Pre-Analysis Plan: Skill-Biased Inequality and Redistributive Preferences

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August 9, 2023

1 Introduction

The rewards for different skills and the resulting skill-biased inequality are often determined by exogenous market mechanisms over which individuals cannot exert control. We refer to this driver of income inequality as market luck. Figurative examples of market luck are profound macroeconomic developments such as globalization, skill-biased technological change, and automation that have caused substantial shifts in the demand and thereby the valuation of different skills in the labor market. From the perspective of economic efficiency, skill-biased inequality might appear justified as higher rewards reflect higher productivity. However, according to the principles of meritocracy, inequalities are only justified if they are due to differences in individual effort and performance but not due to factors outside of individuals' control. Skill-biased inequality, therefore, introduces a trade-off in distributive fairness.

In this paper, we design a parsimonious experiment to study this trade-off in fairness views and improve the understanding of individuals' preferences for redistribution by asking the following research question: Are inequalities arising from market luck perceived as fair? In our experiment, we design a setting where skill-biased inequality between workers arises because exogenous shocks to market demand make certain skills more valuable. We hypothesize that there are fundamental features of market-driven inequalities that increase individuals' inequality acceptance, even though they are fully aware that market-driven inequalities result from exogenous and random factors.

We investigate two main aspects of market luck that we consider crucial for our research question: i) the worker's output is needed for production that generates a profit for a producer, and ii) some

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workers can satisfy the producer's demand that reflects the producer's choice. We design different treatments to vary the two dimensions, allowing us to study their effects on inequality acceptances separately. As our primary outcome, we measure the redistributive behavior of separately recruited third-party spectators, who can decide how much income they want to redistribute between two workers who earned unequal incomes.

2 Experimental Design

For our experiment, we separately recruit three types of subjects; workers, producers, and spectators. Our main outcome of interest is the redistributive behavior of the spectators, which are randomly assigned to one of the experimental treatments.

2.1 Workers

Subjects recruited as workers are randomly assigned to specialize in one of two tasks: *odd* or *even*. In both tasks, workers have to translate a fixed number of letter sequences into sequences of digits with the help of a decryption key they are given. The tasks only differ in that some workers receive a decryption key that translates letters into odd digits, while other workers receive a decryption key that translates letters into even digits. This design allows us to hold effort constant across all workers since it appears reasonable to argue that translating letters into odd or even digits requires the same amount of effort. This is crucial to rule out that beliefs about differences in effort drive our results.

2.2 Producers

Subjects recruited as producers are assigned to the production of products *ODD* or *EVEN*. For the production, producers need to upload sequences of digits by checking them off in a matrix of randomly ordered number sequences. The production of the two products differs as for product *ODD*, producers need to upload sequences of odd digits, while for product *EVEN*, producers need to upload sequences of even digits. Hence, producers need sequences of odd or even digits; in other words, they need a specific input for their production.

2.3 Spectators

We apply a between-subject design for our experiment so that subjects recruited as spectators are randomly allocated to one of the treatments described below. Within each treatment, spectators

are matched with a pair of workers, where one worker earned the high income (USD 6), and the other worker earned no additional income. It is emphasized to the spectators that their choice has real-life consequences. The spectators' choices are probabilistically incentivized, meaning that the decision of one out of ten spectators is payoff-relevant for a pair of workers. Spectators receive detailed information about the context of their decision, i.e., they are fully informed about the information provided to the workers and the producers, and how earnings are allocated. Spectators are also informed that workers are at no point informed about their earnings to minimize the role of worker expectations in the decision of the spectators.[1] Spectators also know that there should not be any differences in effort between the two workers, except for the benchmark effort treatment, as both workers translate the same number of sequences. Spectators have to answer comprehension questions to make sure that they fully understand the context of their decision. Finally, spectators decide whether and how they want to redistribute earnings from the high-income worker to the low-income worker.[2]

2.4 Treatments

Our experiment consists of our main treatment, a control treatment, a benchmark treatment, a mechanism treatment, and a robustness treatment. The benchmark treatment allows us to compare our main treatment effect to the treatment effect of control luck versus effort experiments. We implement a mechanism treatments to isolate the channels that potentially drive our main treatment effect.

In all treatments, workers are randomly assigned to one of the two tasks, odd or even, and are subsequently matched with another worker who worked on the other task, i.e., who has a different skill. After both workers provided effort and completed the task, we induce income inequality between the two workers, as one of the two workers earns a high income (USD 6), whereas the other worker earns nothing for completing the task. The treatments differ in the source of the induced inequality. Table 1 gives an overview of all experimental treatments.

Main Treatment: Market Luck

In the market luck treatment, the pair of workers is randomly matched with a producer who produces either product *ODD* or *EVEN*. Producers are assigned to the production based on their choice. For

[1] Workers do not know their initial earnings but are only told that a third person, the spectator, will be informed and will be allowed to redistribute the initial earnings.

[2] Spectators can choose between seven possible income distributions, (6,0), (5,1), (4, 2), (3, 3), (2, 4), (1, 5), and (0, 6).

that, producers are asked at the beginning of the experiment whether they prefer to work with odd or even numbers. Producers who choose even numbers have to upload even number sequences, and producers who choose odd numbers have to upload odd number sequences.

The producers need to buy the sequences from the workers to earn an additional income (USD 2) from the production. It is explained to the workers, producers, and spectators that producers have to buy the number sequences they need from the worker for a fixed price (USD 6). Buying the right sequences allows the producers to produce, i.e., to upload the number sequences. This generates an additional earnings for the producer (USD 8). In other words, the producers make a profit of USD 2.

Income inequality between the two workers is induced by the matching of workers with a producer. Only one of the two workers translated the number sequences the producer needs for her production. This worker can sell her sequences to the producer and earns the high income. The other worker who cannot sell her sequences earns no additional income.

Control Treatment: Luck

In this treatment, a coin flip decides which worker earns the high income and which worker earns no additional income after they complete the task. There are no producers, i.e., we do not model the market demand side.

Benchmark Treatment: Effort

In this treatment, the assignment of the high income is based on workers' relative productivity. In other words, this is the only treatment where the source of inequality is endogenous with respect to the workers' behavior. Both workers work on the translation task for the same amount of time, and the worker who translates more sequences earns the high income. As in the control condition, there are no producers in this treatment.

Mechanism Treatment

The treatment effect comparing the control luck and the market luck treatment could be driven by the fact that i) producers earn an additional income from their production and that ii) producers could choose what they want to produce. We design mechanism treatment which allows us to identify the effect of each potential mechanism separately.

The mechanism treatment is almost identical to the market luck treatment, with the only difference that producers are randomly assigned to their production, in contrast to the assignment being

based on their choice. Producers still earn an additional income (USD 2), and hence, this mechanism treatment isolates the effect of the producers' profit.

Robustness Treatment

Compared to the control luck treatment, the decision context of the spectators in the market luck treatment involves an additional actor, the producer, and the initial inequality between workers depends on the random matching with producers. A potential concern could be that a treatment effect is already driven by this difference. To address this concern, we design an additional robustness treatment.

The difference between this treatment and the control treatment is that we include producers. Producers do not make a choice and are randomly assigned to the *ODD* or *EVEN* production. They do not earn additional profits from the production. The worker pair is still matched with a producer, and the worker who translated the same type of letters as the matched producer uploads earns the high income, whereas the other worker earns no additional income.

Table 1: Overview of Experimental Treatments

	Producer	Profit	Choice	Source of inequality
Market Luck	✓	✓	✓	exogenous
Control Luck	✗	✗	✗	exogenous
Benchmark Effort	✗	✗	✗	endogenous
Mechanism	✓	✓	✗	exogenous
Robustness	✓	✗	✗	exogenous

3 Primary Outcome Variable

We are interested in the redistributive behavior of the spectators. The main outcome is the inequality I implemented by spectator j:

$$I_j = \frac{|\text{income worker } A_j - \text{income worker } B_j|}{\text{total income}} \in [0, 1]$$

The interpretation of inequality I is similar to the one of a Gini coefficient, where a higher value indicates a more unequal distribution of incomes.

4 Other Variables

- Open-text explanations: We ask spectators to briefly explain their redistribution choice in an open-text format.
- Policy support and government attitudes: We include questions that elicit spectators' support for different redistributive policies such as for example, a higher top marginal tax rate, an increase in the minimum wage, the universal basic income, or an increase in unemployment benefits, and their attitudes toward the government and government policies addressing trade, technological progress, and immigration.
- Beliefs: We include questions that elicit spectators' beliefs about inequality and the role of luck versus effort.
- Socio-demographic characteristics: We elicit characteristics of the spectators, such as age, gender, ethnicity, income, education, employment, and political orientation.
- Comprehension: We include an incentivized factual comprehension check before the elicitation of spectators' redistributive choices. This check consists of several questions about treatment-relevant information, i.e., the context of spectators' decisions. In addition to the participation fee, spectators receive \$0.25 if they correctly answer all comprehension questions.
- Randomness: We ask spectators whether the initial income allocation between workers was random.

5 Hypotheses

We use pairwise treatment comparisons to test our hypotheses. In the following, we formulate our hypotheses and explain for each hypothesis which treatments we can compare to test the hypothesis.

5.1 Main Hypothesis

As our main hypothesis, we want to test whether the fact that a worker can satisfy a producer's demand that is based on the producer's choice and generates a profit for the producer increases the spectators' inequality acceptance, even though the source of inequality is completely beyond the

workers' control. For that, we test whether spectators implement a higher level of inequality in the market luck treatment compared to the control luck treatment.

Hypothesis 1. *Spectators implement higher levels of inequality in the market luck than in the control luck treatment.*

5.2 Benchmark Hypothesis

To benchmark our main treatment effect, we can compare spectators' implemented inequality in the market luck treatment to the implemented inequality in the effort treatment.

Hypothesis 2. *Spectators implement similar levels of inequality in the market luck and the effort treatment.*

5.3 Mechanism Hypotheses

We use the mechanism treatment to get a better understanding of the drivers of the main treatment effect.

Profit producers

We want to test whether the fact that producers can make additional profits because they bought the sequences from the workers affects the spectators' redistribution behavior.

Hypothesis 3. *Spectators implement higher levels of inequality in the mechanism than in the control luck treatment.*

Choice of producers

We also want to test whether the fact that one worker can satisfy the producer's demand that is based on her choice affects the spectators' redistribution behavior.

Hypothesis 4. *Spectators implement higher levels of inequality in the market luck than in the mechanism treatment.*

5.4 Robustness Hypothesis

We want to test that other differences in the decision context of the spectators besides our main mechanisms do not affect the redistribution behavior of the spectators.

Hypothesis 5. *Spectators implement similar levels of inequality in the control luck and in the robustness treatment.*

6 Data Collection

We use the software Qualtrics to implement the online experiment. Our data will then be collected through the panel provider Prolific from a representative sample of the US population, cross-stratified on gender, age, and ethnicity.[3] Participants are randomized within the experiment into one of the treatment arms. All participants receive a fixed participation compensation which amounts to an hourly wage of USD 12. Spectators receive a participation compensation of USD 4, whereas workers and producers receive a participation compensation of USD 1. Depending on the treatment assignment and the spectators' decision, workers and producers receive additional bonus payments.

We already ran a pilot study with 423 spectators where we tested versions of the control luck, the market luck, and the robustness treatment. The data of the pilot study help us to determine sample sizes for our data collection. We plan to collect the experimental data in two waves.

Wave 1. In the first wave, we collect data for the control luck, market luck, and effort benchmark treatment. We plan to collect data from 200 spectators per treatment arm. This sample size allows us to detect the effect size from the pilot study at the conventional level of significance (5%) and power (80%). As only 10% of the spectators are matched with a worker pair, we recruit 120 workers and at least 20 producers through Prolific.[4]

Wave 2. The implementation of wave 2 depends on the results of wave 1. We implement wave 2 if we find a significant treatment effect between the control luck and the market luck treatment in wave 1. In that case, we collect data for the mechanism and the robustness treatment in wave 2. We expect the treatment effect of the mechanism treatment to be smaller, as this treatment tests the effect of only one dimension. Therefore, we will use the data from wave 1 to derive the sample size for wave 2 that would allow us to detect half of the main treatment effect from wave 1 at the conventional level of significance (5%) and power (80%). We will also replicate the control luck and the market luck treatment, choosing sample sizes such that when pooling these treatments across wave 1 and 2 they have similar sample sizes as the mechanism and robustness treatment.

[3] Workers and producers will not be recruited from a representative sample.

[4] We might have to collect more data from producers as we need ten producers who chose odd numbers and ten producers who chose even numbers.

7 Main Analysis

To test our hypotheses we use the following empirical specification for pairwise treatment comparisons:

$$I_j = \alpha + \beta T_j + \gamma X_j + \epsilon_j$$

where I_j is the income inequality implemented by spectator j, T_j is an indicator variable for spectator j being in a certain treatment, and X_j is a vector of control variables (e.g., income, gender, age, ethnicity, education, and political affiliation). To test our main hypothesis, T_j would be an indicator for the Market Luck treatment and the Control Luck treatment would be the reference category. The coefficient β isolates the causal treatment effect. We further run regressions, where we estimate the treatment effect of several treatments simultaneously, i.e. include multiple treatment indicators.[5]

Pooling data. We pool the data across waves if implemented inequality within the control luck and the market luck treatment does not significantly differ across waves.

8 Exploratory Analyses and Heterogeneity

We will also conduct some exploratory analyses

- Support for real-life policies, attitudes toward the government, and beliefs about inequality: We will use these survey items to investigate how allocation choices and treatment effects vary with policy support, attitudes toward the government, and beliefs about inequality[6].
- Socio-demographic characteristics: We collect data on demographic characteristics such as gender, income, and political orientation. We will use these variables to investigate how allocation choices and treatment effects vary along these socio-demographic dimensions.
- Exploratory analyses of open-text data: We ask participants about their reasoning for their allocation choices and investigate these data to get a better understanding of participants' underlying motivation of their inequality acceptance.

[5] As a robustness check, we plan to replicate the main analyses using the subsample of participants who answered all comprehension questions correctly.

[6] For these analyses, we will look at the individual survey items separately but also create indices for general policy support, attitudes toward the government, and inequality beliefs.

9 Exclusion Criteria

We will restrict the spectator sample along the following dimensions:

- Attention check: We will implement an attention check before the spectators read the instructions of the experiment. Any spectator that does not pass the attention check will be excluded from the analysis and not count towards the number of completes.
- Completion time: We exclude spectators whose completion time deviates by 2 standard deviations from the mean completion time.
- Previous participation: We exclude participants that already have participated in the pilot study. Participants from wave 1 will be excluded from data collection in wave 2.