could then be tested by comparing several scenarios in which similar distributions would be due to 1. pure randomness, 2. pure difference in performance, 3. different exogenous factors with equal performance, and finally, a combination of these factors. The reason for not doing so is the trade-off between expected survey responses and the variation of data. As the survey is already quite long and cognitively demanding, I decided against adding further scenarios. At the same time, the five scenarios of the experimental design need to have the assigned specifications to ensure the possibility of the Descriptive Analysis (Hypotheses 1 2).

6.3 The Egalitarian Pull

Hypotheses 4.1 and 4.2 are aiming to test for what Cappelen et al. (2022b) refer to as "Egalitarian Pull". It describes a situation in which meritocratic spectators assign an egalitarian distribution due to uncertainty regarding the source of inequality. Similar to Hypotheses 3.1 and 3.2, I am trying to conceptually replicate previous scientific findings. Compared to Cappelen et al. (2022b), in which uncertainty is given by varying the probability that a given distribution is due to luck or performance, my conceptual replication of the Egalitarian Pull aims to investigate whether the *Inequality of Opportunity* treatment creates *perceived* uncertainty. Compared to the more general Hypotheses 1 and 2, the following two hypotheses test whether spectators might react differently to a treatment in a specific situation.

Hypothesis 4.1:

The proportion of distributive decisions that are equivalent to an egalitarian outcome (thus cases in which spectators decide to do a 50/50 split) will be smaller[15] in the

[15] Here, I typed a mistake in the Pre-Analysis Plan. In the text of Hypothesis 4.1 it is written that the share of egalitarian outcomes will be **higher** in the mentioned scenarios of the *Price* treatment. It is supposed to be smaller. The formalized H_1 is correctly written in the plan. This is also in line with Hypothesis 4.2 in which the share of Meritocratic outcomes is higher in the *Price* treatment. With the idea that Soft Myopic Meritocrats behave according to the Egalitarian Pull and therefore will be more likely to assign an egalitarian distribution in the prevalence of the different *Factors*.

scenarios of the *Price* treatment compared to the *Inequality of Opportunity* treatment. This hypothesis focuses on the scenarios in which Worker A performed better than Worker B. The idea is that these are the situations in which meritocrats would optimize their utility by assigning Worker A more than half of the money.

H_0: % of 50/50 splits (Price') = % of 50/50 splits (IneqOp')

H_1: % of 50/50 splits (Price') < % of 50/50 splits (IneqOp')

Hypothesis 4.2:

Additionally, I expect that the % of meritocratic outcomes will be higher in the *Price* treatment of these scenarios.

H_0: % meritocratic outcomes (Price') = % meritocratic outcomes (IneqOp')

H_1: % meritocratic outcomes (Price') > % meritocratic outcomes (IneqOp')

Price' refers to Scenarios 3 and 5 of the *Price* treatment, and IneqOp' refers to Scenarios 3 and 5 of the *Inequality of Opportunity* treatment.

Meritocratic outcomes are defined as outcomes in which the relative share distributed is equivalent to the relative effort of the two workers measured in the number of tasks solved in both rounds. If for example, Worker A solves twice as many tasks as Worker B, a meritocratic decision would assign Worker A twice as many SEK as Worker B (67/33).

6.4 Regressions

Regression - Descriptive Analysis

In order to identify evidence that could tell how variables might affect behavior, I will run several regressions.

The first two regressions will be based on the following equation:

$$D_i = \alpha_0 + \alpha_1 \text{Perf}(B)_i + \alpha_2 \text{IneqOp}_i + e_i \tag{10}$$

In the first regression, the dependent variable D_i stands for the avg. amount SEK distributed to Worker A. In the second regression, D_i stands for the % of active decisions. $\text{Perf}(B)_i$ is a binary variable that equals 1 in case Worker B performed better than Worker A (that is the case in scenarios 2 and 4 in each treatment). IneqOp_i is a binary variable that equals 1 in case the observed scenario is in the Inequality of Opportunity treatment, and 0 in case that the observed scenario is in the Price treatment. i refers to spectator i who makes a decision in this observation. In both regressions, the coefficients of both binary variables are expected to be statistically significant. In both cases, α_1 and α_2 are expected to be negative.

Regression - Merit Primacy Effect

Equation (6) will be run a third time, with D_i being the avg. adjusted transfer to Worker A in SEK (defined as in Hypothesis 3.1). Here, the coefficient α_1 is expected to be negative and statistically significant. Since Worker B, has external luck either in the form of a better Factor or a better Price by default, better performance with always interact with "luck".

In addition, I will run a regression that aims to identify whether Worker B's interaction of luck and merit is more influential in cases where merit and luck interact in the same round.

$$\begin{aligned} D_i = \alpha_0 + \alpha_1 \text{Perf}(B1)_i + \alpha_2 \text{Perf}(B2)_i + \alpha_3 \text{IneqOp}_i \\ + \alpha_4 \text{Perf}(B1)_i \times \text{IneqOp}_i + \alpha_5 \text{Perf}(B2)_i \times \text{IneqOp}_i + e_i \end{aligned} \tag{11}$$

$\text{Perf}(B1)_i$ and $\text{Perf}(B2)_i$ are binary variables that indicate whether Worker B performed better than Worker A in scenarios 2 and 4 respectively.

Equation (7) will be run a second time, with D_i being the % of active decisions. In both cases, the coefficients of the interaction terms are the main focus. α_4 is expected to be positive and while α_5 is expected to be negative. As this regression is mainly exploratory, I do not explicitly expect the coefficients to be statistically significant.

Regression - Egalitarian Pull

To further investigate what variables might drive the Egalitarian Pull, I will run logistic regressions in which the dependent variable is the probability of an egalitarian outcome (defined as a 50/50 split of the 100 SEK). To make the interpretation of the coefficients easier, I will define the binary variables $\text{Perf}(A)_i$, $\text{Perf}(A1)_i$, and $\text{Perf}(A2)_i$ indicating better performances of Worker A. The regressions are based on the following two equations:

$$P(50/50) = \alpha_0 + \alpha_1 \text{Perf}(A)_i + \alpha_2 \text{IneqOp}_i + e_i \tag{12}$$

$$\begin{aligned} P(50/50) = \alpha_0 &+ \alpha_1 \text{Perf}(A1)_i + \alpha_2 \text{Perf}(A2)_i + \alpha_3 \text{IneqOp}_i \\ &+ \alpha_4 \text{Perf}(A1)_i \times \text{IneqOp}_i + \alpha_5 \text{Perf}(A2)_i \times \text{IneqOp}_i + e_i \end{aligned} \tag{13}$$

The regression based on equation (8) is exploratory which is why I do not explicitly expect the coefficients to be statistically significant or to go in a certain direction.
The regression based on equation (4) could reveal whether the Egalitarian Pull could be driven by interactions of performance and source of inequality. This regression is mainly exploratory, which is why I do not expect coefficients to be statistically significant. I do, however, expect α_4 to be positive and α_5 to be negative. Additionally, I will run equations (8) and (9) with the probability of a meritocratic outcome as the dependent variable. Again, both regressions are mainly exploratory, but I expect in the regression based on equation (9), coefficients α_4 to be negative and α_5 to be positive. I focus on coefficients of interaction terms because I am interested if the better performance of Player A in an early round, combined with an exogenous disadvantage due to a worse *Factor* might increase the probability of an egalitarian outcome while decreasing the probability of a meritocratic outcome. All regressions are intended to extend the analytical scope of the previous Hypothesis tests.

7 Results

Before focusing on the analytical results, Table 4 offers a descriptive overview of the data obtained during the experiment:

Table 4. Descriptive Data Hypotheses 1 and 2

	Price	Opportunity
Share of Active Decisions Scenario 1	0.98	0.94
Share of Active Decisions Scenario 2	0.98	0.96
Share of Active Decisions Scenario 3	0.98	0.96
Share of Active Decisions Scenario 4	0.95	0.96
Share of Active Decisions Scenario 5	1	0.98
Share of Active Decisions Total	0.98	0.96
Avg. Transfer Scenario 1	46.9	50.1
Avg. Transfer Scenario 2	41.5	44.1
Avg. Transfer Scenario 3	53.0	54.2
Avg. Transfer Scenario 4	41.1	45.9
Avg. Transfer Scenario 5	53.6	54.7
Avg. Transfer Total	47.2	50.0

Note: Avg. Transfer in SEK to Worker A

The table shows that the share of active decisions is close to 1 in any of the scenarios. On average, the share of active decisions is slightly higher in the *Price* treatment. The average transfers in SEK across scenarios show a clear pattern. Here, the numbers are quite similar in case of Scenario 2 and Scenario 4, in which Worker B performed superior[16]. Similar to this, the average transfers in Scenario 3 and Scenario 5 are similar and even exceed 50 SEK, implying a considerable share of Meritocrats in both treatments. As average transfers are higher in the *Inequality of Opportunity* treatment, one can already infer that the direction of Hypothesis 2 will go towards the other direction[17]. Also, it seems unlikely that the higher average share of active decisions in the *Price* treatment will be statistically different.

[16]At the same time the average transfer is much closer to 50 SEK than to the initially assigned 25 SEK, to an extent that must be driven by (Soft) Egalitarians (as (Soft) Meritocrats would assign only assign 40 SEK in total

[17]The data shows that one spectator in the *Price* treatment assigned 0 SEK to Worker A in each of the scenarios, which can partly explain the difference. Since I did not specify to exclude such extreme values in the pre-analysis plan, the observation is considered for all tests.

7.1 Hypothesis Tests

Table 5a shows the results of the six hypothesis tests with clustered standard errors.

Table 5a. Hypothesis Testing Results

Hypothesis Test	H_0	H_1	t-statistic	p-value	Outcome
1	$p_1 = p_2$	$p_1 > p_2$	0.49	0.62	Fail to reject H_0
2	$\mu_1 = \mu_2$	$\mu_1 > \mu_2$	-1.46	0.14	Fail to reject H_0
3.1	$\mu_1 = \mu_2$	$\mu_1 < \mu_2$	4.46	0.0001	Fail to reject H_0
3.2	$p_1 = p_2$	$p_1 < p_2$	-0.52	0.6	Fail to reject H_0
4.1	$p_1 = p_2$	$p_1 < p_2$	1.66	0.1	Fail to reject H_0
4.2	$p_1 = p_2$	$p_1 > p_2$	0.82	0.41	Fail to reject H_0

Note: H_0 stands for null hypothesis. T-statistic and p-values calculated with clustered SE

From Table 5a it can be seen, that all tests fail to reject the null hypothesis. Although test 3.1 shows a p-value below the threshold of 0.005, it fails to reject the null hypothesis as the effect does not go in the expected direction. Table 5b. shows that when using robust standard errors, the results in remain largely unchanged.

Table 5b. Hypothesis Testing Results (Robust)

Hypothesis Test	H_0	H_1	t-statistic	p-value	Outcome
1	$p_1 = p_2$	$p_1 > p_2$	0.48	0.63	Fail to reject H_0
2	$\mu_1 = \mu_2$	$\mu_1 > \mu_2$	-1.46	0.14	Fail to reject H_0
3.1	$\mu_1 = \mu_2$	$\mu_1 < \mu_2$	4.45	0.0001	Fail to reject H_0
3.2	$p_1 = p_2$	$p_1 < p_2$	-0.52	0.6	Fail to reject H_0
4.1	$p_1 = p_2$	$p_1 < p_2$	1.65	0.1	Fail to reject H_0
4.2	$p_1 = p_2$	$p_1 > p_2$	0.82	0.41	Fail to reject H_0

Note: H_0 stands for null hypothesis. T-statistic and p-values calculated with robust SE

Merit Primacy Effect

Table 6 gives offers more information on the statistical tests of Hypotheses 3.1 and 3.2.

Table 6. Merit Primacy Effect

	Price	Opportunity	Diff	p-value
Number of responses	190	285		
Avg. Adj. Transfer (in SEK)	18.42	12.70	5.72	0.0001
Share of Active Decisions	0.96	0.97	-0.01	0.63

Note: P-values based on Clustered SE

190 responses were recorded for scenarios in which Worker B had a superior Performance, the number of responses in which Worker B did not have a superior Performance was 285. As can be seen in Table 6., the p-value indicates a significant difference. However, the direction of the effect is wrong. Indeed, the average adjusted transfer, in scenarios in which Worker B performed better, was significantly higher with an average adjusted transfer of 18.42 SEK (compared to 12.70 SEK in scenarios where B had no superior performance). The share of active decisions was not significantly different with 96,3 % in cases where B had superior performance compared to 97.2 %.

Egalitarian Pull

Further information regarding the statistical testing of the Egalitarian Pull can be found in Table 7.

Table 7. Egalitarian Pull

	Price	Opportunity Treatment	Diff	p-value
Number of responses	82	108		
Share of Egalitarian Decisions	0.34	0.23	0.11	0.098
Share of Meritocratic Decisions	0.27	0.16	0.11	0.411

Note: p-values based on Clustered SE

82 responses were recorded for scenarios in which Worker A had a superior Performance within the Price treatment, compared to 108 responses in the Inequality of Opportunity treatment. When comparing the proportion of egalitarian outcomes, we can see that the share is higher in the *Price* treatment. Therefore, the effect does not go in the expected direction. When comparing the proportions one can see that the share of Egalitarian outcomes is higher in the *Price* treatment with an average of 34 % participants who decide for an Egalitarian outcome compared to 23 % in the *Inequality of Opportunity* treatment. The p-value, however, is too high to reject H_0 making this effect statistically insignificant. Perhaps a large enough sample would have been powerful enough to detect a significant difference between the two groups.

When focusing on the share of meritocratic outcomes, the p-value is too high in order to reject the H_0. The shares are different in both treatments in the expected direction of H_1 as specified in the pre-analysis plan (27% in the *Price* and 16% in the *Inequality of Opportunity* treatment). The relative difference between the two treatments seems quite significant at first sight but the clustered standard errors of 0.13474728 were on a similar level as the difference between the two proportions[18].

7.2 Regression results

In the following, I will present the different regression results. All regressions were conducted by using clustered standard errors on an individual level. As specified in the pre-analysis plan, I run each regression by using robust standard errors as a robustness check. The additional regression for the results of Table 10-15 with robust standard errors can be found in Appendix A.

Descriptive Analysis

Tables 8 and 9 report the first two regression results based on equation (6) from the hypothesis section. Each regression is run in four specifications. In both regression tables, column (1) presets the regression with Clustered standard errors while (2) presents the same regression with robust standard errors. Columns (3) and (4) run the regression control variables with clustered and robust standard errors, respectively.

Table 8 shows the results of the linear regression based on equation (6) with an "amount SEK transferred to Worker A"[19] as outcome variable. Here, only the $Perf(B)_i$ indicates significance which is not surprising since the anchor, set by the initially assigned distribution is much lower compared to cases in which $Perf(B)_i$ equals 0. Additionally, columns (3) and (4) show suggestive evidence that male[20] participants on average, transfer less money to Worker A. Alternatively, one could say that female participants on average, transfer more money to Worker A.

Table 9 shows the results of a logistic regression based on equation (6) with an "active decision" made by the spectator as a binary outcome variable. It is important to note, that this presents a deviation from the pre-analysis plan in which the equation is presented as a

[18]When testing Hypothesis 4.2 with normal standard errors, I found a t-statistic of 1.8321 and a p-value of 0.06886 suggesting high correlation within individual responses.

[19]Strictly speaking, this also represents a deviation from the pre-analysis plan, since I specified "**avg.** amount transferred to Worker A" as the outcome variable of the regression. This, however due to the binary predictor variable $Perf(B)_i$ would not yield meaningful results.

[20]In the pre-analysis plan I designed gender to be a categorical variable with three levels. But since no observation of non-binary participants was recorded, I coded the gender variable binary.

Table 8. Regression Results I

	Dependent variable: Amount SEK transferred to Worker A			
	(1)	(2)	(3)	(4)
Perf(B)	-8.241^{***}	-8.241^{***}	-10.177^{***}	-10.177^{***}
	(1.251)	(1.262)	(1.101)	(1.105)
treatment	1.541	1.541	1.939	1.939
	(1.756)	(1.236)	(1.644)	(1.056)
age			0.038	0.038
			(0.134)	(0.102)
male			-3.051^{*}	-3.051^{*}
			(1.469)	(1.115)
Experience			0.987	0.987
			(1.590)	(1.039)
Constant	49.621^{***}	49.621^{***}	50.234^{***}	50.234^{***}
	(1.405)	(1.060)	(3.960)	(2.942)
Note:			*p<0.05; **p<0.005; ***p<0.001	

Table 9. Regression Results II

	Dependent variable: Log Odds of Active Decision			
	(1)	(2)	(3)	(4)
Perf(B)	−0.281	−0.281	−0.009	−0.009
	(0.420)	(0.528)	(0.014)	(0.017)
treatment	0.387	0.387	0.011	0.011
	(0.854)	(0.557)	(0.025)	(0.015)
age			0.0003	0.0003
			(0.002)	(0.001)
male			−0.057*	−0.057***
			(0.027)	(0.015)
Experience			−0.044	−0.044*
			(0.029)	(0.017)
Constant	3.399***	3.399***	1.020***	1.020***
	(0.653)	(0.426)	(0.049)	(0.033)

Note: *p<0.05; **p<0.005; ***p<0.001

linear regression. Since the outcome variable is binary, I decided to change the regression to a logistic regression model. I still calculated the results based on a linear regression which can be found in Appendix A. Compared to Table 8, Worker B's superior performance does not seem to significantly influence the probability of an active decision. Again, the control variable *male* shows evidence to be associated with a lower probability of an active decision. In column (4) in which the logistic regression was calculated with robust standard errors, the coefficient was even below a p-value threshold of 0.001. This again, indicates that individual correlations underestimate bias standard errors of a regression.

Merit Primacy Effect

The following two tables show regressions that aimed to further investigate the relationship between variables that might be associated with the Merit Primacy Effect.

Table 10 presents the linear regression results based on equations (6) and (7). Again, instead of "average adjusted transfer" as written in the pre-analysis plan, the outcome variable shall be "adjusted transfer".

As already implied in the hypothesis test, $Perf(B)_i$ is positively associated with higher adjusted transfers on average. Similar to the results in Tables 8 and 9, the control variable *male* shows suggestive evidence to be associated with lower adjusted transfers on average. It is interesting to note that, when running the more specified equation (7), neither superior performance in a single round nor the interaction of Worker B's superior performance with the *Inequality of Opportunity* treatment are significantly associated with the dependent variable.

Table 11 presents the logistic regression results based on equation (7) with "active decision" as the binary outcome variable. Again, the pre-analysis plan described this regression as linear. Results based on a linear regression model with both clustered and robust standard errors can be found in Appendix A.

One can take from Table 11, that neither the coefficients of Worker B performing better nor the coefficient of the treatment variable show statistical significance.

Egalitarian Pull

Table 12 presents the results of the logistic regression investigating the Egalitarian Pull

Table 10. Regression Results - Merit Primacy Effect I

	Dependent variable: Adjusted transfer to Worker A			
	(1)	(2)	(3)	(4)
Perf(B)	5.714***	5.714***		
	(1.369)	(1.369)		
Perf(B1)			5.024	5.024
			(2.674)	(2.674)
Perf(B2)			4.585	4.585
			(2.349)	(2.349)
treatment	2.733	2.621	2.093	1.876
	(1.937)	(1.861)	(1.954)	(1.835)
age		−0.173		0.036
		(0.127)		(0.122)
male		−4.575*		−2.432
		(1.932)		(1.505)
Experience		−0.337		0.590
		(1.895)		(1.800)
Perf(B1):treatment			0.519	0.519
			(3.254)	(3.254)
Perf(B2):treatment			2.680	2.680
			(2.807)	(2.807)
Constant	11.149***	18.312***	11.512***	11.983***
	(1.612)	(3.772)	(1.713)	(3.477)
Note:				*p<0.05; **p<0.005; ***p<0.001

Table 11. Regression Results - Merit Primacy Effect II

	Dependent variable: Log Odds of Active Decision	
	(1)	(2)
Perf(B1)	0.715	0.021
	(0.586)	(0.012)
Perf(B2)	0.715	0.021
	(0.586)	(0.012)
treatment	1.106	0.033
	(0.917)	(0.028)
age		0.0003
		(0.002)
male		-0.058^{*}
		(0.029)
Experience		-0.045
		(0.030)
Perf(B1):treatment	-1.842^{*}	-0.049
	(0.938)	(0.029)
Perf(B2):treatment	-2.270^{**}	-0.070^{*}
	(0.720)	(0.031)
Constant	3.135^{***}	1.010^{***}
	(0.586)	(0.050)
Note:		*p<0.05; **p<0.005; ***p<0.001

with "egalitarian outcome" as the dependent variable. Columns (1) and (2) are based on equation (8) of the Hypotheses section, and columns (3) and (4) are based on equation (9).

Table 12. Regression Results - Egalitarian Pull I

	Dependent variable: Log Odds of Egalitarian Outcome			
	(1)	(2)	(3)	(4)
Perf(A)	0.270	0.270		
	(0.158)	(0.158)		
Perf(A1)			-0.931^{**}	-0.934^{**}
			(0.286)	(0.286)
Perf(A2)			-0.931^{***}	-0.934^{***}
			(0.272)	(0.272)
treatment	-0.510^{*}	-0.506^{*}	-1.451^{***}	-1.450^{***}
	(0.185)	(0.186)	(0.247)	(0.247)
male		-0.098		-0.111
		(0.198)		(0.226)
Experience		-0.175		-0.198
		(0.181)		(0.205)
age		0.014		0.015
		(0.024)		(0.027)
Perf(A1):treatment			3.198^{***}	3.209^{***}
			(0.371)	(0.371)
Perf(A2):treatment			1.184^{***}	1.188^{***}
			(0.327)	(0.328)
Constant	-0.414^{**}	-0.599	0.049	-0.159
	(0.142)	(0.669)	(0.154)	(0.757)
Note:				*p<0.05; **p<0.005; ***p<0.001

Table 12 shows that there seems to be suggestive evidence that the *Inequality of Opportunity* treatment has a negative impact on the probability of an egalitarian outcome. When specifying the regression by incorporating binary variables of Worker A performing better in round 1 or round 2, both round coefficients, the treatment coefficient, and the two interaction terms become statistically significant. The directions of the coefficients suggest that while superior round performance and the treatment generally decrease the probability of an egalitarian outcome, the interaction of both is associated with an increase in the log odds. In case of the interaction of superior performance in round 1 and being in the *Inequality of Opportunity* treatment, the coefficient outweighs the negative coefficients of the two dummy variables.

Table 13 runs a similar logistic regression as in Table 12, but with "meritocratic outcome" as the dependent variable. Here, the coefficient of $Perf(A)$ is highly significant and positive, suggesting a positive relationship between Worker A's superior performance and the probability of a meritocratic outcome. Further, the more specified regression shows evidence of a positive relationship between the probability of a meritocratic outcome and the interaction of treatment and with Worker A performing superior in round 1. In the pre-analysis plan[21], I expected this interaction coefficient to be negative. The coefficient of the treatment variable is statistically significant and negative.

[21] Also, in the pre-analysis plan, focused my expectation only on the coefficient of the interaction term, instead of the combined effect of the interaction term and its components.

Table 13. Regression Results - Egalitarian Pull II

	Dependent variable: Log Odds Meritocratic Outcome			
	(1)	(2)	(3)	(4)
Perf(A)	0.456***	0.458***		
	(0.136)	(0.137)		
Perf(A1)			−0.412	−0.414
			(0.254)	(0.255)
Perf(A2)			−0.522*	−0.525*
			(0.262)	(0.263)
treatment	−0.612**	−0.607**	−1.409***	−1.409***
	(0.197)	(0.197)	(0.271)	(0.270)
male		−0.007		−0.007
		(0.211)		(0.242)
Experience		−0.132		−0.153
		(0.196)		(0.225)
age		−0.027		−0.032
		(0.026)		(0.030)
Perf(A1):treatment			2.931***	2.950***
			(0.379)	(0.378)
Perf(A2):treatment			0.272	0.275
			(0.372)	(0.374)
Constant	−0.613***	0.133	−0.245	0.617
	(0.140)	(0.694)	(0.140)	(0.776)

Note: *p<0.05; **p<0.005; ***p<0.001

8 Discussion

In my thesis, I investigate the impact of two sources of inequality on spectators' perception of distributive fairness based on 95 responses. Overall, the results do not show any evidence that spectators perceive unequal outcomes due to inequality of opportunity different compared to inequality in prices. All pre-registered hypothesis tests fail to detect statistically significant differences in the predicted direction. Neither the average amount of money transferred, nor the share of active decisions is statistically different between the two treatment conditions.

For one of the hypothesis tests where I am to conceptually replicate the "merit primacy effect", I do find a statistically significant result. However, this result is in the wrong direction compared to previous research. While I predicted the average adjusted transfer to Worker A will be lower in scenarios in which Worker B performs better than Worker A, the adjusted transfer in these scenarios is instead significantly higher. Here, I believe that the hypothesis test was not ideally conceptualized since it does not account for different initial allocations between scenarios. But since the initial allocation can be seen as the reference point for determining the "adjusted amount transferred", it is likely that inequity aversion strongly opposes the merit primacy effect. In all tests comparing the share of active decisions between treatments or in a mixed design, the shares are almost identical. I have to note, that the share of active decisions is very high in general, with each scenario having a share of active decisions between 94% and 100%. According to the theoretical framework, this would suggest a very small share of spectators that can be identified as libertarians or myopic meritocrats. Compared to other studies[22] this share of active decisions is much higher. Here, I cannot disentangle whether this effect is due to the hypothetical character of this experiment, potentially amplifying a so-called "action bias" (Patt and Zeckhauser, 2000), or whether it is due to the circumstance that this experiment initially assigns a libertarian outcome instead of initially allocating all money to one worker. A "winner-takes-it-all" competition might influence a spectator's perception of the necessity to redistribute. This is for example supported by Bartling et al. (2018) who state that a winner-takes-it-all market results in more spectators taking

[22]To name an example, in Preuss et al. (2022) the share of spectators that actively redistribute across conditions is 84.1% and 90.1%

the view that the winner deserves all the payoff because earnings were determined due to her performance.

The regression results mainly support the findings of the hypothesis tests. When including controls, *male* seems to be associated with lower transfers on average, which would align with women being more inequity-averse than men (Carlsson et al., 2005). However, I cannot rule out that this might be due to an unequal distribution of male and female participants across treatment conditions[23].

Regarding the egalitarian pull, I designed the two tests to show that additional (perceived) uncertainty in the *Inequality of Opportunity* treatment, might lead to more participants deciding for an egalitarian outcome. This could be the case as spectators might be more likely to hesitate to change the initial distribution in a way that the initial high earner would eventually receive less than half of the 100 SEK. Here, I expected this effect to be driven by myopic meritocrats. Those would incorporate different *Prices* but not different *Factors* in their distributive decision. Eventually, this should lead to more distributive decisions that are according to a meritocratic outcome in the *Prices* treatment, compared to the *Inequality of Opportunity* treatment. As a potential mechanism, I reasoned that direct inequality due to different *Prices* would be easier to recognize compared to an unfair advantage due to different *Factors*. This is because *Factors* will only directly impact the *Multiplier*, not however the amount of Token. This could eventually lead to a cognitive dissonance (Konow, 2000) in which a spectator perceives the initial outcome as unfair on a rational level, but intuitively not entirely unfair as the *Multiplier* appears to be determined by performance in round 1 and after that is given in round 2. As a result, I expected the share of egalitarian distributions to be higher in the *Inequality of Opportunity* treatment. The idea behind this is, that a decision for an egalitarian distribution presents a solution for the spectator's cognitive dissonance that is created between the "rational" meritocratic fairness ideal and the "emotional" hesitation to assign Worker B less than half of the 100 SEK, despite him having earned more Token(due to the higher *Multiplier* that was earned in the past round).

The results show, however, that spectators in the *Price* treatment are actually more likely to assign an egalitarian distribution which makes it unlikely that the higher share

[23]While male participants are similarly distributed between treatment, there is a significantly lower share of women in the *Price* treatment. Therefore, *male* could be confounded with *Price*

of meritocratic distributions is driven by spectators hesitating to assign a meritocratic distribution. Focusing on the regression results, in particular, the regression with specific binary variables for Worker A performing better in rounds 1 and 2 respectively, I find that the interaction term of better performance in round 1 and being in the *Inequality of Opportunity* treatment is positively associated with the log odds of an egalitarian outcome. Here, the coefficient of the interaction term outweighs the combined coefficients of its components. This could be an evident indicator that an egalitarian outcome becomes more likely with a high *Multiplier*.

Finally, I am going to discuss some important limitations of this thesis. First, there is the hypothetical character of this experiment which, compared to similar studies, does not offer financial incentives. The lack of financial incentives, the circumstance that spectators are aware of the hypothetical nature of the study, as well as the (for a Master's Thesis) cognitively demanding and long survey experiment could make the data less reliable (Meade and Craig, 2012). Additionally, it is important to note that the vast majority of previous research tests the spectator's decision from a "winner-takes-it-all" environment in which one worker initially gets all money assigned, while the other worker does not receive any money before the spectator makes her decision. Another important difference compared to similar papers is that inequality of opportunity is implemented without any uncertainty regarding its impact on the initial distribution. This could lead to a smaller effect size compared to studies that utilize uncertainty when testing inequality of opportunity in a distributive setting. The only (perceived) uncertainty that might arise in spectators' decision-making would be due to factors outside my influence. An example would be that a spectator might be uncertain regarding a worker's effort when solving tasks in round 1 and round 2 (e.g. a talented worker could solve 10 tasks in two minutes while not exhibiting much effort, while a worker with less talent would need to exhibit higher effort in order to solve the same number of tasks in two minutes). Due to the RCT design, I assume that this kind of uncertainty would be equally prevalent in both treatment conditions. Additionally, the experiment suffers from a low response rate while also having a high attrition rate. This results in a small sample size of only 95 used observations, making it not feasible to test for medium-sized effects.

I believe that the sample size and quality of data could be significantly improved by conducting a similar study with monetary incentives, either in an economic laboratory or alternatively on an online research platform such as Prolific. Besides reaching a larger and more reliable sample, the introduction of monetary incentives would also allow for testing for differences in considering different prices vs different opportunities in a non-spectator setting. This can be done by letting workers decide the distribution of resources in the form of a dictator game. As in Cappelen et al. (2010), one would randomly decide whose workers' dictator game decision will be implemented.

I think that such a non-spectator design could be an interesting topic for future research as it could reveal whether dictators, who benefit from exogenous inequalities, would be more willing to neglect *Inequality of Opportunity* in their distributive decision. This research design might find evidence for increased "self-serving behavior" (Babcock and Loewenstein, 1997) in the case of indirect sources of inequality compared to more direct sources of inequality (such as different prices).

I believe that such findings would shed light on an important aspect of inequality acceptance in society. In specific, one could show whether people are more likely to perceive unequal outcomes as fair in case the source of inequality lays more in the past and/or influences the outcome through an additional intermediate step. In particular, could identify causal evidence regarding the extent people consider differences in opportunities to cause differences in productivity when doing their moral and economic reasoning and potentially show that people might have more difficulties incorporating certain types of "unfair" sources of inequality in their moral decision making of distributive fairness, even in the absence of uncertainty. Additionally, by further investigating this bias, one could learn more about its potential interactions with other biases and heuristics. Furthermore, the investigation of inequality of opportunities in a laboratory experiment would contribute to the existing literature on fairness perception and how it relates to differences in decision-making. Significant findings from the laboratory could be used to develop and conduct field experiments and empirical studies aiming to increase the external validity of these findings. Finally, it could be used to conceptualize economic models that better describe peoples' utility, considering "selfish" and "fairness" aspects. Such models could, for example, be used to provide a better understanding of the trade-offs people face when deciding between two policies that impact their utility from both directions. Especially in cases where those policies require the cooperation of public and private institutions or the acceptance of a large part of the population, researchers and decision-makers must obtain a better understanding of how individuals (heterogeneously) endorse such information in their perception of fairness. As Cappelen et al. (2022a) conclude, further research should set potential study results in the context of moral psychology that could help to identify the (cognitive) mechanisms of these biases and thus, help public decision-makers to overcome these biases by more effectively framing public information highlighting the causality between unequal opportunities and economic outcomes and how people might struggle to perceive these causalities. In practice however, this will be very challenging since there are many additional obstacles to overcome due to the many factors that amount to inequality of opportunity as well as its many interactions with uncertainty, social norms, or memory. While this experimental design models inequality of opportunity in a very simple and abstract way, it would be a first step investigating a novel aspect of fairness perception that might have important implications for society.

9 Conclusion

In a hypothetical survey experiment with impartial spectators, I used a novel design involving two types of inequality influencing workers' performances in two rounds of real-effort tasks. The experimental treatment conditions come without any uncertainty when providing spectators with information regarding workers' performances and sources of inequality. In my simple theoretical framework, I define several types of spectators based on different fairness ideals, including the novel type of "myopic meritocrats" that exert different distributive behaviors depending on what type of inequality influences the initial payoff of the workers. All hypothesis tests were specified in an analysis plan that was preregistered before the survey experiment was sent out and any data gathered. All tests fail to find statistically significant findings in the predicted direction. The analysis was partly constrained due to a low response rate and high attrition rate, resulting in a sample size of 95 used observations which is much lower than the minimum sample size needed to detect a medium-size effect[24]. An economic experiment in a laboratory using monetary incentives could provide a larger sample size and arguably more reliable responses. In case such an experiment would demonstrate that people perceive inequality of opportunity differently, it would add to the existing literature on the perception of distributive fairness and might inspire follow-up studies that would contribute to the understanding and external validity of this topic. A myopic view of given economic circumstances is characteristic of most distributive situations, which is why a deep understanding of the many factors contributing to this myopia is crucial in understanding how unequal opportunities impact people's perception and behavior.

[24]Based on the specifications of my power calculation.

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Appendix

A Appendix - Tables

Table A0.1 presents the regression results of Table 10 with robust standard errors.

Table A0.1: Merit Primacy Effect I (Robust)

	Dependent variable: Adj. Transfer to Worker A			
	(1)	(2)	(3)	(4)
Perf(B)_i	5.714^{***}	5.714^{***}		
	(1.369)	(1.369)		
Perf(B1)_i			5.024	5.024
			(2.674)	(2.674)
Perf(B2)_i			4.585	4.585
			(2.349)	(2.349)
treatment	2.733	2.621	2.093	1.876
	(1.937)	(1.861)	(1.954)	(1.835)
age		−0.173		0.036
		(0.127)		(0.122)
male		-4.575^{*}		−2.432
		(1.932)		(1.505)
Experience		−0.337		0.590
		(1.895)		(1.800)
$\text{Perf(B1)}_i : treatment$			0.519	0.519
			(3.254)	(3.254)
$\text{Perf(B2)}_i : treatment$			2.680	2.680
			(2.807)	(2.807)
Constant	11.149^{***}	18.312^{***}	11.512^{***}	11.983^{***}
	(1.612)	(3.772)	(1.713)	(3.477)
Note:		*p<0.05; **p<0.005; ***p<0.001		

Table A0.2 shows the results of the regression table 11 as linear regression with both clustered and robust standard errors in columns (1)+(2) and (3)+(4) respectively. This table is included since the pre-analysis plan unintentionally described the regression based on a linear model.

Table A0.2: Merit Primacy Effect II (Linear Model)

	Dependent variable: Active Decision			
	(1)	(2)	(3)	(4)
Perf(B1)	0.021	0.021	0.021	0.021
	(0.012)	(0.012)	(0.027)	(0.026)
Perf(B2)	0.021	0.021	0.021	0.021
	(0.012)	(0.012)	(0.027)	(0.026)
treatment	0.027	0.033	0.027	0.033
	(0.025)	(0.028)	(0.020)	(0.021)
age		0.0003		0.0003
		(0.002)		(0.001)
male		-0.058^{*}		-0.058^{***}
		(0.029)		(0.015)
Experience		-0.045		-0.045^{*}
		(0.030)		(0.017)
Perf(B1):treatment	-0.049	-0.049	-0.049	-0.049
	(0.029)	(0.029)	(0.041)	(0.040)
Perf(B2):treatment	-0.070^{*}	-0.070^{*}	-0.070	-0.070
	(0.031)	(0.031)	(0.046)	(0.045)
Constant	0.958^{***}	1.010^{***}	0.958^{***}	1.010^{***}
	(0.023)	(0.050)	(0.017)	(0.033)

Note: *p<0.05; **p<0.005; ***p<0.001

Table A0.3. presents the results of table 11 with robust standard errors.(This time again based on a logistic regression model)

Table A0.3: Merit Primacy Effect II (Robust)

	Dependent variable: Log Odds Ratio of Active Decision	
	(1)	(2)
Perf(B1)	0.021	0.021
	(0.027)	(0.026)
Perf(B2)	0.021	0.021
	(0.027)	(0.026)
treatment	0.027	0.033
	(0.020)	(0.021)
age		0.0003
		(0.001)
male		-0.058^{***}
		(0.015)
Experience		-0.045^{*}
		(0.017)
Perf(B1):treatment	-0.049	-0.049
	(0.041)	(0.040)
Perf(B2):treatment	-0.070	-0.070
	(0.046)	(0.045)
Constant	0.958^{***}	1.010^{***}
	(0.017)	(0.033)
Note:		*p<0.05; **p<0.005; ***p<0.001

Table A0.4 and A0.5 present the results of Table 12 and 13 with robust standard errors.

Table A0.4: Egalitarian Pull I (Robust)

	Dependent variable: Log Odds Ratio Egalitarian Outcome			
	(1)	(2)	(3)	(4)
Perf(A)	0.270	0.270		
	(0.140)	(0.141)		
Perf(A1)			-0.931^{***}	-0.934^{***}
			(0.275)	(0.275)
Perf(A2)			-0.931^{***}	-0.934^{***}
			(0.275)	(0.276)
treatment	-0.510^{***}	-0.506^{***}	-1.451^{***}	-1.450^{***}
	(0.137)	(0.138)	(0.190)	(0.190)
male		-0.098		-0.111
		(0.154)		(0.165)
Experience		-0.175		-0.198
		(0.142)		(0.152)
age		0.014		0.015
		(0.017)		(0.019)
Perf(A1):treatment			3.198^{***}	3.209^{***}
			(0.374)	(0.375)
Perf(A2):treatment			1.184^{**}	1.188^{**}
			(0.382)	(0.384)
Constant	-0.414^{***}	-0.599	0.049	-0.159
	(0.119)	(0.481)	(0.128)	(0.532)
Note:				*p<0.05; **p<0.005; ***p<0.001

Table A0.5: Egalitarian Pull II (Robust)

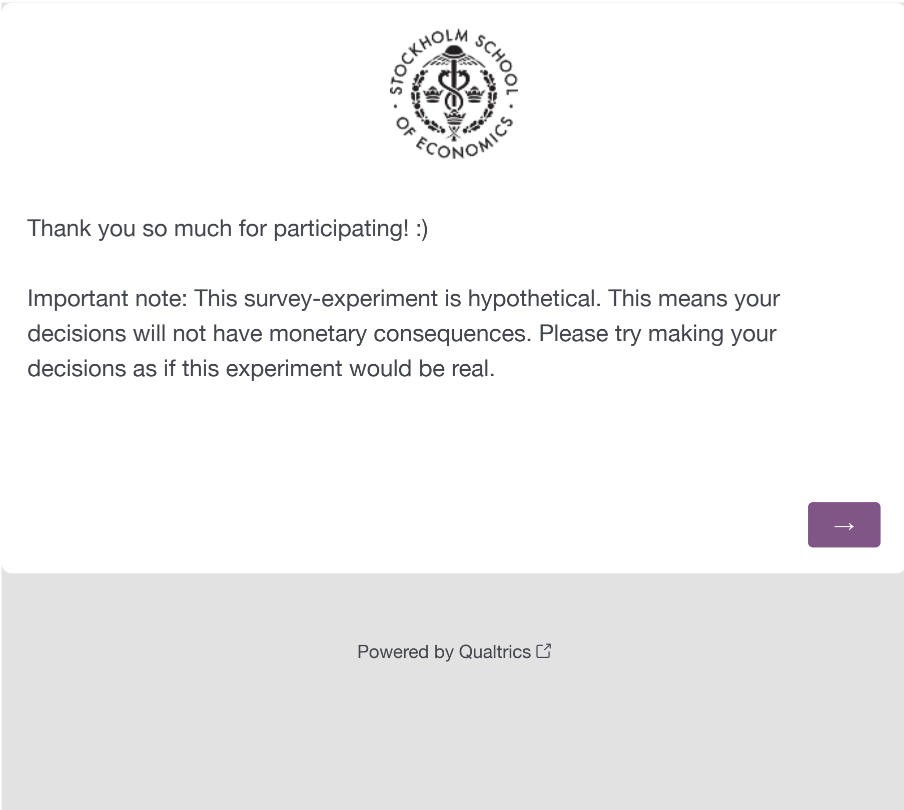
	Dependent variable:Log Odds Ratio of Meritocratic Outcome			
	(1)	(2)	(3)	(4)
Perf(A)	0.456**	0.458**		
	(0.144)	(0.144)		
Perf(A1)			−0.412	−0.414
			(0.267)	(0.267)
Perf(A2)			−0.522	−0.525
			(0.271)	(0.270)
treatment	−0.612***	−0.607***	−1.409***	−1.409***
	(0.142)	(0.143)	(0.199)	(0.200)
male		−0.007		−0.007
		(0.164)		(0.173)
Experience		−0.132		−0.153
		(0.150)		(0.161)
age		−0.027		−0.032
		(0.020)		(0.020)
Perf(A1):treatment			2.931***	2.950***
			(0.373)	(0.375)
Perf(A2):treatment			0.272	0.275
			(0.423)	(0.423)
Constant	−0.613***	0.133	−0.245	0.617
	(0.123)	(0.542)	(0.129)	(0.552)

Note: *p<0.05; **p<0.005; ***p<0.001

B Appendix - Experimental Instructions

A1 Game Description

Content that all Participants see


Thank you so much for participating! :)

Important note: This survey-experiment is hypothetical. This means your decisions will not have monetary consequences. Please try making your decisions as if this experiment would be real.

→

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Figure B.1: Game Description - Page 1

First, please answer the following questions:

What is your current age?

Gender: How do you identify?

Male

Female

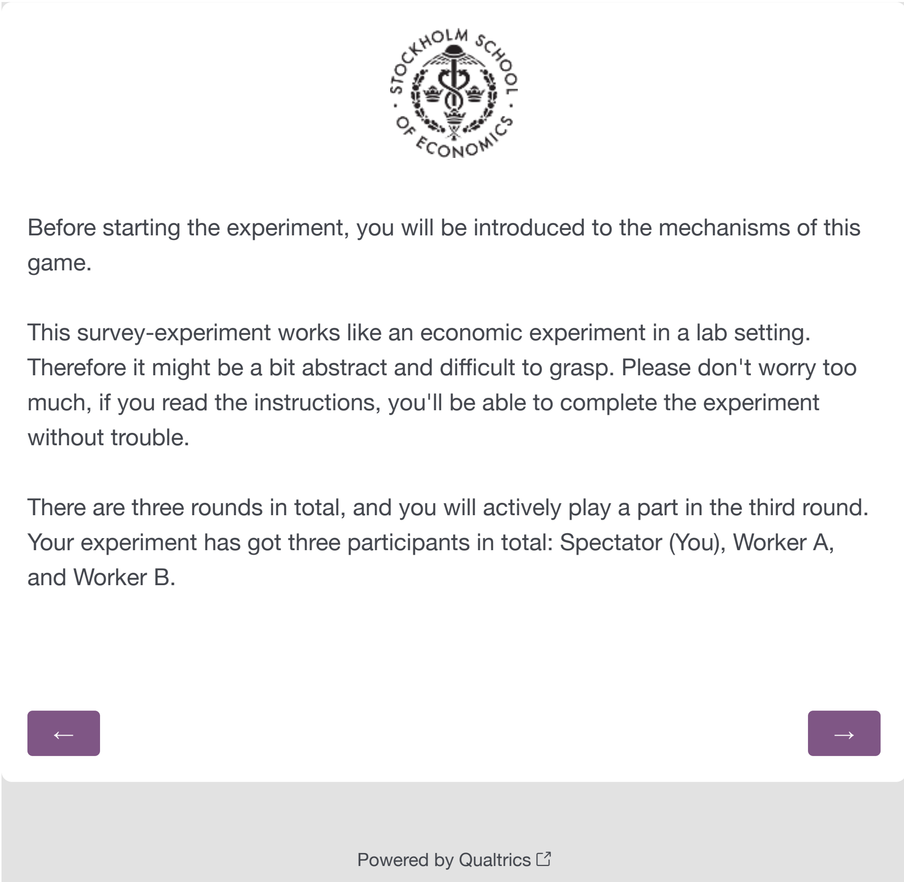
Non-binary

Are you familiar with the methods in Experimental (Behavioral) Economics or Experimental Psychology? For example by having taken a class in Behavioral Economics or having read papers with experimental lab studies.

Yes

No

Figure B.2: Game Description - Page 2



Before starting the experiment, you will be introduced to the mechanisms of this game.

This survey-experiment works like an economic experiment in a lab setting. Therefore it might be a bit abstract and difficult to grasp. Please don't worry too much, if you read the instructions, you'll be able to complete the experiment without trouble.

There are three rounds in total, and you will actively play a part in the third round. Your experiment has got three participants in total: Spectator (You), Worker A, and Worker B.

← →

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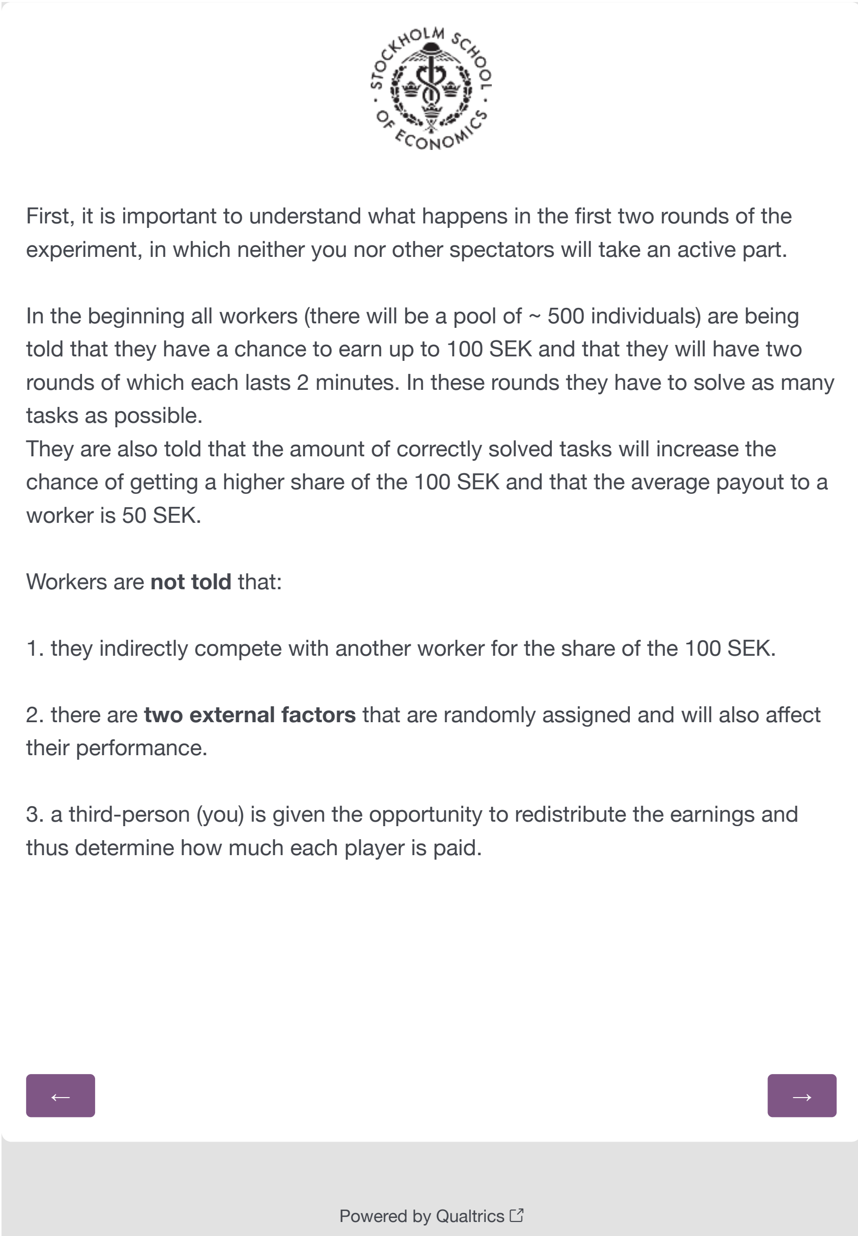
Figure B.3: Game Description - Page 3



First, it is important to understand what happens in the first two rounds of the experiment, in which neither you nor other spectators will take an active part.

In the beginning all workers (there will be a pool of ~ 500 individuals) are being told that they have a chance to earn up to 100 SEK and that they will have two rounds of which each lasts 2 minutes. In these rounds they have to solve as many tasks as possible.
They are also told that the amount of correctly solved tasks will increase the chance of getting a higher share of the 100 SEK and that the average payout to a worker is 50 SEK.

Workers are **not told** that:

1. they indirectly compete with another worker for the share of the 100 SEK.

2. there are **two external factors** that are randomly assigned and will also affect their performance.

3. a third-person (you) is given the opportunity to redistribute the earnings and thus determine how much each player is paid.

← →

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Figure B.4: Game Description - Page 4

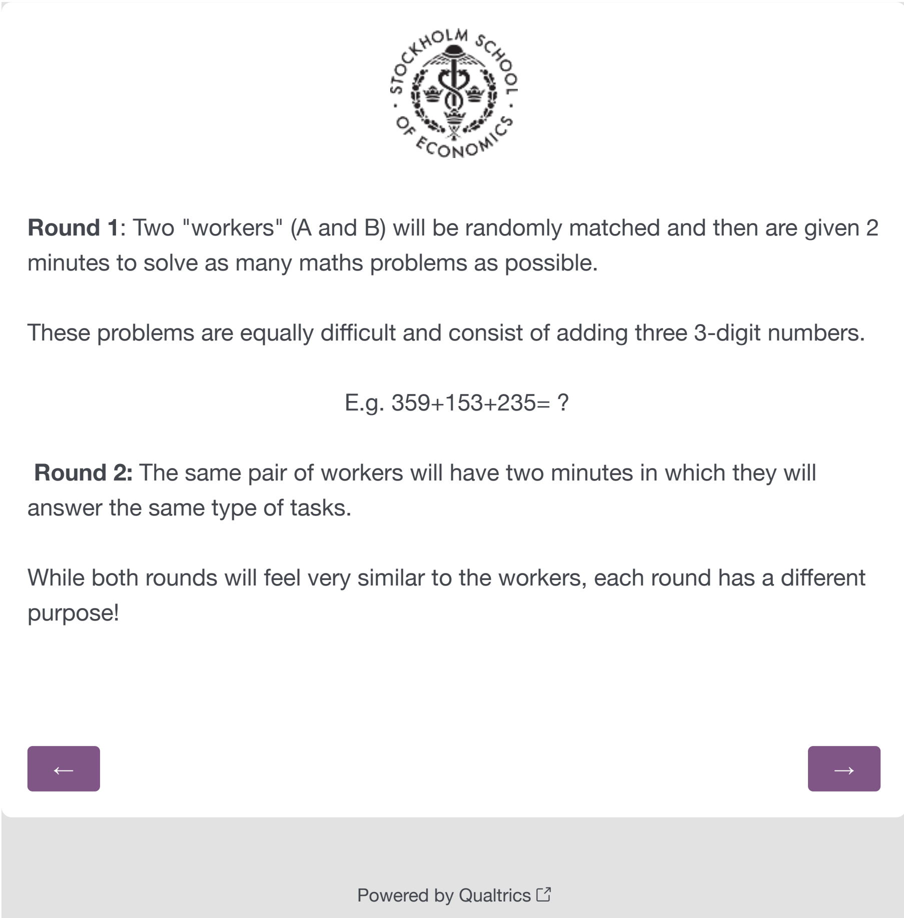

Figure B.5: Game Description - Page 5

Round 1: Both Workers are assigned a so called ***Factor*** (of which they are not aware of) and each correctly solved question is going to increase their ***Multiplier*** that will become important in Round 2.

By the end of Round 1 the Multiplier will be equal to:

[Number of correctly solved tasks (Round 1) x Factor]

E.g.: A Worker who solves 10 tasks in round 1 and is assigned a Factor of 2 is going to get a Multiplier of 20.

10 x 2 = 20

← →

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Figure B.6: Game Description - Page 6

Round 2: Both Workers are assigned a so called ***Price*** (of which they are not aware of) and each correctly solved question is going to earn them **Token**. The Price indicates the amount of Token they will earn for each correct answer in round 2.
On top of that, the **Multiplier** that a workers previously achieved will multiply the amount of Token .

By the end of Round 2 the Amount of Token will be equal to:

[Number of correctly solved tasks (Round 2) x Price x Multiplier]

E.g.: A Worker who solves 10 tasks in round 2 while having assigned a Price of 2, and who got a Multiplier of 20 in the previous round, is going earn 400 Token.

10 x 2 x 20 = 400

← →

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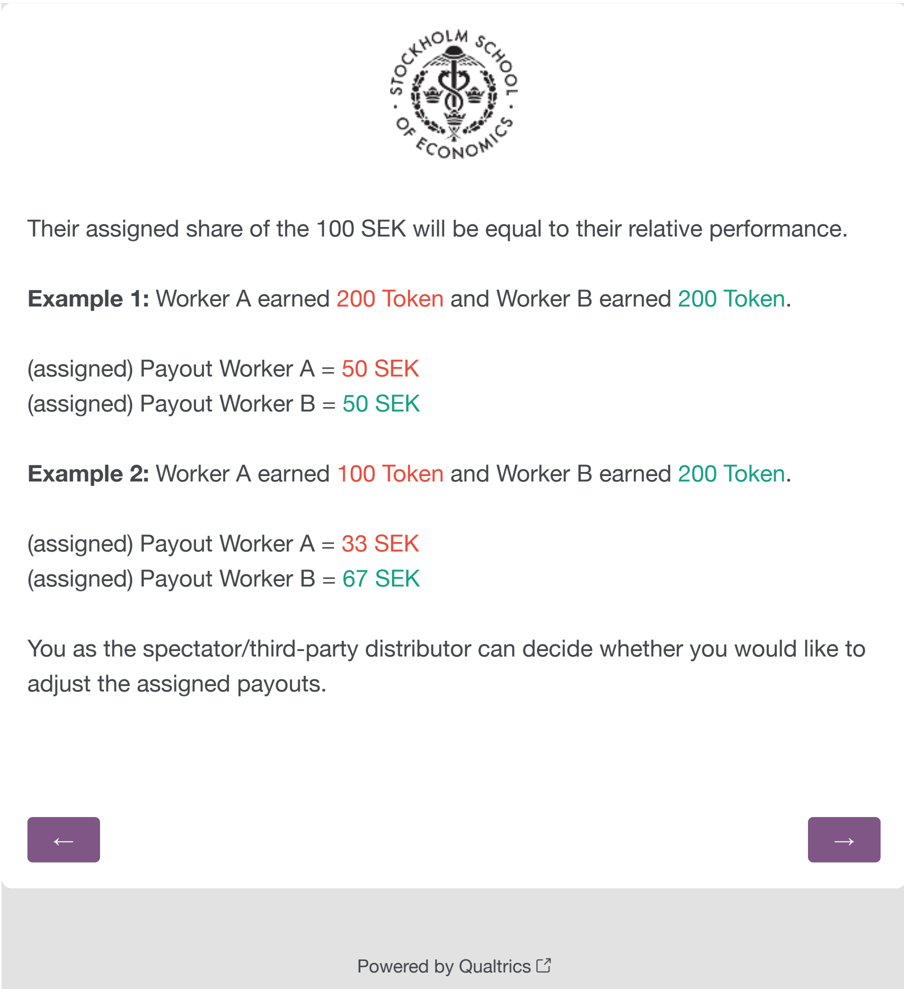
Figure B.7: Game Description - Page 7



Their assigned share of the 100 SEK will be equal to their relative performance.

Example 1: Worker A earned 200 Token and Worker B earned 200 Token.

(assigned) Payout Worker A = 50 SEK
(assigned) Payout Worker B = 50 SEK

Example 2: Worker A earned 100 Token and Worker B earned 200 Token.

(assigned) Payout Worker A = 33 SEK
(assigned) Payout Worker B = 67 SEK

You as the spectator/third-party distributor can decide whether you would like to adjust the assigned payouts.

← →

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Figure B.8: Game Description - Page 8

You are going to observe **five different scenarios** in the following way:

Worker A	**Worker B**
Factor:	Factor:
Multiplier:	Multiplier:
Price:	Price:
Earning: (in Token)	Earning: (in Token)

There will be no direct reporting of the number of correctly solved tasks of each worker.

If you want to change the assigned share of SEK, you can do so by adjusting the share of Worker A.
Keep in mind that if you increase/decrease Worker A's share the share of Worker B is going to decrease/increase by the exact same amount! (Fixed amount of 100 SEK to distribute)

(Example): Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK

0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

← →

Figure B.9: Game Description - Page 9

Before starting the experiment, please quickly answer the three control questions. As soon as you answer the three questions correctly, the five scenarios will be displayed.

No problem in case you might have a wrong answer! In that case there will be a pop-up answer that explains the mechanism. Please read the correct solution and select the correct answer in order to proceed.

If you assign 100 SEK to Worker A, how much money does Worker B receive?

- 100 SEK
- 0 SEK
- It depends

Round 1: Worker A has an "Accumulation Factor" of 2 and solves 10 tasks correctly. What is her Multiplier?

- 10
- 20
- It depends

Round 2: Worker B has a Price of 2 and a Multiplier of 10. She solves 10 tasks correctly (in Round 2). What is her earning in TOKEN?

- 200
- 300
- It depends

Figure B.10: Game Description - Page 10

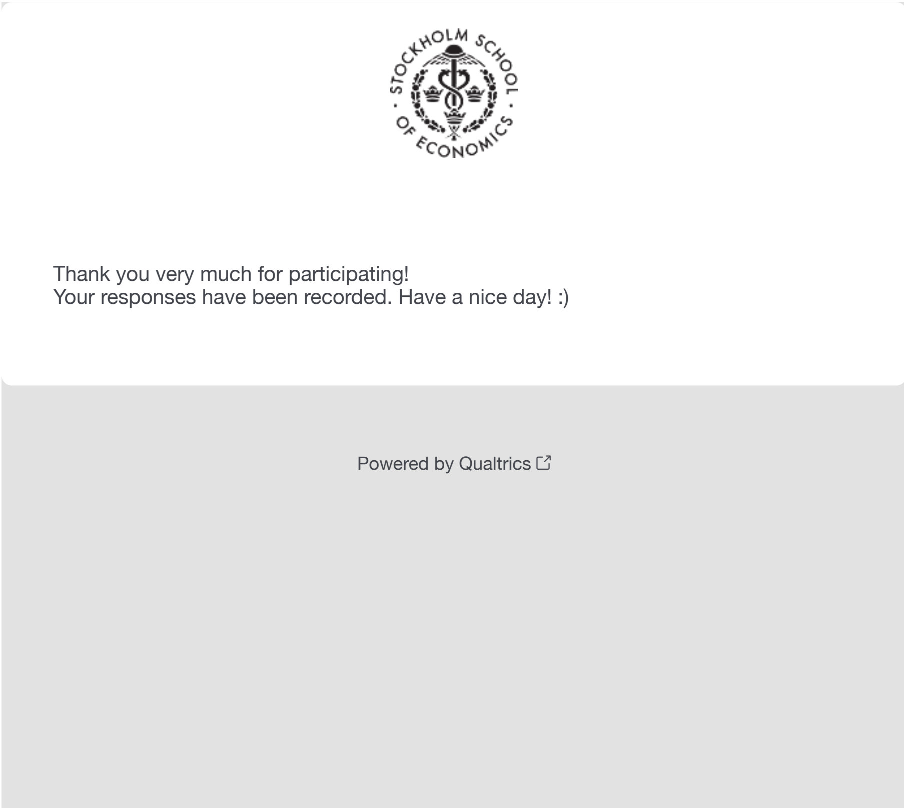
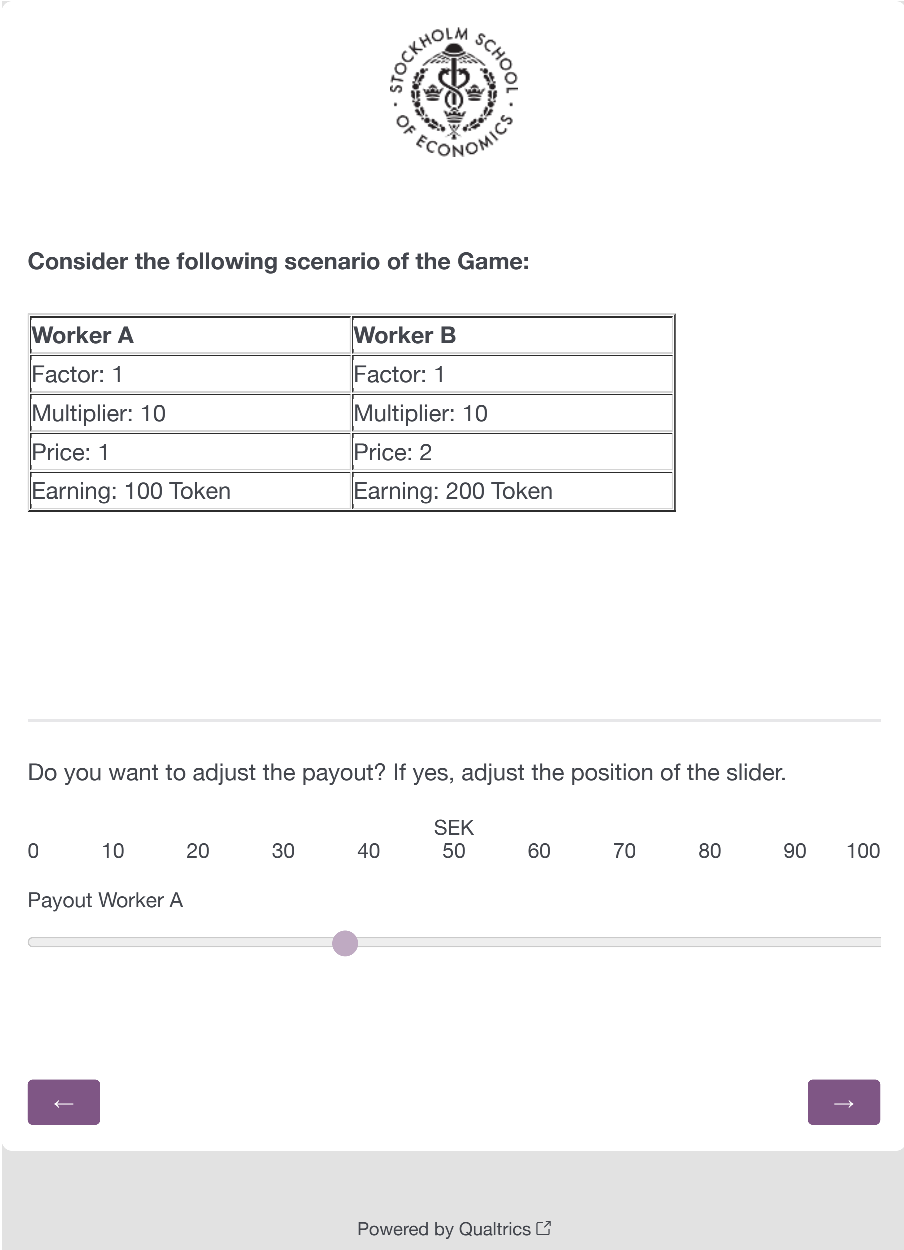


Figure B.11: End of Survey

A2 Price Treatment

Scenarios that only Participants in the Price Treatment will see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Consider the following scenario of the Game:

Worker A	**Worker B**
Factor: 1	Factor: 1
Multiplier: 10	Multiplier: 10
Price: 1	Price: 2
Earning: 100 Token	Earning: 200 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK

0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

← →

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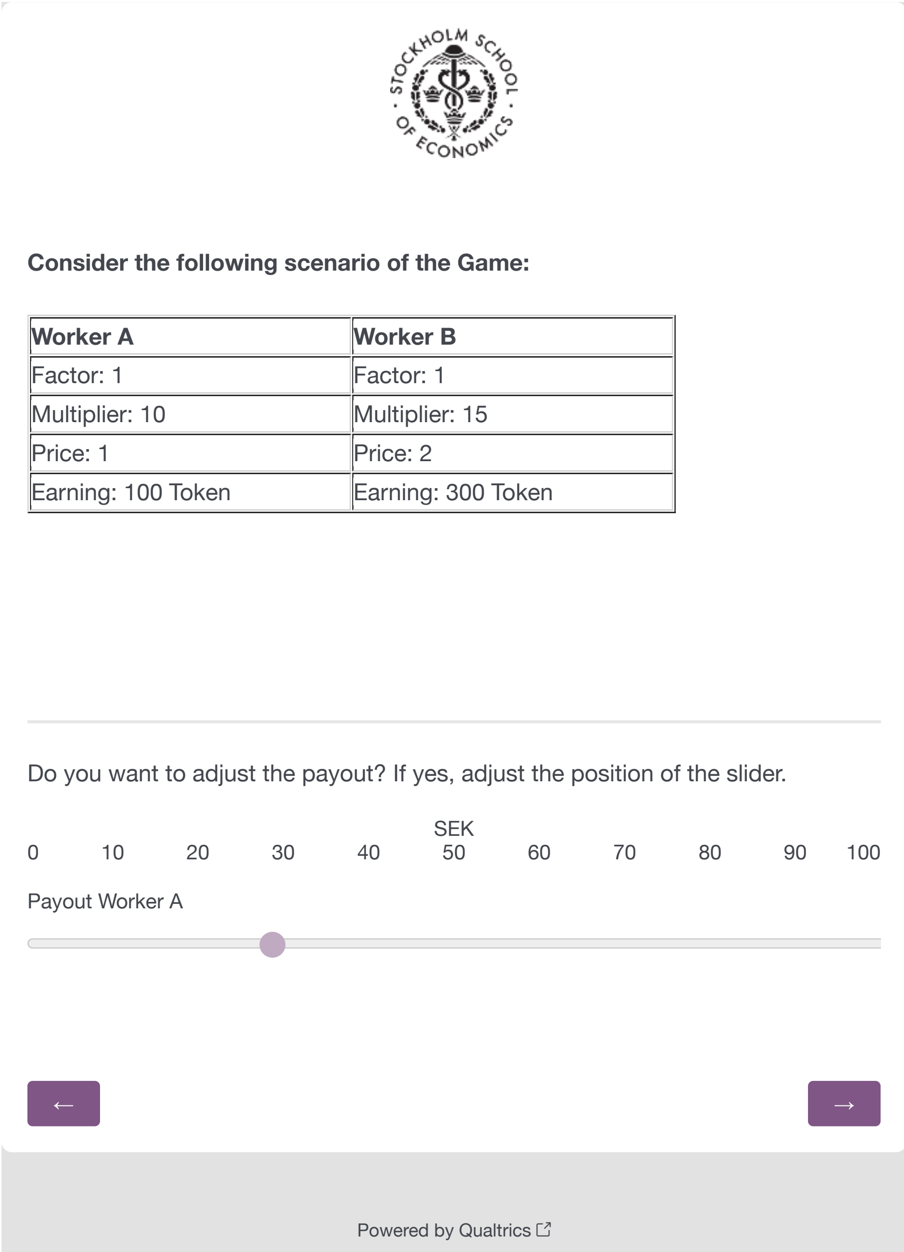
Figure B.12: Price Treatment - Scenario 1 (Base Case)



Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 1
Multiplier: 10	Multiplier: 15
Price: 1	Price: 2
Earning: 100 Token	Earning: 300 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK
0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

← →

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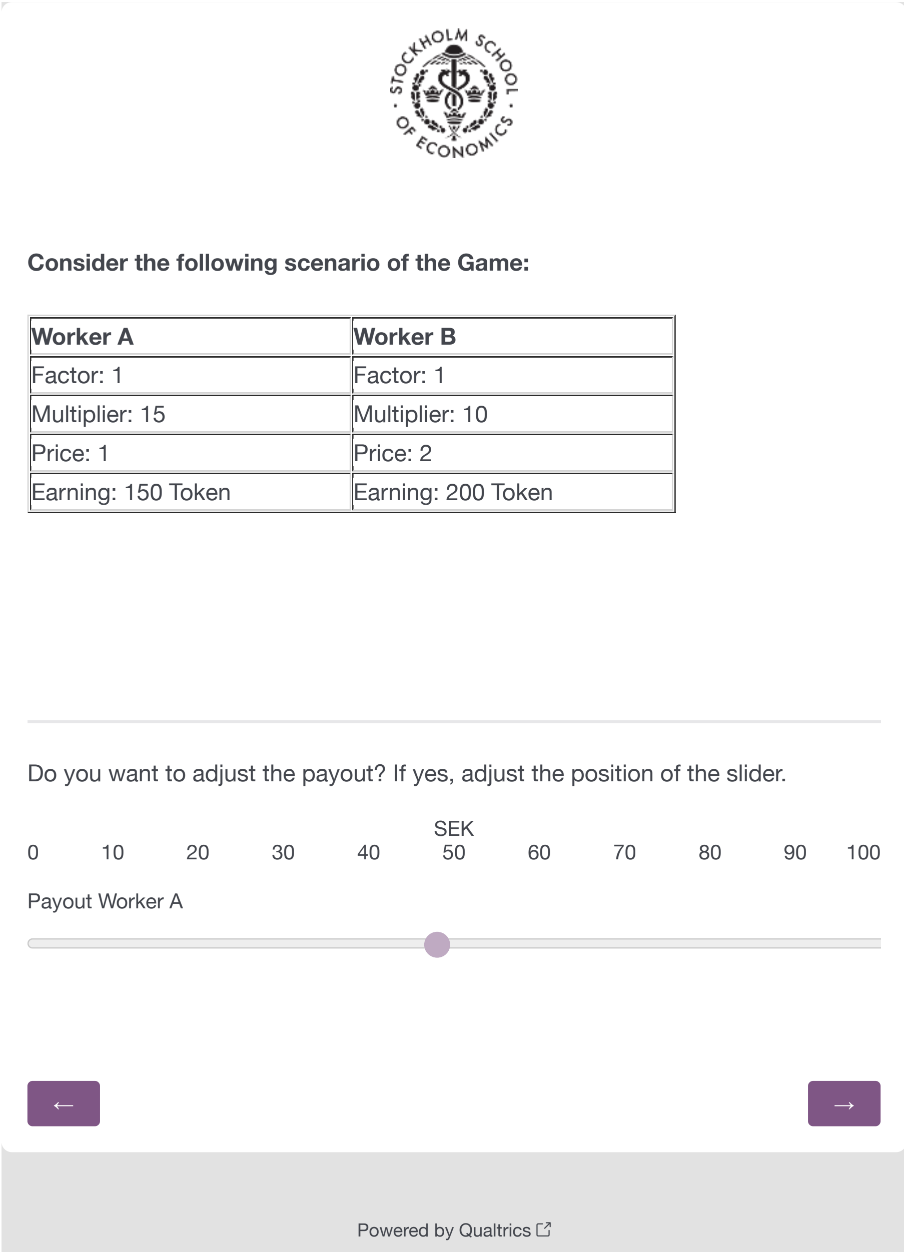
Figure B.13: Price Treatment - Scenario 2



Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 1
Multiplier: 15	Multiplier: 10
Price: 1	Price: 2
Earning: 150 Token	Earning: 200 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK

0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

← →

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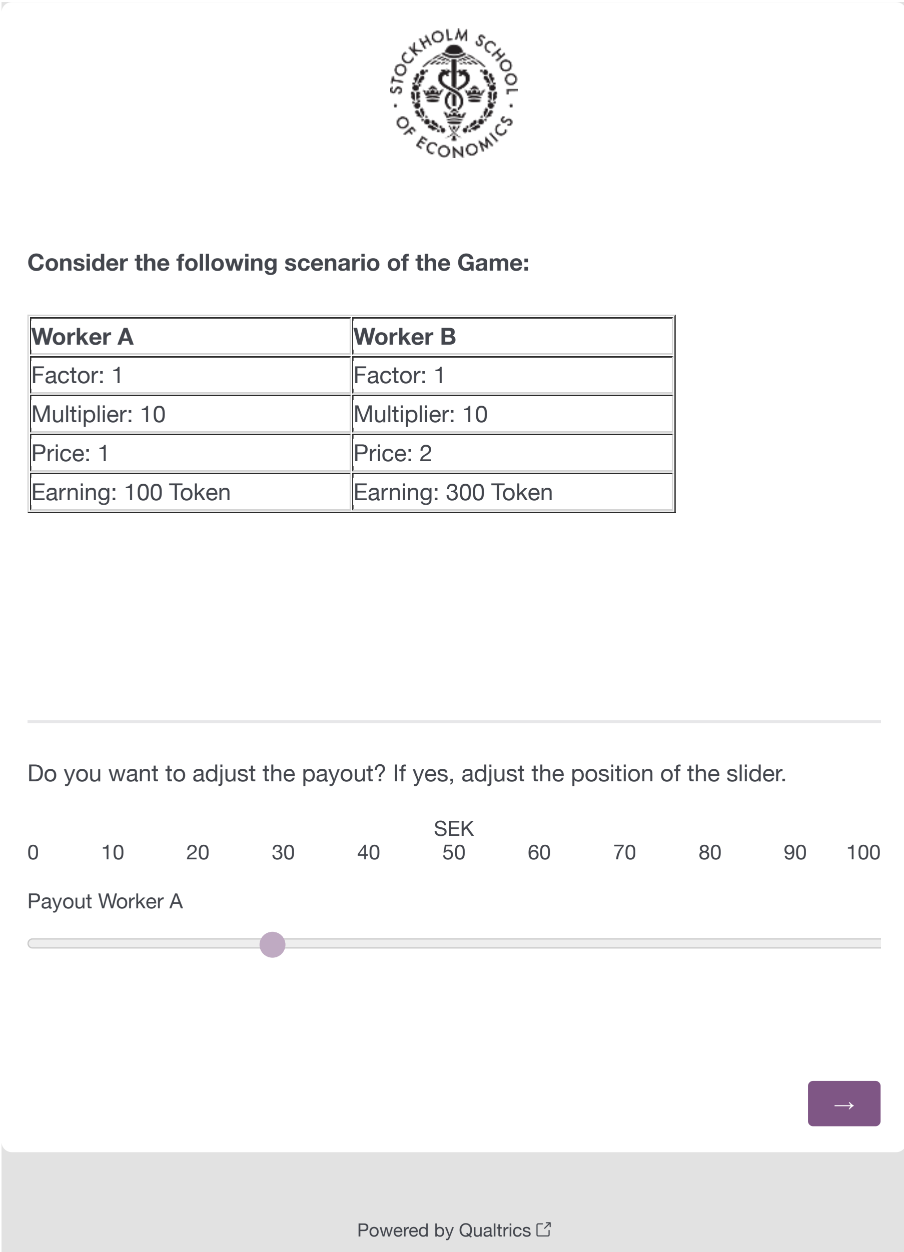
Figure B.14: Price Treatment - Scenario 3



Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 1
Multiplier: 10	Multiplier: 10
Price: 1	Price: 2
Earning: 100 Token	Earning: 300 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK
0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

→

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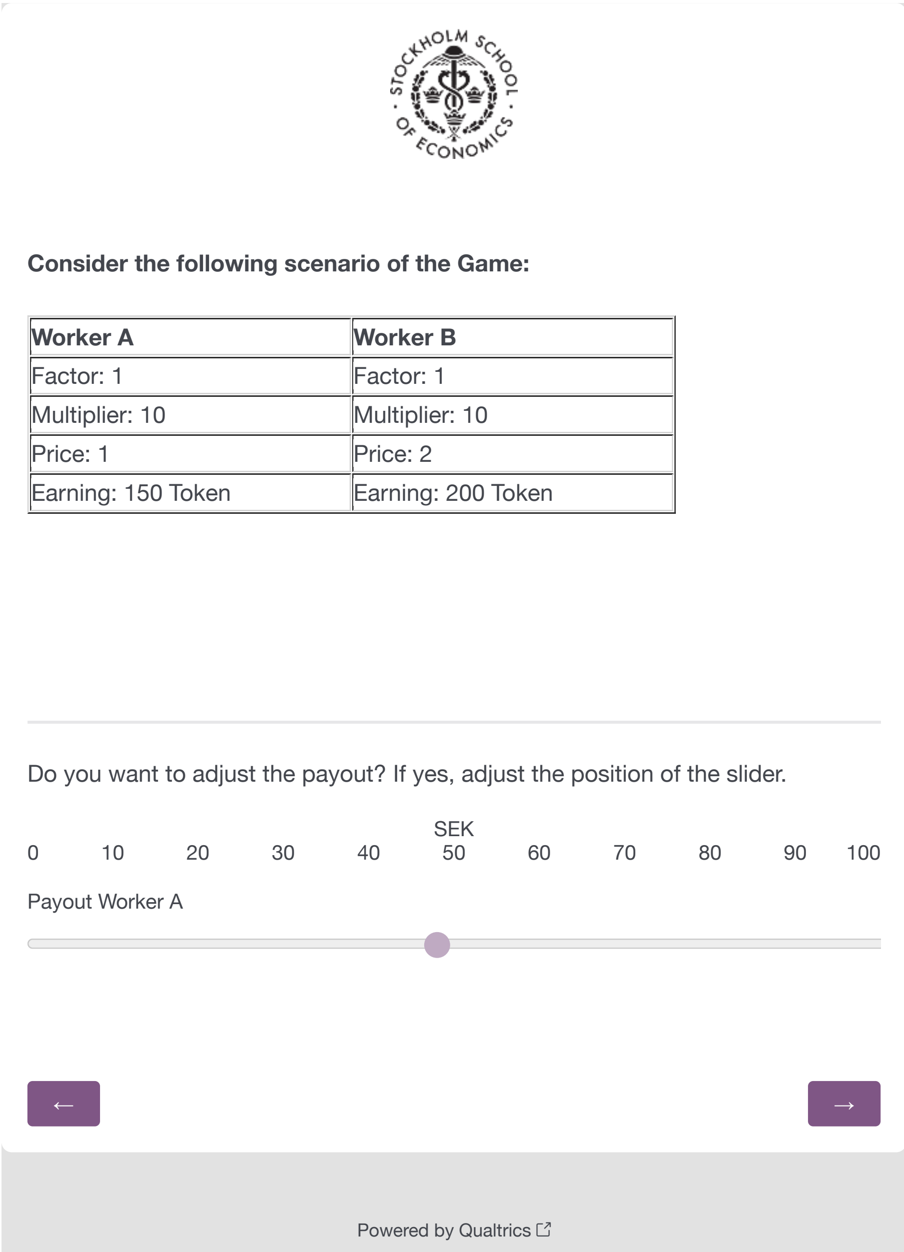
Figure B.15: Price Treatment - Scenario 4



Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 1
Multiplier: 10	Multiplier: 10
Price: 1	Price: 2
Earning: 150 Token	Earning: 200 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK

0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

← →

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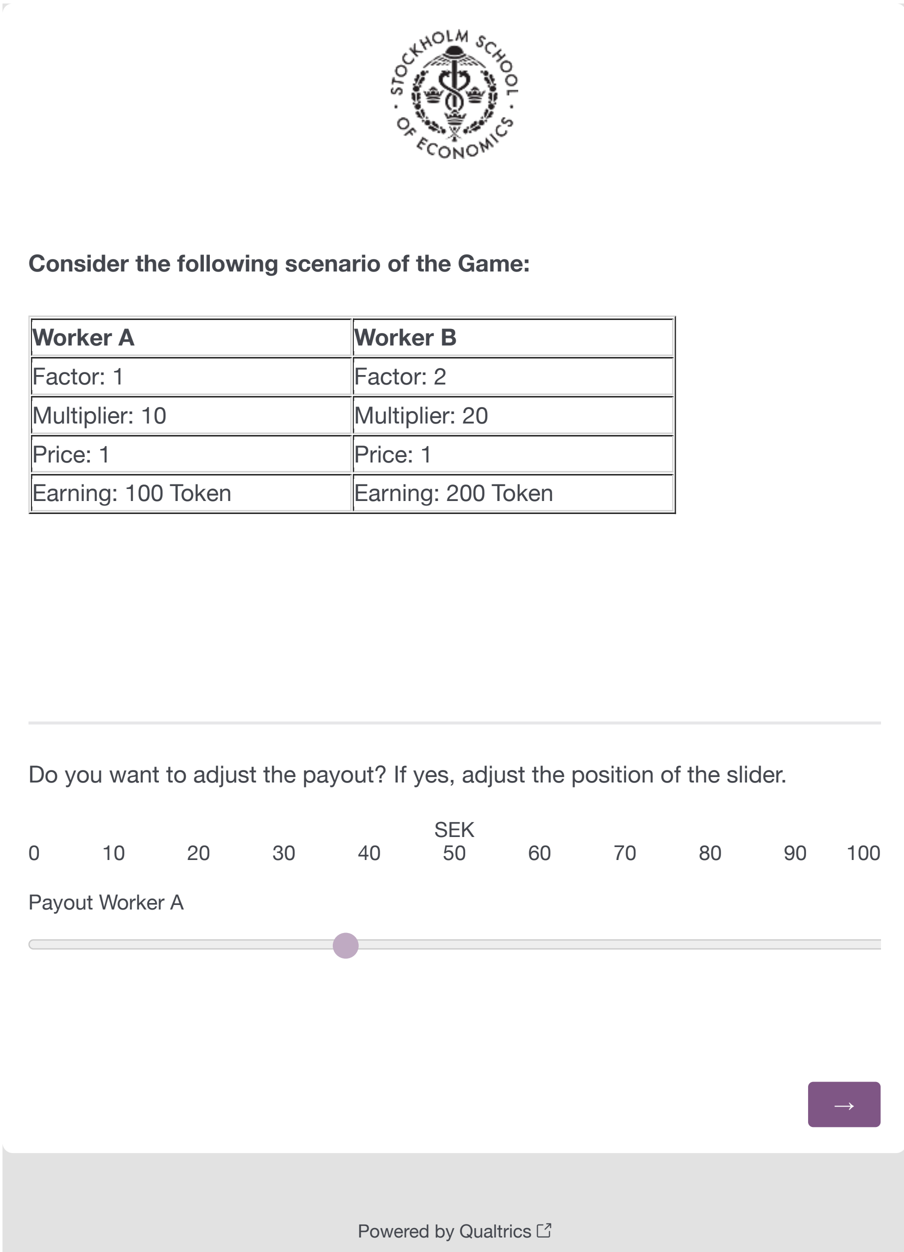
Figure B.16: Price Treatment - Scenario 5

A3 Inequality of Opportunity Treatment

Scenarios that only Participants in the Inequality of Opportunity Treatment will see



STOCKHOLM SCHOOL OF ECONOMICS

Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 2
Multiplier: 10	Multiplier: 20
Price: 1	Price: 1
Earning: 100 Token	Earning: 200 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK

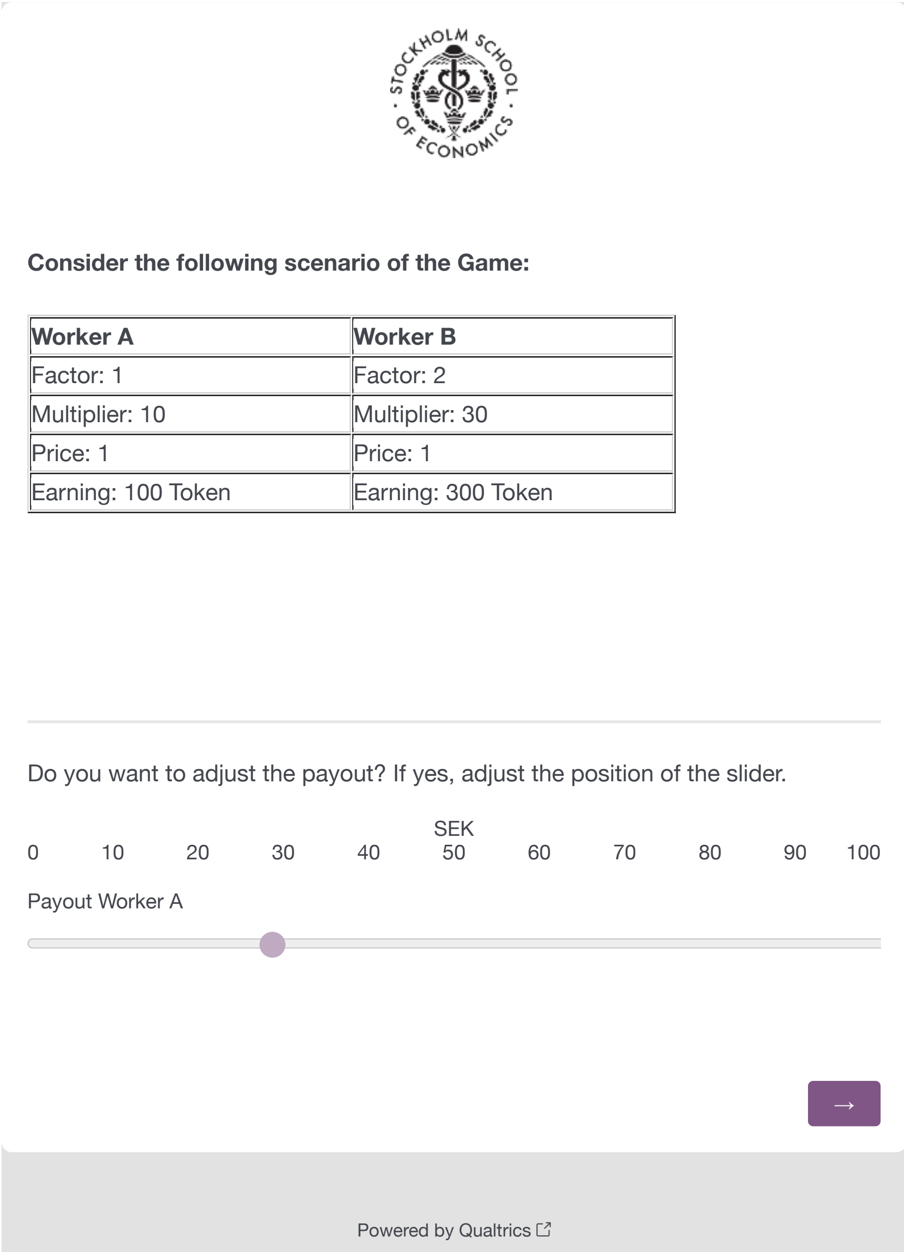
0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

→

Powered by Qualtrics

Figure B.17: Inequality of Opportunity - Scenario 1 (Base Case)



Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 2
Multiplier: 10	Multiplier: 30
Price: 1	Price: 1
Earning: 100 Token	Earning: 300 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK

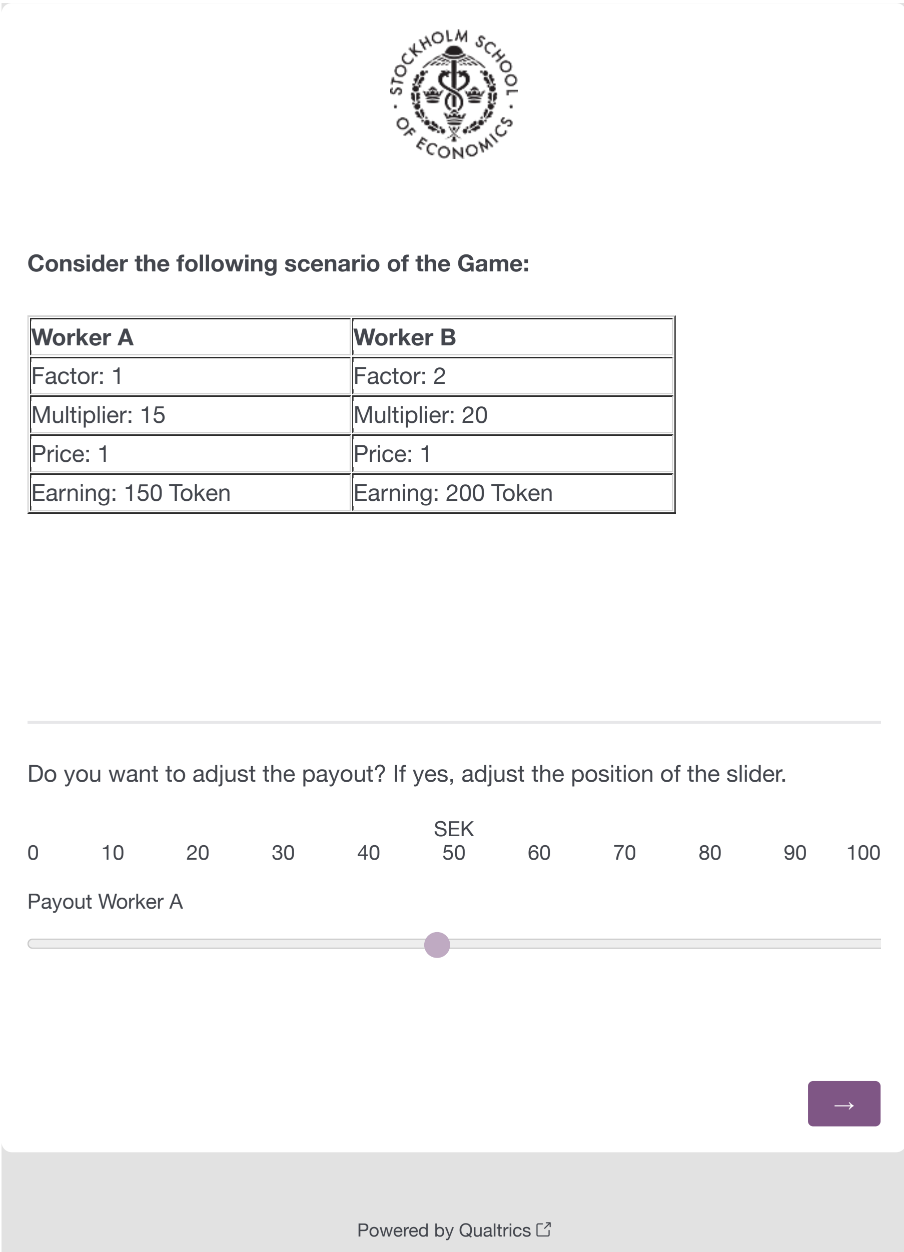
0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

→

Powered by Qualtrics

Figure B.18: Inequality of Opportunity - Scenario 2



Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 2
Multiplier: 15	Multiplier: 20
Price: 1	Price: 1
Earning: 150 Token	Earning: 200 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK

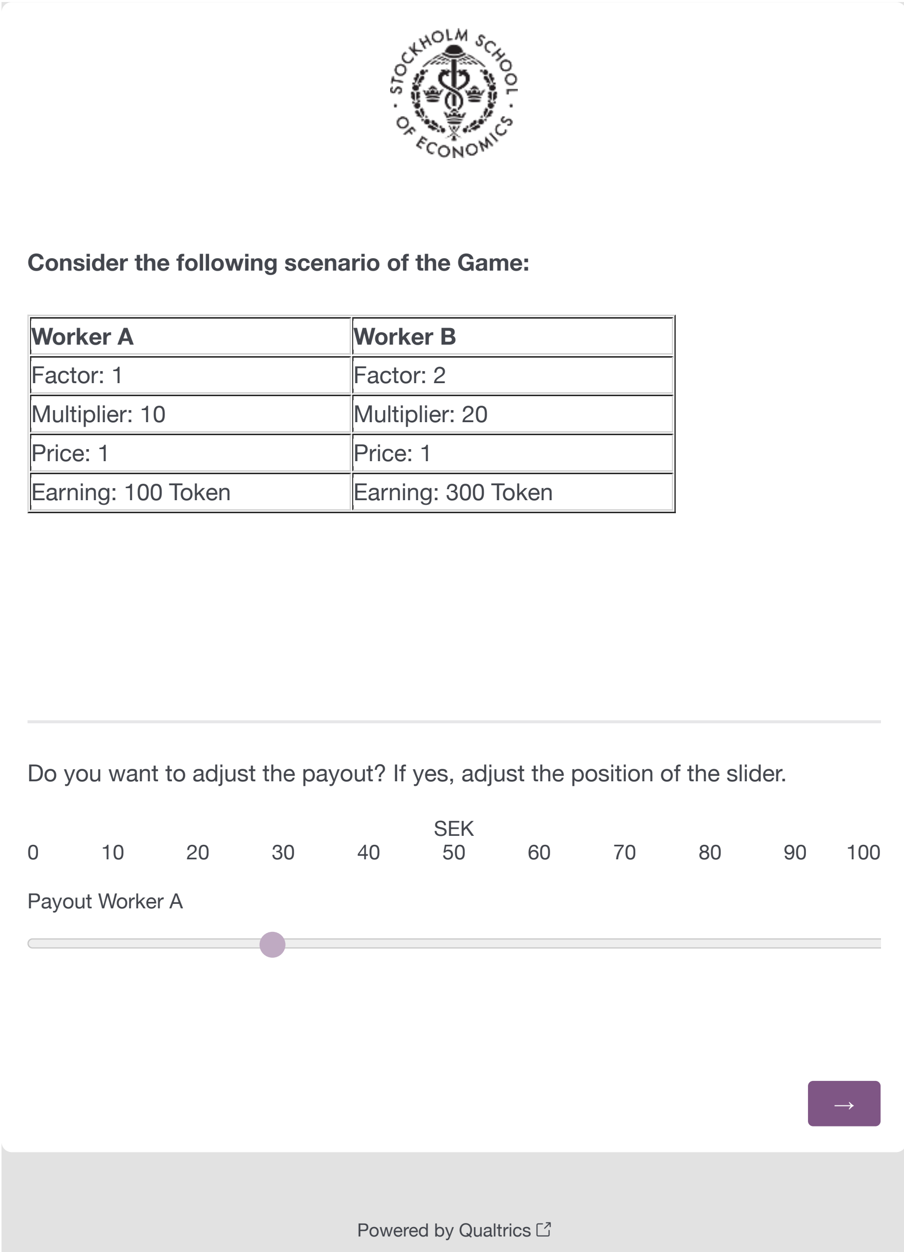
0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

→

Powered by Qualtrics

Figure B.19: Inequality of Opportunity - Scenario 3



Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 2
Multiplier: 10	Multiplier: 20
Price: 1	Price: 1
Earning: 100 Token	Earning: 300 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

SEK

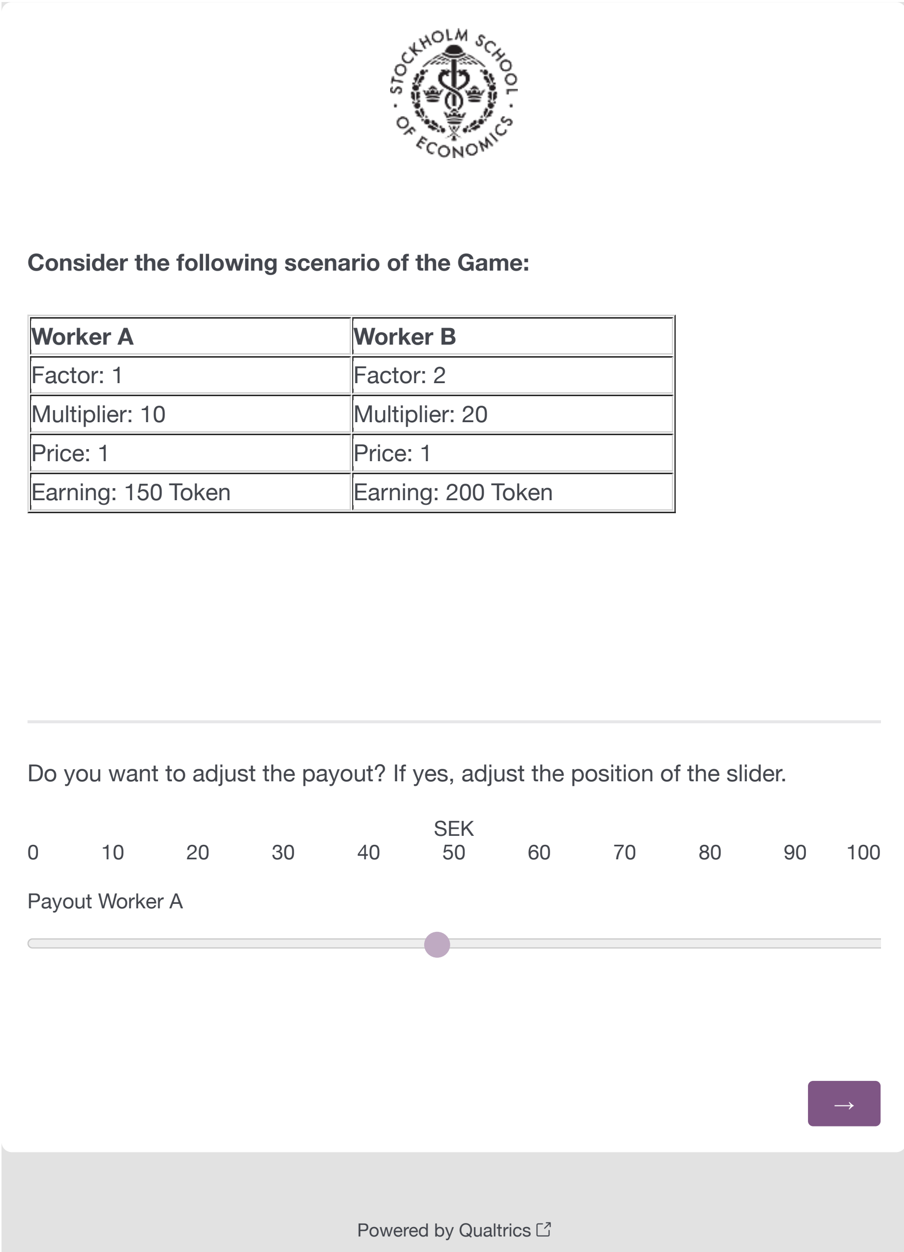
0 10 20 30 40 50 60 70 80 90 100

Payout Worker A

→

Powered by Qualtrics

Figure B.20: Inequality of Opportunity - Scenario 4



Consider the following scenario of the Game:

Worker A	Worker B
Factor: 1	Factor: 2
Multiplier: 10	Multiplier: 20
Price: 1	Price: 1
Earning: 150 Token	Earning: 200 Token

Do you want to adjust the payout? If yes, adjust the position of the slider.

Figure B.21: Inequality of Opportunity - Scenario 5

C Appendix - Pre-Analysis Plan

Master Thesis Pre-Analysis Plan for an Empirical Study using a Survey Experiment

Milan-Avin Johannes Spendel

April 12, 2023

1 Introduction

The thesis project uses an novel survey approach for collecting experimental data. The project combines the elements from several experimental designs of studies using real effort tasks (Cappelen et al. 2010, Preuss et al. 2022). The experimental survey game will be distributed via Qualtrics in Mid-April 2023. This pre-analysis plan was completed before the survey was distributed and data gathered.
There will be one type of real participants in the experiment, namely spectators. The spectators are asked to redistribute money from hypothetical "workers" as if these workers are real participants of a study.

2 Power Calculation - Determining the Minimum Sample Size

Due to the novel approach of this thesis, there are no studies that aim to detect an similar effect. The most similar studies are those of Dong et al. (2022) and Preuss et al. (2022) which both use a spectator design in which a spectator is asked to distribute money from a common money pool. Two workers contributed to the money pool by doing a real-effort task. However, in both papers there is only one round of real-effort task, and luck might entirely determine the outcome. Also, in their studies, the spectator has to redistribute from a "Winner-takes-it-all" scenario in which the worker with better performance is assigned the whole pool of money. In this study, the default distribution is equal to a libertarian outcome. In order to calculate the necessary sample size, I assume that the "true" effect size is equal to a Cohen's d of 0.5, which would be equal to a medium-size effect. The statistical significance level will be 0.005 with a power of 90%. The main statistical tests used will be two-sided t-tests between two conditions - even if there is a clear expectation of the direction of the effect. This would imply a target sample size of roughly 136 participants in each of the treatment groups. Since the study has two treatments and no control, the minimum sample size of this study is 272 divided in the two treatments conditions *Price* and *Inequality of Opportunity*.

3 Recruiting and Data Collection

Observations in this experiment will be collected using a Qualtrics survey. This survey will initially be emailed out to students at the Stockholm School of Economics. If this gives enough observations, the collection of observations will be cut off here. If this does not give enough observation, the collection will have to be extended to neighboring universities such as the KTH, Karolinska Institute, or Stockholm University. The choice to stop or continue gathering observations will be solely dependent on the number of (usable) observations gathered, not the results they lead to. The minimum number of observations will be 272 in total, thus at least 136 participants in each of the two treatment groups. The number is calculated in order to achieve 90% statistical power in a two-sided t-test with a p-value of 0.005 (assuming a Cohen's d of 0.5).
There will be no prior inspection of the data before ten days after the survey is released. If the number of usable observation reaches or exceeds 272, the collection of data will be stopped. If there are not enough observations ten days after the survey was released, the survey will be resent to SSE students (including a message to not participate a second time). This extended data collection will continue for five more days.

If there are not enough observations after the extended period, the survey will be sent out to students from other universities. As soon as data analysis begins, there will be no further observations considered.

3.1 Outliers and Exclusions

Subjects' responses will be excluded if the survey has not been finished. Thus, only complete survey will be included. In case the control variable *Age* is included in a regression. Answers that are under 15 and above 60 will be excluded since the survey is only send out to (PhD) students, numbers outside this range are unlikely to be true.

4 Design

The spectators will be introduced to this survey experiment. In the mail that invites spectators to participate in the study, they will be informed that this survey experiment is hypothetical, but that they should imagine that it is a real study. The study design is according to the experimental procedure.
The experiment which the spectators are asked to observe and make their third-party decision on consists of two rounds on a real effort task. A pre-production phase in which the workers exert effort in order to increase their Multiplier. And a production phase in which the workers earn Token (experimental currency) which relative share translate into SEK. It is explained to the spectator, that any amount of Token a worker received is determined by the number of tasks he or she correctly solves in the production phase as well as the Multiplier that she obtained in the pre-production phase. In total, there will be two treatment conditions. Spectators are not informed regarding the number and nature of treatment conditions.
Spectators are informed about the kind of information, that workers will receive, and not receive.
Before seeing the different scenarios, participants have to answer three questions regarding their gender, age and experience with economic experiments. Then, after reading the experimental instructions, they have to answer three mandatory control questions. These control questions are a forced-choices in Qualtrics. If one or more than one questions are answered incorrectly, a text will pop-up and explain how to calculate the question and how one should think about it in terms of the survey. Participants will have to switch to the correct answer before being able to proceed with the survey. Participants who answer the control-questions wrong will not be automatically excluded.
In order to ensure that differences in transfer do not arise due to learning or fatigue effects, the different scenarios within a treatment will be done in different orders. Placement into treatments and order in which the five scenarios are being displayed is randomly allocated by the software.

5 Statistical Tests and Standard Errors

Hypotheses will tested by a two-sided t-test between two conditions. For both, hypotheses and regressions I will use normal clustered standard errors on individual level to account for potential within-subject correlation. In the regressions, parameters will be estimated under a pooled OLS regression.

6 Hypotheses

The Design of the study with it's fixed scenarios yielding libertarian outcomes and it's RCT assumption allows to test for several Hypotheses.

6.1 Descriptive Data

Hypothesis 1: The *Price* treatment will lead more spectators to adjust the payout compared to the *Inequality of Opportunity* treatment. This is tested by comparing the number of active decisions (decisions where Spectators adjusted the payout). Every decision in which the payout is unequal to the initially assigned outcome will count as an active decision. Here, the null hypothesis is that the sum of active decisions is going to be the same between the two treatments:

$$H0 : \text{\% of active decisions (Price)} = \text{\% of active decisions (IneqOp)}$$

$$H1 : \text{\% of active decisions (Price)} > \text{\% of active decisions (IneqOp)}$$

Here, (Price) refers to all five scenarios within the Price treatment and (IneqOp) refers to all five scenarios within the Inequality of Opportunity treatment.
In addition I will calculate the % of active decisions of each scenarios of each treatment. Since this is exploratory, I do not state a specific hypothesis.

Hypothesis 2: The *Price* treatment will lead to adjustments that are more in favor of Player A, compared to the *Inequality of Opportunity* treatment. Hypothesis 2 is tested by comparing the average amount transferred to Worker A (measured in SEK).

$$H0 : \text{Avg. amount SEK transferred to A (Price)} = \text{Avg. amount SEK transferred to A (IneqOp)}$$

$$H1 : \text{ Avg. amount SEK transferred to A (Price)} > \text{Avg. amount SEK transferred to A (IneqOp)}$$

In addition I will calculate the average amount transferred to Worker A in each scenarios in both treatments. Since this is exploratory, I do not state a specific hypothesis.

6.2 The Merit Primacy Effect

The following tests are aiming to test whether the Merit Primacy Effect - a situation that Cappelen et al. (2023) refers to as a positive complementarity between luck and merit in the reward of the high earner - can be identified in this experiment. ”Luck” in this study never fully determines the outcome after the working stage, but in each treatment, Worker B has ”Luck” on his side since Factor or Price are in his favor.

Hypothesis 3: Comparing the four scenarios (two in each treatment group) in which Worker B performs better than Worker A, to the remaining six scenarios in which Worker B has a worse or equal performance, the average adjusted transfer to Worker A will be less in case that Worker B performed better. Average adjusted transfer to Worker A is defined as the difference between actual transfer to Worker A and the initially assigned amount which would be transferred in case the Spectator decides not to adjust the distribution. Here, the primary test will be a comparison between the averages of these two groups:

H_0: Avg. adjusted transfer to A in SEK (B performs better) =
Avg. adjusted transfer to A in SEK (B does not perform better)

H_1: Avg. adjusted transfer to A in SEK (B performs better) <
Avg. adjusted transfer to A in SEK (B does not perform better)

”B performs better” includes the scenarios Price_2, Price_4, IneqOp_2, IneqOp_4, whereas ”B does not perform better” includes the scenarios Price_1, Price_3, Price_5, IneqOp_1, IneqOp_3, IneqOp_5.
Additionally, the Merit Primacy Effect shall be tested by comparing % of active decisions made by the spectator. Here, the four scenarios in which Worker B performs better are expected to have a lower share of active decisions.

H_0: % of active decisions (B performs better) = % of active decisions (B does not perform better)

H_1: % of active decisions (B performs better) < % of active decisions (B does not perform better)

6.3 The Egalitarian Pull

The following tests are aiming to test for what Cappelen et al. (2022) refer to as "Egalitarian Pull" that might occure in distributive situations due to uncertainty regarding the source of inequality.

Hypothesis 4: The proportion of distributive decisions that are equivalent to an egalitarian outcome (thus cases in which spectators decide to do a 50/50 split) will be higher scenarios in the *Price* treatment compared to the *Inequality of Opportunity* treatment. This hypothesis focuses on the scenarios in which Worker A performed better than Worker B.

$$H_0\text{: \% of 50/50 splits (Price')} = \text{\% of 50/50 splits (IneqOp')}$$

$$H_1\text{: \% of 50/50 splits (Price')} < \text{\% of 50/50 splits (IneqOp')}$$

Additionally, I expect that the % of meritocratic outcomes will be higher in the *Price* treatment of these scenarios.

$$H_0\text{: \% meritocratic outcomes (Price')} = \text{\% meritocratic outcomes (IneqOp')}$$

$$H_1\text{: \% meritocratic outcomes (Price')} > \text{\% meritocratic outcomes (IneqOp')}$$

Price' refers to Price_3 and Price_5, IneqOp' refers to IneqOp_3, IneqOp_5. Meritocratic outcomes are defined as outcomes in which the relative share distributed is equivalent to the relative effort of the two workers measured in number of tasks solved in both rounds. If both solved the same number of tasks, the meritocratic outcome would be equal to an egalitarian outcome. If Worker A solves twice as many tasks as Worker B, a meritocratic outcome would assign Worker A twice as many SEK than Worker B.

7 Regressions

In order to identify evidence that could tell how variables might affect behavior, I will runs several regressions. The first two regressions will be based on the following equation:

$$D_i = \alpha_0 + \alpha_1 \text{Perf}(B)_i + \alpha_2 \text{IneqOp}_i + e_i \tag{1}$$

In the first regression, the dependent variable D_i, stands for the avg. amount SEK distributed to Worker A. In the second regression D_i stands for the % of active decisions. $\text{Perf}(B)_i$ is a binary variable that equals 1 in case that Worker B performed better than Worker A (that is the case in scenarios 2 and 4 in each treatment). IneqOp_i is a binary variable that equals 1 in case that the observed scenario is in the Inequality of Opportunity treatment, and 0 in case that the observed scenario is in the Price treatment. i refers to spectator i who makes a decision in this observation. In both regressions, the coefficients of both binary variables are expected to be statistically significant. In both cases, α_1 and α_2 are expected to be negative.

7.1 The Merit Primacy Effect

Equation (1) will be run a third time but with D_i being the avg. adjusted transfer to Worker A in SEK (as tested in Hypothesis 3). Here, the coefficient α_1 is expected to be negative and statistically significant. Since Worker B, has external luck either in form of a better Factor or a better Price by default, better performance with always interact with "luck".

In addition, I will run a more specific regression that aims to identify whether spectators behave in accordance to the Merit Primacy Effect especially in cases where merit and luck interact in the same round. Thus, equation (2) includes binary variables that specify the round of better Performance of Worker B and two interaction terms with the binary variable IneqOp_i

$$\begin{aligned} D_i = {} & \alpha_0 + \alpha_1 \text{Perf}(B1)_i + \alpha_2 \text{Perf}(B2)_i + \alpha_3 \text{IneqOp}_i \\ & + \alpha_4 \text{Perf}(B1)_i \times \text{IneqOp}_i + \alpha_5 \text{Perf}(B2)_i \times \text{IneqOp}_i + e_i \end{aligned} \tag{2}$$

In this equation, $\text{Perf}(B1)_i$ and $\text{Perf}(B2)_i$ are binary variables that indicate whether Worker B performed better than Worker A in scenario 2 and 4 respectively.
Equation (2) will be run a second time with dependent variable D_i being the % of active decisions. In both cases, the coefficients of the interaction terms are the main focus. α_4 is expected to be positive and while α_5 is expected to be negative. As this regression is mainly exploratory, I do not explicitly expect the coefficients to be statistically significant.

7.2 The Egalitarian Pull

To further investigate what variables might drive the Egalitarian Pull, I will run a logistic regression model in which the dependent variable is the probability of an egalitarian outcome (50/50 split) of the 100 SEK. To make the interpretation of the coefficients easier, I will change the variables, $\text{Perf}(B)_i$, $\text{Perf}(B1)_i$ and $\text{Perf}(B2)_i$ to $\text{Perf}(A)_i$, $\text{Perf}(A1)_i$ and $\text{Perf}(A2)_i$ where the binary variables indicates better performances of Worker A. To investigate the Egalitarian Pull, I will run the following two regressions:

$$P(50/50) = \alpha_0 + \alpha_1 \text{Perf}(A)_i + \alpha_2 \text{IneqOp}_i + e_i \tag{3}$$

$$\begin{aligned} P(50/50) = \alpha_0 &+ \alpha_1 \text{Perf}(A1)_i + \alpha_2 \text{Perf}(A2)_i + \alpha_3 \text{IneqOp}_i \\ &+ \alpha_4 \text{Perf}(A1)_i \times \text{IneqOp}_i + \alpha_5 \text{Perf}(A2)_i \times \text{IneqOp}_i + e_i \end{aligned} \tag{4}$$

The regression based on equation (3) is exploratory which is why I do not explicitly expect the coefficients to be statistically significant or to go in a certain direction.
The regression based on equation (4) could reveal whether the Egalitarian Pull could be driven by interactions of performance and source of inequality. This regression is mainly exploratory which is why I do not explicitly expect coefficients to be statistically significant. I do however, expect α_4 to be positive and α_5 to be negative. Additionally, I will run equations (3) and (4) with the probability of a meritocratic outcome as dependent variable. Again, both regression are mainly exploratory, but I expect in the regression based on equation (4), coefficient α_4 to be negative and α_5 to be positive.

8 Robustness Checks

As an robustness check, I will run the Hypotheses and regressions with robust clustered standard errors on individual level.

To further test the robustness of the results, I will include three control variables (Age, Gender, Experience with Experimental Economics/Psychology) in the pooled OLS regressions. Age will be treated as a continuous (integer) variable, while Gender will be treated as a categorical variable with three levels: male, female, and non-binary to account for non-binary participants. Moreover, Experience with Experimental Economics or Experimental Psychology will be treated as a binary variable, where 1 indicates experience and 0 indicates no experience.

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