Misperceiving Economic Success: Experimental Evidence on Meritocratic Beliefs and Inequality Acceptance *

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October 31, 2021

Abstract

Meritocratic beliefs are often invoked as a justification of inequality. We provide evidence on how meritocratic beliefs are shaped by economic status and how they contribute to the moral justification of inequality. In a large-scale study that samples the general US population, we show that economic success causes a change in beliefs about success depending on effort rather than luck, and participants to accept substantially more inequality. Exploiting exogenous variation in meritocratic beliefs in a two-stage analysis, suggests that meritocratic beliefs have a strong bearing on inequality acceptance. We find no evidence that these effects are moderated by political orientation.

Keywords: meritocratic beliefs, inequality acceptance, fairness, political views
JEL classification numbers: D31, D63, H23, H24

*We thank Alexander Cappelen, Bertil Tungodden, Sarah Necker, Joerg Oechssler, Yannick Reichlin, and Yilong Xu as well as the seminar audience at FAIR (Bergen), Heidelberg University, HEIRS Illusion of Merit Workshop, and ZEW Public Finance Conference for valuable comments. We are also grateful to Christian König-Kersting for his technical support in implementing the experiment. We gratefully acknowledge the Deutsche Forschungsgemeinschaft (DFG) for financial support through individual grant FE 1452/3-1 (Fehr). This study is registered in the AEA RCT Registry under AEARCTR-0004455. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.
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1 Introduction

Meritocratic beliefs are often invoked as a justification of inequality. People tend to accept more inequality if they think it reflects hard work, talent, and skill rather than external circumstances such as luck (Fong, 2001; Alesina and Angeletos, 2005; Cappelen et al., 2007; Almås, Cappelen and Tungodden, 2020). This meritocratic ideal explains, for example, variation in income inequality and redistributive policies across countries (Alesina and Glaeser, 2004; Alesina and Angeletos, 2005; Mijs, 2019), and is at the core of the "American Dream," i.e. the notion that success can be attained by all who work sufficiently hard. Although the meritocratic ideal is appealing for fairness and aspirational reasons, there is a growing concern that a belief in meritocracy reinforces pre-existing inequality.

Meritocratic beliefs help people making sense of economic status. As the gap between the rich and poor widens, those at the top of the distribution may want to persuade themselves that they have worked hard and to believe that they deserve their fortune. While this narrative that economic status and success reflect merit is widespread, well-identified evidence on the link from status to meritocratic beliefs is scarce because of several challenges. First, economic status is typically the combined result of effort, talent, and life circumstances. Identification is thus complicated by the difficulty to specify and to quantify the relative impact of luck and effort ex-post. Second, it is difficult to gather data on individuals' meritocratic beliefs around the same time as they achieve economic success, and if it is possible, any observed variation in beliefs is likely endogenous with respect to economic success and behavior. Third, it is rarely possible to create random variation of economic success in naturally occurring settings.[1]

In this paper, we provide causal evidence on how economic status shapes meritocratic beliefs and contributes to the moral justification of inequality using a large-scale experiment with a sample of the general population of the United States. To overcome the identification challenges outlined above, we design a work assignment that allows us to introduce the necessary exogenous variation in economic status, while also mapping important aspects of socio-economic reality. We recruited a large and diverse sample of workers from an online labor market platform, to work on a simple code-entry task. The code-entry task requires no prior knowledge or specific skills such that performance should depend almost entirely on exerted effort. There are two versions of the task

[1] A rare example for random variation in success is research funding. Some agencies and research foundations have started to experiment with randomized selection processes for grant proposals, for example the Swiss National Science Foundation, the Volkswagen Foundation in Germany, or the Health Research Council of New Zealand.

that differ in their difficulty and that are calibrated such that working on the easy version of the task results with near certainty in a better performance and thus economic success. We then create the necessary random variation to identify the impact of status on meritocratic beliefs by randomly assigning workers to the two tasks, without disclosing this assignment to the workers. The random assignment captures external circumstances that are often decisive for success and mirrors the fact that one rarely faces a level playing field, which is an essential ingredient of a fair meritocratic race. While effort and hard work are a necessary requirement for having a chance of economic success, some have better life circumstances that give them an advantage over others. Because the random assignment to the tasks basically predetermines economic status, we can identify the impact of success and failure on meritocratic beliefs and inequality acceptance.

Following this work assignment, we investigate how economic status shapes meritocratic beliefs and inequality acceptance. To this end, we elicit participants' beliefs about the role of effort and luck in economic success. We do this after they completed their work assignment and after revealing the bonus payment (i.e. economic success), but before they learned about the measurement of inequality acceptance. Therefore, participants cannot strategically manipulate their beliefs to justify subsequent (self-serving) behavior. After eliciting beliefs, we measure inequality acceptance based on participants' decisions to redistribute the earnings from the work assignment. Finally, we are interested in participants willingness to resolve the uncertainty of task assignment and elicit their willingness to pay for this information in an incentive-compatible way.

This setup allows us to estimate the *first-stage* effect of economic status on meritocratic beliefs and the *reduced-form* effect of economic status on inequality acceptance. Both effects consistently point to a strong impact of economic status on beliefs and behavior. First, we observe that economic success leads to a 16 percentage point higher belief that success in the work assignment depends on effort. While unsuccessful participants have well calibrated meritocratic beliefs and view success equally as the result of luck *and* effort, successful participants believe that success is to a large degree the product of effort. Thus, the successful downplay the role of luck and predominantly attribute their success to hard work, although it is very salient that success is mostly random in our setting. Second, our reduced-form results reveal a strong divergence in inequality acceptance that is driven by economic status. Successful participants implement a highly unequal distribution of income. Specifically, the implemented inequality is twice as high as the inequality that unsuccessful participants would implement.

Combining the first stage and reduced form in a two-stage analysis, we show that meri-

tocratic beliefs have a causal impact on inequality acceptance. The two-stage effects exploit the variation in meritocratic beliefs induced by the random task assignment. The estimates show that a higher belief in the importance of effort in achieving success leads to more inequality acceptance. Therefore, the two-stage effects suggest that meritocratic beliefs have a strong bearing on inequality acceptance. Importantly, these beliefs, while morally justifying participants' status, are not biased towards justifying self-serving behavior because participants could not condition their beliefs on subsequent behavior.

While the observed differences in inequality acceptance may be interpreted as reflecting self-serving behavior, we present additional evidence in favor of a prominent role of fairness and meritocratic concerns in driving inequality acceptance. We first note that participants implement an income distribution that is far from an equal and a completely unequal distribution that self-interested unsuccessful and successful participants would implement. Moreover, the difference in inequality acceptance of successful and unsuccessful participants can be fully explained by the treatment difference in *prior* beliefs regarding deservingness. Participants with a higher prior belief that they deserve the bonus payment accept more inequality when they are successful, but less inequality if they are unsuccessful. Again, because we elicited these beliefs before participants learned about the bonus payment and the redistribution stage, they solely reflect how participants perceive their performance in the work assignment and are not distorted toward self-serving redistributive decisions. Together with the two-stage results, this provides more evidence against purely self-serving motives in the decision about inequality acceptance.

The two-stage effects suggest that the resulting inequalities can feedback into meritocratic beliefs creating a "meritocratic" trap (Markovits, 2019). We present further evidence that can fuel such a vicious cycle. A significant share of participants are highly willing to remain in the dark about the relative importance of merit for their success. About 50 percent of participants are unwilling to forego even 1 cent to obtain information regarding task difficulty, the main determinant of economic status. Moreover, the willingness to pay for this piece of information is significantly lower for successful than for unsuccessful participants, indicating that individuals are more than willing to maintain false perceptions about the causes of their success, misperceptions that justify greater inequality.

Finally, given that ideological dispositions on fairness views and inequality differ strongly between liberal and conservative voters and appear as critical inputs for government tax policy policies (Alesina and Glaeser, 2004; Congdon, Kling and Mullainathan, 2009), we also examine

how meritocratic beliefs and inequality acceptance differ in relation to political orientation.[2] Our findings cast doubt on the broadly held notion that liberals are less likely to equate success with merit than conservatives. While it is true that there are differences in the level of meritocratic beliefs and inequality acceptance, these differences are economically small and unaffected by the treatment.[3] In other words, the impact of success on meritocratic beliefs and inequality acceptance is unaffected by political orientation. Moreover, liberals assign as little importance to learning about the role of luck in their success as conservatives. But if they pay for this information, they are more responsive than conservative and more likely revise their initial decision.

The findings of our paper contribute to several strands in the literature. Most importantly, we add to the voluminous literature on fairness preferences and fairness views. A cornerstone in this domain is the theoretical work on redistribution by Piketty (1995), Alesina and Angeletos (2005), and Benabou and Tirole (2006) illustrating the role of meritocratic beliefs on preferences for redistribution. This channel has also received much attention in the empirical literature. An important and consistent finding that has emerged in observational studies (Fong, 2001; Alesina and Angeletos, 2005; Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011) and laboratory studies alike (Konow, 2000; Cappelen et al., 2013, 2017; Fisman et al., 2020) is that people tend to accept greater inequality if it is the result of effort rather than luck. While the importance of the source of inequality is well documented, the formation of meritocratic beliefs is less understood. Some evidence suggests that motivational reasons and intragenerational mobility play a role and that values and beliefs about the role of effort are inherited from one's parents (e.g. Gärtner, Mollerstrom and Seim, 2018; Cohn et al., 2020; Hvidberg, Kreiner and Stantcheva, 2021; Lobeck, 2021). We focus here on economic status and present evidence on how status alters meritocratic beliefs in a setting that allows us to clearly assess the impact of luck and effort on economic success.[4] The variation in meritocratic beliefs that we observe has a bearing on inequality acceptance. While this appears consistent with self-serving fairness norms and behavior described in the prior literature (e.g. Babcock et al., 1995; Engelmann and Strobel, 2004; Croson and Konow, 2009; Konow, 2009; Cappelen et al., 2013; Durante, Putterman and Van der Weele, 2014; Deffains, Espinosa and Thöni,

[2]Political affiliation appears, in general, as a strong indicator of how people perceive and navigate political and economic issues (Campbell, 1960; Bartels, 2002).

[3]This is in line with correlational evidence from a few other studies suggesting that inequality accepting is negatively related to liberal ideology (Fisman, Jakiela and Kariv, 2017; Cappelen, Haaland and Tungodden, 2018; Almås, Cappelen and Tungodden, 2020).

[4]Although the relative importance of luck and performance in determining outcomes is ambiguous in most settings, only very few papers study inequality acceptance in such situations (e.g. Cappelen et al., 2017; Cappelen, De Haan and Tungodden, 2020). Well-identified evidence of economic success on meritocratic beliefs is largely missing (but see, for example, Albertazzi, Lown and Mengel 2021 for evidence on the impact of relative income on meritocratic beliefs).

2016), our setting leaves no room to manipulate beliefs in a self-serving way.[5] They rather reflect pure meritocratic concerns embodied in the narrative that economic success or social position reflect merit and, in contrast to the prior work, we can show that these meritocratic beliefs shape inequality acceptance.

Our paper also belongs to a growing literature in economics that is concerned with individuals' perceptions about social norms, inequality, and relative income (e.g. Cruces, Perez-Truglia and Tetaz, 2013; Kuziemko et al., 2015; Karadja, Mollerstrom and Seim, 2017; Alesina, Stantcheva and Teso, 2018; Alesina, Miano and Stantcheva, 2020; Fehr, Muller and Preuss, 2021; Fehr, Mollerstrom and Perez-Truglia, 2021; Fehr and Reichlin, 2021; Hvidberg, Kreiner and Stantcheva, 2021). This literature has mostly focused on documenting that people have biased views about factual reality and that these misperceptions have consequences for a range of political preferences, as opposed to focusing on the formation of meritocratic beliefs that are to some extent more subjective. Our study not only adds to the previous evidence on misperceptions by documenting that successful participants misperceive the relative importance of effort and luck, but also that these misperceptions are caused by participants' economic status.

Finally, we also contribute to a nascent empirical literature on self-image and motivated beliefs. Evidence suggests that people bias their beliefs, for example, to deceive others (e.g., Schwardmann and Van der Weele, 2019; Charness, Rustichini and Van de Ven, 2018) or to maintain a self-image of moral integrity (e.g., Di Tella et al., 2015). These motivated beliefs prevail despite the frequent feedback that people typically receive, for example because people selectively recall the feedback (e.g., Zimmermann, 2020) and tend to actively avoid negative feedback (Castagnetti and Schmacker, 2020). Our findings on meritocratic beliefs resonate with both findings. We observe that people inflate the significance of the role of effort in their economic success, despite the large portion of luck involved and that they tend to avoid information that may or may not threaten their meritocratic beliefs.[6] These findings cannot be traced to motivational or material reasons. We also add evidence on the open debate about how people react to information, i.e. whether they put more weight on positive or negative information. In our specific setting, people react to success as well as failure to a similar extent, though they are more likely to avoid potential negative feedback if they were successful.

[5]For example, in related work Deffains, Espinosa and Thöni (2016) show that experiencing success lowers redistribution among subjects in a lab experiment, but in their experimental design it is not possible to identify an effect on beliefs, as subjects are aware of the redistribution task and thus can bias their beliefs.

[6]These findings are also consistent with theories suggesting that people derive consumption utility from distorting beliefs (Bénabou and Tirole, 2002; Brunnermeier and Parker, 2005; Köszegi, 2006).

2 Experimental Design

The study combines a work assignment with a survey and incentivized decision tasks (screenshots of the survey and all tasks are available in the Appendix A.9). We pre-registered the study and posted a pre-analysis plan in the AEA RCT Registry (AEARCTR-0004455).

Setup: We first elicited some basic socio-demographic information and personality traits. More details and a complete list of all covariates can be found in Appendix A.1. After the survey, participants worked on a job assignment for 3 minutes. The assignment consists of retyping a series of randomly generated sequences of upper- and lower-case letters. Working on this assignment reflects effort, as the task is tedious and unpleasant and, importantly, participants can quit working anytime and immediately find other work on the online platform that we use to run our study.

We implemented two types of tasks: An *Easy Task* consisting of five-letter sequences and a *Hard Task* consisting of 15-letter sequences (see Figure 1 for an example). We informed participants that there are two task types and that they would be randomly assigned to one of the two (treatment assignment). While participants know that the *Easy Task* involves shorter sequences and the *Hard Task* involves longer sequences, they are not told the exact number of letters in each task type, thus engendering uncertainty about their task assignment. We intentionally designed the tasks to ensure divergence between participant scores based on the random task assignment, rather than (endogenous) effort. Specifically, due to the length of the sequences, participants in the *Hard Task* will retype fewer sequences than participants assigned to the *Easy Task*.

We paid participants according to their relative performance. That is, we randomly matched a participant working on the *Easy Task* with a participant working on the *Hard Task* and compared their scores. The participant with the higher score receives a bonus payment of $2 and the participant with the lower score receives $0. Note that the matching protocol is public knowledge, i.e. participants are uncertain about the difficulty of their task, but know their matching partner is doing the other task (whether *Hard* or *Easy*).

After the work assignment we elicited our outcomes of interest: (meritocratic) beliefs, inequality acceptance and willingness to pay for information on task difficulty and performance in pairs. Importantly, we introduce and measure these outcomes sequentially, i.e. participants were, for example, not aware about our interest in inequality acceptance. Directly after the work assignment, but before we reveal the outcome of the performance comparison (i.e. the bonus payment), we elicit

beliefs about the task assignment in a randomized order. We remind participants that there was a 50 percent chance to be in either task and ask them to estimate the likelihood that they worked through the *Hard Task* ("Prior Belief, Task Difficulty"). They estimate how many of 100 participants performing the same task achieved a lower score ("Prior Belief, Relative Performance"), and how much they think they deserve the $2 bonus payment based on their score ("Prior Belief, Deserving Bonus"). After revealing the bonus payment, we ask the same questions again ("Posterior Beliefs"). In addition, we elicit our measure for meritocratic beliefs, i.e. the extent to which participants think that the bonus payment depends on luck or effort ("Effort Determines Success"). Building on evidence suggesting that complex incentivation rules do not outperform introspection (e.g. Trautmann and van de Kuilen, 2015; Charness, Gneezy and Rasocha, 2021; Danz, Vesterlund and Wilson, 2020), we do not remunerate the elicitation of these beliefs in order to avoid complicating the tasks and to keep the study within a reasonable time frame.

Next, participants had to decide about how much inequality they want to implement in the matched pair, i.e. they had to indicate the share of the bonus payment (between 0 and 100 percent) that will be equally redistributed within their pair. Using an interactive slider, participants can immediately see how their decision will affect their income and that of the other person. To incentivize the decision, we randomly select and implement one decision within each pair at the end of the study.[7] The variable of interest is then the inequality participant i implements, which we calculate as the ratio of the absolute income difference in the pair and total income:

$$inequality_i = \frac{\mid income\ success - income\ failure \mid}{total\ income} \tag{1}$$

where *income success* is the income after redistribution of the successful participant and *income failure* is the income after redistribution of the unsuccessful participant. Note that this ratio corresponds to the Gini-coefficient in this two-person situation, which is equal to one if the successful player keeps his entire bonus and zero if the bonus is fully redistributed resulting in equal incomes.

Finally, we offered participants an opportunity to buy information about task difficulty and the task performance of the other participant. We elicit their willingness to pay ("WTP") for this information with a simple price list. In this price list, we present participants with eight scenarios in which they have to decide between seeing the information or receiving extra money,

[7] Note that this procedure elicits participants' true inequality acceptance given that participants are consequentialists and care about final outcomes. This assumption seems reasonable in our setting as merit considerations typically overlay ex-ante fairness concerns (Cappelen et al., 2013; Durante, Putterman and Van der Weele, 2014; Cappelen et al., 2017).

with amounts ranging from \$0.01 to \$0.50.[8] For instance, in Scenario 1 they have to choose between seeing information and receiving \$0.01, and in Scenario 8 they have to choose between seeing information and receiving \$0.50. To incentivize participants, we randomly pick one of the eight scenarios for each participant and implement their choice in this scenario. That is, a participant will either receive the information immediately after the price-list decision or receive the extra money at the end of the survey. In a last step, all participants who have received the information and a random subset of the remaining participants (50 percent) have the opportunity to revise their decision about how much inequality to implement.

Implementation: We recruited participants via Amazon Mechanical Turk (MTurk) in summer 2019 and implemented the study using the open source software oTree (Chen, Schonger, and Wickens 2016). The MTurk platform offers access to a quite diverse population (e.g. Berinsky, Huber and Lenz, 2012; Buhrmester, Kwang and Gosling, 2011; Arechar, Kraft-Todd and Rand, 2017) and mounting evidence suggests that the findings of studies run on MTurk are robust to results using other subject populations, such as student, convenience, and nationally representative samples (e.g. Horton, Rand and Zeckhauser, 2011; Arechar, Gächter and Molleman, 2018; Coppock and McClellan, 2019; Snowberg and Yariv, 2021). To ease recruitment we offered a relatively high flat payment of \$0.75 and promised additional payments (participants could expect to earn \$1.50). Considering the average duration of the study of 10 minutes, incentives were substantially above the hourly minimum wage in all US states in 2019.

Attrition and balance: In total, 2,026 workers started the work assignment and 1,845 participants finished all tasks.[9] The overall attrition rate is about 9 percent, which is comparatively low for this type of study.[10] Importantly, attrition is random across the treatment assignment (10 percent in the *Hard Task* and 8 percent in the *Easy Task*, t-test, $p = 0.25$). The low level of attrition illustrates the effectiveness of the implemented measures to minimize dropouts and suggests that the treatment assignment did not cause participants to quit our HIT. A regression of an indicator for dropouts on

[8] This procedure is similar to elicitation procedure in Fuster et al. (2018), which also tested understanding in cognitive interviews, and Fehr, Mollerstrom and Perez-Truglia (2021).

[9] A total of 2,535 workers accepted our HIT. Of those, 383 failed on a simple CAPTCHA in the beginning, which served as a first robot control, and 105 did not finish the demographics survey. Our work assignment served as a second robot control as we displayed the letter sequences in non-machine-readable format and 21 MTurkers dropped out after the demographics survey but before the work assignment resulting in our final sample of 2,026.

[10] For example, Kuziemko et al. (2015) report an attrition rate of 15 percent in an early survey experiment on MTurk and Arechar, Gächter and Molleman (2018) report an attrition rate of 18 percent in a long and interactive repeated public good game on MTurk.

the treatment indicator shows no difference in the likelihood of attrition between the *Easy* and *Hard Task* (see Table A1).[11] Moreover, comparing socio-demographic characteristics (including political views) of dropouts and non-dropouts reveals no differences (see Table A2). Across 30 tests, there is no single t-statistic above 1.96. Taken together, attrition is therefore unlikely to affect our results.

In our final sample, we dropped 20 participants, because they ended up with the same score such that the bonus was split equally within pairs. This leaves us with 1,825 observations. In Table A3, we show summary statistics in column 1. In the remaining columns we show that participants do not differ along a large set of observables in the two tasks. A joint test for all observables being equal to zero reveals an F-statistic of 1.09 ($p = 0.35$).

Data quality: To ensure high data quality, we took several precautionary measures. Among them we required a minimum of 1000 completed Human Intelligence Tasks (HITs) and an acceptance rate of at least 98 percent, implemented bot screens (e.g., non-machine-readable content), and enforced strict timeouts on each question to minimize inattention of participants due to multitasking and switching between several HITs (for more details on these measures see Appendix A.2). Comparing our MTurk sample with data from the US census reveals remarkable similarities along a large set of observables. Our sample closely matches the US population in terms of age, gender, marital status, household size and income, and geographic location, but white and educated people are overrepresented (see Table A4). Another way to check data quality is to look at violations of monotonicity in the WTP elicitation task (i.e. switching multiple times between buying information and keeping the offered amount of money). The share of inconsistent participants is 3 percent, which is clearly at the lower end of the range observed in other studies.[12] We take this as an indication that data quality is high and that participants are attentive throughout the study, as this WTP measure is elicited towards the end of the survey.

Summary statistics of work assignment: Table 1 summarizes participants' performances in the two tasks. In the *Easy Task*, participants coded, on average, substantially more sequences of letters compared to participants in the *Hard Task* (35 vs. 10). However, the scores in the two tasks overlap

[11] The coefficient for the treatment indicator is -0.015 (s.e. 0.013). The same is true if we run the same regression but only consider dropouts after participants learned about the bonus assignment (coefficient -0.013, s.e. 0.009).

[12] Using a similar procedure, for example, Fehr, Mollerstrom and Perez-Truglia (2021) report 5 percent inconsistent choices in a high-quality panel study, the German Socio-Economic Panel (SOEP). The same is true for the sample used in Fuster et al. (2018), the SCE Housing Survey of the Federal Reserve Bank of New York, whereas Cullen and Perez-Truglia (2018) report 15 percent among employees of a large international bank.

to some extent. That is, the 10th percentile in the *Easy Task* is 16, while the 90th percentile in the *Hard Task* is 17. This has some implication on the empirical strategy as explained below.

3 Empirical Strategy

Our treatment involves the random assignment of participants to an *Easy* and *Hard Task*. Participants know at the outset that they will be assigned to one of the two tasks with equal probability and that they will be randomly matched to a participant completing the other task. Importantly, they do not learn and cannot infer the difficulty of the task from the task itself. We calibrated the difficulty of the two tasks such that the participant assigned to the *Easy Task* can easily outperform his or her counterpart assigned to the *Hard Task*. Consequently, economic success (i.e. receiving the $2 bonus payment) should coincide with the random assignment to the *Easy Task*, while failure coincides with the random assignment to the *Hard Task*. Thus, the random assignment to the two tasks allows us to identify the causal effect of economic status on meritocratic beliefs and inequality acceptance.

Assuming that the random assignment to the *Easy Task* always leads to economic success, we could simply regress the outcomes on the bonus payment. In practice, treatment compliance was, however, not perfect. As indicated above, the performance distribution in the two tasks overlap to some extent resulting in non-compliance to the treatment assignment in about 6 percent of cases. That is, in these cases a participant assigned to the *Hard Task* outcompeted a participant in the *Easy Task*, such that the bonus is paid to the participant in *Hard Task*, instead of the participant in the *Easy Task*. To deal with this non-compliance, we use the treatment assignment (*Easy* or *Hard Task*) to estimate *intention-to-treat (ITT)* effects. The general regression framework thus takes the following form:

$$Y_i = \beta_0 + \beta_1 EasyTask_i + \gamma \mathbf{X}_i + \varepsilon_i \tag{2}$$

where Y_i is the outcome variable of interest, $EasyTask_i$ indicates if a participant i was randomly assigned to the *Easy Task*, $\mathbf{X}$ is a set of standard controls (including gender, age, marital status, education level, ethnicity, employment status, and household income), and ε_i is an individual-specific error term.[13]

We use this framework to estimate the "first stage" effect of treatment assignment on meritocratic beliefs (i.e. beliefs about the role of luck and effort for economic success). In a second

[13] We control for the false discovery rate (FDR) using the the two-stage linear step-up procedure by Benjamini, Krieger and Yekutieli (2006).

step we estimate the "reduced form" of treatment assignment on inequality acceptance and then combine the two effects to estimate the effect of a change in meritocratic beliefs on inequality acceptance using two-stage least squares. We interpret the results from estimating equation 2 as the effect of the bonus announcement or economic success throughout the paper as non-compliance is low and IV estimates using treatment assignment as an instrument for the bonus announcement are very similar in magnitude (see Appendix A.3 for details).

We also explore the relationship between economic status and political views. Public opinion polls consistently reveal that liberals and conservatives differ in their views about the causes of success: liberals emphasize the role of luck, whereas conservatives believe in the role of effort. A persistent concern is that these views do not necessarily reflect what people think and do when they are forced to appraise their own success. Therefore, in some specifications we consider participants' political views by splitting the sample along these views or by including its interaction with the treatment. For this purpose, we asked participants about their political orientation ranging from "strongly liberal" to "strongly conservative" (on a 6-point scale) and classify them as liberal if they indicate that they are "strongly liberal" , "moderately liberal" or "slightly liberal."[14]

4 Main Results

4.1 First Stage: Effects on Meritocratic Beliefs

We begin by documenting participants' subjective assessments of their performance in the work assignment measured before *and* after revealing their status (i.e. whether they received the bonus payment or not). While estimating the causal effect of economic status on meritocratic beliefs is our main focus in this section, our setup also enables us to look at how individuals update their beliefs about the task assignment upon receiving a perfect signal about success or failure (information about bonus assignment).

In Figure 2, we present a graphical illustration of the prior beliefs that we elicited directly after the work assignment. At this point participants did not know whether they were successful or not. They only experienced the task and knew that they had a 50 percent chance to complete either task. Looking at the priors about task difficulty reveals that participants had some notion of their task assignment. In both tasks they shift their beliefs into the right direction, but this shift is far from

[14] We also asked participants about their party affiliation (Republican, Democrat, other). Our results do not change if we use this information or a combination of both questions in our analysis.

perfect. In particular, updating is lower for participants in the *Easy Task* (63 percent) than in the *Hard Task* (68 percent, $p < 0.001$, two-sided t-test). This suggests that there is significant uncertainty about the task assignment and in predicting success, and this is particularly pronounced in the *Easy Task*.

Figure 2 also reveals that participants in the *Easy Task* find themselves as more deserving of the bonus compared to participants in the *Hard Task* (75.2 percent vs. 71.9 percent, $p < 0.05$, two-sided t-test). This is notable, as it suggests that performance (i.e. coding a large number of sequences) creates a perception that one worked hard and thus deserves the bonus. This view is supported by the observation that actual task performance and *prior* perceptions of deservingness are strongly correlated in both tasks. Specifically, we see that each point increase in task performance is associated with a 0.48 percentage points higher prior belief in deservingness in the *Hard Task* and with a 0.26 percentage points higher prior in the *Easy Task* (see Figure A2).[15] Moreover, we can rule out that the higher deservingness in the *Easy Task* is due to participants correctly anticipating their success. In fact, we observe a negative relationship between prior beliefs about task difficulty and prior beliefs of deservingness, i.e. more certain participants think they are less deserving (see Figure A3). Finally, coding more sequences is related to the impression that one ranks higher in the performance distribution. Participants in the *Easy Task* thought they outperformed 54 percent of other participants completing the *Easy Task*, whereas participants in the *Hard Task* thought they were better than 52 percent of those completing the *Hard Task*. Although this difference is small, it is statistically significant ($p < 0.05$, two-sided t-test).

Next, we look at belief updating and examine the effect of the bonus announcement on the beliefs about the task assignment. For Bayesian individuals, the information about the bonus payment is a perfect signal about task difficulty and should resolve any remaining uncertainty about task difficulty leading to an update towards the signal (i.e. to the *Easy Task* when receiving the bonus and to the *Hard Task* otherwise). On the other hand, the information about the bonus payment should neither affect beliefs about relative performance nor deservingness as it contains no relevant news. This is because performance beliefs are measured relative to other participants doing the same task and because the signal does not reveal new information about the score in the task (beliefs about deservingness are assessed relative to one's score).

Figure 3 presents the changes in beliefs (posterior minus prior).[16] One can clearly see that

[15] Note that we rescaled the scores in the two tasks to have a common scale ranging from 0-100, see Figure A2 for details.

[16] In Table A5 we show the estimates of the task assignment on belief updating by regressing the difference between

individuals do update all three beliefs in response to the information about the bonus payment, but the updating is either insufficient or too excessive. Looking first at beliefs about task difficulty, one can see that failure leads to far too little updating (about 4 percentage points) and success results in updating away from the signal, i.e. participants in the *Easy Task* become less certain about the task difficulty (by about 2 percentage points). Second, although the information about the bonus payment is not informative for beliefs about deservingness and relative performance, we observe belief updating. In both cases, success increases posteriors, while failure decreases posteriors. Successful participants increase their posteriors about deservingness by 5 percentage points, while at the same time, failure induces participants to decrease perceived deservingness by almost 6 percentage points. This widens the belief gap in deservingness across tasks from 3 to 14 percentage points (*Easy Task* 80 percent vs. *Hard Task* 66 percent). Similarly, successful participants increase their beliefs about their performance relative to others doing the same task by 6 percentage points, while unsuccessful participants reduce their beliefs by 4 percentage points. The resulting belief gap across conditions amounts to 13 percentage points, i.e. in the *Easy Task* participants think they performed better than 60 percent of participants doing the same task, while in the *Hard Task* this belief drops to 47 percent.[17]

We now move on to discuss in greater detail meritocratic beliefs and focus on how the random task assignment – i.e. revealing the bonus payment – influences beliefs about the role of effort and luck in determining success. The belief updating on deservingness suggests that success reinforces a belief that those who work hard deserve the rewards they earn. Given this view, we would also expect to see an effect on meritocratic beliefs. Figure 4 presents a graphical illustration of the impact of economic success on the belief that success is due to effort in the task. Panel (a) of Figure 4 shows a strong divergence in beliefs across tasks. While participants in the *Hard Task* give effort and luck almost equal weight in determining success (54 percent effort), this is not the case for participants in the *Easy Task*. Successful participants think that success is predominantly the result of effort (70 percent). Panel (b) of Figure 4 shows the same relationship but splits the relationship along political orientation. Using pre-treatment information on participants' self-assessment in the political left-right spectrum, we observe a common pattern for liberals and conservatives. Both

posterior and prior beliefs on a treatment indicator.

[17]There is also evidence that being successful triggers overconfidence. If we compare the posteriors about relative performance with individuals' true rank in the performance distribution, we see substantially more overestimation of relative performance in the *Easy Task* than in the *Hard Task* (0.59 vs. 0.46; t-test, $p<0.01$). This is not the case if we instead consider prior beliefs. In this case, the share of participants who overestimate their performance is nearly the same in both tasks (0.52 vs. 0.50; t-test, $p<0.37$).

liberals and conservatives think that effort more likely determines success if they are successful, though liberals to a slightly lesser degree. Conversely, if they are unsuccessful both place only slightly more weight on effort than on luck.

In Table 2, we present estimates supporting the observations from Figure 4. In the table, we regress meritocratic beliefs on a treatment indicator, participants' political beliefs, and its interaction with the treatment indicator. It is apparent that receiving the bonus affects beliefs about the role of effort and luck. Successful participants more likely believe that effort determined success than unsuccessful participants. The magnitude of the effect, 16 percentage points, is large (column 1) and matches what we have seen in Figure 4. Adding political orientation reveals that conservatives, compared to liberals, consistently think that effort is more important than luck in determining success. This conservative-liberal gap is, however, small and amounts to 4 percentage points and we observe virtually no treatment heterogeneity along political orientation. This implies that the magnitude of the treatment effect (16 percentage points) is the same for liberals and conservatives. Thus, the impact of success on meritocratic beliefs is unaffected by political orientation.

4.2 Reduced Form: Effects on Inequality Acceptance

We now turn to examining how economic status affects inequality acceptance. As we did in the analysis of the first stage and meritocratic beliefs, we look first at the pooled sample and then at heterogeneous effects of political orientation using pre-treatment information on participants' self-reported classification as liberal or conservative.

In Figure 5, we present the implemented inequality among participants in the *Easy Task* and *Hard Task*. Panel (a) of Figure 5 shows a strong divergence of implemented inequality across the two conditions: the implemented inequality in the *Easy Task* is twice as high as in the *Hard Task* (0.79 vs. 0.40). Panel (b) of Figure 5 illustrates that economic status affect inequality acceptance irrespective of political views: conservatives *and* liberals prefer a lower level of inequality when they are unsuccessful and they accept more inequality when they are successful.[18] However, it is also true that liberals tend to implement lower inequality than conservatives. Specifically, the difference in the implemented inequality is about 0.08 points in the *Hard Task* (t-test, $p < 0.01$), while it is about 0.03 points in the *Easy Task* (t-test, $p = 0.06$).

In Table 3, we present regressions showing how economic status shapes inequality acceptance. The first column confirms that the implemented inequality is about 0.4 points higher if

[18] Figures A4 and A5 show the distribution of implemented inequality (pooled and separated by political orientation).

participants received the bonus. Including covariates does not change the estimate (column 2). In the remaining specifications, we consider the role of political orientation. One can see in columns 3–4 that, on average, liberals implement slightly less inequality than conservatives corroborating the observations from Figure 5. However, the difference in the implemented inequality between successful and unsuccessful liberals remains substantial. Interacting treatment status with political views in columns 5–6, illustrates that the liberal-conservative gap gets smaller in the *Easy Task*. The coefficient estimate is positive (but statistically insignificant) and roughly half of the difference between liberals and conservatives in the *Hard Task*. That is, while liberals tend to implement less inequality than conservatives, this difference is substantially smaller in the *Easy Task* than in the *Hard Task*.

Despite the strong divergence in inequality acceptance that we observe, it is apparent that fairness and merit considerations matter. First, if participants were purely selfish, we would expect an equal income distribution (i.e. a Gini coefficient of 0) *and* a completely unequal distribution (i.e. a Gini coefficient of 1), depending on treatment status. The implemented inequality is, however, far from these extremes in both conditions. Second, in both conditions the implemented inequality seems to reflect a widespread view that hard work entitles participants to what they earn. While this is most obvious in the behavior of the successful, even unsuccessful share this view as they do not equalize income within pairs. To more thoroughly examine to what extent this behavior is driven by the view that one deserves or is entitled to the rewards of hard work, we take advantage of our belief elicitation procedure that elicits beliefs about deservingness prior to the bonus announcement and *before* participants learned about the measurement of inequality acceptance. Thus, these beliefs reflect heterogeneity in participants' deservingness that are unaffected by the bonus announcement and that cannot reflect a preference for self-serving redistributive behavior.

Table 4 presents the results and reproduces, for comparison, the treatment effect of economic status on inequality acceptance in columns 1-2. We focus on the specifications in columns 5–6, which include the prior beliefs about the deservingness of the bonus.[19] One can see that the magnitude of the treatment effect becomes substantially smaller and statistically insignificant compared to the estimates in columns 1–2 and that prior beliefs about deservingness explain the differences in inequality acceptance across conditions. This is true if we consider only prior beliefs

[19]Prior beliefs about task difficulty and relative performance are also related to inequality acceptance (columns 3–4). We first note that in both treatments, participants who are more certain about task difficulty react more strongly by demanding less (*Hard Task*) and more inequality (*Easy Task*), respectively. Similarly, believing in stronger relative performance is associated with demanding a larger share of the pie. Importantly, in both cases we observe a large and significant treatment effect.

about deservingness (column 5) or all three prior beliefs simultaneously (column 6). A higher prior belief in deserving the bonus payment is associated with a lower acceptance of inequality for unsuccessful participants, but not for successful participants. More precisely, a 1 percentage point higher prior belief in deserving the bonus payment is associated with a 0.2 percentage point lower inequality for unsuccessful participants, but a 0.4 percentage point higher inequality for successful participants. Given the effect size of the interaction term, the joint effect of prior beliefs is positive and significant as well (Wald test, $p < 0.01$). Together, this suggests that meritocratic views are a major driver of the observed inequality acceptance.

4.3 Two-Stage Estimates: The Effect of Meritocratic Beliefs on Inequality Acceptance

Thus far we have seen that random variation in the task assignment caused: (i) a change in beliefs about economic success depending on effort rather than luck (meritocratic beliefs), and (ii) a strong divergence in inequality acceptance, with successful participants accepting substantially higher inequality than unsuccessful participants. We next combine the first-stage and reduced-form effects in a two-stage analysis that allows us to estimate the causal impact of meritocratic beliefs on inequality acceptance.

More precisely, we predict participants' inequality acceptance using their meritocratic beliefs, instrumenting these beliefs with the treatment, i.e. the random assignment to the *Easy Task* and the *Hard Task*. We begin by estimating the two-stage model using the pooled sample with and without controls. In Table 5, column 1, one can see that for each percentage point higher belief about the role of effort and luck for success inequality acceptance increases by 0.02 points. Including controls has virtually no effect on this estimate (column 2). In the remaining columns of Table 5, we split the sample along political orientation. Columns 3–4 display the results for liberals and columns 5–6 display the results for conservatives. For both subsamples we obtain results that are similar in magnitude to the results of the pooled sample, with and without controls. In summary, our findings show that meritocratic beliefs are shaped by economic status and that these meritocratic beliefs have a causal impact on how much inequality participants accept.

4.4 Willingness to Revise Meritocratic Beliefs

In our setting economic success depends on the random assignment to the *Easy* and *Hard Task*. The preceding analysis has demonstrated that success shifts beliefs about the role of effort and luck for

success and that these meritocratic beliefs causally affect how much inequality participants accept. As there is substantial uncertainty about the task difficulty and the performance of opponents, it is easy to maintain distorted beliefs to morally justify one's success.

In a next step, we examine whether participants are willing to pay for information that would allow them to update their beliefs about task difficulty and thus to verify their perceptions about the role of effort and luck in success. We proceed in two steps. First, we analyze participants willingness to pay for information resolving their uncertainty about the determinant of success and, second, we examine whether such information changes the inequality they implement.

We elicited participants' willingness to pay (WTP) with the help of an incentivized price list in the last part of the survey. That is, participants had to choose between receiving an additional sum of money (which varied between 1, 3, 5, 7, 10, 20, 35, and 50 cents) or information about the difficulty of the completed task and the score of their opponent. Figure 6 shows the distribution of participants' WTP with consistent answers, separated by task.[20] It is apparent that in both tasks a significant share of the participants are not interested in the information and always opt for the money (46 percent in the *Hard Task* and 52 percent in the *Easy Task*) and that WTP is lower in the *Easy Task*. At the same time, there is a sizable share of participants who are interested in learning about task difficulty. In Table 6, we use interval regressions to provide statistical support for these observations. Column 1 reveals that the average WTP in the *Hard Task* is about 7.4 cents, and about 1 cent lower in the *Easy Task*, a 14 percent lower WTP. Adding controls in column 2 leaves the coefficient of the treatment variable nearly unchanged. Moreover, we see that political views play no role in willingness to obtain information: liberals and conservatives display a similar willingness to pay. These findings suggest that participants are more likely to prefer remaining ignorant when they are successful, possibly to maintain their meritocratic beliefs, and this applies to liberals and conservatives in equal degree.

We next examine whether obtaining information about task difficulty and the opponents' score leads to a revised view on inequality. All participants who received the information (approx. 25 percent of the whole sample) and a random subset of the remaining participants (approx. 50 percent) had the possibility to reconsider their implemented inequality. This results in a sample of *N=1,130*. In a slight deviation from our pre-analysis plan, we look here at the likelihood of participants changing their inequality acceptance *and* the magnitude of a change.[21] In all regression

[20] As indicated in the discussion about data quality in Section 2, a few participants (3 percent) displayed inconsistent behavior by switching multiple times between buying information and keeping the offered amount of money.

[21] In the pre-analysis plan we proposed to look only at the revision of inequality acceptance.

specifications, we include our standard controls and additionally control for WTP as participants with a higher WTP have a higher probability of receiving the information. In other words, receiving information is only random after conditioning on WTP.

Table 7 displays the results. Conditional on WTP, receiving information increases the likelihood of revising the implemented inequality by 8 percentage points (a 27 percent change, column 1). The estimate does not change much once we control for treatment status (column 2). Splitting the sample along political orientation reveals that receiving information about task difficulty only had an impact among liberals (column 3), but not among conservatives (column 4). In the remaining columns of Table 7, we examine the magnitude of these changes in inequality acceptance. Again, we see that receiving information about task difficulty leads to larger changes in inequality acceptance than not receiving information (about 4 points, see column 5). Controlling for treatment status indicates that changes are smaller in the *Easy Task*. In the last two columns, we again split the sample by political orientation and show that the observed effects are completely driven by liberals.

In the appendix, we explore the robustness of these results. The effect of receiving information about task difficulty may depend on participants' prior about task difficulty. For example, a participant who is relatively certain about having worked on the *Hard Task* will not be too surprised to learn that she was in fact assigned to the *Hard Task*, thus making her less likely to revise the implemented inequality. Thus, we estimate the effect of misperceptions about task difficulty on the likelihood of revising inequality acceptance and its magnitude (see Appendix A.4 for more details). The results in Table A6 largely confirm our findings presented above. While the information shock has no effect on the likelihood of changing the implemented inequality, there is a significant and positive effect on the size of change in inequality. Learning that the task difficulty is 10 percentage point higher than previously thought results in a 3.5 percentage point larger magnitude of change (column 5). Again, differentiating between political views, we see that liberals drive this effect: they react strongly to the information shock, while conservatives do not react at all.

5 Conclusion

There is widespread support for meritocratic principles in modern societies. Few would disagree that people should be able to climb the ladder of success and reap its associated rewards, if they only work hard enough. Using a large-scale online experiment, we provide evidence that economic

status shapes meritocratic beliefs and inequality acceptance. Specifically, we document that success induces people to more likely believe that they deserve their success and to misperceive the cause of success by placing unduly low weight on the role of luck. Success and failure also have implications on inequality acceptance: success results in twice as high inequality as failure. Leveraging our experimental design, we then show that higher inequality acceptance is driven by stronger beliefs in meritocratic ideals. Taken together, our findings suggests the existence of a feedback loop: economic success leads to the impression that success is deserved because of hard work, and this justifies more inequality acceptance, which in turn reinforces economic status.

Our results contribute to and add new causal evidence to a rekindled debate about the merits of meritocracy. Some have drawn a bleak picture of meritocratic ideals in this debate, arguing that it benefits those who are already in advantageous positions (e.g. Frank, 2016; Sandel, 2020; Markovits, 2019). While meritocracy emphasizes the importance of a level playing field, reality diverges sharply from this ideal in most countries. In the United States, for example, social mobility is among the lowest across developed countries (Corak, 2006; Chetty et al., 2014, 2017). These unequal opportunities are strikingly visible in the college admission process. The most selective colleges in the US, which also offer the best earning prospects, predominantly enroll students from affluent families. The share of students at elite colleges coming from families in the top 1% of the income distribution is higher than the share from the bottom 50% (Chetty et al., 2020). Although these students from privileged families have to work hard for the admission, they clearly have a much easier route to success than others. Our setting mimics this socio-economic reality and our findings illustrate how success in the college admission race can easily reinforce an impression that one has worked hard and that other factors are less relevant. Because this belief creates a perception of deservingness, it can widen inequality and further strengthen the meritocratic ideal.

Meritocratic beliefs also potentially have a dark side. According to our data, successful participants accept more inequality because they feel entitled to their high income. Their success may, however, also distort their perception of others' meritocratic credentials. The psychological literature suggests that people are more likely remember the obstacles they faced than the advantages they had (e.g. Davidai and Gilovich, 2016). This asymmetry may induce people to attribute others' failure to a lack of effort and perseverance, and this tendency may be particularly pronounced in successful people who have managed to overcome the hurdles they faced. In this way, our results suggest that attribution of success solely to personal merit may be an important impediment to encouraging greater fairness and equality in socioeconomic outcomes.

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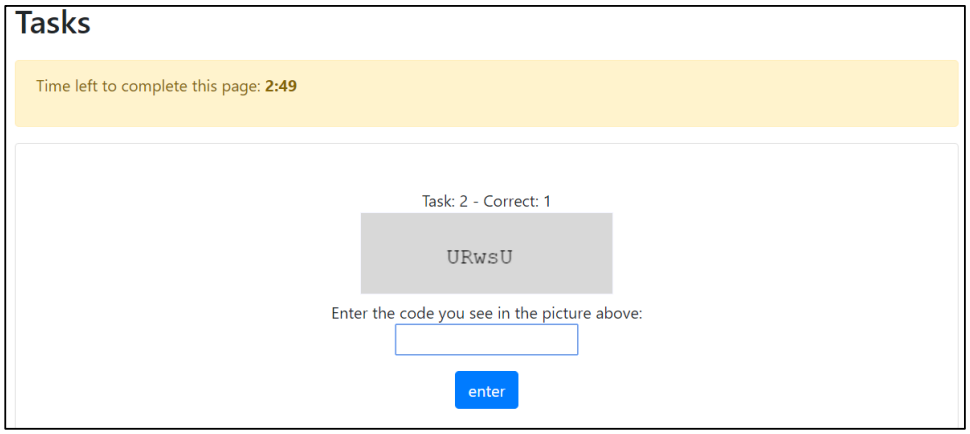
Figures and Tables

Figure 1: Examples for the Two Tasks

(a) Easy Task



(b) Hard Task

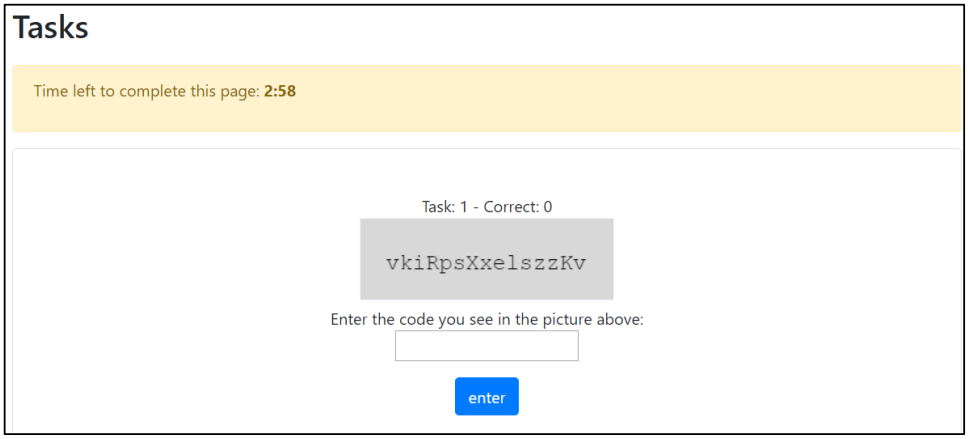
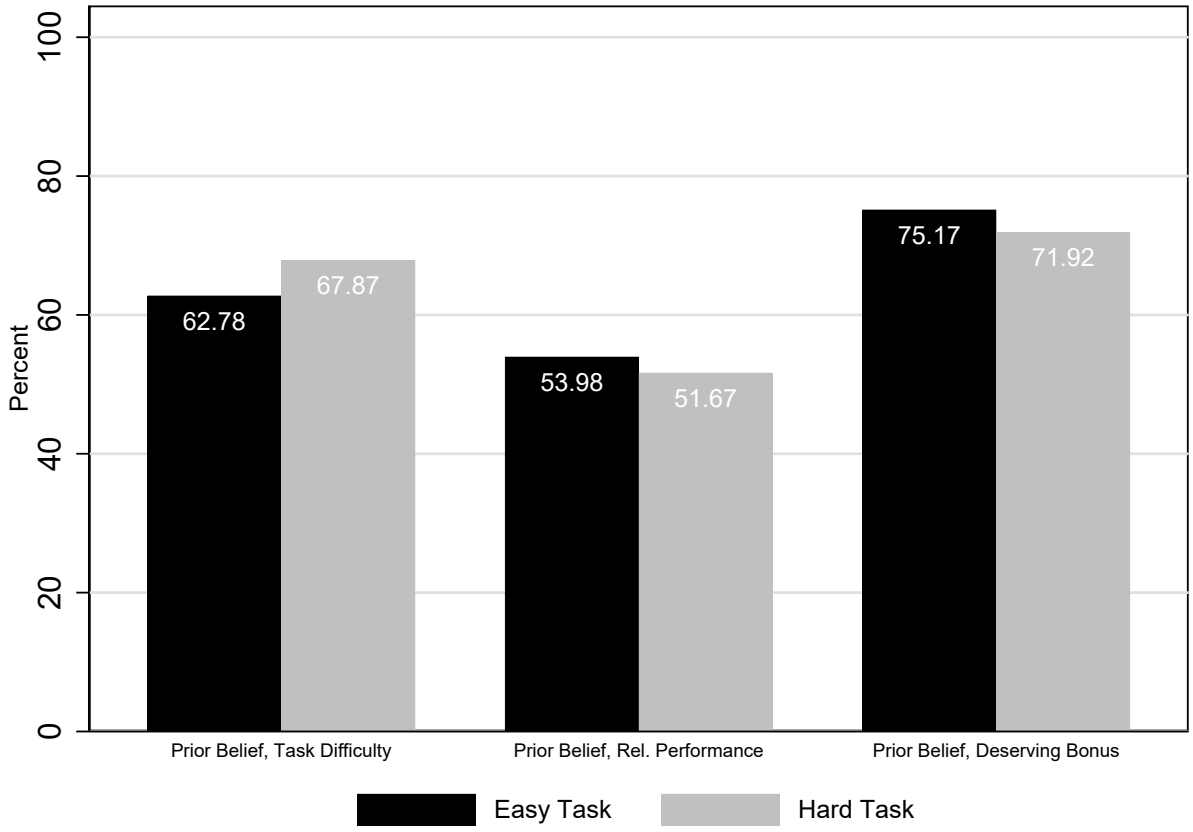
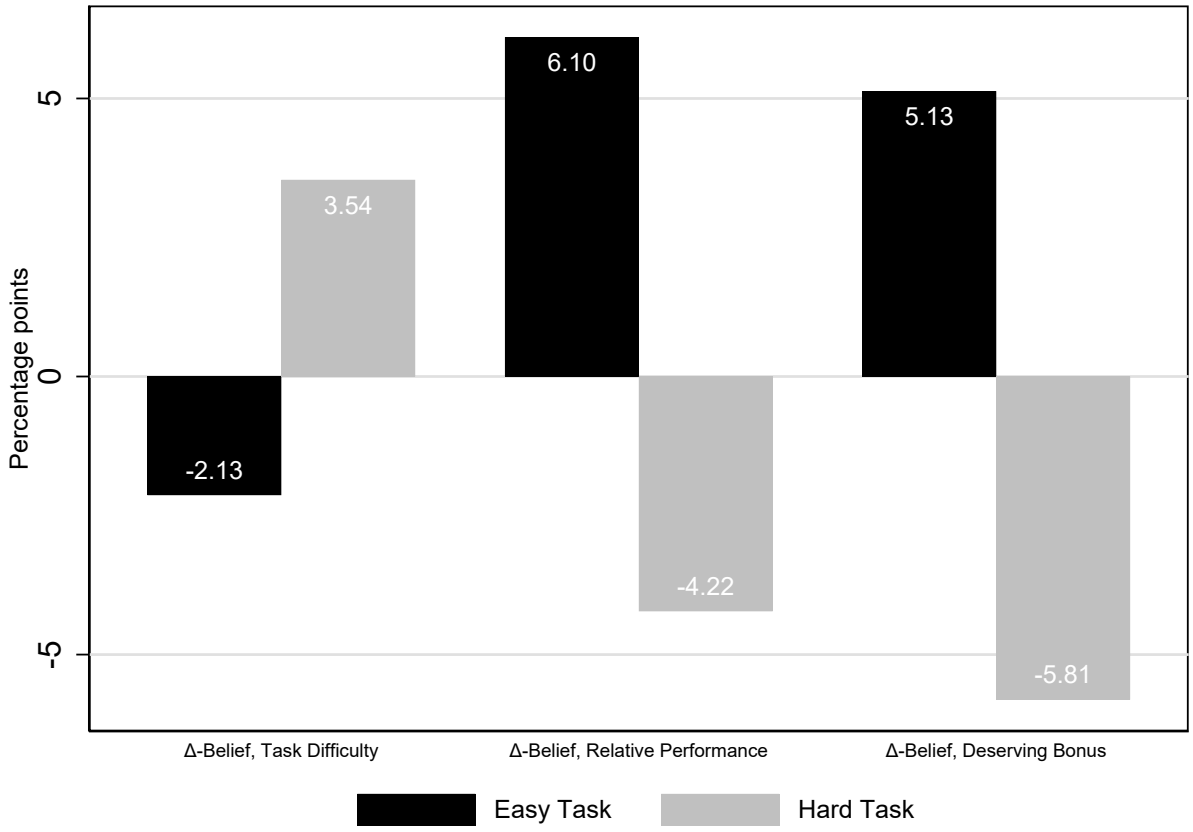


Figure 2: Prior Beliefs about Task Assignment by Treatment



Notes: The figure shows prior beliefs about task difficulty, relative performance, and deservingness that we elicited after the task, but before revealing the bonus assignment in the two conditions. All beliefs are measured on a scale from 0 – 100: "Prior Belief, Task Difficulty:" likelihood of performing in the *Easy (Hard) Task* in %; "Prior Belief, Deserving Bonus:" deserving the $2 bonus payment in %.

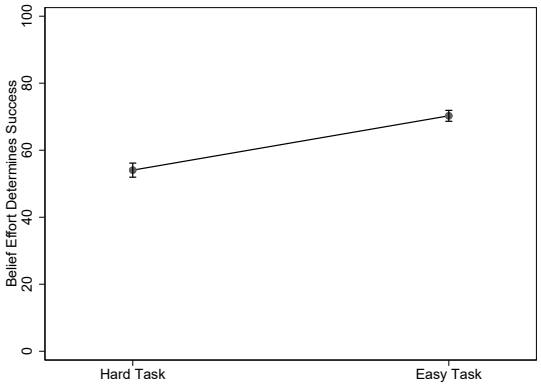
Figure 3: Updating Beliefs about Task Assignment



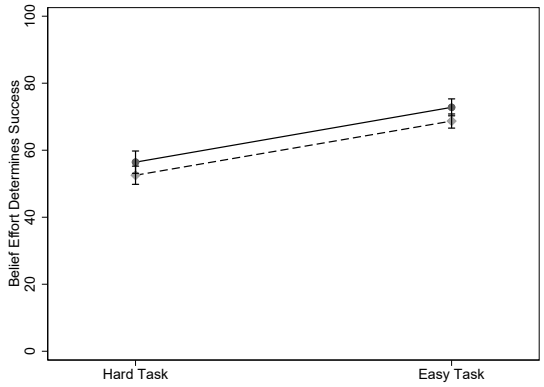
Notes: The figure shows the difference between posterior and prior beliefs about task difficulty, relative performance, and deservingness in the two conditions. All beliefs are measured on a scale from 0 – 100: "Δ-Belief, Task Difficulty:" likelihood of performing in the *Easy (Hard) Task* in %; "Δ-Belief, Relative Performance:" perceived number of participants performing the same task with a lower score; "Δ-Belief, Deserving Bonus" deserving the $2 bonus payment in %.

Figure 4: Meritocratic Beliefs by Treatment and Political Orientation

(a) Treatment effect



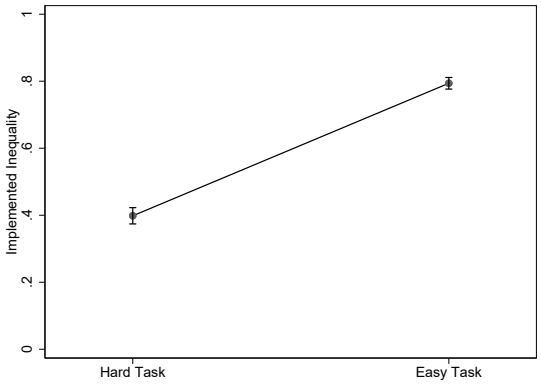
(b) Split by political orientation



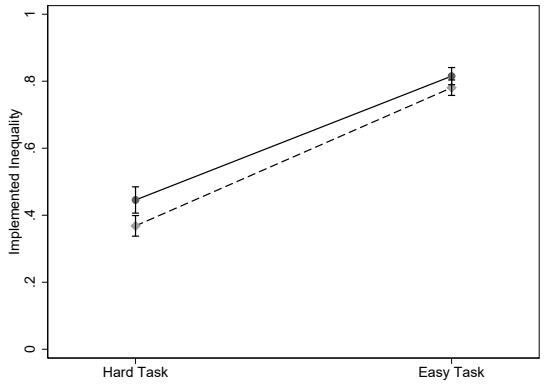
Notes: The figure shows meritocratic beliefs across the different conditions. Panel (a) displays the belief "Effort Determines Success" across treatments (*Hard Task* and *Easy Task*). Panel (b) displays the belief "Effort Determines Success" across treatments (*Hard Task* and *Easy Task*) split by political orientation: conservatives (solid black line) and liberals (dashed light-gray line). Error bars denote 95% confidence interval.

Figure 5: Inequality Acceptance by Treatment and Political Orientation

(a) Treatment effect

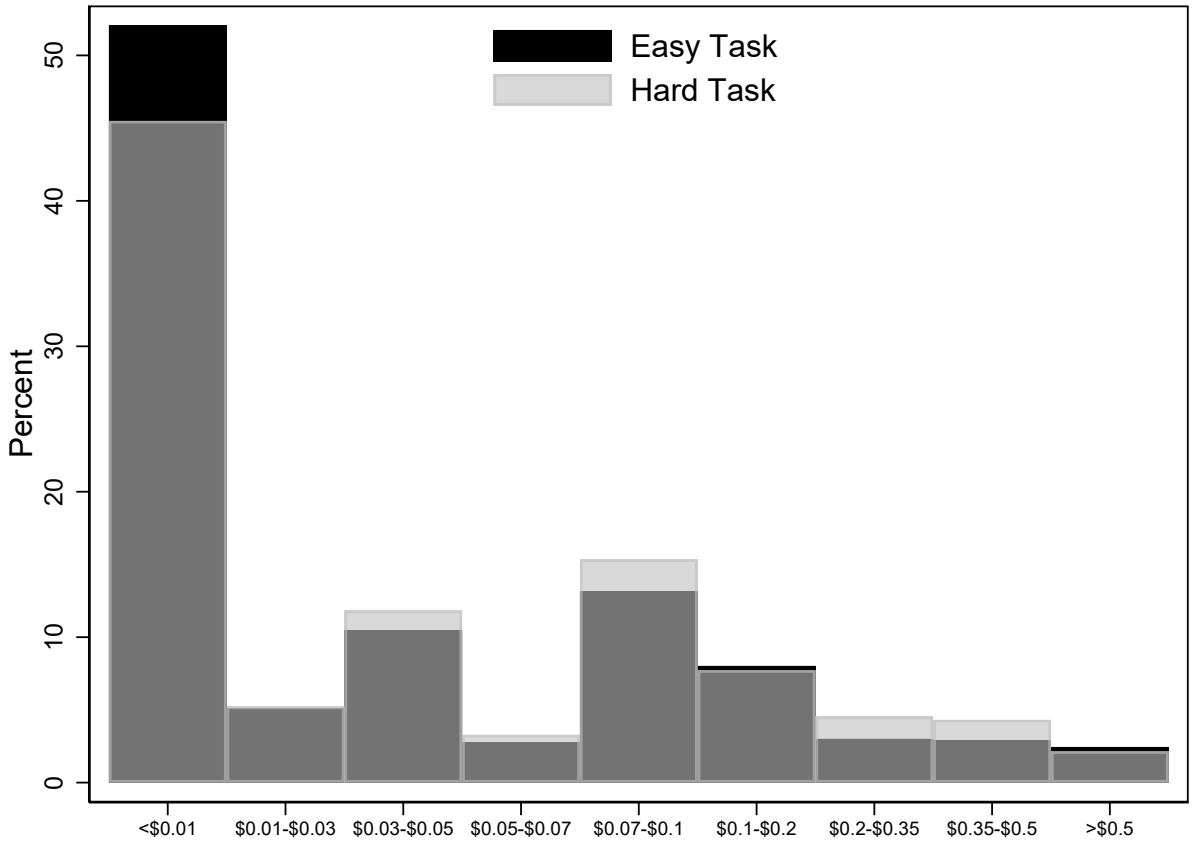


(b) Split by political orientation



Notes: The Figure shows inequality acceptance across the different conditions. Panel (a) displays implemented inequality across treatments (*Hard Task* and *Easy Task*) and panel (b) shows the implemented inequality across conditions split by political orientation: conservatives (solid black line) and liberals (dashed light-gray line). Error bars denote 95% confidence interval.

Figure 6: Willingness-to-Pay for Information on Task Difficulty



Notes: The figure shows the distribution of participants' willingness to pay (WTP) for information about the task difficulty (using all participants with consistent answers: N=1,776), separated by condition. The black bars indicate the WTP in the *Easy Task* and the overlaying gray bars the WTP in the *Hard Task*. An amount smaller than \$0.01 indicates that the participant always preferred money over information and vice versa for an amount larger than \$0.50.

Table 1: Work Performance by Task Difficulty (Treatment)

	Mean	**S.D.**	$\mathbf{P_{10}}$	$\mathbf{P_{50}}$	$\mathbf{P_{90}}$
Hard Task	10.25	5.45	4	10	17
Easy Task	34.86	15.47	16	33	56

Notes: Mean, standard deviation (S.D.) and the 10th, 50th, and 90th percentile of correctly typed letter sequences by treatment

Table 2: Regression: First Stage – Effects on Meritocratic Beliefs

	Effort Determines Success		
	(1)	(2)	(3)
Easy Task	16.213***/#	16.358***/#	16.465***/#
	(1.355)	(2.123)	(2.126)
Liberal		-3.905*	-4.014*
		(2.184)	(2.216)
Liberal**Easy Task*		-0.185	0.081
		(2.752)	(2.751)
Constant	54.054***	56.439***	40.207***
	(1.072)	(1.691)	(10.422)
Observations	1,825	1,825	1,822
Controls	No	No	Yes
R-squared	0.07	0.08	0.09

Notes: OLS-regressions with robust standard errors in parentheses. Meritocratic Beliefs ("Effort Determines Success:") are elicited after the bonus assignment (posterior) and are measured on a scale from 0 – 100: likelihood that the $2 bonus payment depends on exerted effort in %. "*Easy Task*" is an indicator for random assignment to the *Easy Task*. "Liberal" is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and a set of indicator variables for white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; # indicates significance, when using the two-stage linear step-up procedure by Benjamini, Krieger and Yekutieli (2006) that controls for a false discovery rate at q=0.05 for the treatment variable *Easy Task*.

Table 3: Regression: Reduced Form – Inequality Acceptance and Political Views

	Inequality Acceptance					
	(1)	(2)	(3)	(4)	(5)	(6)
Easy Task	0.395***/#	0.394***/#	0.396***/#	0.395***/#	0.370***/#	0.367***/#
	(0.015)	(0.015)	(0.015)	(0.015)	(0.024)	(0.024)
Liberal			-0.056***	-0.058***	-0.077***	-0.080***
			(0.015)	(0.016)	(0.025)	(0.026)
Liberal**Easy Task*					0.043	0.045
					(0.031)	(0.031)
Constant	0.398***	0.279**	0.432***	0.330***	0.446***	0.338***
	(0.012)	(0.114)	(0.016)	(0.114)	(0.020)	(0.115)
Observations	1825	1822	1825	1822	1825	1822
Controls	No	Yes	No	Yes	No	Yes
R-squared	0.272	0.277	0.277	0.282	0.278	0.283

Notes: OLS-regressions with robust standard errors in parentheses. "Inequality Acceptance" is the implemented inequality in a group, measured on a scale from 0 to 1. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. "Liberal" is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; # indicates significance, when using the two-stage linear step-up procedure by Benjamini, Krieger and Yekutieli (2006) that controls for a false discovery rate at q=0.05 for the treatment variable *Easy Task*.

Table 4: Regression: Prior Beliefs and Inequality Acceptance

	Inequality Acceptance					
	(1)	(2)	(3)	(4)	(5)	(6)
Easy Task	0.395***/#	0.394***/#	0.273***/#	0.160***/#	0.068	-0.064
	(0.015)	(0.015)	(0.041)	(0.038)	(0.044)	(0.058)
Prior Belief, Task Difficulty			-0.002***			-0.001**
			(0.000)			(0.000)
Easy Task x Prior Belief, Task Difficulty			0.002***			0.002***
			(0.001)			(0.001)
Prior Belief, Relative Performance				-0.003***		-0.001**
				(0.001)		(0.001)
Easy Task x Prior Belief, Relative Performance				0.004***		0.002***
				(0.001)		(0.001)
Prior Belief, Deserving Bonus					-0.002***	-0.001***
					(0.000)	(0.001)
Easy Task x Prior Belief, Deserving Bonus					0.004***	0.003***
					(0.001)	(0.001)
Constant	0.398***	0.279**	0.395***	0.430***	0.490***	0.573***
	(0.012)	(0.114)	(0.117)	(0.116)	(0.116)	(0.119)
Observations	1825	1822	1822	1822	1822	1822
Controls	No	Yes	Yes	Yes	Yes	Yes
R-squared	0.272	0.277	0.284	0.297	0.303	0.310

Notes: OLS-regressions with robust standard errors in parentheses. "Inequality Acceptance" is the implemented inequality in a group, measured on a scale from 0 to 1. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. Prior beliefs elicited before the bonus assignment and measured on a scale from 0 – 100: "Prior Belief, Task Difficulty:" likelihood of performing in the easy/hard task in %; "Prior Belief, Relative Performance:" relative performance rank among participants performing the same task; "Prior Belief, Deserving Bonus:" deserving the $2-bonus payment in %. Controls include sex, age, household size, log income and a set of indicator variables for white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; # indicates significance, when using the two-stage linear step-up procedure by Benjamini, Krieger and Yekutieli (2006) that controls for a false discovery rate at q=0.05 for the treatment variable *Easy Task*.

Table 5: Regression: Two-Stage Estimates of Inequality Acceptance

	Inequality Acceptance					
	All subjects		Liberals		Conservatives	
	(1)	(2)	(3)	(4)	(5)	(6)
Effort Determines Success	$0.024^{***/\#}$	$0.024^{***/\#}$	$0.025^{***/\#}$	$0.025^{***/\#}$	$0.023^{***/\#}$	$0.022^{***/\#}$
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	1825	1822	1122	1121	703	701
Controls	No	Yes	No	Yes	No	Yes

Notes: Two-stage estimates of the effects of meritocratic beliefs on inequality acceptance. The first stage estimates the impact of the exogenous task assignment on meritocratic beliefs and the second stage used variation in meritocratic beliefs induced by the exogenous task assignment to estimate the effect of meritocratic beliefs on inequality acceptance. "Inequality Acceptance" is the implemented inequality in a group, measured on a scale from 0 to 1. "Effort Determines Success" is the likelihood that the $2 bonus payment depends on exerted effort in %. Columns 1-2 include the whole sample, columns 3-4 restricts the sample to liberals (participants who self-identified as strongly liberal, moderately liberal and slightly liberal) and columns 5-6 restricts the sample to conservatives (participants who self-identified as strongly conservative, moderately conservative and slightly conservative). Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; # indicates significance, when using the two-stage linear step-up procedure by Benjamini, Krieger and Yekutieli (2006) that controls for a false discovery rate at q=0.05.

Table 6: Regression: Willingness to Pay for Information

	Willingness to Pay			
Easy Task	-0.991*	-1.109**/#	-0.990*	-1.109**/#
	(0.535)	(0.531)	(0.534)	(0.531)
Liberal			-0.635	-0.213
			(0.565)	(0.579)
Constant	7.367***	-0.892	7.760***	-0.697
	(0.403)	(3.832)	(0.558)	(3.816)
Observations	1776	1773	1776	1773
Controls	No	Yes	No	Yes

Notes: Interval-Regression with robust standard errors in parentheses. The sample includes only participants with consistent answers, i.e. we dropped 49 participants who switched multiple times between a monetary amount and receiving information. "Willingness to Pay" (in cents) is the willingness to pay for receiving information about the task difficulty and the score of the other participant, categorized in 9 intervals $[0,1]$; $[1,3]$; $[3,5]$; $[5,7]$; $[7,10]$; $[10,20]$; $[20,35]$; $[35,50]$; $[50,\infty)$. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. "Liberal" is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Mid-west, West).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; # indicates significance, when using the two-stage linear step-up procedure by Benjamini, Krieger and Yekutieli (2006) that controls for a false discovery rate at q=0.05 for the treatment variable *Easy Task*.

Table 7: Regression: Revising Inequality Acceptance

	Change Inequality				Δ-Inequality			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	All	Lib.	Cons.	All	All	Lib.	Cons.
Received Info	0.081**/#	0.077**/#	0.101**/#	0.054	0.039***/#	0.037***/#	0.040***/#	0.042
	(0.036)	(0.036)	(0.046)	(0.060)	(0.013)	(0.013)	(0.013)	(0.026)
Easy Task		-0.047	-0.023	-0.071		-0.026***	-0.008	-0.049***
		(0.029)	(0.036)	(0.048)		(0.009)	(0.010)	(0.018)
WTP	0.278*	0.278*	0.404*	0.079	0.019	0.018	0.022	-0.010
	(0.149)	(0.148)	(0.206)	(0.217)	(0.050)	(0.049)	(0.053)	(0.090)
Constant	0.609***	0.631***	0.394	0.882**	0.022	0.034	-0.057	0.149
	(0.221)	(0.221)	(0.282)	(0.379)	(0.068)	(0.068)	(0.075)	(0.135)
Observations	1094	1094	681	413	1094	1094	681	413
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.047	0.049	0.061	0.064	0.035	0.042	0.049	0.063

Notes: OLS-regressions with robust standard errors in parentheses. The sample includes all participants who had the opportunity to reconsider the implemented inequality, i.e. all participants who received information about the task difficulty and a random subset of participants who did not receive this information. "Change Inequality" is an indicator for revising the initially implemented inequality and "Δ-Inequality" is the absolute difference between the initially implemented inequality and the revised inequality. "Received Info" is an indicator for participants who received information about the task difficulty and the performance of the other participant. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. "Liberal" is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. "WTP" (in cents) is the willingness to pay for receiving information about the task difficulty and the score of the other participant, categorized in 9 intervals $[0,1]$; $[1,3]$; $[3,5]$; $[5,7]$; $[7,10]$; $[10,20]$; $[20,35]$; $[35,50]$; $[50,\infty)$. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; # indicates significance, when using the two-stage linear step-up procedure by Benjamini, Krieger and Yekutieli (2006) that controls for a false discovery rate at q=0.05 for *Received Info*.

Appendix – For Online Publication Only

Misperceiving Economic Success: Experimental Evidence on Meritocratic Beliefs and Inequality Acceptance

Dietmar Fehr and Martin Vollmann

A.1 List of Covariates

- Gender (Male / Female / Other / I prefer not to say)
- Age (in years)
- Marital status (Single / Married)
- Education (Not completed high school/ High school/ Some college/ 2-year college degree/ 4-year college degree/ Masters degree/ Doctoral degree/ Professional degree (JD, MD))
- Ethnicity (White/European-American / Black/African-American / Asian/Asian-American/Pacific Islander / Hispanic/Latino / Other)
- Number of household members
- Political beliefs (Strongly liberal / Moderately liberal / Slightly liberal / Slightly conservative / Moderately conservative / Strongly conservative)
- Political party identification (Democratic Party/ Republican Party/ Other)
- US residence (Yes / No)
- Home state (list of US states)
- Employment status (Full-time employee / Part-time employee / Self-employed or small business owner / Unemployed and looking for work / Student / Not in labor force)
- Household income ($0 - $9,999 / $10,000 - $14,999 / $15,000 - $19,999 / $20,000 - $29,999 / $30,000 - $39,999 / $40,000 - $49,999 / $50,000 - $74,999 / $75,000 - $99,999 / $100,000 - $124,999 / $125,000 - $149,999 / $150,000 - $199,999 / $200,000 and more)

A.2 Details on Study Implementation

We used Amazon Mechanical Turk (MTurk) to recruit workers to complete a work assignment and survey in summer 2019. We offered a relatively high flat payment of $0.75 and promised additional payments (participants could expect to earn $1.50). Considering the average duration of the study of 10 minutes, incentives were substantially above the hourly minimum wage in all US states in 2019.

To address concerns about data quality, in particular due to automated responses (bots) and inattention (Chmielewski and Kucker, 2020; Ahler, Roush and Sood, 2019), we took several precautionary measures. First, we limited participation to MTurkers based in the US with more than 1000 performed Human Intelligence Tasks (HITs) and an acceptance rate of at least 98%. Second, we used a simplified CAPTCHA (adding two numbers) to screen for bots, i.e. only participants that correctly answered this question could access our survey. In addition, the letter sequences in the work assignment were in non-machine-readable format, providing another layer of protection against bots. Third, to minimize inattention due to multitasking and switching between several HITs, we requested that participants should exclusively work on our HIT, and stated that they have a total of 20 minutes to complete the HIT, that there are timeouts on each question, and that any payment is conditional on completing the HIT within the time limit.

There are also some practical challenges associated with running experiments on an online platform such as MTurk. First, participants typically do not arrive simultaneously. While we designed our experiment such that the survey and the work assignment can be completed independently, the bonus payment required a comparison of the work performance between two participants. For this reason, every participant entered a virtual waiting room before the announcement of the bonus payment. If a suitable matching partner was already waiting, participants were immediately matched and each could independently work through the rest of the survey. If there was no matching partner available, participants had to wait for a minimum of three minutes. As soon as a suitable matching partner arrived in the waiting room, they were matched.[22]

Second, we aimed to minimize the risk of participants dropping out before completing the survey. Despite numerous possibilities for dropping out voluntarily or involuntarily (e.g. if no matching partner is available), internal validity is only threatened by dropouts after the

[22] Participants had the possibility to end the survey after three minutes (if no suitable matching partner had arrived), in which case they only received the base payment. Alternatively, they could continue waiting until they were matched (but they ran the risk of exceeding the HIT time limit, in which case they received no payment).

announcement of the bonus payment (which depends on the random task assignment). As long as such dropouts are random across the treatment, our treatment estimates remain unbiased (as it is the case, as shown in Section 2 and Table A1). Nevertheless, we took some steps to minimize this risk ex-ante and informed participants that they would not receive any payment *and* no HIT approval if they dropped out due to a time out. Evidence suggests that these are sensible requirements, as MTurkers are sensitive to rejections (a low approval rate prevents them from participating in HITs that require a high approval rate, see Hara et al. (2018)).

A.3 IV-Estimates: Effect of the treatment on the treated

We identify the causal impact of economic status on meritocratic beliefs and inequality acceptance through the random assignment of participants to the *Easy Task* and *Hard Task*. Recall that we calibrated the two tasks such that completing the *Easy Task* should always result in a better performance than completing the *Hard Task*. Consequently, economic success should always coincide with the random task assignment.

However, treatment compliance was imperfect. That is, about 6 percent of participants assigned to *Hard Task* had a better performance than their matched counterparts in the *Easy Task*. Therefore, we reported intention-to-treat (ITT) effects in the paper and presented first-stage and reduced-form effects along with the two-stage effects in Section 4. In the following, we present the effects of the treatment on treated (i.e. the effect of receiving the bonus – economic success – on meritocratic beliefs and inequality acceptance) by using our random assignment to the two tasks as an instrument. In specifications that include an interaction term between economic success and political view, we also instrument the interaction term with the interaction between random task assignment and political view.

The results are presented in Tables A7, A8 and A9. The exercise reveals that the effects and the magnitude of the IV estimates are very similar to the ITT estimates that we reported in the paper. For example, the effect of economic success on meritocratic beliefs is 18 percentage points (Table A7, column 1) compared to 16 percentage points in the intent-to-treat framework presented in Table 2.

A.4 Robustness: Impact of Correcting Misperceptions on Inequality Acceptance

Given the variation in beliefs about task difficulty, the impact of information disclosure may differ substantially across participants. For example, a participant who is relatively certain about having worked on the *Hard Task* will not be too surprised to learn that she was in fact assigned to the *Hard Task*, thus making her less likely revise her inequality acceptance. To capture this effect and to account for the fact that a subset of participants received no information and therefore could not update their beliefs, we estimate the following regression model:

$$Y_i = \beta_1 \cdot \left(100 - b_i^{posterior}\right) \cdot R_i + \beta_2 \cdot \left(100 - b_i^{posterior}\right) + WTP_i + \gamma \mathbf{X} + \varepsilon_i$$

where Y_i is an indicator for revising the implemented inequality (or not), or the absolute value of the change in inequality. $b_i^{posterior}$ is the posterior belief about task difficulty and R_i is a binary variable, indicating whether a participant received information or not. The parameter of interest is β_1, which shows the causal effect (conditional on WTP) of receiving information on task difficulty, i.e. the effect of learning that the likelihood of being in the *hard/Easy Task* is 1 percentage point higher than previously thought. The variable $\left(100 - b_i^{posterior}\right)$ controls for non-random variation in misperceptions about the task difficulty, which ensures that β_1 is identified by random variation in receiving information about task difficulty. This analysis is exploratory, as we did not specify it in our pre-analysis plan.

In Table A6, column 1, we see that the information shock has no effect on the likelihood of changing the implemented inequality. The coefficient is close to zero and precisely estimated. Controlling for treatment status (column 2) reveals that participants in the *Easy Task* are less likely to revise the implemented inequality. This negative effect on inequality acceptance is present among liberals and conservatives (columns 3 and 4). In contrast to these results, the information shock has a significant and positive effect on the size of change in inequality. Learning that the task difficulty is 10 percentage point higher than previously thought results in a 3.5 point larger magnitude of change (column 5). This is sizable given that the average bias is about 33 percentage points. Again, controlling for the treatment status reveals that changes are smaller in the *Easy Task*. If we differentiate between political views, we see that liberals drive the effect of correcting misperceptions on inequality acceptance. They react strongly to the information shock (column 7),

while conservatives do not react at all (column 8). To summarize, the information shock has no influence on the decision to revise implemented inequality, but if participants revise their inequality acceptance, changes are larger for liberals who experienced a larger information shock.

A.5 Locus of Control

A person's locus of control (LoC) describes the degree to which they feel to have control over the outcomes in their life. We elicit LoC with a 7-item battery listed below, measured on a 7-point scale (1=Disagree strongly – 7=Agree strongly).

1. I have little control over the things that happen to me.
2. There is really no way I can solve some of the problems I have.
3. There is little I can do to change many of the important things in my life.
4. I often feel helpless in dealing with the problems of life.
5. Sometimes I feel that I'm being pushed around in life.
6. What happens to me in the future mostly depends on me.
7. I can do just about anything I really set my mind to do.

Following (Cobb-Clark and Schurer, 2013), we summarize the responses in a single measure that ranges between seven (full control over life, i.e. internal LoC) and 49 (no control over life, i.e. external LoC). This single measure (LoC-Index) is constructed by summing the responses to the five external items (1–5), subtracting the sum of responses to the two internal items (6–7) and adding 16. Specifically,

$$LoC - Index_i = \sum_{j=1}^{5} eLoC_{i,j} + \sum_{j=6}^{7} iLoC_{i,j} + 16 \tag{3}$$

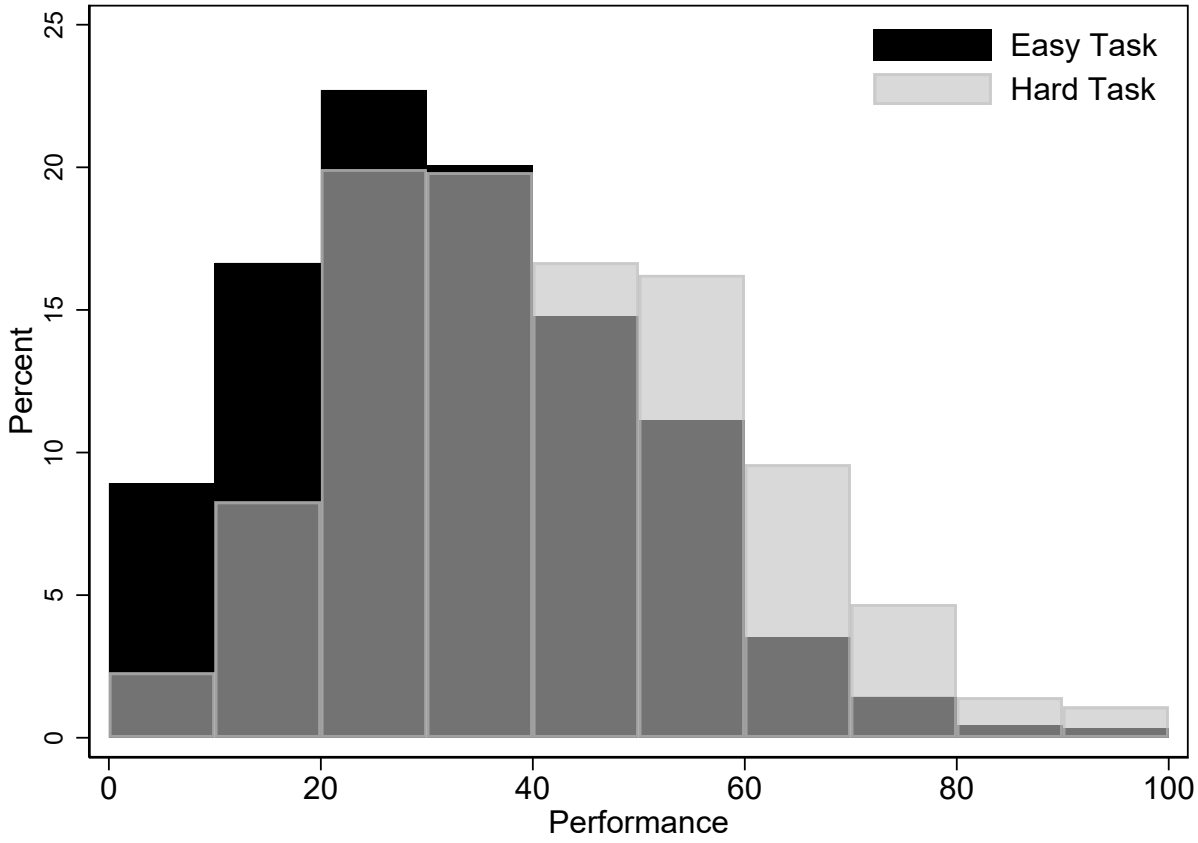
This index is therefore increasing in external control tendencies and is bounded between 7 (internal) and 49 (external).

We begin our analysis with looking at the relationship between political orientation and locus of control. Table A10 presents this correlation: liberals are more likely to believe life outcomes are the result of fate or luck, and therefore beyond one's control. We also observe that LoC is

associated with meritocratic beliefs (see Table A11). That is, a higher external LoC is associated with a lower belief in the importance of effort for achieving success in both the *Easy Task* and *Hard Task*. Both findings are consistent with the finding that liberals are less likely to believe that the bonus payment is the result of effort (see Table 2). In Table A12), we regress inequality acceptance on our treatment, LoC, and the interaction of the two and find no measurable effect of LoC on inequality acceptance.

A.6 Additional Figures

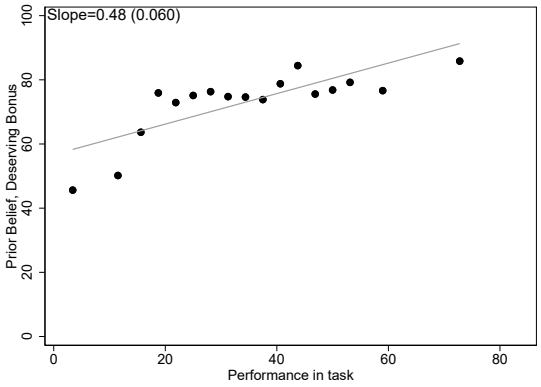
Figure A1: Distribution of Performance



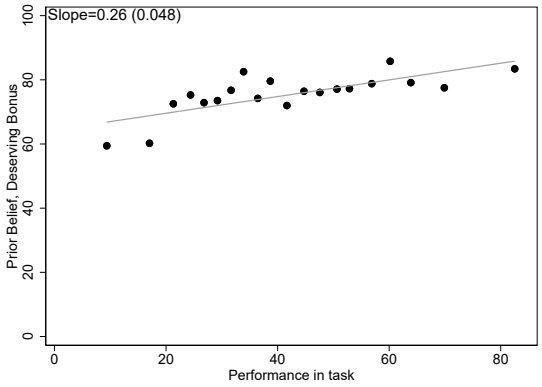
Notes: Histograms showing the distribution of performance separated by *Easy Task* and *Hard task* (N=1,825).

Figure A2: Relationship between Task Performance and Prior Belief about Deservingness

(a) Hard Task



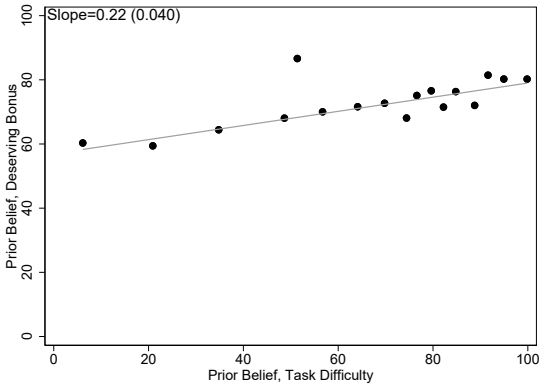
(b) Easy Task



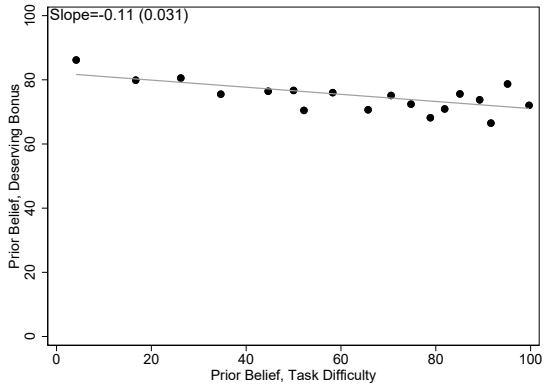
Notes: Binned scatterplots of the relationship between Task Performance and Prior Belief, Deserving Bonus, which is elicited before the revelation of the bonus payment. The left panel shows the distribution for the *Hard Task* and the right panel for the *Easy Task*. Task performances in the two tasks are rescaled to have a common scale (ranging from 0-100) by taking the ratio of the difference between the actual score and the minimum score and the difference between the maximum and the minimum score multiplied by the upper limit of the rescaled variable (100).

Figure A3: Relationship between Priors about Task Difficulty and Deservingness

(a) Hard Task

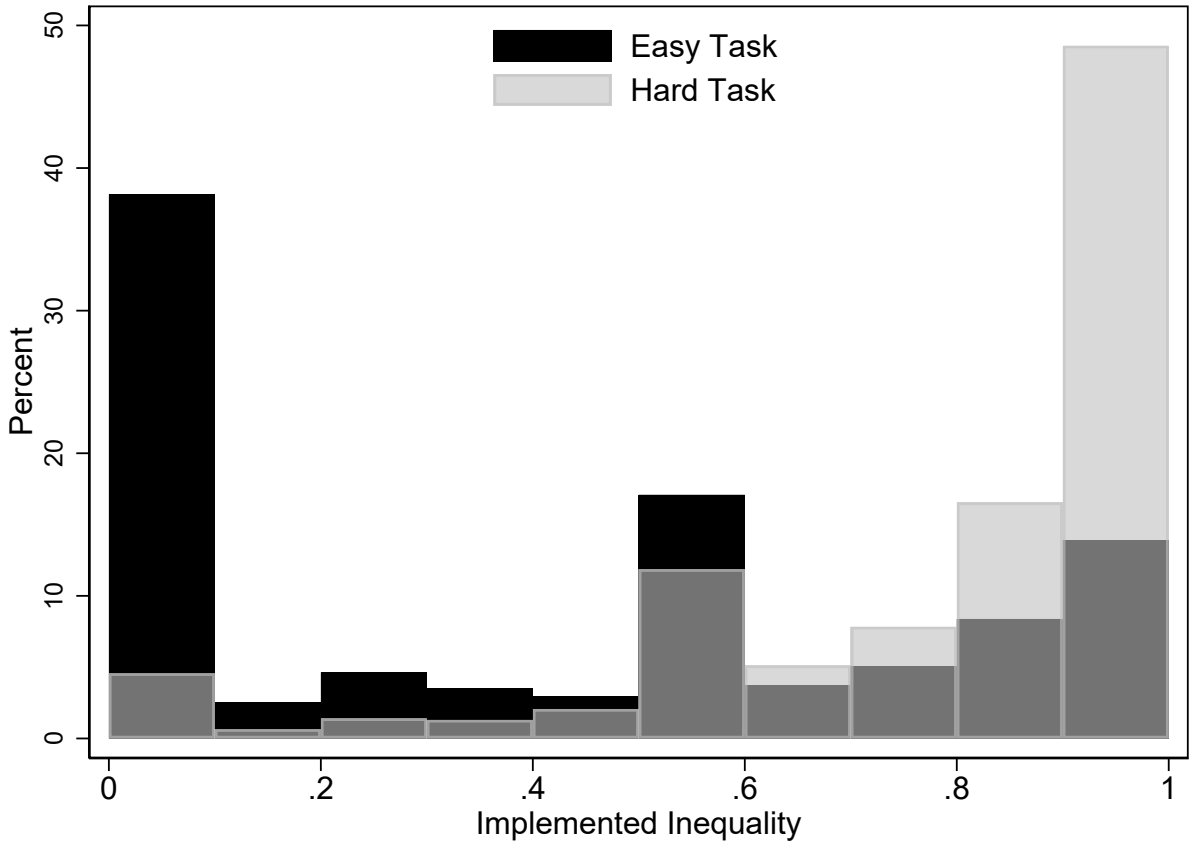


(b) Easy Task



Notes: Binned scatterplots of the relationship between Prior belief, Task Difficulty and Prior Belief, Deserving Bonus. Both beliefs are elicited before the revelation of the bonus payment. The left panel shows the distribution for the *Hard Task* and the right panel for the *Easy Task*.

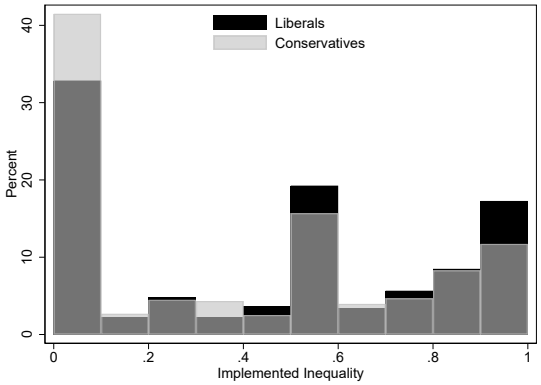
Figure A4: Distribution of Inequality Acceptance in the Hard and Easy Task



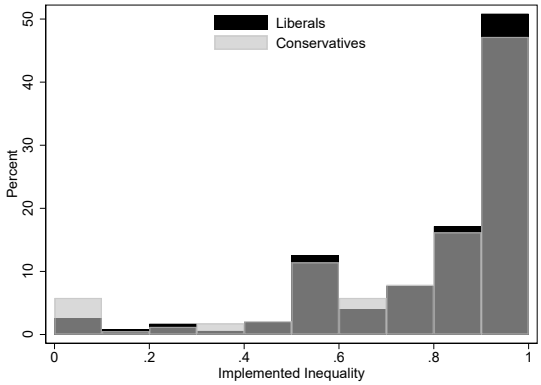
Notes: Histograms showing the distribution of inequality acceptance separated by *Easy Task* and *Hard task* (N=1,825).

Figure A5: Distribution of Inequality Acceptance by Political Orientation

(a) Hard Task



(b) Easy Task



Notes: Histograms showing the distribution of inequality acceptance separated by treatment and political orientation. The left panel shows the distribution for the *Hard Task* and the right panel for the *Easy Task* (N=1,825).

A.7 Additional Tables

Table A1: Regression: Dropout on Easy Task

	Dropout	
	(1)	(2)
Easy Task	-0.015	-0.018
	(0.013)	(0.012)
Constant	0.097***	0.007
	(0.009)	(0.009)
Observations	2026	1993
Controls	No	No
R-squared	0.001	0.001

Notes: OLS-regressions with robust standard errors in parentheses. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. Column 1 considers all participants who start with the work assignment and column 2 considers all participants who remained after learning about the bonus assignment.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Balance between No-Dropouts and Dropouts

Variable	(1) Study sample Mean/SD	(2) Dropouts Mean/SD	T-test P-value (1)-(2)
L-o-C-Index	4.042	4.016	0.548
	(0.581)	(0.686)	
Age	39.167	37.653	0.098*
	(12.406)	(11.661)	
Female (in %)	52.438	45.050	0.046**
	(49.954)	(49.878)	
White (in %)	76.658	73.267	0.282
	(42.313)	(44.366)	
Married (in %)	45.205	41.584	0.326
	(49.783)	(49.409)	
People in Household	2.660	2.698	0.720
	(1.425)	(1.372)	
Full-Time Employed (in %)	61.370	67.822	0.073*
	(48.703)	(46.832)	
Part-Time Employed (in %)	11.342	11.386	0.985
	(31.720)	(31.843)	
Self-Employed (in %)	11.123	8.911	0.339
	(31.451)	(28.561)	
Not-in-Labor-Force (in %)	9.753	5.941	0.078*
	(29.677)	(23.697)	
Income (in $)	64784.932	62202.970	0.412
	(42589.057)	(40993.245)	
Strongly Liberal (in %)	18.137	15.842	0.420
	(38.543)	(36.604)	
Moderately Liberal (in %)	22.301	24.752	0.429
	(41.638)	(43.265)	
Slightly Liberal (in %)	21.041	21.287	0.935
	(40.771)	(41.035)	
Slightly Conservative (in %)	20.274	19.307	0.745
	(40.215)	(39.569)	
Moderately Conservative (in %)	12.658	13.861	0.627
	(33.259)	(34.640)	
Strongly Conservative (in %)	5.589	4.950	0.706
	(22.977)	(21.746)	
Democrats (in %)	52.877	54.455	0.670
	(49.931)	(49.925)	
Republicans (in %)	28.274	25.743	0.447
	(45.045)	(43.830)	
No/ Other Political Party (in %)	18.849	19.802	0.743
	(39.121)	(39.950)	
Northeast Region (in %)	19.045	21.782	0.350
	(39.244)	(41.379)	
South Region (in %)	38.364	37.624	0.837
	(48.601)	(48.564)	
Midwest Region (in %)	20.746	18.812	0.519
	(40.527)	(39.178)	
West Region (in %)	21.844	21.782	0.984
	(41.296)	(41.379)	
Only High school Degree (in %)	8.986	7.426	0.459
	(28.606)	(26.284)	
Only Some College (in %)	24.274	21.287	0.346
	(42.886)	(41.035)	
2-Year College Degree (in %)	12.219	12.376	0.948
	(32.760)	(33.013)	
4-Year College Degree (in %)	38.356	45.050	0.064*
	(48.639)	(49.878)	
Master Degree (in %)	12.219	11.386	0.731
	(32.760)	(31.843)	
Doc/Professional Degree (in %)	3.671	1.980	0.215
	(18.811)	(13.967)	
N	1825	202	
F-test of joint significance (F-statistic)			0.728
F-test, number of observations			2027

Notes: The table shows average (std. dev.) of covariates by dropout status (columns 1–2). Column 3 shows p-values from t-test for differences between *Easy Task* and *Hard Task*. F-statistic is from a test of joint significance of all covariates.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Summary Statistics and Balance between Easy and Hard task

Variable	(1) Hard task Mean/SD	(2) Easy task Mean/SD	T-test P-value (1)-(2)
L-o-C-Index	4.064	4.020	0.111
	(0.596)	(0.565)	
Age	39.295	39.040	0.661
	(12.403)	(12.415)	
Female (in %)	53.142	51.743	0.550
	(49.929)	(49.997)	
White (in %)	75.744	77.560	0.360
	(42.887)	(41.741)	
Married (in %)	46.968	43.464	0.133
	(49.936)	(49.598)	
People in Household	2.627	2.693	0.326
	(1.367)	(1.479)	
Full-Time Employed (in %)	61.632	61.111	0.819
	(48.655)	(48.776)	
Part-Time Employed (in %)	10.805	11.874	0.472
	(31.061)	(32.365)	
Self-Employed (in %)	12.569	9.695	0.051*
	(33.168)	(29.605)	
Not-in-Labor-Force (in %)	9.592	9.913	0.817
	(29.464)	(29.900)	
Income (in $)	64812.569	64757.625	0.978
	(41972.517)	(43212.434)	
Strongly Liberal (in %)	17.641	18.627	0.585
	(38.138)	(38.954)	
Moderately Liberal (in %)	21.499	23.094	0.414
	(41.105)	(42.166)	
Slightly Liberal (in %)	21.940	20.153	0.349
	(41.407)	(40.136)	
Slightly Conservative (in %)	19.956	20.588	0.737
	(39.989)	(40.457)	
Moderately Conservative (in %)	12.900	12.418	0.757
	(33.538)	(32.997)	
Strongly Conservative (in %)	6.064	5.120	0.380
	(23.880)	(22.052)	
Democrats (in %)	52.701	53.050	0.881
	(49.955)	(49.934)	
Republicans (in %)	28.335	28.214	0.954
	(45.087)	(45.028)	
No/ Other Political Party (in %)	18.964	18.736	0.901
	(39.223)	(39.042)	
Northeast Region (in %)	20.418	17.689	0.137
	(40.311)	(38.134)	
South Region (in %)	38.080	38.646	0.804
	(48.558)	(48.667)	
Midwest Region (in %)	20.199	21.287	0.567
	(40.148)	(40.912)	
West Region (in %)	21.303	22.379	0.578
	(40.944)	(41.656)	
Only High school Degree (in %)	9.592	8.388	0.369
	(29.464)	(27.736)	
Only Some College (in %)	23.705	24.837	0.573
	(42.550)	(43.230)	
2-Year College Degree (in %)	12.900	11.547	0.378
	(33.538)	(31.976)	
4-Year College Degree (in %)	37.376	39.325	0.392
	(48.407)	(48.874)	
Master Degree (in %)	12.238	12.200	0.980
	(32.791)	(32.747)	
Doc/Professional Degree (in %)	4.190	3.159	0.242
	(20.046)	(17.500)	
N	907	918	
F-test of joint significance (F-stat)			1.149
F-test, number of observations			1825

Notes: The table shows average (std. dev.) of covariates by treatment status (columns 1–2). Column 3 shows p-values from t-test for differences between *Easy Task* and *Hard Task*. F-statistic is from a test of joint significance of all covariates.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Comparison of Demographics of Study Sample and U.S. Population

Variable	**Study Sample**	**U.S. Population**
Median Age (in years)	36.0	38.2
Female (in %)	52.4	50.8
White (in %)	76.7	60.4
Married (in %)	45.21	49.78
People in Household	2.66	2.52
Median Household Income (in $)	62,500	61,937
Bachelor's degree or higher (in %)	68.7	32.6
Northeast Region (in %)	19.0	17.1
Midwest Region (in %)	20.8	20.8
West Region (in %)	21.8	23.9
South Region (in %)	38.4	38.4

Notes: Data on U.S. Population comes from the U.S. Census Bureau (https://data.census.gov/cedsci/): Data on median age, white, married, household income, education is from 2018 and data on gender, people in household and region is from 2019.

Table A5: Regression: Change in Beliefs about Task Assignment (Posterior – Prior)

	Δ-Task Difficulty		Δ-Relative Performance		Δ-Deserving	
	(1)	(2)	(3)	(4)	(5)	(6)
Easy Task	-5.672***	-5.668***	10.324***	10.252***	10.943***	10.768***
	(0.934)	(0.936)	(0.713)	(0.717)	(0.882)	(0.882)
Constant	3.538***	-5.104	-4.223***	-1.409	-5.811***	-8.638
	(0.683)	(7.514)	(0.530)	(5.833)	(0.710)	(6.995)
Observations	1,825	1,822	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes	No	Yes
R-squared	0.02	0.02	0.10	0.11	0.08	0.08

Notes: OLS-regressions with robust standard errors in parentheses. "Δ" is the difference between posterior and prior beliefs. Beliefs are elicited before the bonus assignment (prior) and after the bonus assignment (posterior). All beliefs are measured on a scale from 0 – 100: "Prior Belief, Task Difficulty:" likelihood of performing in the *Hard Task* in %; "Prior Belief, Relative Performance:" perceived number of participants performing the same task with a lower score; "Prior Belief, Deserving Bonus:" deserving the $2 bonus payment in %. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. Controls include sex, age, household size, log income and a set of indicator variables for white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Regression: Misperception about Task Difficulty and Revising Inequality Acceptance

	Change Inequality				Δ-Inequality			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	All	Lib.	Cons.	All	All	Lib.	Cons.
Misperception*Received Info	0.001	0.001	0.001	0.001	0.036**	0.034**	0.043**	0.030
	(0.001)	(0.001)	(0.001)	(0.001)	(0.016)	(0.016)	(0.017)	(0.034)
Misperception	-0.000	0.001	0.001	0.000	-0.043***	-0.025	-0.029	-0.029
	(0.000)	(0.001)	(0.001)	(0.001)	(0.014)	(0.017)	(0.019)	(0.030)
Easy Task		-0.087***	-0.071*	-0.098*		-2.393**	-0.706	-4.204**
		(0.033)	(0.043)	(0.054)		(1.099)	(1.203)	(2.053)
WTP	0.344**	0.343**	0.547***	0.082	6.735	6.708	7.511	5.438
	(0.136)	(0.134)	(0.185)	(0.200)	(4.530)	(4.483)	(5.090)	(7.870)
Constant	0.610***	0.601***	0.340	0.844**	7.294	7.056	-2.720	19.628
	(0.221)	(0.221)	(0.278)	(0.378)	(6.986)	(6.965)	(7.461)	(13.776)
Observations	1128	1128	694	434	1128	1128	694	434
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.041	0.047	0.059	0.066	0.031	0.036	0.042	0.055

Notes: OLS-regressions with robust standard errors in parentheses. Sample consists of all participants who received information about the task difficulty and a random subset of participants who did not receive this information. "Change Inequality" is an indicator for revising the initially implemented inequality and "Δ-Inequality" is the absolute difference between the initially implemented and the revised inequality acceptance. "Misperception" indicates the difference between the actual task difficulty and the posterior belief about task difficulty in percentage points. "Received Info" is an indicator for participants who received information about the task difficulty and the performance of the other participant. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. "WTP" (in cents) is the willingness to pay for receiving information about the task difficulty and the score of the other participant, categorized in 9 intervals $[0,1]$; $[1,3]$; $[3,5]$; $[5,7]$; $[7,10]$; $[10,20]$; $[20,35]$; $[35,50]$; $[50,\infty)$. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). Columns labeled "All" uses all data and columns labeled "Lib." ("Cons.") restricts the sample to liberals (conservatives).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Regression: Meritocratic Beliefs (Treatment on the Treated)

	Effort Determines Success		
	(1)	(2)	(3)
Economic Success	18.231***	18.340***	18.417***
	(1.509)	(2.369)	(2.364)
Liberal*Economic Success		-0.120	0.285
		(3.067)	(3.054)
Liberal		-3.918*	-3.965*
		(2.294)	(2.315)
Constant	53.009***	55.400***	40.586***
	(1.127)	(1.774)	(10.375)
Observations	1,825	1,825	1,822
Controls	No	No	Yes
R-squared	0.09	0.09	0.11

Notes: 2SLS-Regression with robust standard errors in parentheses. "Economic Success" instrumented with treatment assignment. Meritocratic Beliefs ("Effort Determines Success:") are elicited after the bonus assignment (posterior) and are measured on a scale from 0 – 100: likelihood that the $2 bonus payment depends on exerted effort in %. "Economic Success" is an indicator for the bonus payment. "Liberal" is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and a set of indicator variables for white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$;

Table A8: Regression: Inequality Acceptance and Political Views (Treatment on the Treated)

	Inequality Acceptance					
	(1)	(2)	(3)	(4)	(5)	(6)
Economic Success	0.445***	0.444***	0.445***	0.445***	0.414***	0.411***
	(0.017)	(0.017)	(0.016)	(0.017)	(0.026)	(0.026)
Liberal			-0.055***	-0.054***	-0.081***	-0.082***
			(0.015)	(0.015)	(0.026)	(0.026)
Liberal*Economic Success					0.050	0.055
					(0.033)	(0.034)
Constant	0.373***	0.290**	0.407***	0.338***	0.422***	0.350***
	(0.013)	(0.115)	(0.016)	(0.115)	(0.020)	(0.115)
Observations	1825	1822	1825	1822	1825	1822
Controls	No	Yes	No	Yes	No	Yes
R-squared	0.318	0.323	0.323	0.328	0.323	0.328

Notes: 2SLS-Regression with robust standard errors in parentheses. "Economic Success" instrumented with treatment assignment. "Inequality Acceptance" is the implemented inequality in a group, measured on a scale from 0 to 1. "Economic Success" is an indicator for the bonus payment. "Liberal" is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Regression: Prior Beliefs and Inequality Acceptance (Treatment on the Treated)

	Inequality Acceptance					
	(1)	(2)	(3)	(4)	(5)	(6)
Economic Success	0.445***	0.444***	0.281***	0.171***	0.077	-0.093
	(0.017)	(0.017)	(0.045)	(0.043)	(0.052)	(0.066)
Prior Belief, Task Difficulty			-0.002***			-0.002***
			(0.000)			(0.000)
Economic Success*Prior Belief, Task Difficulty			0.002***			0.002***
			(0.001)			(0.001)
Prior Belief, Relative Performance				-0.003***		-0.002***
				(0.001)		(0.001)
Economic Success*Prior Belief, Relative Performance				0.005***		0.003***
				(0.001)		(0.001)
Prior Belief, Deserving Bonus					-0.003***	-0.001***
					(0.000)	(0.001)
Economic Success*Prior Belief, Deserving Bonus					0.005***	0.003***
					(0.001)	(0.001)
Constant	0.373***	0.290**	0.429***	0.470***	0.510***	0.615***
	(0.013)	(0.115)	(0.116)	(0.115)	(0.115)	(0.116)
Observations	1825	1822	1822	1822	1822	1822
Controls	No	Yes	Yes	Yes	Yes	Yes
R-squared	0.318	0.323	0.334	0.355	0.357	0.371

Notes: 2SLS-Regression with robust standard errors in parentheses. "Economic Success" instrumented with treatment assignment. "Inequality Acceptance" is the implemented inequality in a group, measured on a scale from 0 to 1. "Economic Success" is an indicator for the bonus payment. Prior beliefs elicited before the bonus assignment and measured on a scale from 0 – 100: "Prior Belief, Task Difficulty:" likelihood of performing in the easy/hard task in %; "Prior Belief, Relative Performance:" relative performance rank among participants performing the same task; "Prior Belief, Deserving Bonus:" deserving the $2-bonus payment in %. Controls include sex, age, household size, log income and a set of indicator variables for white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Midwest, West). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$;

Table A10: Regression: Locus of Control and Political Views

	Locus of Control (LoC)	
	(1)	(2)
Liberal	1.308***	1.084**
	(0.438)	(0.443)
Constant	20.145***	49.977***
	(0.341)	(3.370)
Observations	1,825	1,822
Controls	No	Yes
R-squared	0.00	0.07

Notes: OLS-regressions with robust standard errors in parentheses. "Locus of Control" is the degree to which one feels to have control over one's life outcomes and ranges between 7 (full control over life or internal LoC) and 49 (no control over life or external LoC). "Liberal" is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Mid-west, West).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Regression: Meritocratic Beliefs and Locus of Control

	Effort Determines Success		
	(1)	(2)	(3)
Easy Task	16.213***	16.866***	16.681***
	(1.355)	(3.489)	(3.492)
LoC		-0.289**	-0.260**
		(0.121)	(0.123)
LoC**Easy Task*		-0.037	-0.015
		(0.154)	(0.155)
Constant	54.054***	60.166***	50.286***
	(1.072)	(2.799)	(11.016)
Observations	1,825	1,825	1,822
Controls	No	No	Yes
R-squared	0.07	0.08	0.09

Notes: OLS-regressions with robust standard errors in parentheses. Meritocratic Beliefs ("Effort Determines Success:") are elicited after the bonus assignment (posterior) and are measured on a scale from 0 – 100: likelihood that the $2 bonus payment depends on exerted effort in %. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. "LoC" is the degree to which one feels to have control over one's life outcomes and ranges between 7 (full control over life or internal LoC) and 49 (no control over life or external LoC). Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Mid-west, West).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

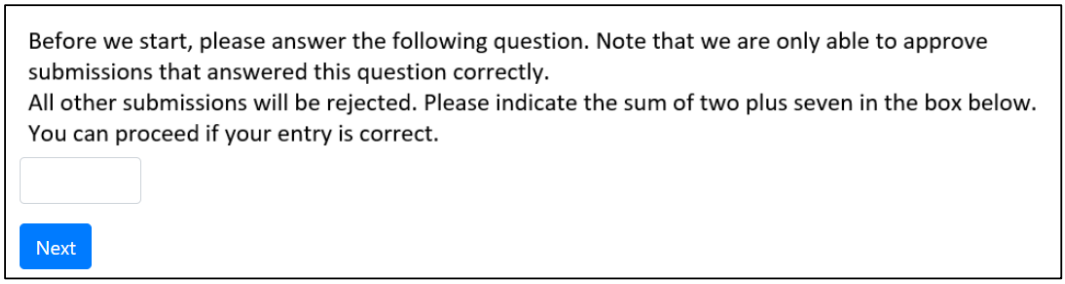
Table A12: Regression: Inequality Acceptance and Locus of Control

	Inequality Acceptance					
	(1)	(2)	(3)	(4)	(5)	(6)
Easy Task	0.395***	0.394***	0.395***	0.394***	0.435***	0.433***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.039)	(0.039)
LoC			-0.001	-0.001	0.000	0.000
			(0.001)	(0.001)	(0.001)	(0.001)
*Easy Task**LoC					-0.002	-0.002
					(0.002)	(0.002)
Constant	0.398***	0.279**	0.411***	0.310**	0.391***	0.297**
	(0.012)	(0.114)	(0.022)	(0.122)	(0.032)	(0.123)
Observations	1825	1822	1825	1822	1825	1822
Controls	No	Yes	No	Yes	No	Yes
R-squared	0.272	0.277	0.272	0.277	0.273	0.277

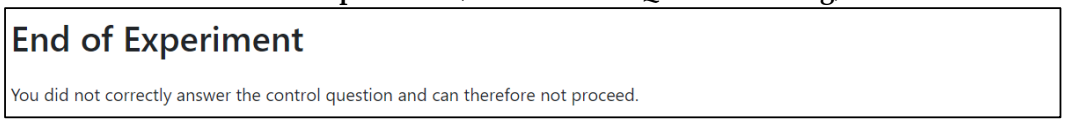
Notes: OLS-regressions with robust standard errors in parentheses. "Inequality Acceptance" is the implemented inequality in a group, measured on a scale from 0 to 1. "*Easy Task*" is an indicator for participants randomly assigned to the *Easy Task*. "LoC" is the degree to which one feels to have control over one's life outcomes and ranges between 7 (full control over life or internal LoC) and 49 (no control over life or external LoC). Controls include sex, age, household size, log income and dummy variables indicating white/European-American ethnicity, college degree, working, married and U.S.-regions (North, East, South, Mid-west, West).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.8 Screenshots of the Experiment

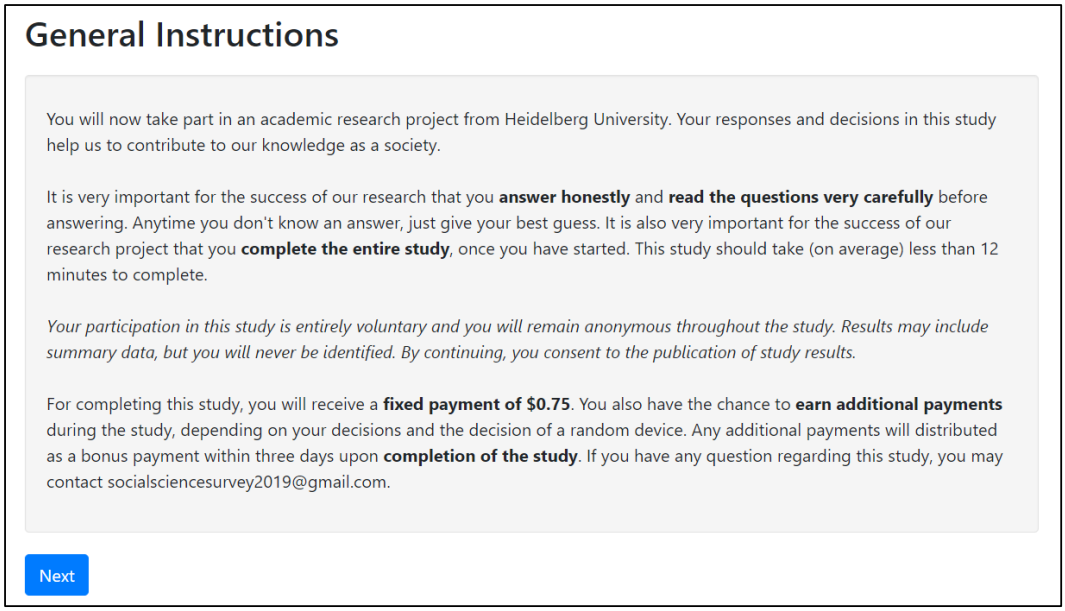
Bot Control-Question



End of Experiment (if Bot Control-Question wrong)



General Instructions



Locus-of-Control Questionnaire

Questionnaire

The following statements apply to different attitudes towards life and the future. To what degree do you personally agree with the following statements.

	Disagree strongly	Disagree moderately	Disagree a little	Neither agree nor Disagree	Agree a little	Agree moderately	Agree strongly
I have little control over the things that happen to me.	○	○	○	○	○	○	○
There is really no way I can solve some of the problems I have.	○	○	○	○	○	○	○
There is little I can do to change many of the important things in my life.	○	○	○	○	○	○	○
I often feel helpless in dealing with the problems of life.	○	○	○	○	○	○	○
Sometimes I feel that I'm being pushed around in life.	○	○	○	○	○	○	○
What happens to me in the future mostly depends on me.	○	○	○	○	○	○	○
I can do just about anything I really set my mind to do.	○	○	○	○	○	○	○

Next

Demographic Questionnaire

Questionnaire

Please select your gender.
- Male
- Female
- Other
- I prefer not to say.

Please enter your age.

Please indicate your marital status.
- Single
- Married

How many persons live in your household (including you)?

What is the highest level of education you have completed?
- Not completed high school
- High school
- Some college
- 2-year college degree
- 4-year college degree
- Masters degree
- Doctoral degree
- Professional degree (JD, MD)

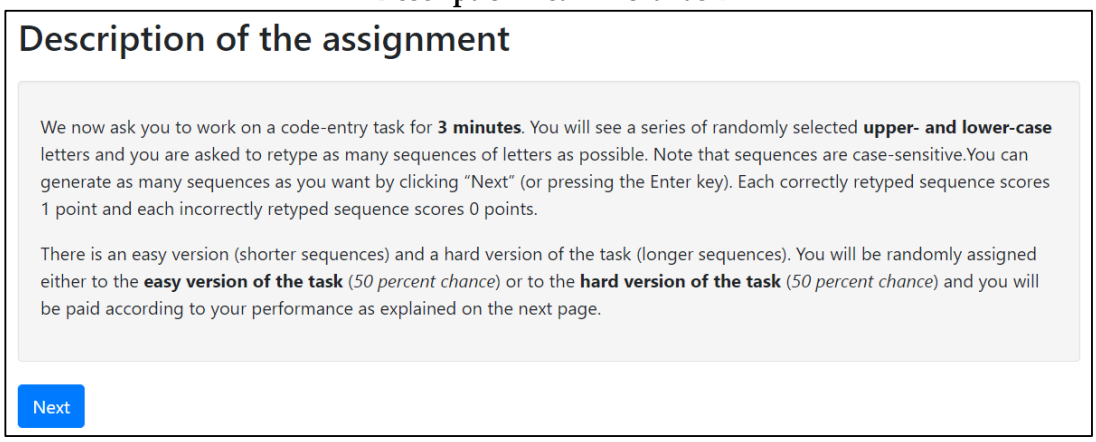
What is your current employment status?
- Full-time employee
- Part-time employee
- Self-employed or small business owner
- Unemployed and looking for work
- Student
- Not in labor force (for example: retired, full-time parent)

What was your TOTAL household income, before taxes, last year (2018)?
- $0 - $9,999
- $10,000 - $14,999
- $15,000 - $19,999
- $20,000 - $29,999
- $30,000 - $39,999
- $40,000 - $49,999
- $50,000 - $74,999
- $75,000 - $99,999
- $100,000 - $124,999
- $125,000 - $149,999
- $150,000 - $199,999
- $200,000 and more

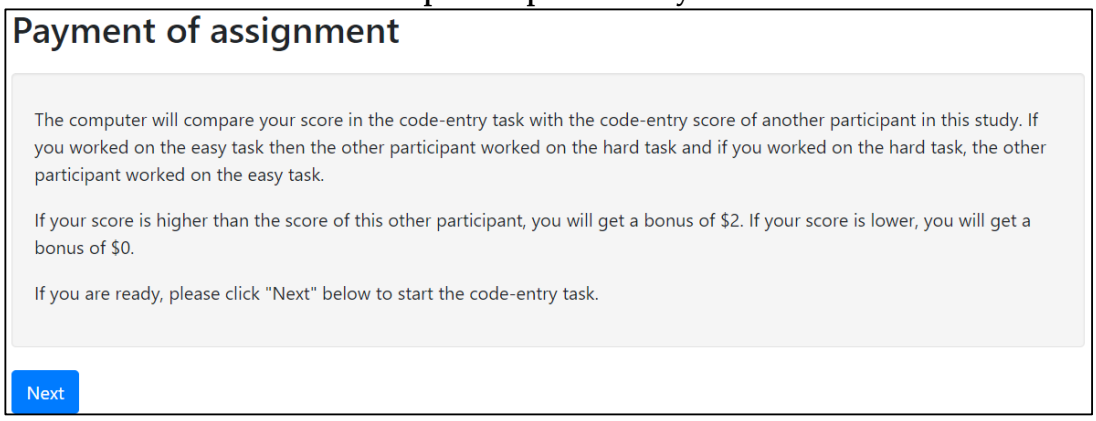
What is your ethnicity?	○ White/European-American ○ Black/African-American ○ Asian/Asian-American/Pacific Islander ○ Hispanic/Latino ○ Other
On a continuum from liberal to conservative, how would you describe your political beliefs?	○ Strongly liberal ○ Moderately liberal ○ Slightly liberal ○ Slightly conservative ○ Moderately conservative ○ Strongly conservative
Which of the following political parties do you identify with most?	○ Democratic Party ○ Republican Party ○ Other
Do you live in the United States?	○ Yes ○ No
In which state do you live?	---------

Next

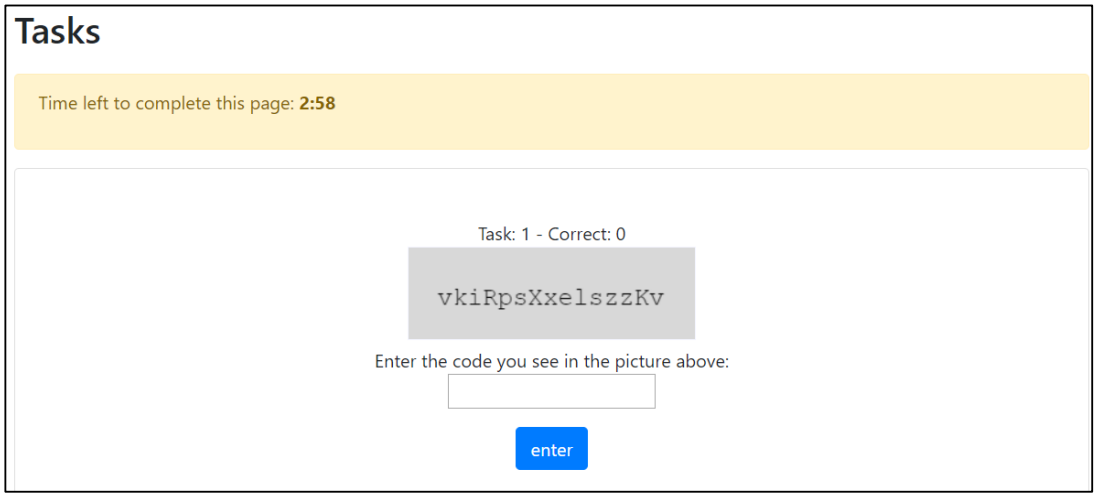
Description Real Effort Task



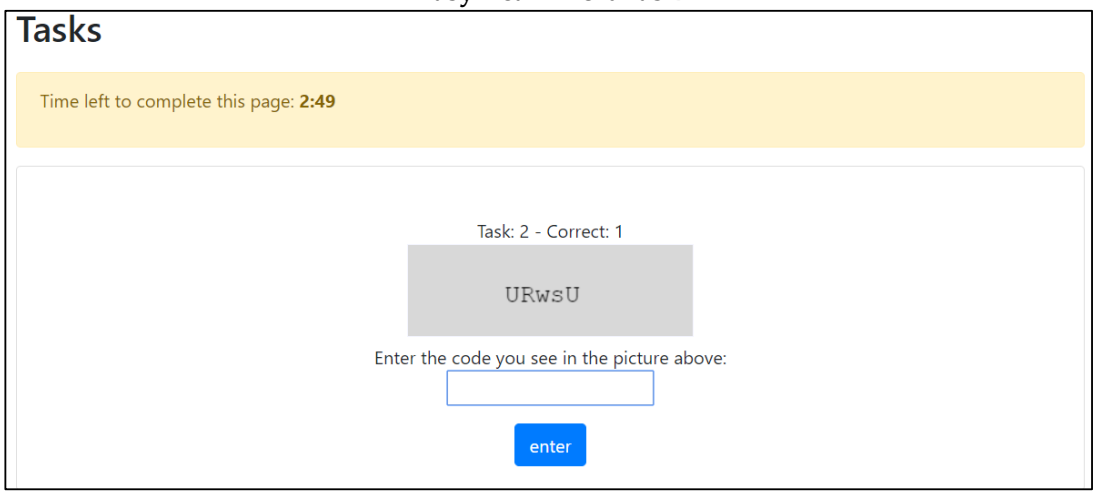
Description Experiment Payment



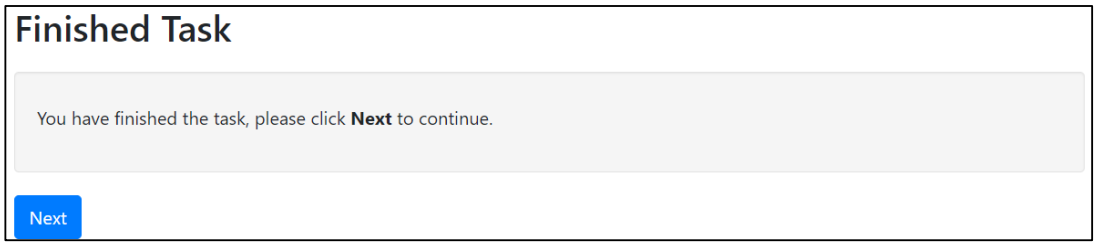
Hard Real Effort Task



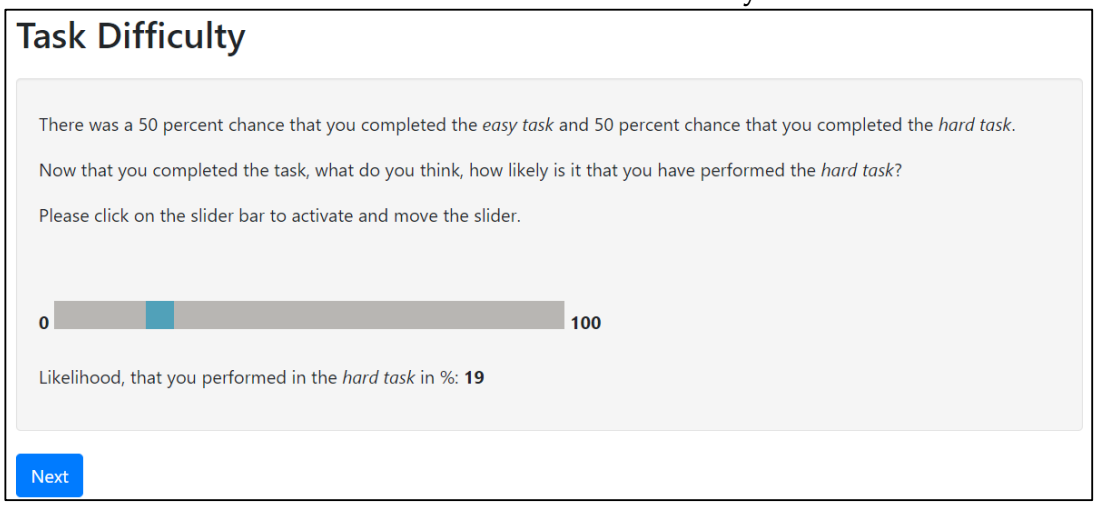
Easy Real Effort Task



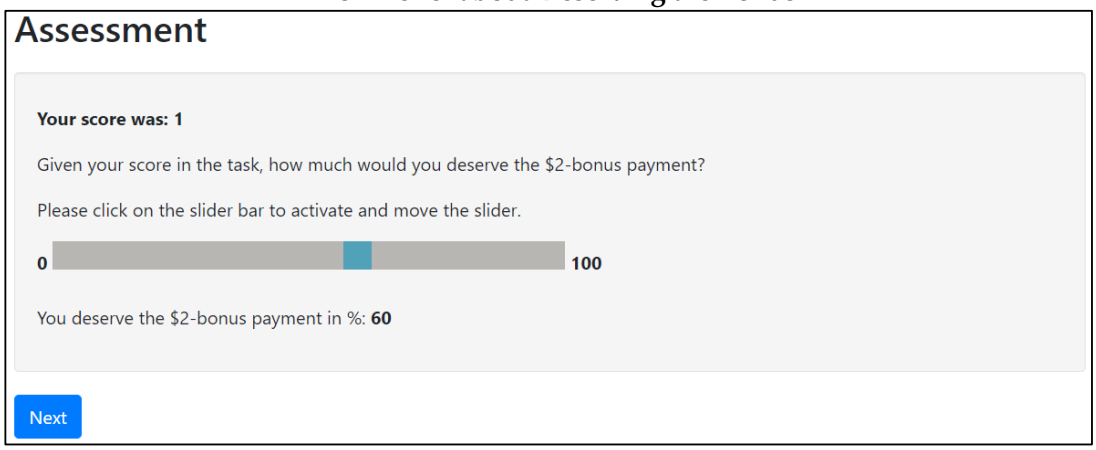
Information Real Effort Task Finished



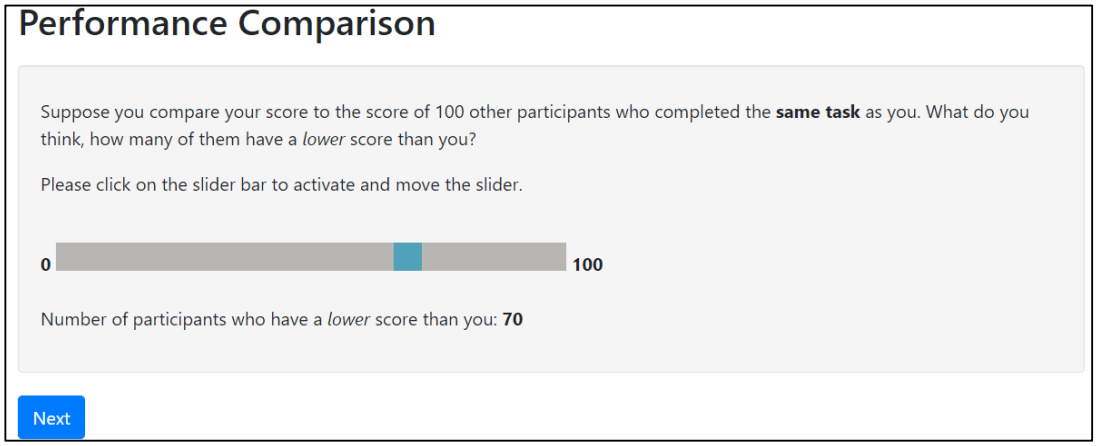
Prior-Belief about Task Difficulty



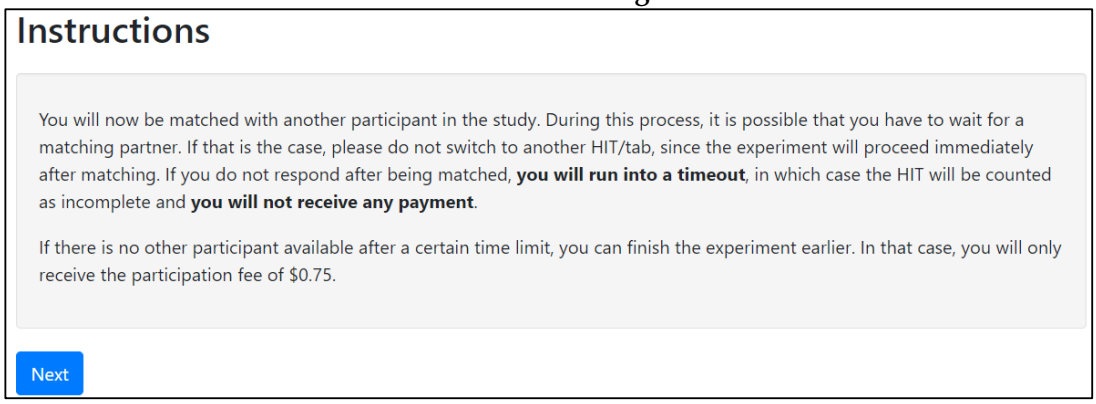
Prior- Belief about Deserving the Bonus



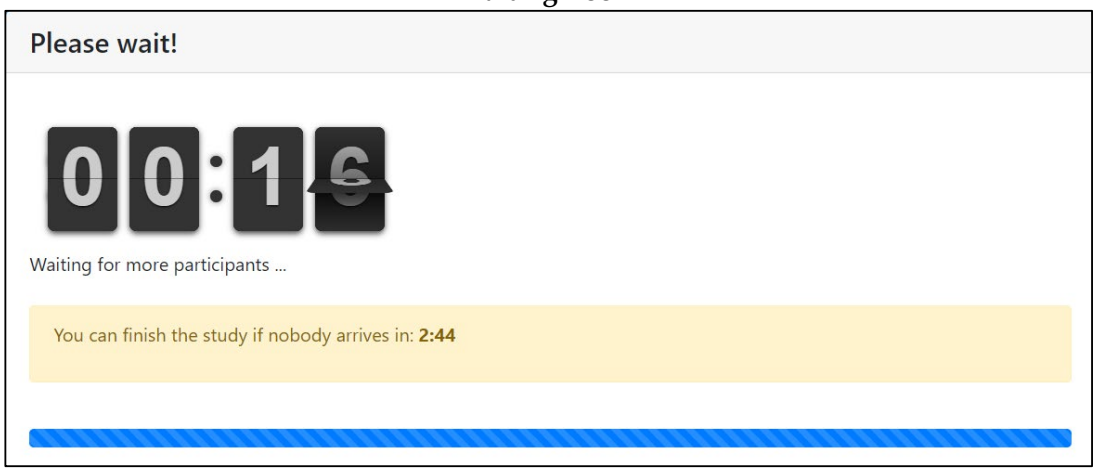
Prior-Belief about Relative Performance



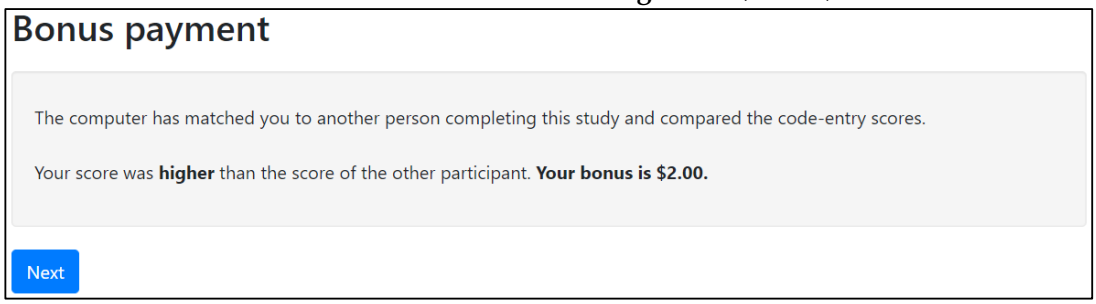
Instructions about Matching Mechanism



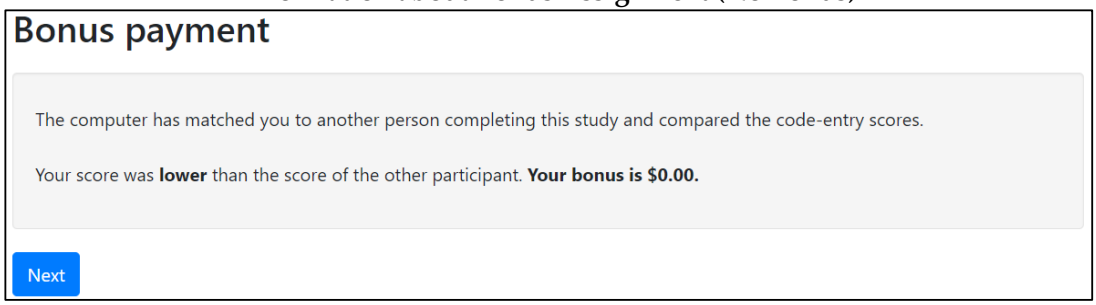
Waiting Room



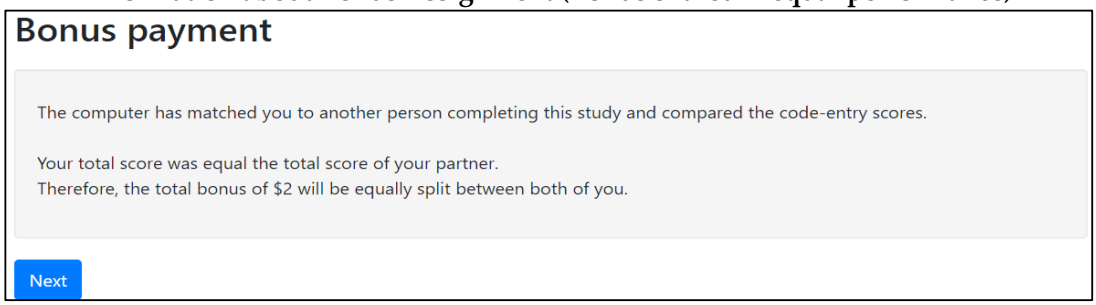
Information about Bonus Assignment (Bonus)



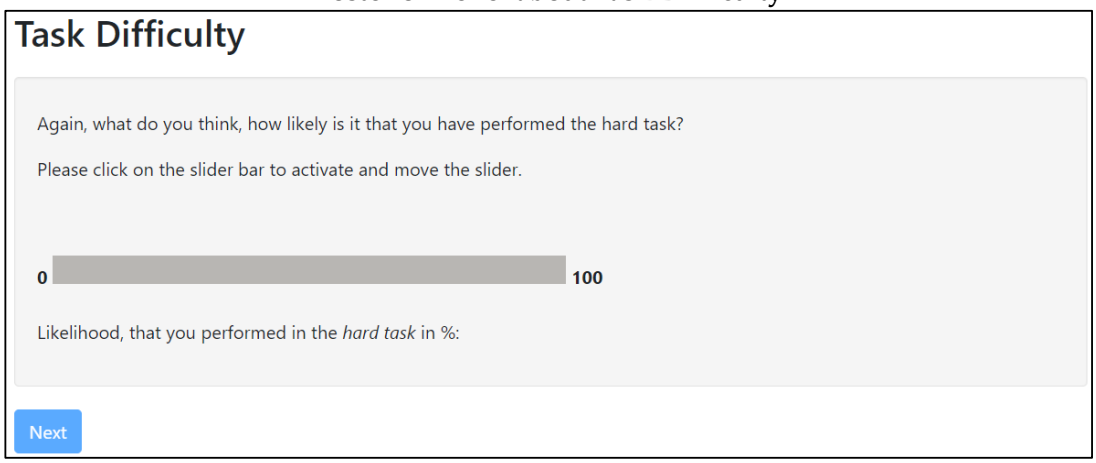
Information about Bonus Assignment (No Bonus)



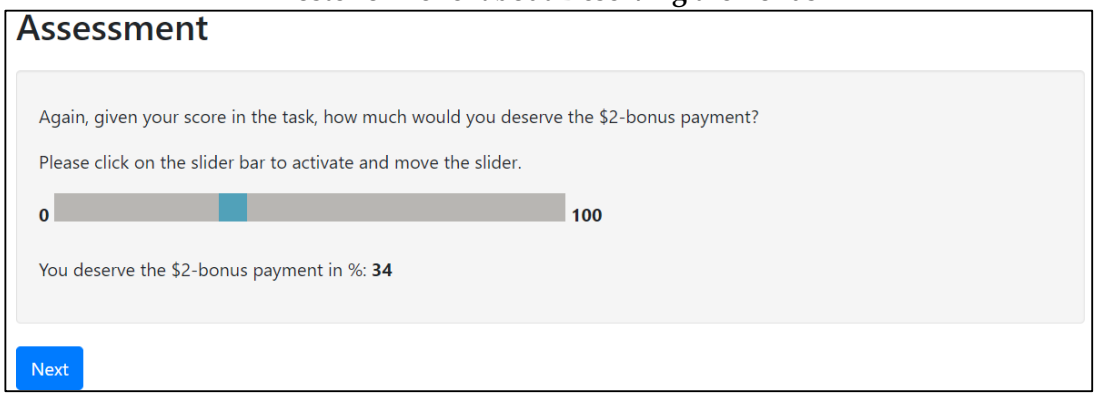
Information about Bonus Assignment (Bonus shared if equal performance)



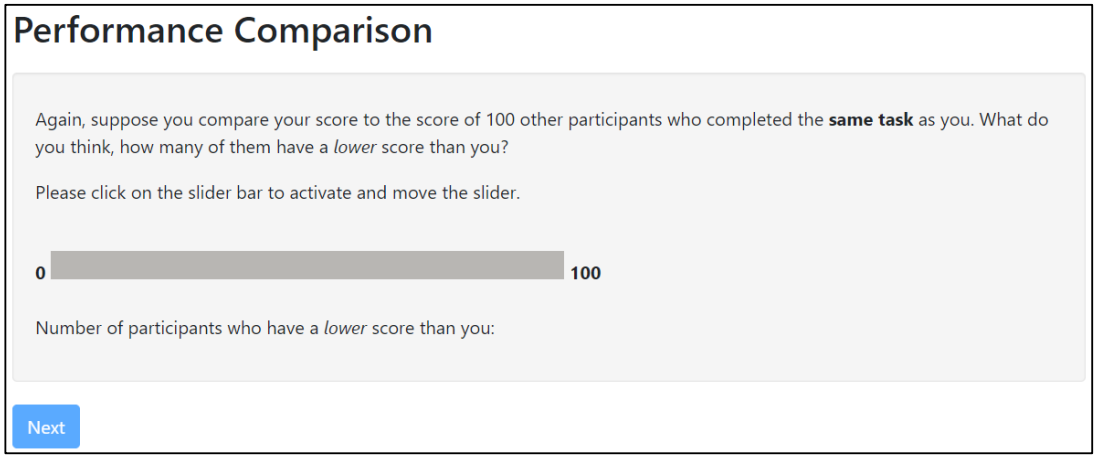
Posterior-Belief about Task Difficulty



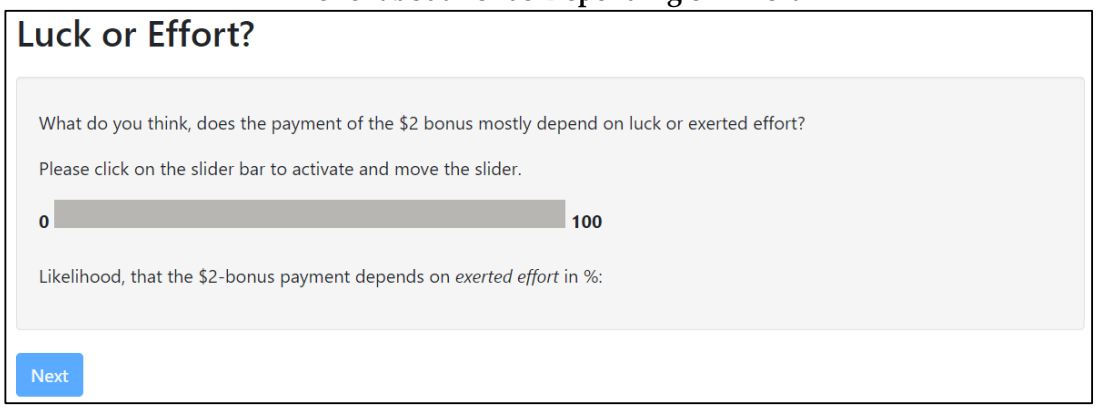
Posterior-Belief about Deserving the Bonus



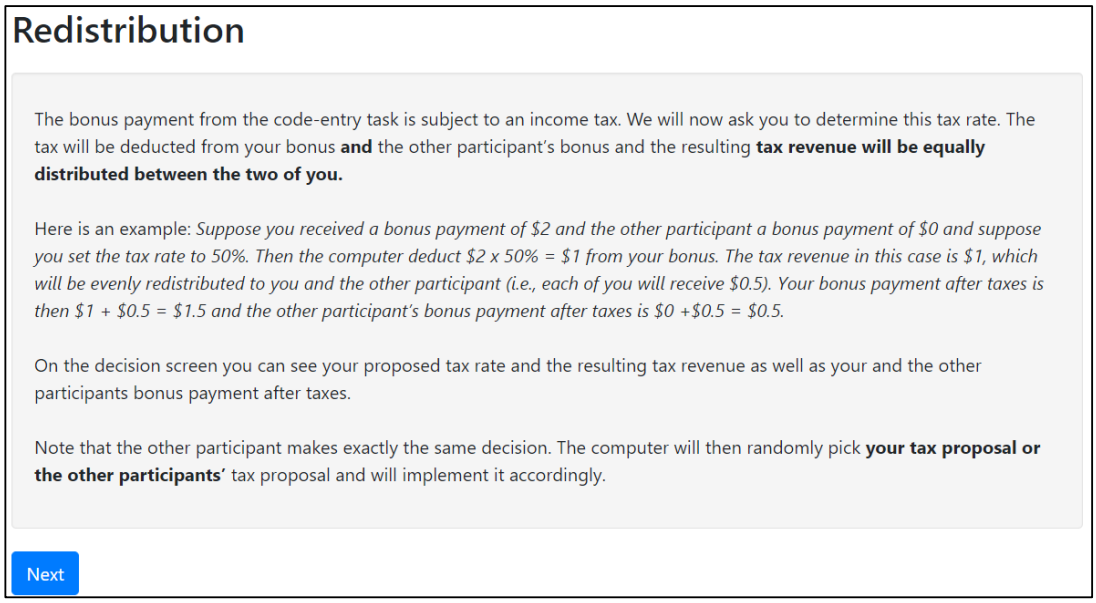
Posterior-Belief about Relative Performance



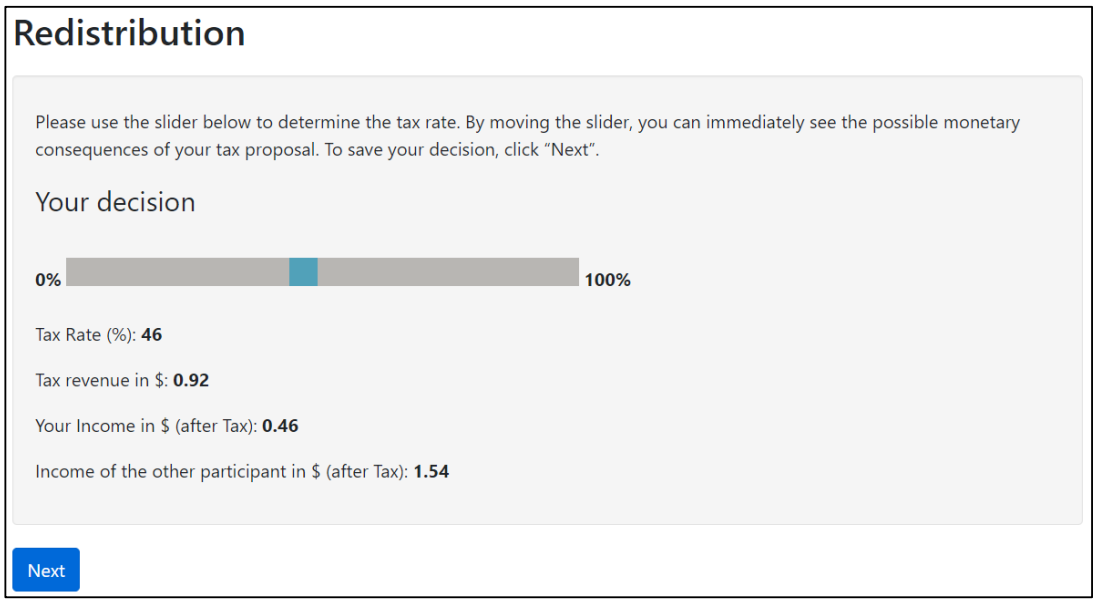
Belief about Bonus Depending on Effort



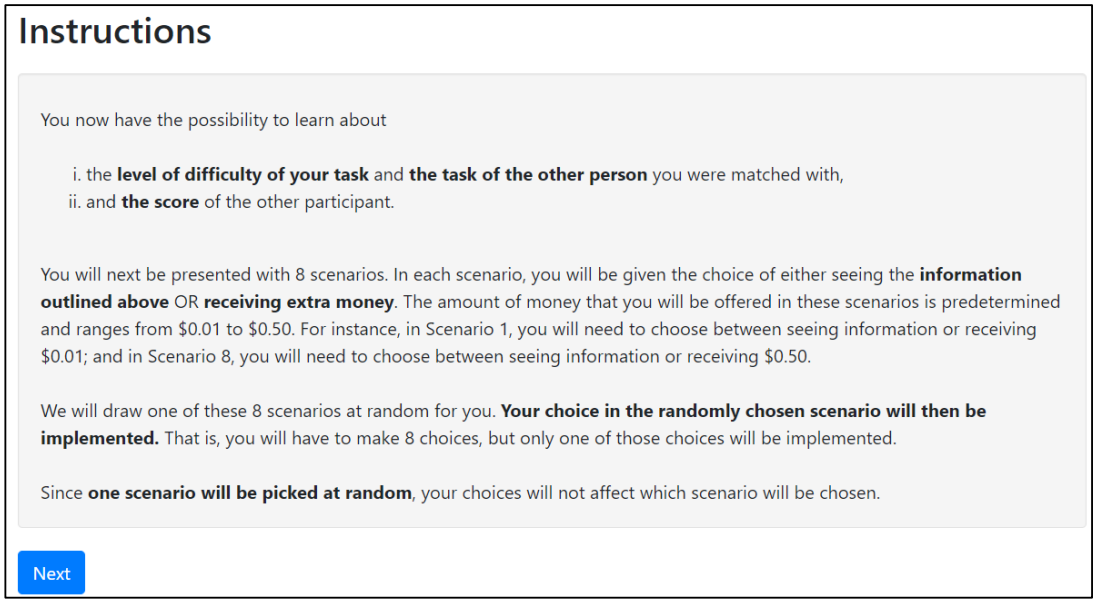
Information about Redistribution Mechanism



Redistribution First Time



Information about Price List to Receive additional Information about Partner



Price List to Receive additional Information about Partner

Scenarios

You will now be asked to make a decision for each of the **8 scenarios**.
Note: One of the 8 scenarios is randomly chosen for you, and your choice in this scenario will be implemented. If you choose the information, you will see it on the next page. Instead, if you choose the money, you will receive the money on top of your other earnings.

Scenario 1:
Would you like to see information about your relative performance OR receive $0.01?
(•) see Information ( ) receive $ 0.01

Scenario 2:
Would you like to see information about your relative performance OR receive $0.03?
(•) see Information ( ) receive $ 0.03

Scenario 3:
Would you like to see information about your relative performance OR receive $0.05?
(•) see Information ( ) receive $ 0.05

Scenario 4:
Would you like to see information about your relative performance OR receive $0.07?
(•) see Information ( ) receive $ 0.07

Scenario 5:
Would you like to see information about your relative performance OR receive $0.10?
( ) see Information (•) receive $ 0.10

Scenario 6:
Would you like to see information about your relative performance OR receive $0.20?
( ) see Information (•) receive $ 0.20

Scenario 7:
Would you like to see information about your relative performance OR receive $0.35?
( ) see Information (•) receive $ 0.35

Scenario 8:
Would you like to see information about your relative performance OR receive $0.50?
( ) see Information (•) receive $ 0.50

Next

Result of Price List Decisions (see Information)

Result

Time left to complete this page: **0:04**

Scenario 2 was picked at random for you.

You had chosen to receive information about the assignment.

i. you completed the **hard code-entry task** (i.e., retyping sequences of **15** upper- and lower-case letters) and the other person completed the **easy code-entry task** (i.e., retyping sequences of **5** upper- and lower-case letters),
ii. and the score of the other participant in the *easy code-entry task* was **2**. (Your score was: **0)**

Next

Result of Price List Decisions (receive Money)

Result

Scenario 5 was picked at random for you.

You had chosen to receive $0.10.

Next

Redistribution Second Time

Redistribution

You now have a non-recurring chance to revise your proposal.

Your earlier proposal was 83%.

If you do not want to revise your earlier proposal, move the slider to 83%, if you want to revise your earlier proposal move the slider to a different position.

Your decision

0% **100%**

Tax Rate (%): **67**

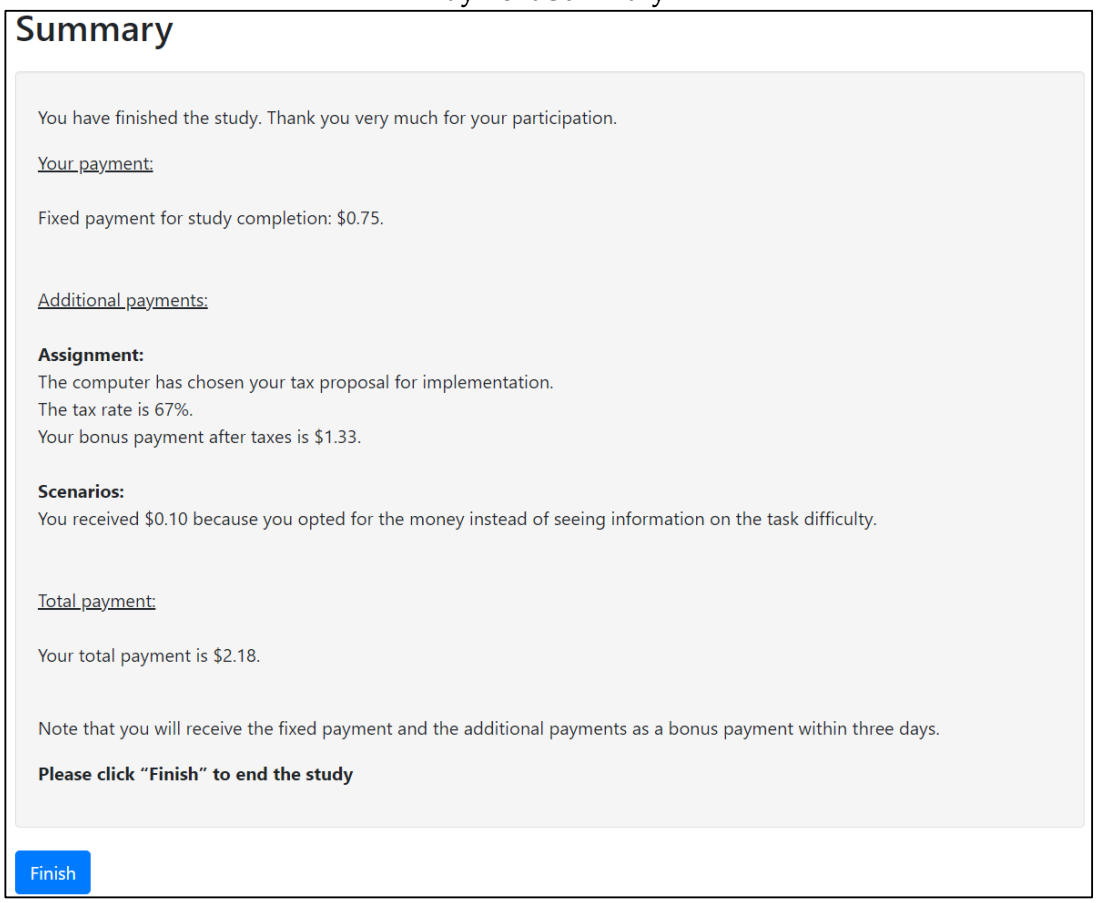
Tax revenue in $: **1.34**

Your Income in $ (after Tax): **1.33**

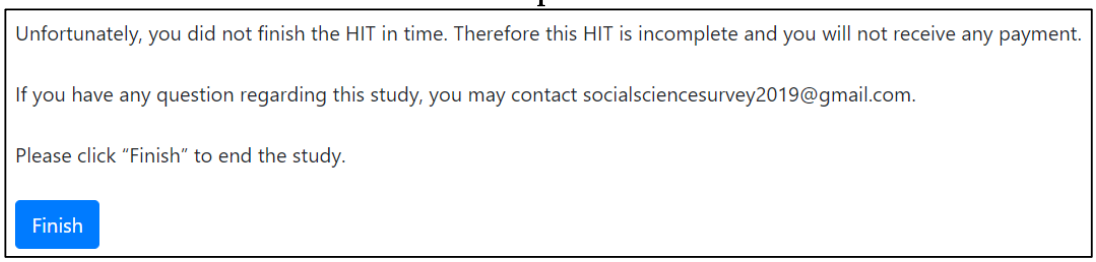
Income of the other participant in $ (after Tax): **0.67**

Next

Payment Summary



Information if Participants run into Timeout



A.9 Pre-Analysis Plan

Dietmar Fehr and Martin Vollmann

Heidelberg University

August 5, 2019

1. Introduction

Increasing levels of inequality around the world have gained a lot of attention from researchers and the general public. A growing literature highlights the importance of individuals' views about the sources of inequality for inequality acceptance. In particular, this literature suggests that people are willing to accept more inequality if it is the result of merit rather than the result of luck (Almås et al., 2019; Bartling et al., 2018; Cappelen et al., 2017; Durante et al., 2014). However, it is often difficult, if not impossible, to relate economic success or inequality to the relative impact of the luck or merit and people may (willingly) misperceive the relative importance of merit on their success.

We study distributional situations in which people exert effort in a real-effort task, but economic success or inequality is largely the result of luck. Our main research question is whether an individual's economic success shapes their acceptance of inequality. We are in particular interested in whether economic success affects how people think about the role of merit and whether it affects their attitudes towards taxation.

2. Research Strategy

We will run the study on Amazon Mechanical Turk (MTurk). Mturk offers a quite diverse population that appears more representative of the general population in the US as most other "convenience samples" (Berinsky et al., 2012; Paolacci et al., 2010). Our study is a combination

of survey and incentivized decision tasks, and consists of four parts: a socio-demographic questionnaire, a real effort task (RET), a redistribution task, and an information acquisition task.

Study design: We first introduce participants to the general details of the study and ask for their consent. Subsequently, we elicit some basic socio-demographic information and personality traits (locus of control). A complete list of all variables can be found in section 3.2.

In the second part, participants work on a real effort task for 3 minutes. The task consists of retyping a series of randomly generated sequences of upper- and lower-case letters (see Figure 1 in the Appendix). Prior to this code-entry task, participants learn that there are two types of the task – an easy and a hard task – and that they will be randomly assigned to one of the two tasks. The easy task consists of sequences of five letters and the hard task consists of longer sequences with 15 letters. While participants know that the easy (hard) task involves shorter (longer) sequences, they do not know and learn the exact number of letters such that there is some uncertainty about the task assignment. We designed the two tasks with the intention to separate the scores in the two tasks as fully as possible. Consequently, due to the length of sequences, participants in the hard task will retype fewer sequences than participants assigned to the easy task, on average.

Participants are paid according to their performance. That is, we randomly match two participants working on the easy and hard task and compare their scores in the task. The participant with the higher score receives a bonus payment of $2 and the participant with the lower performance receives $0. Since we will always match participants working on different tasks, the random assignment to the two tasks basically determines the bonus payment, i.e., participants working on the easy task almost always receive the $2 bonus. Note that the matching protocol is public knowledge, i.e., that they are matched to another participant doing a different task (either the easy or hard task).

Before we reveal the outcome of the performance comparison (i.e., the bonus payment), we ask participants (1) to compare their performance to 100 other participants working on the same task, (2) to estimate the likelihood that they completed the hard task, and (3) how much they would deserve the $2-bonus payment. After revealing the bonus payment, we ask the same questions again. Additionally, we ask participants to assess to what extent the bonus payment depends on luck or effort.

In the third part, both participants in a matched pair have to decide about a redistributive tax scheme, where tax revenues are equally distributed within the pair. Using an interactive slider, participants can indicate a tax rate (0-100%) and immediately see how the tax rate will affect own and other income (see Figure 2 in the Appendix). We randomly select one of the two proposals and implement the choices within the pair.

In the fourth part, we offer participants the possibility to buy information on the task difficulty and the performance of the other participant they are matched with. We elicit their willingness to pay for this piece of information with a simple price list. In this price list, we present participants eight scenarios in which they have to decide between seeing the information or receiving extra money with amounts ranging from \$0.01 to \$0.50. For instance, in Scenario 1, they will need to choose between seeing information or receiving \$0.01, and in Scenario 8, they will need to choose between seeing information or receiving \$0.50 (see Figure 3 in the Appendix). To incentivize participants, we randomly pick one of the eight scenarios for each participant and implement their in this scenario. That is, a participant will either receive the information immediately after the price list or receive the extra money at the end of the survey. In a last step, all participants who have received the information and random subset (50%) of the participants that have not received the information have the opportunity to revise their tax rate. Note that we implement the revised tax rate if the first tax proposal was initially chosen for implementation. Finally, participants receive a detailed overview about the composition of their payoff.

Implementation: We use the open source software oTree (Chen et al., 2016) to program and run the study. We limit participation to Mturkers based in the US, with more than 1000 accepted HITs and an acceptance rate of 98%. In addition, we use a simplified CAPTCHA (adding two numbers) to screen for bots, i.e., only participants that correctly answer this question can access our survey. (Note also that our real-effort task serves as an additional bot check as the sequences of letters are in a non-machine readable format.)

There are some further practical challenges in running experiments on an online platform such as MTurk. First, Mturkers often multitask and work simultaneously on several HITs. To minimize the switching between HITs, we state in the beginning that they should exclusively work on our HIT, that they have a total of 20 minutes to complete the HIT, that there are timeouts on

each question, and that any payment is conditional on completing the HIT within the time limit. Moreover, we pay a relative high flat payment of $0.75 and promise substantial additional payments. On average, participants could expect to earn about $1.90, which is substantially above the minimum wage considering the usual HIT duration of 12 minutes. Second, since participants typically do not arrive simultaneously, we designed the survey as a decision task such that most questions and tasks can be completed independently. There is, however, one important exception. To determine the bonus payment, we need to compare the performance in the real-effort task of two participants. For this purpose, every participant enters a virtual waiting room before the revelation of the bonus payment. If there is already a participant waiting, pairs are immediately matched and each participant in a pair can independently work through the rest of the survey. If there is no matching partner available, participants have to wait for a minimum of three minutes. As soon as a suitable matching partner arrives in the waiting room, they will be matched. Participants have the possibility to end the survey after three minutes (if no suitable matching partner has arrived), in which case they only receive the base payment. Alternatively, they can continue waiting until they are matched (but they run the risk that they will not manage to complete the HIT within the time limit, in which case they receive no payment).

3. Empirical Analysis

3.1 Definition of Outcome Variables

We divide our outcome variables into primary outcomes and secondary outcomes. Our primary variable of interest are:

- Tax rate ($Tax_i, revTax_i$)
    - Proposed tax rate (0-100%, tax revenues will be equally distributed within the matched pair)
- Belief about deserving the bonus ($Des_i^{posterior}, Des_i^{prior} - Des_i^{posterior} (= \Delta Des_i)$)
    - Question: Given your score in the task, how much would you deserve the $2-bonus payment?
        - You deserve the $2-bonus payment in % (0 – 100)
- Belief about luck / effort (Eff_i)

    - Question: What do you think, does the payment of the bonus mostly depend on luck or exerted effort?
        - Likelihood, that the $2-bonus payment depend on exerted effort in % (0 – 100)
- Willingness to pay for information (WTP_i)
    - Price list: eight choices about either seeing information about the task difficulty and score of the other participant or receiving extra money ranging from $0.01 to $0.50.

The secondary outcomes help to shed light on the mechanism and are the following:

- Belief about task difficulty ($Diff_i^{posterior}$, $\Delta Diff_i$)
    - Question: What do you think, how likely is it that you have performed the hard task?
        - Likelihood, that you performed in the hard task in % (0-100)
- Belief about relative performance ($Perf_i^{posterior}$, $\Delta Perf_i$)
    - Question: Suppose you compare your score to the score of 100 other participants who completed the same task as you. What do you think, how many of them have a lower score than you?
        - Number of participants who have a lower score than you (0-100)

3.2 Covariates

We elicit the following the following socio-demographic information.

- Gender (Male / Female / Other / I prefer not to say)
- Age (in years)
- Marital status (Single / Married)
- Education (Not completed high school/ High school/ Some college/ 2-year college degree/ 4-year college degree/ Masters degree/ Doctoral degree/ Professional degree (JD, MD))
- Ethnicity (White/European-American / Black/African-American / Asian/Asian-American/Pacific Islander / Hispanic/Latino / Other)
- Number of household members

- Political beliefs (Strongly liberal / Moderately liberal / Slightly liberal / Slightly conservative / Moderately conservative / Strongly conservative)
- Political party identification (Democratic Party/ Republican Party/ Other)
- US residence (Yes / No)
- Home state (list of US states)
- Employment status (Full-time employee / Part-time employee / Self-employed or small business owner / Unemployed and looking for work / Student / Not in labor force)
- Household income ($0 - $9,999 / $10,000 - $14,999 / $15,000 - $19,999 / $20,000 - $29,999 / $30,000 - $39,999 / $40,000 - $49,999 / $50,000 - $74,999 / $75,000 - $99,999 / $100,000 - $124,999 / $125,000 - $149,999 / $150,000 - $199,999 / $200,000 and more)

We will run standard two-sided t-tests on all demographic variables to check balance between the group assigned to the easy code-entry task and to the group assigned to the hard code-entry task. We will also conduct a joint F-test to see if the coefficients are jointly different from zero.

We will also elicit the following personal trait.

- Locus-of-Control using a seven-items module (Cobb-Clark and Schurer, 2013)
    - "I have little control over the things that happen to me."
    - "There is really no way I can solve some of the problems I have."
    - "There is little I can do to change many of the important things in my life."
    - "I often feel helpless in dealing with the problems of life."
    - "Sometimes I feel that I'm being pushed around in life."
    - "What happens to me in the future mostly depends on me."
    - "I can do just about anything I really set my mind to do."
      (7-point scale; Disagree strongly – Agree strongly)

3.3 Power

We will recruit $n = 1800$ participants through Mturk to draw on a sample of the US population. With $n = 1800$ participants, we have 0.8 power to detect an effect size of 0.14 at a 5-percent significance level in the main analysis and an effect size of 0.2 at a 5-percent significance level in the subgroup analysis.

3.4 Empirical Strategy

The treatment is the random assignment of participants to the easy or hard code-entry task. We randomly match a participant in the easy code-entry task with another participant doing the hard code-entry task and we calibrated the task difficulty such that likelihood of receiving the $2 bonus is vanishingly low for participants assigned to the hard task. Thus, the bonus assignment will coincide with the treatment assignment in almost all cases. This allows us to causally identify the impact of the $2 bonus payment on beliefs and behavior. To deal with non-compliance, i.e., participants in the easy (hard) task who received the $0 ($2) bonus, we use the treatment assignment (easy or hard task) to estimate *intention-to-treat* effects.

The general framework in which we will study the impact of a bonus payment on our outcome variables will take the following form:

$$Y_i = \beta_0 + \beta_1 Treatment_i + \gamma \boldsymbol{X} + \varepsilon_i \quad (1)$$

where Y_i is one of our outcome variables defined above (see Section 3.1.), $Treatment_i$ is a binary variable equaling one if a subject was randomly assigned to the easy task, $\boldsymbol{X}$ is a set of standard controls (including gender, age, marital status, education level, ethnicity, employment status, and household income, see also Section 3.2.) and ε_i is an individual-specific error term. We will run OLS regressions, use robust standard errors, and estimate (1) with and without controls.

To test for heterogeneous effects we expand the regression specification (1):

$$Y_i = \delta_0 + \delta_1 Treatment_i + \delta_2 Het_i + \delta_3 Treatment_i * Het_i + \gamma \boldsymbol{X} + \varepsilon_i \, (2)$$

where Y_i is one of our outcome variables defined above (see Section 3.1.), $Treatment_i$ is a binary variable equaling one if a subject was randomly assigned to the easy task, Het_i is the variables of interest (specified in Section 3.6 below), $\boldsymbol{X}$ is a set of standard controls (including gender, age, marital status, education level, ethnicity, employment status, and household income, see also Section 3.2.) and ε_i is an individual-specific error term. We will run OLS regressions, use robust standard errors, and estimate (2) with and without controls.

3.5 Main Analysis

Our main focus is the question whether economic success affects how people think about the role of merit and whether it affects their attitudes towards taxation. We use the regression

equation (1) to estimate the impact of the treatment on our primary outcomes. In some specifications, we will include prior beliefs to control for possible pre-treatment differences.

We will also investigate participants' willingness to pay (WTP) to learn about the task difficulty and the performance of the other participant. Here, we will use equation (1) and regress WTP on our treatment. In addition, we can use the random variation in the information provision to investigate how participants react to this information and revise their tax proposal ($revTax_i$). For this analysis we use the same regression framework as above and control for WTP_i.

3.6 Heterogeneous effects

Political beliefs: Our treatment may have a different effect on participants depending on their political beliefs. We use pre-treatment information on political beliefs ranging from "strongly liberal" to "strongly conservative" (on a 6-point scale) and will create a binary variable "liberal" which equals 1 for participants indicating "strongly liberal", "moderately liberal" or "slightly liberal" and 0 otherwise. We will estimate equation (2) with our primary outcomes as dependent variables, and use similar specifications as in our main analysis.

Locus of Control: In a second specification, we look at heterogeneity by locus of control. We elicit locus of control before the treatment with a 7-item battery. The responses to this item battery can be summarized in a single measure, by taking the sum of responses to the five external items, subtracting the sum of responses of the two internal items and adding 16 (Cobb-Clark and Schurer, 2013). Here higher values indicate more external control tendencies. We will estimate equation (2) with our primary outcomes as dependent variables, and use similar specifications as in our main analysis. Alternatively, we will use a median split of the single measure of locus of control to indicate respondents with an external locus of control and repeat the analysis outlined above.

3.7 Multiple Hypothesis Adjustment

To deal with multiple hypothesis testing we will use indices and account for the False Discovery Rate (FDR).

Indices: We will create an unweighted index for the two post-treatment belief questions on effort and luck and deservingness ($Des_i^{posterior}$, Eff_i).

False Discovery Rate: Because we have multiple outcomes, we will adjust the p-values of our coefficients of interest using the "sharpened q-value approach" (Anderson, 2012; Benjamini et al., 2006).

Variables with limited variation: We will drop from the analysis variables with limited variation (i.e., variables for which more than 95 percent of observations have the same value). If these variables are part of an index, we will recalculate the index without them.

3.8 Attrition from the Sample

Given the setting, we expect that a small share of participant will drop out during the survey. There are two possibilities to drop out. First, a participant may drop out, if there is no matching partner available. This case is not problematic because this will happen before the announcement of the bonus (which depends on the random task assignment). Second, a participant may drop out because of a timeout after the bonus announcement. As long as this is random across treatment, this is no problem. However, it is possible that the announcement of the bonus payment, leads to differential attrition. For example, if it is more likely that participants with a $0 payment drop out. To minimize this risk ex-ante, these participants will not receive any payment for their effort and participation and no approval of the HIT. For these reasons, we expect that the number of participants quitting after the bonus announcement will be very small.

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Appendix

Figure 1 Real Effort Task (hard)

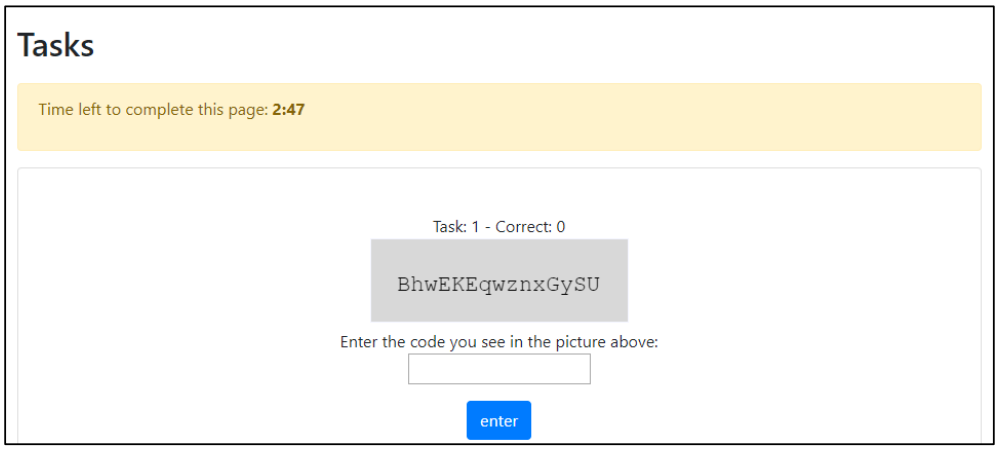


Figure 2 Redistribution Decision

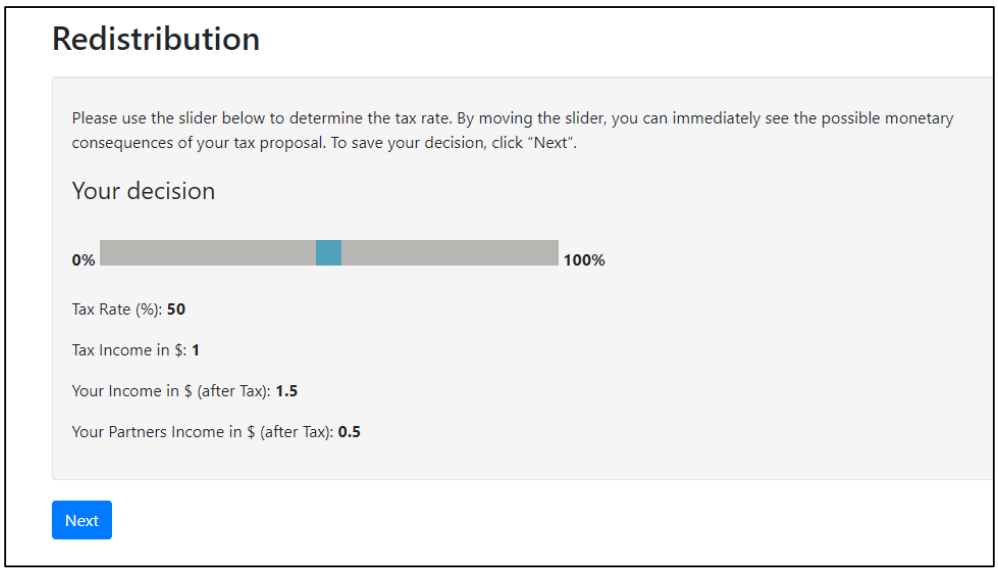
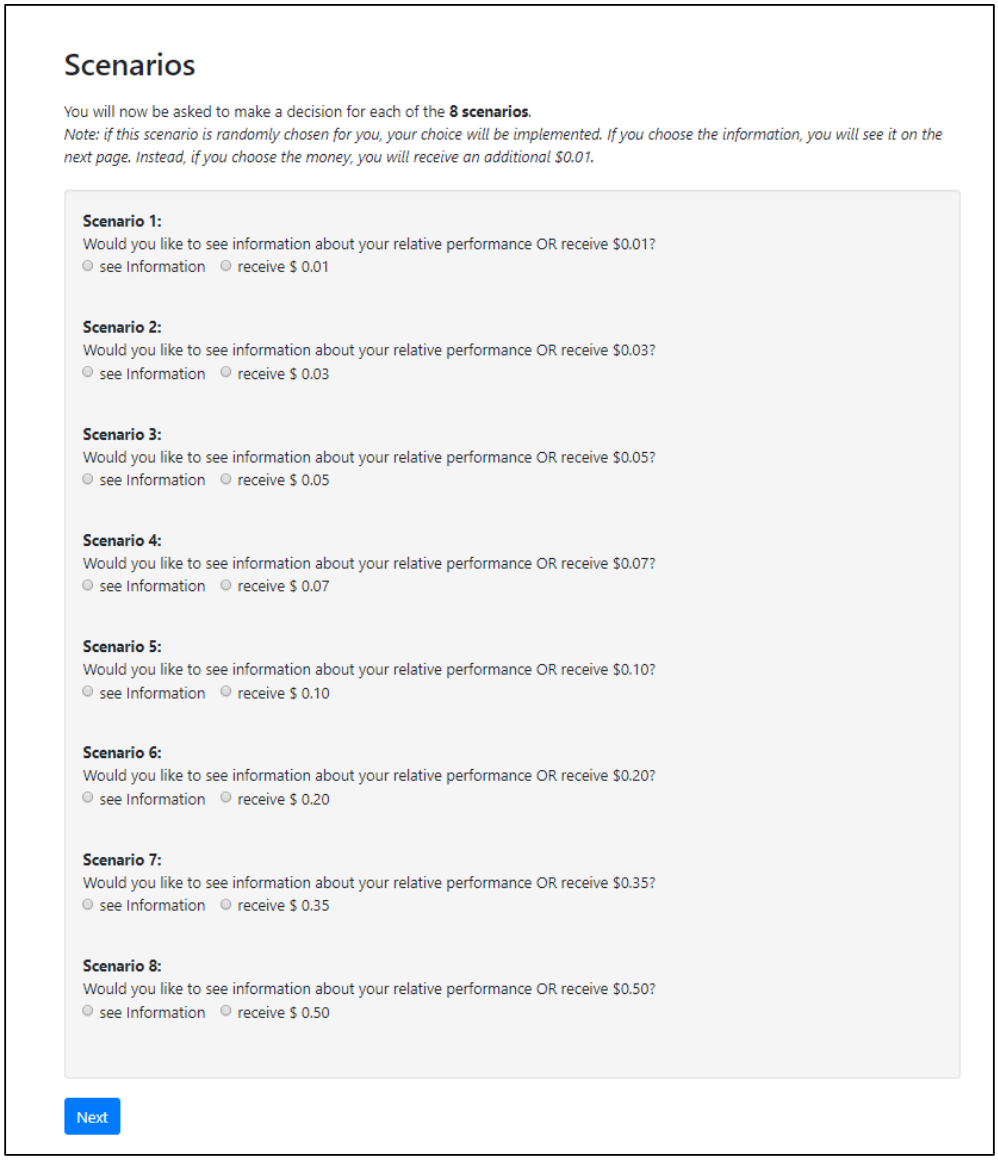


Figure 3 Price List Scenarios



Scenarios

You will now be asked to make a decision for each of the **8 scenarios**.
Note: if this scenario is randomly chosen for you, your choice will be implemented. If you choose the information, you will see it on the next page. Instead, if you choose the money, you will receive an additional $0.01.

Scenario 1:
Would you like to see information about your relative performance OR receive $0.01?
○ see Information ○ receive $ 0.01

Scenario 2:
Would you like to see information about your relative performance OR receive $0.03?
○ see Information ○ receive $ 0.03

Scenario 3:
Would you like to see information about your relative performance OR receive $0.05?
○ see Information ○ receive $ 0.05

Scenario 4:
Would you like to see information about your relative performance OR receive $0.07?
○ see Information ○ receive $ 0.07

Scenario 5:
Would you like to see information about your relative performance OR receive $0.10?
○ see Information ○ receive $ 0.10

Scenario 6:
Would you like to see information about your relative performance OR receive $0.20?
○ see Information ○ receive $ 0.20

Scenario 7:
Would you like to see information about your relative performance OR receive $0.35?
○ see Information ○ receive $ 0.35

Scenario 8:
Would you like to see information about your relative performance OR receive $0.50?
○ see Information ○ receive $ 0.50

Next

Figure 4 Result Summary