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

Dietmar Fehr and Martin Vollmann

January 18, 2021

Abstract

Most people tend to equate success with merit, a tendency that is particularly pronounced among conservatives. Using a large-scale study that samples the general US population, we investigate whether individuals misperceive the importance of luck for success, and how this mediates their meritocratic beliefs and inequality acceptance. We show that economically successful participants overweight the role of effort in their success, perceiving high income as more deserved than unsuccessful participants. Subsequently, they demand less redistributive taxation and also show little interest in receiving information about the true determinants of their success. These general findings hold true regardless of political orientation.

Keywords: *inequality; fairness; deservedness; political views; cognitive dissonance*
JEL codes: *C93, D63*

* We thank Alexander Cappelen, Bertil Tungodden, Sarah Necker, Joerg Oechssler, and Yilong Xu as well as the seminar audience at FAIR (Bergen) and Heidelberg University 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.
Contact details: Heidelberg University, Alfred-Weber-Institute for Economics,
E-mail: dietmar.fehr@awi.uni-heidelberg.de; martin.vollmann@awi.uni-heidelberg.de

1 Introduction

People tend to accept more inequality if it reflects hard work, effort, and performance (Fong 2001; Alesina and La Ferrara 2005; Cappelen et al. 2007; Almås, Cappelen, and Tungodden 2020). This widely held meritocratic fairness ideal may explain variation in income inequality and redistributive policies across countries (Alesina and Glaeser 2004; Alesina and Angeletos 2005), and it is at the core of the "American Dream," i.e. the notion that success can be attained by all who work sufficiently hard. Against this backdrop, it is not surprising that many tend to equate success with merit and disregard the role of luck (Frank 2016; Gauriot and Page 2019; Mijs 2019). This tendency is paralleled by a strong political polarization. Public opinion polls consistently reveal that liberals emphasize the role of luck in economic success, while conservatives typically support the view that success is the result of hard work, which makes any resulting inequality morally fair (Dunn 2018; Pew Research Center 2019).[1] However, it is difficult, if not impossible, to discern the relative contributions made by luck and effort to economic success

This paper provides causal evidence on how economic success shapes meritocratic beliefs and contributes to the moral justification of inequality using a large-scale interactive online study with a sample of the general population of the United States. Given that ideological dispositions on fairness views and inequality differ so strongly between liberal and conservative voters and appear as critical inputs for government tax policy policies (e.g. Alesina and Glaeser 2004; Congdon, Kling, and Mullainathan 2009), we also examine how meritocratic beliefs and redistributive taxes differ in relation to political orientation. A persistent concern is that these dispositions do not necessarily reflect what people think and do when they are forced to appraise their own success. Do liberals still believe in luck when they are successful, and are conservatives still proponents of meritocracy when they are unsuccessful?

There is little disagreement in the public and academic discourse that those who work hard should be entitled to their earnings. However, as success is typically the combined result of effort, talent and life circumstances, it is notoriously difficult to credibly identify the causal relationship between economic success and perceptions surrounding the role of merit. First, identification is complicated by the difficulty to identify and to quantify the relative impact of luck and effort ex-post, let alone to study the associated beliefs. Second, it is difficult to gather data on individuals' meritocratic beliefs before and after 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, a correlation between political orientation and meritocratic beliefs may indicate

[1] More generally, political affiliation appears as a strong indicator of how people perceive and navigate political and economic issues (Campbell 1960; Bartels 2002).

that causality runs in both directions with political orientation informing such beliefs, and vice versa.

We overcome these identification challenges by designing a work assignment that captures some important aspects of socio-economic reality, while also allowing us to introduce the necessary exogenous variation in economic success. The work assignment is a simple code-entry task, for which we recruited a large sample of workers from an online labor market platform. The code-entry task requires no prior knowledge or specific skills such that performance should depend almost entirely on exerted effort. We randomly match workers into pairs and pay them by their relative performance. To capture the luck component of economic success and to create the necessary random variation to identify the impact of success on meritocratic beliefs, we randomly assign workers in a pair to either an *easy* or a *hard* version of the task, without disclosing this assignment to the workers. The two versions of the task are calibrated such that working on the *easy task* results with near certainty in economic success i.e. receiving a bonus payment, which results in highly unequal incomes within pairs. Thus, everyone has to exert effort to have a chance of success (i.e. receiving the bonus payment), while some have a larger exogenous advantage than others. Because this exogenous advantage basically predetermines success, we can identify its impact on meritocratic beliefs and support for redistributive taxes.

After participants complete the work assignment, but before they learn about their success or failure, we elicit their beliefs about task difficulty, their relative performance, and the extent to which they deserve the bonus payment. After revealing the bonus payment (i.e. economic success), we measure these beliefs again. Eliciting beliefs *before* and *after* disclosing the bonus payment allows us to account for heterogeneous effects in behavior by prior beliefs and to measure the causal impact of success on meritocratic beliefs. In a next step, we investigate how economic success and meritocratic beliefs shape support for redistributive taxes and to what extent participants are willing to resolve the uncertainty of task assignment. Specifically, we elicit participants' preferences for a redistributive tax scheme and their willingness to pay for information about task difficulty and performance. In addition, we gather information on a broad range of socio-economic characteristics from participants, including political orientation and party affiliation, before the start of the work assignment.

The experiment generates two main findings. First, we observe that economically successful participants assign excess weight to the role of effort, leading to a strong polarization in attitudes. That is, we document a strong treatment effect on meritocratic beliefs. Economic success leads to a 14 percentage point higher belief that receiving the bonus payment is deserved. Similarly, successful participants are 16 percentage points more likely than unsuccessful participants to think that success in the work assignment depends on effort. Therefore, participants

predominantly attribute their success to hard work, although it is very salient that success is random in our setting.

Economic success in our setting also conditions preferences for redistributive taxes. Specifically, successful participants prefer a 40 percentage points lower tax rate than unsuccessful participants, a difference equivalent to a three-times lower tax revenue. The difference in tax rates preferred by successful and unsuccessful participants can be fully explained by the treatment difference in prior beliefs regarding merit. Participants with a higher prior belief that they deserve the bonus payment demand less redistribution (i.e. prefer a lower tax rate) when they are successful, but more redistribution if they are unsuccessful. Because we elicited these meritocratic beliefs before participants learned about the redistribution stage, they solely reflect how participants attribute the outcome of their work assignment to luck and effort and are not distorted toward self-serving tax decisions.

Consistent with the relationship between perceptions of personal merit and preferred tax rates, we document that 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 success. 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.

Second, the findings bring empirical evidence to the divisive political debate regarding fairness views and economic issues. In particular, we cast doubt on the broadly held notion that liberals are less likely to equate success with merit than conservatives. In fact, when liberals are economically successful, they advocate meritocracy just as frequently as conservatives, despite the overwhelming role played by luck in our setting. In other words, meritocratic beliefs and behavior do not differ by political orientation: when they are successful, liberals and conservatives both identify merit as the cause of success, and they both prefer lower redistributive taxes. Moreover, liberals assign as little importance to learning about the role of luck in their success as conservatives, they are less likely to revise their tax preferences, and if they revise them, the magnitude of change is smaller when compared to that of conservatives.

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. An important and consistent finding that has emerged in observational studies (Fong 2001; Alesina and Angeletos 2005; Karadja, Mollerstrom, and Seim 2017) and laboratory studies alike (Konow 2000; Cappelen et al. 2013; Cappelen et al. 2017) is that people tend to accept greater inequality if it is the result of

effort rather than luck.[2] While the importance of the source of inequality is well documented, empirical evidence on inequality acceptance when individuals are uncertain or have limited information about the source of inequality is scarce (but see, for example, Cappelen et al. 2017; Cappelen, De Haan, and Tungodden 2020). Unlike most of these papers, we present causal evidence on how economic success impacts meritocratic beliefs when individuals can "mentally" justify their success by attributing it to their own actions. The subsequent selfish behavior that we observe is consistent with self-serving fairness norms described in the prior literature (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 2016). We advance this literature by showing that participants display little interest in correcting their views about merit. We also show that this lack of interest applies to liberals and conservatives alike.

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 et al., 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.[3] 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.

Finally, our paper belongs to a growing literature in economics that documents political polarization on a host of social and economic issues. Recent studies show that this polarization is not confined to political attitudes or fairness views alone (e.g. Gromet, Hartson, and Sherman 2015; Cappelen et al. 2020), but also applies to perceptions of factual reality, including inequality (Kuziemko et al. 2015), relative income (Cruces, Perez-Truglia, and Tetaz 2013; Karadja, Mollerstrom, and Seim 2017; Fehr, Mollerstrom, and Perez-Truglia 2019), social mobility (Alesina, Stantcheva, and Teso 2018; Fehr, Muller, and Preuss 2020), and immigration (Alesina, Miano, and Stantcheva 2020; Grigorieff, Roth, and Ubfal 2020). Other studies suggest that liberals tend to be less

[2] There is also evidence that rich people accept more inequality if they experienced upward mobility compared to rich people who inherited their wealth (Cohn et al. 2019).

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

accepting of inequality (Fisman, Jakiela, and Kariv 2017; Cappelen, Haaland, and Tungodden 2019; Almås, Cappelen, and Tungodden 2020). We find no evidence of political polarization when people are not merely required to reveal their attitudes but are actually confronted with the situation. Although liberals are somewhat more open to redistributive taxation, we find no difference in how liberals and conservatives react to economic success – that is, liberals display the same meritocratic beliefs and behavior as conservatives.

The paper proceeds as follows. In the next section we describe our study design and implementation procedures focusing on some practical advice on conducting successful interactive online experiments. Section 3 lays out our empirical strategy and Section 4 presents the results. We conclude by discussing our findings in Section 5.

2 Experimental Design

Our study, which combines a survey and incentivized decision tasks, consists of four parts: a socio-demographic questionnaire, a work assignment, a redistribution task, and an information acquisition task. Screenshots of the survey and all tasks are available in the Appendix. We pre-registered the design and a pre-analysis plan in the AEA RCT Registry (AEARCTR-0004455).

Setup: In the first part, we introduce participants to the general details of the study and ask for their consent. We then elicit some basic socio-demographic information and personality traits. More details and a complete list of all covariates can be found in Appendix A1. 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. There are two task types: An *easy task* consisting of five-letter sequences and a *hard task* consisting of 15-letter sequences. 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 task assignment, rather than 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* (see Section 4.1 for more details).

Participants are paid according to their performance. That is, we randomly match a participant working on the *easy task* with a participant working on the *hard task* and compare 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*).

Before we reveal the outcome of the performance comparison (i.e. the bonus payment), we ask participants: (1) to estimate the likelihood that they worked through the *hard task* ("Prior Belief, Task Difficulty"), (2) how much they think they deserve the $2 -bonus payment ("Prior Belief, Deserving Bonus"), and (3) to estimate how many of 100 participants performing the same task achieve a lower score ("Prior Belief, Relative Performance"). After revealing the bonus payment, we ask the same questions again ("Posterior Beliefs").[4] Additionally, we ask participants to assess the extent to which they think the bonus payment depends on luck or effort ("Belief 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 Rasoscha 2020; 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.

In the third part, both participants in the matched pair have to decide about a redistributive tax rate, in which the tax revenue is equally distributed between the pair. This implies in our setting that the successful participant pays half of the tax revenue as tax while the unsuccessful participant receives half of the tax revenue. Using an interactive slider, participants can indicate a tax rate ("Tax Rate") between 0% and 100% and immediately see how the tax rate will affect their income and that of the other person. We randomly select one of the two proposed tax rates and apply the choice to the matched pair at the end of the study.[5]

In the fourth part, we offer 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, with amounts ranging from $0.01 to $0.50. 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,

[4] While it is common practice to elicit both prior and posterior beliefs, a potential concern is that belief updating is not genuine but the result of a demand effect, i.e. respondents may think they are expected to update their beliefs in response to the bonus announcement. We believe that this is unlikely the case in our setting. As we document in Section 4.2, respondents display nuanced responses to the three belief questions, and the observed differences in posterior beliefs already existed before the treatment. Moreover, recent studies suggest that demand effects play little role in online studies and if evidence occurs they are quantitatively small (de Quidt et al. 2018; Mummolo and Peterson, 2019).

[5] Note that this procedure elicits participants' true preferences for redistributive taxation 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).

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%) have the opportunity to revise their tax rate ("Revised Tax Rate"). Note that we only implement the revised tax rate if the first tax proposal from that participant was initially chosen for implementation. Finally, participants receive a detailed overview about the composition of their final payout.

Implementation: We used the open source software oTree (Chen, Schonger, and Wickens 2016) to program and run the study. We recruited and paid participants via Amazon Mechanical Turk (MTurk) in summer 2019. This platform offers access to a quite diverse population (e.g. Berinsky, Huber, and Lenz 2011; 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 2020). However, some researchers have noted that data quality has recently declined, in particular due to automated responses (bots) and inattention (Ahler, Roush, and Sood 2020; Chmielewski and Kucker 2020). To address these concerns, 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. Third, the letter sequences in the work assignment were in non-machine-readable format, providing another layer of protection against bots.

We also took great care to address other practical challenges associated with running experiments on an online platform such as MTurk. First, MTurkers often multitask and work simultaneously on several HITs. To minimize inattention due to switching between HITs, we requested in the beginning 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. The timeouts are set such that participants have sufficient time to thoughtfully answer our questions, yet they must remain attentive. Moreover, we paid a relatively high flat payment of $0.75 and promised substantial additional payments. On average, participants earned about $1.90, which is substantially above the US minimum wage considering our usual HIT duration of 12 minutes.

Second, since participants typically do not arrive simultaneously, we designed the experiment such that the survey and the work assignment can be completed independently. There was, however, one important exception. To determine the bonus payment, it is necessary to compare two participants' performances in the real-effort task. 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. 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).

Finally, 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 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 below). However, we also took some steps to minimize this risk ex-ante. We 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).

Attrition and sample characteristics: The overall attrition rate was about 9 percent, which is comparatively low for this type of study.[6] In total, 2,026 participants started the work assignment and 1,845 participants finished all tasks.[7] Importantly, attrition was random across the treatment assignment (10 percent in the *hard* 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 the treatment indicator shows no difference in the likelihood of attrition between the *easy* and *hard* treatment (see Appendix Table A1).[8] Moreover, comparing socio-demographic characteristics (including political views) of dropouts and non-dropouts reveals no

[6] For example, Kuziemko et al. (2015) report an attrition rate of 15 percent in survey experiment and Arechar, Gächter, and Molleman (2018) report an attrition rate of 18 percent in an interactive online experiment.
[7] A total of 2,535 MTurkers accepted our HIT. Of those, 383 failed on the simple CAPTCHA, 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 tasks in non-machine-readable format and 21 MTurkers dropped out after the survey but before the work assignment resulting in our final sample of 2,026.
[8] 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).

differences (see Appendix Table A2). Across 30 tests, there is no single t-statistic above 1.96. Therefore, attrition is unlikely to affect our results.

In our final sample, we dropped 20 participants, because they ended up with the same score and the bonus was split equally within pairs. This leaves us with 1,825 observations. In Appendix Table A3, we show that the 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). Moreover, 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 Appendix Table A4).

3 Empirical Strategy

Our treatment involves the random assignment of participants to an *easy* and *hard task*.[9] 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*. This allows us to identify the causal effect of economic success on meritocratic beliefs and behavior.

In practice, treatment compliance was, however, not perfect. About 6 percent of participants assigned to the *hard task* had a better performance than their matched counterparts in the *easy task* (for details, see Section 4.1). 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:

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

where Y_i is one of our outcome variables (i.e. our belief measures and the tax rate), $EasyTask_i$ indicates if a participant 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), and ε_i is an individual-specific error term. In some specifications, we consider participants' political views by including its interaction with the treatment. For this purpose, we

[9] This is similar variation to Deffains, Espinosa and Thöni (2016), though our empirical strategy does not require a full separation of effort in the two tasks and our design allows us to identify the causal effect of economic success on meritocratic beliefs.

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."[10] We run OLS regressions, use robust standard errors, and estimate (1) with and without controls.

Because non-compliance is low, we report ITT estimates throughout the paper, and relegate and discuss the IV estimates (effects of the treatment on the treated) to Online Appendix A5. These estimates are very similar in magnitude to the ITT estimates. Therefore, we interpret our results reported below as the effect of the bonus assignment or economic success. We have prespecified the analysis in our pre-analysis plan (AEARCTR-0004455) and we follow this plan if not stated otherwise.

4 Results

Our aim is to explore whether economic success affects how people think about the role of merit and whether it affects inequality acceptance (i.e. participants' attitudes toward redistributive taxation). We present three sets of results. First, we document participants' perception about merit in the work assignment and examine how these perceptions change with the exogenous bonus assignment Second, we examine how perceptions of merit affect redistributive choices. Third, we are interested in participants' willingness to learn about the underlying determinant of their success.

4.1 Work Assignment and Prior Beliefs

We start by looking at participants' performance in the two tasks. Table 1 provides an overview. It is apparent that, on average, participants in the *easy task* coded substantially more sequences of letters compared to participants in the *hard task* (35 vs. 10). However, as indicated above, the scores in the two tasks overlap to some extent. That is, the 90th percentile in the *hard task* is 17, while the 10th percentile in the *easy task* is 16. This overlap results in a non-compliance to the treatment assignment in about 6 percent of cases, because the bonus is paid to a participant completing the *hard task*, instead of the participant performing the *easy task*.

Figure 1 shows participants' beliefs regarding task difficulty, their deservingness of the bonus, and their relative performance prior to the announcement of the bonus payment. As shown in the figure, actually performing the task was a weak signal of task difficulty, as intended. Nevertheless, participants had some notion of their task assignment: 67.9 percent of participants in

[10] 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.

the *hard task* thought they had been assigned to the *hard task*, which is significantly above 50 percent ($p < 0.001$, two-sided t-test). Similarly, 62.8 percent of participants in the *easy task* thought they had been assigned to the *easy task*. Again, this is significantly different from chance ($p < 0.001$, two-sided t-test).

At the same time, we observe that participants in the *easy task* find themselves as more deserving of the $2 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 a bonus. Indeed, performance and perceptions of deservingness are strongly correlated (each point increase in performance increases beliefs in deservingness by approximately 0.28 percentage points; see Figure A1). In line with this finding, we observe that coding more sequences, on average, is related to the impression that one ranks higher in the performance distribution. Specifically, participants in the *easy task* thought they outperformed 54 percent of other participants completing the same 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). Interestingly, political views are not related to beliefs about deservingness and performance. That is, these beliefs do not differ between liberals and conservatives.

4.2 Effects on Posterior Beliefs

Figure 2 displays the difference between posterior and prior beliefs and thus illustrates how economic success (i.e. bonus assignment) changes beliefs. Notably, the bonus announcement does not change perceptions of task difficulty. However, we observe that bonus announcement results in significant changes in perceived deservingness and relative performance. We see that economic success increases perceived merit by 5 percentage points, while at the same time, failure decreases perceived merit by almost 6 percentage points. This further increases the wedge in merit perceptions between successful and unsuccessful participants. Economic success results in a 14 percentage point higher belief that receiving the bonus payment is deserved.

Similarly, success increases belief in relative performance but decreases it for those who are left empty-handed. Participants in the *easy task* think their performance is better than 60 percent of others, while participants in the *hard task* think their performance is only better than 47 percent of others. This suggests that being successful also triggers overconfidence. Indeed, if we compare how participants' posterior beliefs about relative performance compare to their true rank in the performance distribution of all participants completing the same task, we see that a higher share of participants overestimates their 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 before the bonus announcement, i.e. if we compare prior

beliefs about relative performance to the true rank. In this case, the share of participants who overestimate their performance is nearly the same in the *easy task* and in the *hard task* (0.52 vs. 0.50; t-test, $p<0.37$).

Table 2 presents rigorous statistical evidence on how economic success impacts these perceptions. We regress the difference between posterior and prior beliefs on a treatment indicator, participants' political beliefs, and its interaction with the treatment indicator. To compare the results from this exercise with the observed patterns in the raw data we include a specification without political beliefs and covariates. There are several things to note. First, it is apparent that the regressions confirm the results presented above. Receiving the bonus has no effect on the perceived task difficulty, while it increases participants' perceptions that they deserve the bonus and that they performed better than others. Second, one can see in columns 3, 6, and 9 that controlling for participants demographic and economic status (such as gender, age, education, income, household size, ethnicity, employment status, marital status, and geographic indicators) does not meaningfully affect the estimated treatment effects. Third, political views are largely unrelated to changes in beliefs. In particular, we observe equally strong feelings of deserving the bonus among liberals and conservatives, and they do not differ in their perceptions of task difficulty.

Overall, our treatment resulted in strong effects on beliefs. Most notably, there is a sizable impact on perceptions of deservingness that is independent of political views.

4.3 Behavioral Measure: Redistributive Taxes

We now address how misperceptions of economic success translate into tax preferences. Panel a. of Figure 3 shows a strong divergence of tax rates across the two conditions: the average tax rate in the *easy task* is about 20.6 percent and in the *hard task* about 60.2 percent. Despite this divergence, it is apparent that fairness considerations matter. That is, tax rates are far from the extremes of zero and full redistribution. In Table 3, we present regressions showing how success and failure shape redistributive tax-rate decisions. The first column confirms that the proposed tax rate is about 40 percentage points lower if participants received the $2 bonus. This effect is substantial and corresponds to a 3-times lower tax revenue. Including covariates does not change the estimate (column 2).

Next, we examine the relationship between tax-rate decisions and political views using pre-treatment information on participants' self-assessment in the political left-right spectrum. Panel b. of Figure 3 illustrates that economic circumstances affect redistributive preferences irrespective of political views: conservatives *and* liberals prefer high taxes if they are unsuccessful whereas they both choose low taxes if they are successful. However, it is also true that liberals propose, on average, higher tax rates than conservatives. Specifically, the difference in tax rates is

about 8 percentage points in the *hard task* (t-test, $p<0.01$), while it is about 3 percentage points in the *easy task* (t-test, $p<0.06$). While this finding echoes correlational evidence that liberal voters are more favorable toward taxation (Wahlund 1992; Reed 2006; Hardisty, Johnson, and Weber 2010), the differences are small, particularly among those who are successful.[11]

Following our pre-analysis plan, we corroborate these findings using a similar regression specification as above. In Table 3, column 3, we observe that, on average, liberals demand more redistribution, and thus set a higher tax rate than conservatives. Interacting treatment status with political views, we find a negative and statistically insignificant effect, which corresponds to roughly half of the difference between liberals and conservatives in the *hard task*. That is, while liberals tend to set higher tax rates than conservatives, the difference in the *easy task* is substantially smaller than in the *hard task*. Again, adding covariates does not change the coefficient estimates (columns 4 and 6).

4.4 Impact of Beliefs on Redistributive Taxes

Differences in redistribution preferences between liberals and conservatives are often associated with differences in beliefs about the role of effort in economic success. Liberals tend to assign luck a greater role in economic success than effort, while conservatives believe that effort dominates (Gromet, Hartson, and Sherman 2015; Karadja, Mollerstrom, and Seim 2017; Fehr, Muller, and Preuss 2020). Indeed, when asking participants whether they think economic success is the result of luck or effort, liberals are less likely to believe the bonus payment is the result of effort (see Table 2 columns 11 and 12).[12] This finding accords with liberals' "locus of control": that is, liberals are more likely to believe life outcomes are the result of fate or luck, and therefore beyond one's control (see Appendix Table A8). However, the correlation between locus of control (LoC) and political orientation is not strong, and we find that LoC itself has no impact on tax rate preferences. In the Appendix Table A9, we regress the tax rate on our treatment, LoC, and the interaction of the two and find no measurable effect of LoC on tax rate preferences.

To shed light on the factors underlying tax-rate decisions, we examine how they relate to beliefs. We are particularly interested in the heterogeneity with respect to prior beliefs about the task. All beliefs (except beliefs that effort determines success) were elicited *before* the bonus announcement and *before* participants learned about the possibility to redistribute income. Thus,

[11] Figures A2 and A3 in the appendix show the distribution of tax decisions.

[12] The regression also reveals that in both tasks, participants believe that effort is more important than luck for success. However, the results in Table 1, column 10 highlight a strong disparity: successful participants believe to a much greater extent than unsuccessful participants that receiving the bonus is attributable to effort (16-percentage-point difference).

these beliefs reflect heterogeneity in participants' meritocratic views that are unaffected by the bonus announcement and they cannot reflect a preference for self-serving redistributive policies. We include these perceptions about the work assignment one-by-one in the regressions and additionally control for a full set of covariates. Table 4 presents the results and reproduces, for comparison, the treatment effect on taxation in columns 1–2. In line with the previous literature, we find that a stronger belief that effort determines success reduces tax rates in both conditions (column 3). That is, participants are less willing to redistribute if they more strongly believe that the bonus is the result of hard work.

Examining heterogeneous effects offers a more nuanced picture of possible mechanisms, even though we observe in all specifications that beliefs are related to the tax-rate decision. We first note that beliefs about task difficulty are positively related to taxes in the case of failure, while they are negatively related when successful (column 4). That is, in both treatments we see that participants who are more certain about task difficulty react more strongly by demanding more (*hard task*) and less taxes (*easy task*), respectively. There is a similar pattern for relative performance beliefs (column 5). Believing in stronger performance is associated with demanding a larger share of the pie, i.e. beliefs are positively related to taxes for economically unsuccessful participants and negatively related to taxes for the successful. Importantly, in both cases we observe a large and significant treatment effect.

In contrast to these observations, the treatment effect is no longer significant when we include beliefs about deservingness. The regressions in column 6 reveal that a higher belief in deserving the bonus payment is associated with a higher tax rate for unsuccessful participants, but not for successful participants. More precisely, a 1 percentage point higher belief in deserving the bonus payment is associated with a 0.23 percentage point higher tax rate for unsuccessful participants, but a 0.44 percentage point lower tax rate for successful participants. Given the effect size of the interaction term, the joint effect with prior beliefs is negative and significant as well (Wald test, $p<0.01$). This suggests that the treatment effect is mediated by the belief that success is an indicator of deservingness.

4.5 Willingness to Correct Beliefs

Thus far, we have shown that receiving the bonus caused a shift in perceived deservingness of the bonus and in beliefs about the role of effort for success. This shift in beliefs explains the substantial disparity in the willingness to redistribute, with successful participants proposing a lower tax rate than unsuccessful participants. Recall that we randomly assigned participants to the *easy* and *hard task* and that they only learned whether they received the $2 bonus or not, but neither received information on which task they completed, nor the score of their opponent. This uncertainty in

relation to task difficulty and performance allows participants to maintain distorted and self-serving beliefs about whether they deserve the bonus.

In a next step, we therefore 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 luck in success. 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 4 shows the distribution of participants' WTP with consistent answers, separated by task.[13] 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 5, 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.

Next, we examine whether obtaining information about task difficulty and the opponents' score leads to revised tax-rate preferences. All participants who received the information (approx. 25 percent) and a random subset of the remaining participants (approx. 50 percent) had the possibility to revise their tax decision. 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 the tax rate *and* the magnitude of change. In all regression specifications, we 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 6 displays the results. Conditional on WTP, receiving information increases the likelihood of revising the tax rate by 27 percent.

[13] As is typically the case with this procedure, a few participants displayed inconsistent behavior by switching multiple times between buying information and keeping the offered amount of money. The share of inconsistent participants is 3 percent, which is at the lower end of the range observed in other papers using a similar procedure. For example, Fehr, Mollerstrom, and Perez-Truglia (2019) and Fuster, Perez-Truglia, and Zafar (2018) report 5 percent inconsistent choices, whereas Cullen and Perez-Truglia (2018) report 15 percent. Note that the low rate of inconsistent answers also speaks to the attentiveness of participants.

However, once we control for treatment status and political views (including a full set of interactions) the coefficient estimate becomes substantially smaller and insignificant. Instead, we see that the likelihood of revising the tax rate is lower for liberals (columns 3 and 4). Columns 5–8 present the effects on the magnitude of change. Again, we see that receiving information leads to larger changes in the tax rate than not receiving information. Controlling for treatment status and political views indicates that changes are smaller in the *easy task* and for liberals irrespective of treatment status, while the coefficient on received information is less precisely estimated.

Together, these results suggest that participants in the *easy task* want to maintain their meritocratic beliefs to justify their tax decision, and this tendency is particularly pronounced among liberals.

4.6 Exploratory Analysis: Impact of Correcting Misperceptions on Behavior

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 tax-rate decision. 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:

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

where Y_i is an indicator for revising the tax rate (or not), or the absolute value of the change in the tax rate. $b_i^{posterior}$ 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 7, column 1, we see that the information shock has no effect on the likelihood of changing the tax rate. The coefficient is close to zero and precisely estimated. Controlling for treatment status (column 3) reveals that participants in the *easy task* are less likely to revise the tax rate, which is in line with the estimates in Table 6. This negative effect on taxes is only present among conservatives (column 5), but not among liberals (column 4) when controlling for the news shock (β_1). In contrast to these results, the information shock has a significant and positive effect on the

size of the tax revisions. Learning that the task difficulty is 10 percentage point higher than previously thought results in a 5 percentage point larger change in tax rate (column 6). This is sizable given that the average bias is about 33 percentage points. Adding covariates in column 7 and controlling for treatment status in column 8 leaves the coefficient estimate for information unchanged. If we differentiate between political views, we see that liberals drive this effect. They react strongly to the information shock (column 9), while conservatives do not react at all (column 10). To summarize, the information shock has no influence on the decision to revise the tax, but if participants revise their tax rate, changes are larger for liberals who experienced a larger information shock.

5 Discussion

We conducted a large-scale online experiment to investigate how "randomness" in economic success affects meritocratic beliefs and redistributive taxes when participants have an opportunity to "mentally" justify their success by attributing it to their own effort. Our results demonstrate that experiencing economic success or failure leads to a significant divergence in meritocratic beliefs and inequality acceptance. Successful participants believe they are more deserving of the bonus and demand substantially lower tax rates than unsuccessful participants.

Participants are well aware of the random assignment to one of two tasks that differ in difficulty. Therefore, it is very salient for matched participants that one of them has an easier path to success. Meritocratic fairness views would call for redistribution in such a situation, as circumstances are beyond one's control (e.g. Cappelen et al. 2007). At the same time, however, participants have an incentive to reap the full material benefits of their success. This conflict between self-interest and fairness principles may result in cognitive dissonance (Festinger 1957). To reduce this tension, people may follow different strategies: one may reduce self-interested behavior, or, alternatively, engage in self-deceptive behavior by manipulating their own beliefs (Konow 2000).

The latter strategy appears to be visible in our data, as participants adapt their beliefs to reconcile their wish for maximizing outcomes with the wish for a fair outcome. This is evident based on the share of successful participants who believe they deserve the bonus, which increases substantially in the *easy task* after the bonus announcement. Moreover, it is in accordance with their belief that effort determines success. Consequently, to resolve this cognitive dissonance, participants try to uphold their beliefs in a self-serving manner (Loewenstein et al. 1993). This may also explain why participants in the *easy task* have a lower willingness-to-pay for information about task difficulty and score of the other participant. Köszegi (2006) refers to this as the "self-image protection motive," which impels individuals to avoid information that might distort

existing beliefs (see also Benabou and Tirole 2002). That participants have a fairly good sense of the difficulty of the task they performed is indicative of the strength of this motive.

There is widespread support for meritocratic principles in modern societies. Indeed, 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. Against the backdrop of rising inequality, it is therefore unsurprising that academics, policymakers and voters have repeatedly called for greater equality of opportunity to achieve this ideal. Nevertheless, in most countries, reality diverges sharply from the meritocratic ideal. Social mobility within the United States, for example, is among the lowest across developed countries, in no small part due to inequality of opportunity (Corak 2006; Chetty et al. 2014; Chetty et al. 2017). These unequal opportunities are particularly pronounced 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. Indeed, 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). Given the strong correlation between college affiliation and income, some individuals clearly have a much easier route to success than others. Our setting seeks to replicate this uneven playing field. Although the conditions of unequal opportunity in our setting are arguably more salient than in many real-world settings, our results nevertheless suggest that success is typically viewed as a reward for effort, and not as the result of ability and life circumstances. Consequently, people may cling to the belief that going from rags to riches is possible given enough effort, allowing meritocratic beliefs to prevail despite structurally predetermined unfair outcomes.

This tendency to uphold meritocratic beliefs also illustrates a potentially dark side of meritocracy. According to our data, successful participants self-servingly opt for lower tax rates 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.

6 References

Ahler, Douglas J., Carolyn E. Roush, and Gaurav Sood. 2020. "The Micro-Task Market for Lemons: Data Quality on Amazon's Mechanical Turk." *Working Paper*.

Alesina, Alberto, and George-Marios Angeletos. 2005. "Fairness and Redistribution." *American Economic Review*, 95(4): 960–80.

Alesina, Alberto, and Edward Glaeser. 2004. *Fighting Poverty in the US and Europe. A World of Difference*: Oxford University Press.

Alesina, Alberto, and Eliana La Ferrara. 2005. "Preferences for redistribution in the land of opportunities." *Journal of Public Economics*, 89(5-6): 897–931.

Alesina, Alberto, Armando Miano, and Stefanie Stantcheva. 2020. The Polarization of Reality. Cambridge, MA: *National Bureau of Economic Research.*

Alesina, Alberto, Stefanie Stantcheva, and Edoardo Teso. 2018. "Intergenerational Mobility and Preferences for Redistribution." *American Economic Review*, 108(2): 521–54.

Almås, Ingvild, Alexander W. Cappelen, and Bertil Tungodden. 2020. "Cutthroat Capitalism versus Cuddly Socialism: Are Americans More Meritocratic and Efficiency-Seeking than Scandinavians?" *Journal of Political Economy*, 128(5): 1753–88.

Arechar, Antonio A., Simon Gächter, and Lucas Molleman. 2018. "Conducting interactive experi-ments online." *Experimental Economics*, 21(1): 99–131.

Arechar, Antonio A., Gordon Kraft-Todd, and David G. Rand. 2017. "Turking Overtime: How Participant Characteristics and Behavior Vary Over Time and Day on Amazon Mechanical Turk." *Journal of the Economic Science Association*, 3(1): 1–11.

Babcock, Linda, George Loewenstein, Samuel Issacharoff, and Colin Camerer. 1995. "Biased Judgments of Fairness in Bargaining." *American Economic Review*, 85(5): 1337–43.

Bartels, Larry M. 2002. "Beyond the Running Tally:. Partisan Bias in Political Perceptions." *Political Behavior*, 24(2): 117–50.

Bénabou, Roland, and Tirole, Jean. 2002. "Self-confidence and personal motivation." *Quarterly Journal of Economics*, *117*(3), 871-915.

Benjamini, Yoav, Abba M. Krieger, and Daniel Yekutieli. 2006. "Adaptive Linear Step-up Procedures That Control the False Discovery Rate." *Biometrika*, 93(3): 491–507.

Berinsky, Adam J., Gregory A. Huber, and Gabriel S. Lenz. 2011. "Using Mechanical Turk as a Sub-ject Recruitment Tool for Experimental Research." *Working Paper*.

Brunnermeier, Markus K. and Jonathan A. Parker. 2005. "Optimal Expectations." *American Economic Review*, 95, 1092–1118.

Buhrmester, Michael, Tracy Kwang, and Samuel D. Gosling. 2011. "Amazon's Mechanical Turk. A New Source of Inexpensive, Yet High-Quality, Data?" *Perspectives on Psychological Science*, 6(1): 3–5.

Campbell, Angus. 1960. "Surge and Decline: A Study of Electoral Change." *Public Opinion Quarterly*, 24(3): 397.

Cappelen, Alexander W., Thomas De Haan, and Bertil Tungodden. 2020. "Fairness and limited information: Are people Bayesian meritocrats?" *Working Paper*.

Cappelen, Alexander W., Ranveig Falch, Erik Ø. Sørensen, and Bertil Tungodden. 2020. "Solidarity and Fairness in Times of Crisis." *Working Paper*.

Cappelen, Alexander W., Ingar K. Haaland, and Bertil Tungodden. 2019. "Beliefs About Behavioral Responses to Taxation." *Working Paper*.

Cappelen, Alexander W., Astri D. Hole, Erik Ø. Sørensen, and Bertil Tungodden. 2007. "The Pluralism of Fairness Ideals: An Experimental Approach." *American Economic Review*, 97(3): 818–27.

Cappelen, Alexander W., James Konow, Erik Ø. Sørensen, and Bertil Tungodden. 2013. "Just Luck. An Experimental Study of Risk-Taking and Fairness." *American Economic Review*, 103(4): 1398–413.

Cappelen, Alexander W., Karl O. Moene, Siv-Elisabeth Skjelbred, and Bertil Tungodden. 2017. "The Merit Primacy Effect." *Working Paper*.

Castagnetti, Alessandro, and Renke Schmacker. 2021. "Protecting the Ego: Motivated Information Selection and Updating." *Working Paper*.

Charness, Gary, Aldo Rustichini, and Jeroen van de Ven. 2018. "Self-confidence and strategic behavior." *Experimental Economics*, 21(1): 72–98.

Charness, Gary, Uri Gneezy, and Vlastimil Rasoscha. 2020. "Experimental Methods: Eliciting Be-liefs." *Journal of Economic Behavior & Organization*, forthcoming.

Chen, Daniel L., Martin Schonger, and Chris Wickens. 2016. "oTree—an Open-Source Platform for Laboratory, Online, and Field Experiments." *Journal of Behavioral and Experimental Finance*, 9: 88–97.

Chetty, Raj, John N. Friedman, Emmanuel Saez, Nicholas Turner, and Danny Yagan. 2020. "Income Segregation and Intergenerational Mobility Across Colleges in the United States." *Quarterly Journal of Economics*, 135(3): 1567–633.

Chetty, Raj, David Grusky, Maximilian Hell, Nathaniel Hendren, Robert Manduca, and Jimmy Narang. 2017. "The Fading American Dream: Trends in Absolute Income Mobility Since 1940." *Science*, 356(6336): 398–406.

Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. 2014. "Where Is the Land of Opportunity? the Geography of Intergenerational Mobility in the United States." *Quarterly Journal of Economics*, 129(4): 1553–623.

Chmielewski, Michael, and Sarah C. Kucker. 2020. "An MTurk Crisis? Shifts in Data Quality and the Impact on Study Results." *Social Psychological and Personality Science*, 11(4): 464–73.

Cohn, Alain, Lasse J. Jessen, Marko Klasnja, and Paul Smeets. 2019. "Why Do the Rich Oppose Redistribution? An Experiment with America's Top 5%." *Working Paper*.

Congdon, William, Jeffrey Kling, and Sendhil Mullainathan. 2009. "Behavioral Economics and Tax Policy." *NBER Working Paper*.

Coppock, Alexander, and Oliver A. McClellan. 2019. "Validating the Demographic, Political, Psychological, and Experimental Results Obtained From a New Source of Online Survey Respondents." *Research & Politics*, 6(1): 205316801882217.

Corak, Miles. 2006. "Do Poor Children Become Poor Adults? Lessons from a Cross-Country Comparison of Generational Earnings Mobility." *Research on Economic Inequality*, 13: 143–88.

Croson, Rachel, and James Konow. 2009. "Social Preferences and Moral Biases." *Journal of Economic Behavior & Organization*, 69(3): 201–12.

Cruces, Guillermo, Ricardo Perez-Truglia, and Martin Tetaz. 2013. "Biased Perceptions of Income Distribution and Preferences for Redistribution. Evidence from a survey experiment." *Journal of Public Economics*, 98: 100–12.

Cullen, Zoë, and Ricardo Perez-Truglia. 2018. "How Much Does Your Boss Make? The Effects of Salary Comparisons." *Working Paper*.

Danz, David, Lise Vesterlund, and Alistair Wilson. 2020. "Belief Elicitation: Limiting Truth Telling with Information on Incentives." *NBER Working Paper*.

Davidai, Shai, and Thomas Gilovich. 2016. "The Headwinds/Tailwinds Asymmetry: An Availability Bias in Assessments of Barriers and Blessings." *Journal of Personality and Social Psychology*, 111(6): 835–51.

Deffains, Bruno, Romain Espinosa, and Christian Thöni. 2016. "Political Self-Serving Bias and Redistribution." *Journal of Public Economics*, 134: 67–74.

de Quidt, Jonathan, Johannes Haushofer, and Christopher Roth. 2018. "Measuring and Bounding Experimenter Demand," *American Economic Review*, 108 (11): 3266–3302.

Di Tella, Rafael, Ricardo Perez-Truglia, Andres Babino and Mariano Sigman. 2015. "Conveniently Upset: Avoiding Altruism by Distorting Beliefs About Others' Altruism." *American Economic Review*, 105(11): 3416–3442.

Dunn, Amina. 2018. *Partisans Are Divided Over the Fairness of the U.S. Economy – and Why People Are Rich or Poor*. https://www.pewresearch.org/fact-tank/2018/10/04/partisans-are-divided-over-the-fairness-of-the-u-s-economy-and-why-people-are-rich-or-poor/ (accessed May 22, 2020).

Durante, Ruben, Louis Putterman, and Joël van der Weele. 2014. "Preferences for Redistribution and Perception of Fairness an Experimental Study." *Journal of the European Economic Association*, 12(4): 1059–86.

Engelmann, Dirk, and Martin Strobel. 2004. "Inequality Aversion, Efficiency, and Maximin Prefer-ences in Simple Distribution Experiments." *American Economic Review*, 94(4): 857–69.

Fehr, Dietmar, Johanna Mollerstrom, and Ricardo Perez-Truglia. 2019. "Your Place in the World. The Demand for National and Global Redistribution." *NBER Working Paper*.

Fehr, Dietmar, Daniel Muller, and Marcel Preuss. 2020. "Social Mobility Perceptions and Inequality Acceptance." *Working Paper*.

Festinger, Leon. 1957. *A Theory of Cognitive Dissonance*: Stanford University Press.

Fisman, Raymond, Pamela Jakiela, and Shachar Kariv. 2017. "Distributional Preferences and Political Behavior." *Journal of Public Economics*, 155: 1–10.

Fong, Christina. 2001. "Social Preferences, Self-Interest, and the Demand for Redistribution." *Journal of Public Economics*, 82(2): 225–46.

Frank, Robert H. 2016. *Success and Luck. Good Fortune and the Myth of Meritocracy*. Princeton: Princeton University Press.

Fuster, Andreas, Ricardo Perez-Truglia, and Basit Zafar. 2018. "Expectations With Endogenous Information Acquisition. An Experimental Investigation." *Working Paper*.

Gauriot, Romain, and Lionel Page. 2019. "Fooled by Performance Randomness: Overrewarding Luck." *Review of Economics and Statistics*, 101(4): 658–66.

Grigorieff, Alexis, Christopher Roth, and Diego Ubfal. 2020. "Does Information Change Attitudes Toward Immigrants?" *Demography*, 57(3): 1117–43.

Gromet, Dena M., Kimberly A. Hartson, and David K. Sherman. 2015. "The Politics of Luck. Political Ideology and the Perceived Relationship Between Luck and Success." *Journal of Experimental Social Psychology*, 59: 40–46.

Hara, Kotaro, Abigail Adams, Kristy Milland, Saiph Savage, Chris Callison-Burch, and Jeffrey P. Bigham. 2018. "*A Data-Driven Analysis of Workers' Earnings on Amazon Mechanical Turk*." In Proceedings of the 2018 CHI Conference on Human Factors in Computing Systems, 1–14.

Hardisty, David J., Eric J. Johnson, and Elke U. Weber. 2010. "A Dirty Word or a Dirty World?: Attribute Framing, Political Affiliation, and Query Theory." *Psychological Science*, 21(1): 86–92.

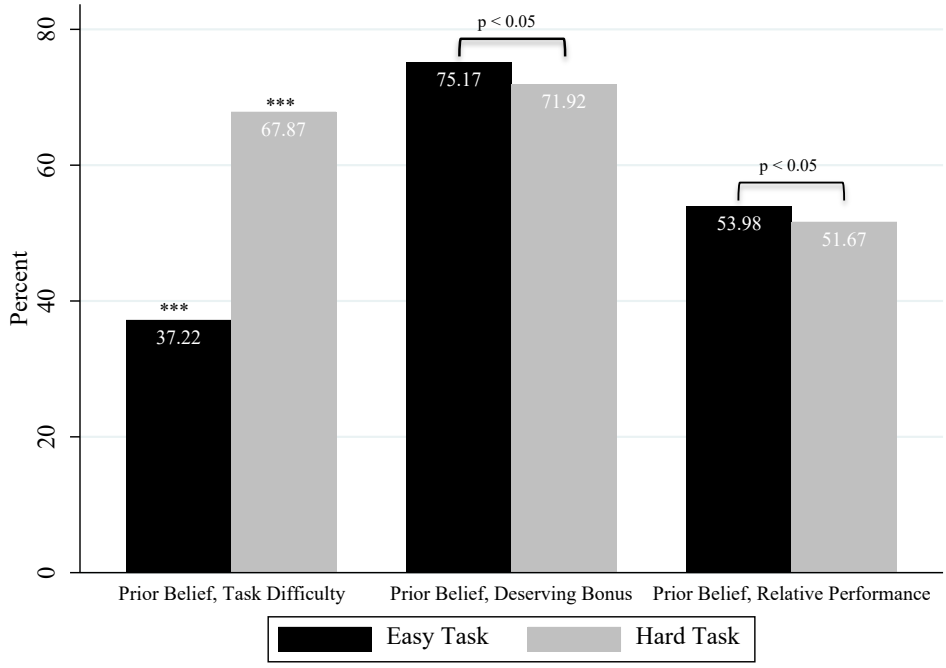
Horton, John J., David G. Rand, and Richard J. Zeckhauser. 2011. "The Online Laboratory: Conducting Experiments in a Real Labor Market." *Experimental Economics*, 14(3): 399–425.

Karadja, Mounir, Johanna Mollerstrom, and David Seim. 2017. "Richer (and Holier) Than Thou? The Effect of Relative Income Improvements on Demand for Redistribution." *Review of Economics and Statistics*, 99(2): 201–12.

Konow, James. 2000. "Fair Shares. Accountability and Cognitive Dissonance in Allocation Decisions." *American Economic Review*, 90(4): 1072–91.
Konow, James. 2009. "Is Fairness in the Eye of the Beholder? An Impartial Spectator Analysis of Justice." *Social Choice and Welfare*, 33(1): 101–27.
Köszegi, Botond. 2006. "Ego Utility, Overconfidence, and Task Choice." *Journal of the European Economic Association*, 4(4): 673–707.
Kuziemko, Ilyana, Michael I. Norton, Emmanuel Saez, and Stefanie Stantcheva. 2015. "How Elastic Are Preferences for Redistribution? Evidence from Randomized Survey Experiments." *American Economic Review*, 105(4): 1478–508.
Loewenstein, George, Samuel Issacharoff, Colin Camerer, and Linda Babcock. 1993. "Self-Serving Assessments of Fairness and Pretrial Bargaining." *Journal of Legal Studies*, 22(1): 135–59.
Mijs, Jonathan J. B. 2019. "The Paradox of Inequality. Income Inequality and Belief in Meritocracy Go Hand in Hand." *Socio-Economic Review*, 108: 521.
Mummolo, Jonathan and Erik Peterson, 2019. "Demand Effects in Survey Experiments: An Empirical Assessment," *American Political Science Review*, 113 (2) 517–529.
Pew Research Center. 2019. *Growing Partisan Divide Over Fairness of the Nation's Tax System*. https://www.people-press.org/2019/04/04/growing-partisan-divide-over-fairness-of-the-nations-tax-system/ (accessed April 21, 2020).
Reed, W. Robert. 2006. "Democrats, Republicans, and Taxes. Evidence that Political Parties Matter." *Journal of Public Economics*, 90(4-5): 725–50.
Schwardmann, Peter, and Van der Weele, Joel. 2019. "Deception and self-deception." *Nature Human Behaviour*, 3 (10): 1055–1061.
Snowberg, Erik, and Leeat Yariv. 2020. "Testing the Waters: Behavior Across Participant Pools." *American Economic Review*, forthcoming.
Trautmann, Stefan T., and Gijs van de Kuilen. 2015. "Belief Elicitation: A Horse Race among Truth Serums." *Economic Journal*, 125(589): 2116–35.
Wahlund, Richard. 1992. "Tax Changes and Economic Behavior: The Case of Tax Evasion." *Journal of Economic Psychology*, 13(4): 657–77.
Zimmermann, Florian. 2020. "The dynamics of motivated beliefs." *American Economic Review*, 110 (2): 337–361.

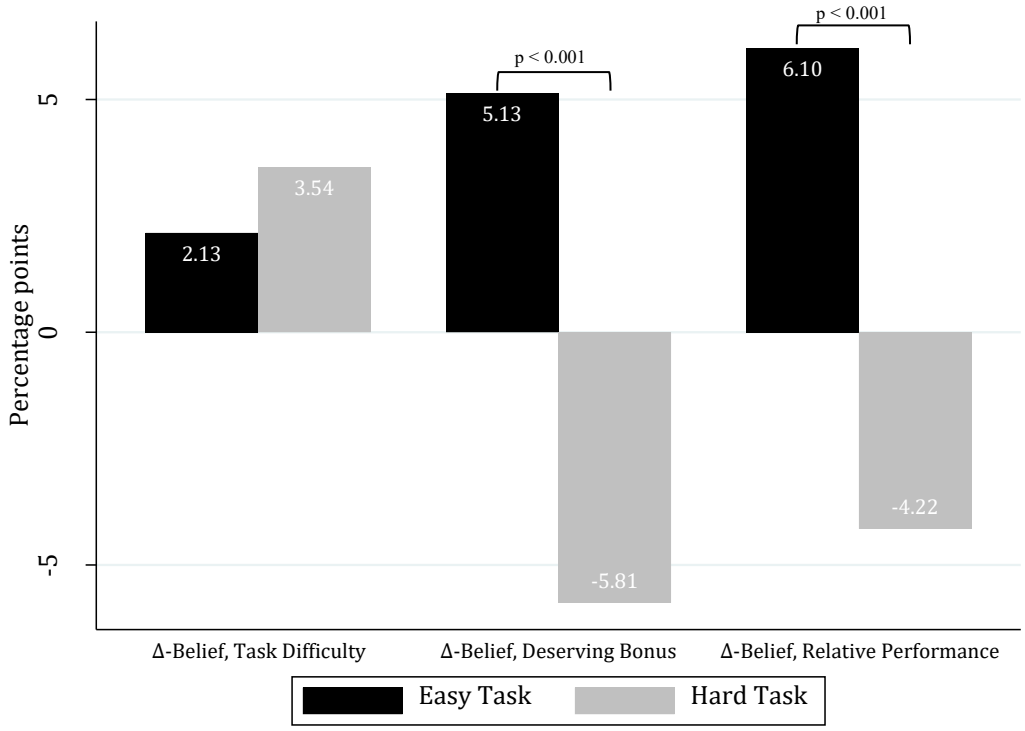
Figures and Tables

Figure 1. Prior Beliefs by Treatment



Notes: The Figure shows prior beliefs about task difficulty, deservingness, and relative performance 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 *hard task* in %; "Prior Belief, Deserving Bonus": deserving the $2-bonus payment in %; "Prior, Belief Relative Performance": perceived number of participants performing the same task with a lower score. *** indicates significant difference from 50% at the 1% level. All p-values based on t-tests.

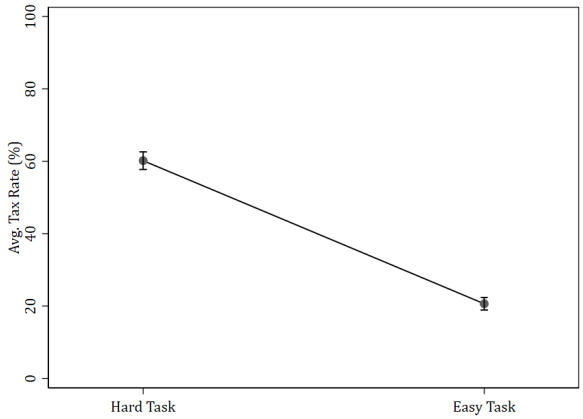
Figure 2. Treatment Effect on Beliefs



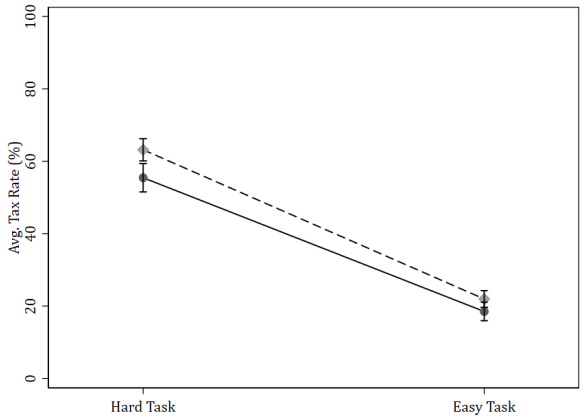
Notes: The Figure shows the difference between posterior and prior beliefs about task difficulty, deservingness, and performance in the two conditions. All beliefs are measured on a scale from 0 – 100: "Δ-Belief, Task Difficulty": likelihood of performing in the *hard task* in %; "Δ-Belief, Deserving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

Figure 3. Tax Rate by Treatment and Political Orientation

a. Treatment effect

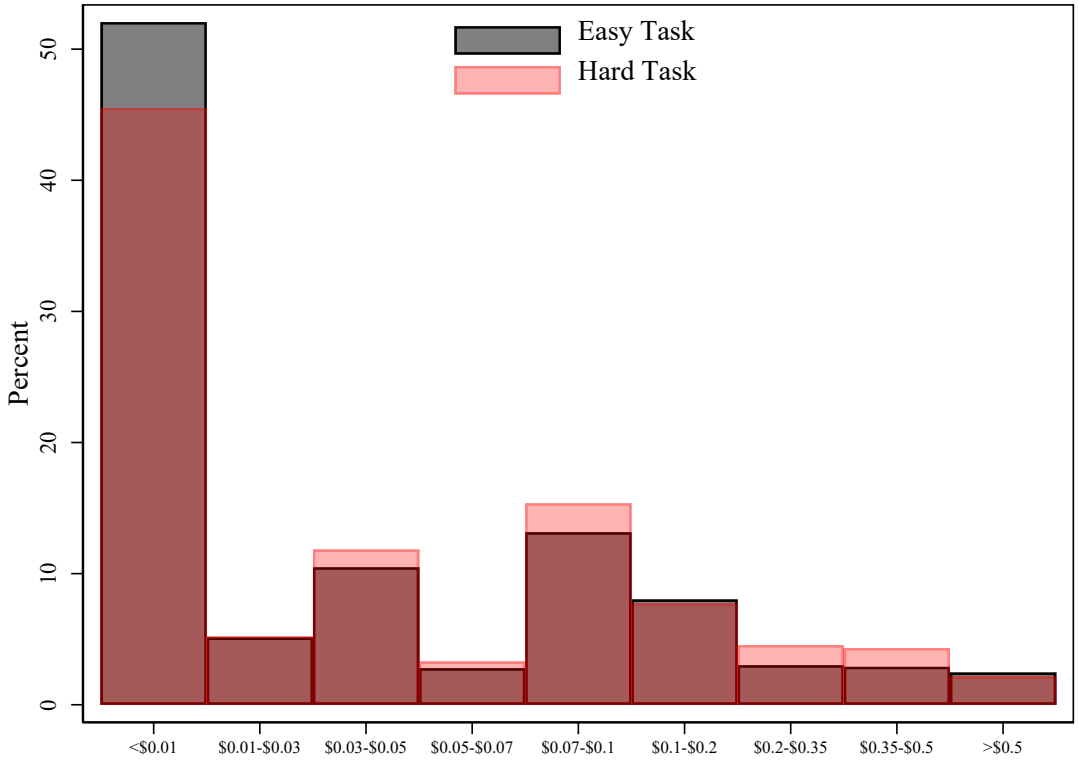


b. Split by political orientation



Notes: The Figure shows the average tax rate across the different conditions. Panel a. displays average tax rates across treatments (*hard task* and *easy task*) and panel b. shows the average tax rates across conditions split by political orientation: conservatives (solid black line) and liberals (dashed light-gray line). Error bars denote 95% confidence interval.

Figure 4. Willingness-to-Pay for Information on Task Difficulty



Notes: The figure shows the distribution of respondents' willingness to pay (WTP) for information about the task difficulty (using all participants with consistent answers: *N=1,776*), separated by condition. The grey bars indicate the WTP in the *easy task* and the overlaying rose 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. Comparison of Exogenous Task Difficulty (Treatment)

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

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

Table 2. Regression: Change in Beliefs (Posterior – Prior)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable:	Δ - Belief Task Difficulty			Δ-Belief Deserving Bonus			Δ - Belief Relative Performance			Effort Determines Success		
Easy task	-1.404	-0.703	-0.745	10.943***/#	11.418***/#	11.150***/#	10.324***/#	8.571***/#	8.404***/#	16.213***/#	16.358***/#	16.465***/#
	(0.934)	(1.374)	(1.373)	(0.882)	(1.478)	(1.472)	(0.713)	(1.167)	(1.173)	(1.355)	(2.123)	(2.126)
Liberal		-0.269	0.001		0.619	0.221		-0.898	-0.897		-3.905*	-4.014*
		(1.349)	(1.370)		(1.496)	(1.493)		(1.069)	(1.082)		(2.184)	(2.216)
Liberal x *Easy task*		-1.130	-1.007		-0.776	-0.620		2.844*	2.995**		-0.185	0.0813
		(1.859)	(1.867)		(1.840)	(1.841)		(1.474)	(1.485)		(2.752)	(2.751)
Constant	3.538***	3.703***	-12.65*	-5.811***	-6.190***	-8.671	-4.223***	-3.674***	-1.402	54.054***	56.439***	40.207***
	(0.683)	(0.979)	(7.446)	(0.710)	(1.221)	(7.064)	(0.530)	(0.809)	(5.827)	(1.072)	(1.691)	(10.422)
Observations	1,825	1,825	1,822	1,825	1,825	1,822	1,825	1,825	1,822	1,825	1,825	1,822
Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
R-squared	0.001	0.002	0.010	0.078	0.078	0.084	0.103	0.105	0.112	0.073	0.077	0.091

Notes: OLS-Regression 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, Deserving Bonus": deserving the $2-bonus payment in %; "Prior Belief, Relative Performance": perceived number of participants performing the same task with a lower score; "Effort Determines Success": likelihood that the $2-bonus payment depends on her exerted effort in %. "*Easy task*" is an indicator for random assignment to the *easy task*. "Liberal" is an indicator for respondents 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<0.01, ** p<0.05, * p<0.1; # indicates significance, when using the adaptive 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: Tax Rate and Political Views

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable:			Tax Rate			
Easy task	-39.543***	-39.445***	-39.588***	-39.486***	-36.959***	-36.730***
	(1.519)	(1.528)	(1.513)	(1.523)	(2.381)	(2.409)
Liberal			5.586***	5.787***	7.729***	8.010***
			(1.540)	(1.608)	(2.535)	(2.561)
Liberal x *Easy task*					-4.276	-4.476
					(3.082)	(3.108)
Constant	60.165***	72.095***	56.753***	67.010***	55.445***	66.226***
	(1.237)	(11.360)	(1.560)	(11.43)	(1.994)	(11.454)
Observations	1,825	1,822	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes	No	Yes
R-squared	0.272	0.277	0.277	0.282	0.278	0.283

Notes: OLS-Regression with robust standard errors in parentheses. "Tax Rate" is the redistribution rate of the $2-bonus payment in percent (0-100). "*Easy task*" is an indicator for respondents randomly assigned to the *easy task*. "Liberal" is an indicator for respondents 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<0.01, ** p<0.05, * p<0.1

Table 4. Regression: Tax Rate and Beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Variable				Tax Rate			
Easy task	-39.543***	-39.445***	-44.599***	-27.695***	-16.049***	-6.828	3.196
	(1.519)	(1.528)	(3.765)	(3.792)	(3.751)	(4.388)	(5.302)
Prior Belief, Task Difficulty				0.176***			0.123**/#
				(0.048)			(0.049)
Easy task x Prior Belief, Task Difficulty				-0.171***			-0.091
				(0.057)			(0.059)
Prior Belief, Relative Performance					0.256***		0.126**
					(0.051)		(0.064)
Easy task x Prior Belief, Relative Performance					-0.444***		-0.226***/#
					(0.065)		(0.079)
Prior Belief, Deserving Bonus						0.228***	0.143***/#
						(0.042)	(0.053)
Easy task x Prior Belief, Deserving Bonus						-0.444***	-0.319***/#
						(0.056)	(0.069)
Effort Determines Success			-0.219***				
			(0.039)				
Easy task x Effort Determines Success			0.125**				
			(0.055)				
Constant	60.165***	72.095***	81.631***	60.496***	57.002***	51.022***	42.71***
	(1.237)	(11.360)	(11.537)	(11.690)	(11.618)	(11.614)	(11.91)
Observations	1,825	1,822	1,822	1,822	1,822	1,822	1,822
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.272	0.277	0.296	0.284	0.303	0.297	0.310

Notes: OLS-Regression with robust standard errors in parentheses. "Tax Rate" is measured in percent (0-100 percent). "*Easy task*" is an indicator for random assignment 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 *hard task* in %; "Prior Belief, Deserving Bonus": deserving the $2-bonus payment in %; "Prior Belief, Relative Performance": perceived number of participants performing the same task with a lower score; "Effort Determines Success": likelihood that the $2-bonus payment depends on her exerted effort 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<0.01, ** p<0.05, * p<0.1; # indicates significance, when using the adaptive linear step-up procedure by Benjamini, Krieger, and Yekutieli (2006) that controls for a false discovery rate at q=0.05 in column (6)

Table 5. Regression: Willingness to Pay for Information

	(1)	(2)	(3)	(4)
Dep. Variable:			WTP	
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	1,776	1,773	1,776	1,773
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. "WTP" is the willingness to pay for receiving information about the task difficulty and the score of the other participant. The variable is categorized in 9 intervals [0¢,1¢]; [1¢,3¢]; [3¢,5¢]; [5¢,7¢]; [7¢,10¢]; [10¢,20¢]; [20¢,35¢]; [35¢,50¢]; [50¢,inf). "*Easy task*" is an indicator for respondents randomly assigned to the *easy task* (treatment). "Liberal" is an indicator for respondents 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, Midwest, West).
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Regression: Revising Tax Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Revising Tax Rate=1				Change in Tax Rate			
Received info	0.078**	0.081**	0.009	0.024	3.787***	3.944***	4.495	4.782
	(0.036)	(0.036)	(0.072)	(0.072)	(1.231)	(1.253)	(3.376)	(3.374)
Easy task			-0.093	-0.078			-4.617**	-4.522**
			(0.06)	(0.06)			(2.087)	(2.042)
Liberal			-0.092*	-0.103*			-4.017**	-4.455**
			(0.055)	(0.057)			(1.954)	(2.003)
Easy task x			0.051	0.042			2.935	3.211
Liberal			(0.075)	(0.074)			(2.376)	(2.326)
Easy task			0.026	0.001			-1.216	-2.085
x Received info			(0.098)	(0.097)			(3.941)	(3.932)
Liberal x			0.084	0.076			-1.417	-1.225
Received info			(0.086)	(0.086)			(3.599)	(3.600)
Easy task x			0.003	0.026			2.567	3.178
Liberal x Received info			(0.123)	(0.122)			(4.525)	(4.519)
WTP	0.313**	0.278*	0.309**	0.271*	4.032	1.858	2.799	0.716
	(0.152)	(0.149)	(0.153)	(0.150)	(4.829)	(4.984)	(4.772)	(4.889)
Constant	0.285***	0.609***	0.374***	0.676***	4.647***	2.170	8.635***	5.833
	(0.018)	(0.221)	(0.045)	(0.226)	(0.534)	(6.809)	(1.690)	(6.953)
Observations	1,096	1,094	1,096	1,094	1,096	1,094	1,096	1,094
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.020	0.047	0.027	0.054	0.019	0.035	0.037	0.054

Notes: OLS-Regression with robust standard errors in parentheses. The sample includes all participants who had the opportunity to revise their initial tax decision, i.e. all participants who received information about the task difficulty and a random subset of participants who did not receive this information. "Revising Tax Rate=1" is an indicator for revising the initially chosen tax rate and "Change in Tax Rate" is the absolute difference between the initial and revised tax rate. "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* (treatment) and "Liberal" is an indicator for participants who self-identified as strongly liberal, moderately liberal and slightly liberal. "WTP" is the willingness to pay for receiving information about the task difficulty and the score of the other participant. The variable is categorized in 9 intervals [0¢,1¢]; [1¢,3¢]; [3¢,5¢]; [5¢,7¢]; [7¢,10¢]; [10¢,20¢]; [20¢,35¢]; [35¢,50¢]; [50¢,inf). 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<0.01, ** p<0.05, * p<0.1

Table 7. Regression: Misperception about Task Difficulty and Revising Tax Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Variable	Revising Tax Rate=1					Change in Tax Rate				
	All	All	All	Liberal	Conservative	All	All	All	Liberal	Conservative
Misperception x	0.001	0.000	0.000	0.001	-0.000	0.048*	0.048*	0.046*	0.081**	0.004
Received Info	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.026)	(0.026)	(0.026)	(0.032)	(0.045)
Misperception	0.002***	0.002***	0.002***	0.001*	0.002***	0.003	0.001	0.011	-0.017	0.044
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.017)	(0.017)	(0.016)	(0.016)	(0.033)
Easy task			-0.084***	-0.055	-0.112**			-3.160***	-1.329	-5.279***
			(0.029)	(0.036)	(0.047)			(0.896)	(0.985)	(1.750)
WTP	0.380***	0.361***	0.343**	0.502***	0.130	7.981*	6.432	5.765	4.831	6.998
	(0.141)	(0.138)	(0.137)	(0.189)	(0.204)	(4.466)	(4.515)	(4.449)	(5.070)	(7.773)
Constant	0.253***	0.567***	0.595***	0.360	0.790**	5.164***	4.169	5.239	-5.223	16.93
	(0.022)	(0.218)	(0.218)	(0.278)	(0.368)	(0.709)	(6.839)	(6.850)	(7.475)	(13.40)
Observations	1,130	1,128	1,128	694	434	1,130	1,128	1,128	694	434
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
R-squared	0.028	0.051	0.059	0.066	0.083	0.014	0.029	0.039	0.051	0.058

Notes: OLS-Regression with robust standard errors in parentheses. The sample includes all participants who had the opportunity to revise their initial tax decision, i.e. all participants who received information about the task difficulty and a random subset of participants who did not receive this information. "Revising Tax Rate=1" is an indicator for revising the initially chosen tax rate and "Change in Tax Rate" is the absolute difference between initial and revised tax rate. "Misperception" indicates the difference between the actual task difficulty and the posterior belief about task difficulty in %. "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* (treatment). "WTP" is the willingness to pay for receiving information about the task difficulty and the score of the other participant. The variable is categorized in 9 intervals [0¢,1¢]; [1¢,3¢]; [3¢,5¢]; [5¢,7¢]; [7¢,10¢]; [10¢,20¢]; [20¢,35¢]; [35¢,50¢]; [50¢,inf). 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<0.01, ** p<0.05, * p<0.1

Online Appendix

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

Dietmar Fehr and Martin Vollmann

Heidelberg University

A1. 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)

A2. Locus-of-Control Module

A person's locus of control describes the degree to which they feel to have control over the outcomes in their life. We elicit locus of control (LoC) with a 7-item battery (Cobb-Clark and

Schurer 2013), and 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).

a. "I have little control over the things that happen to me."
b. "There is really no way I can solve some of the problems I have."
c. "There is little I can do to change many of the important things in my life."
d. "I often feel helpless in dealing with the problems of life."
e. "Sometimes I feel that I'm being pushed around in life."
f. "What happens to me in the future mostly depends on me."
g. "I can do just about anything I really set my mind to do."
(7-point scale; Disagree strongly – Agree strongly)

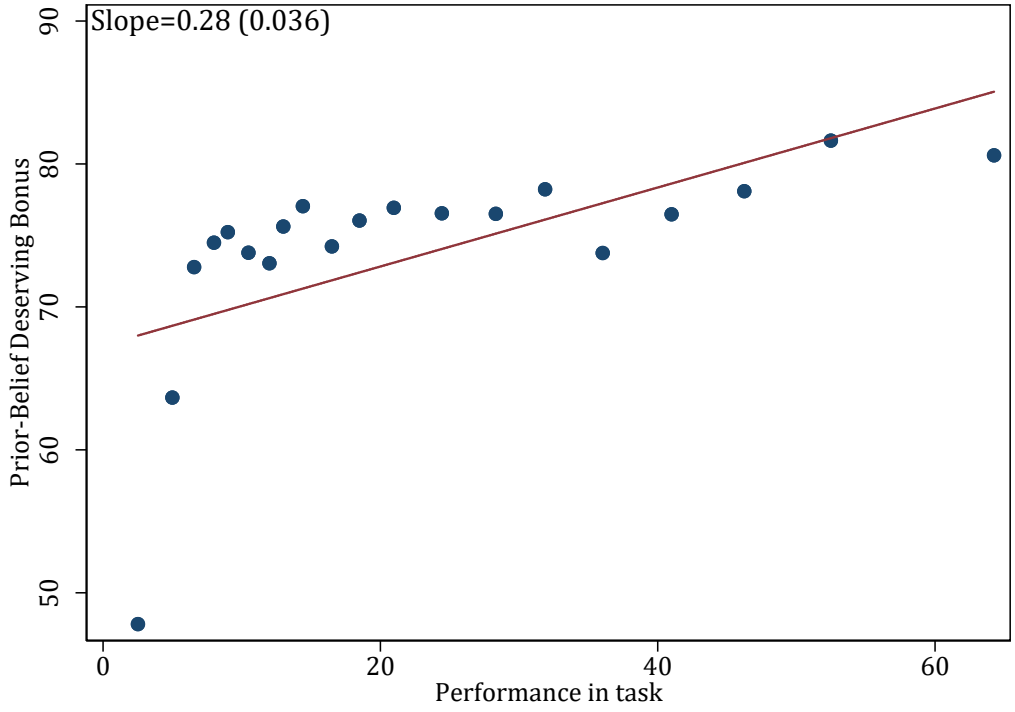
Calculating the combined locus of control index (L-o-C-Index) by summing responses to the five external items (a - e), subtracting the sum of responses to the two internal items (f - g) and adding 16. Specifically,

$$L - o - C - Index_i = \sum_{j=a}^{e} ELOC_{i,j} - \sum_{j=f}^{g} ILOC_{i,j} + 16$$

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

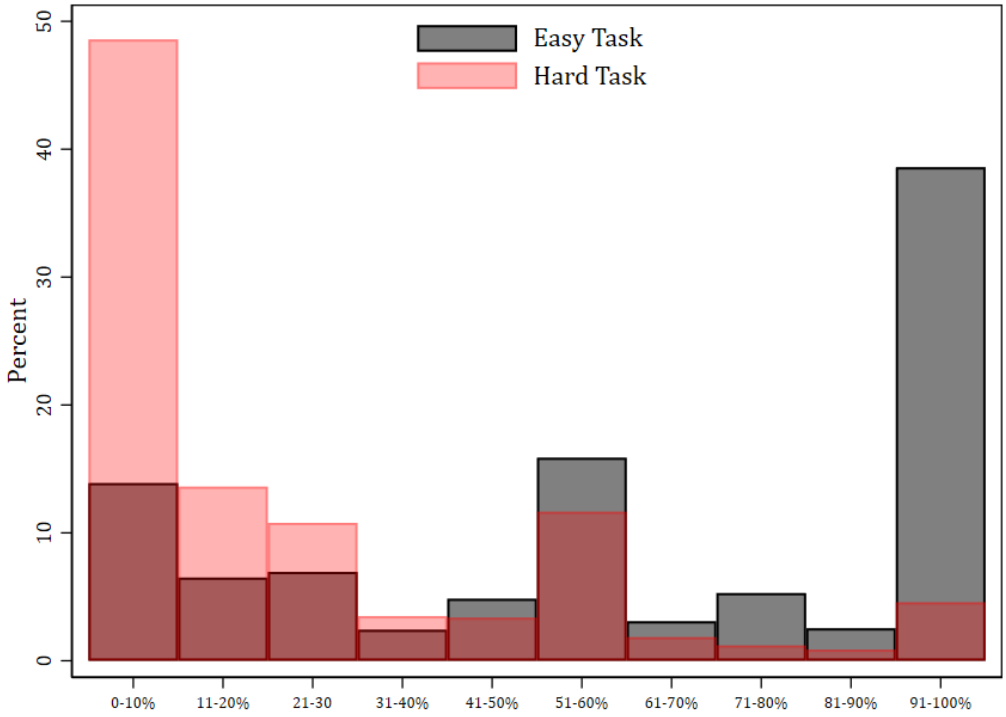
A3. Additional Figures

Figure A1: Relationship between Task Performance and Deservingness of Bonus



Notes: Binned scatterplot showing the relationship between task performance and perceived deservingness of the bonus (Prior-Belief Deserving Bonus). Coefficient estimate on performance with standard error in parentheses in the top-left corner. Estimate based on whole sample (*N=1,825*).

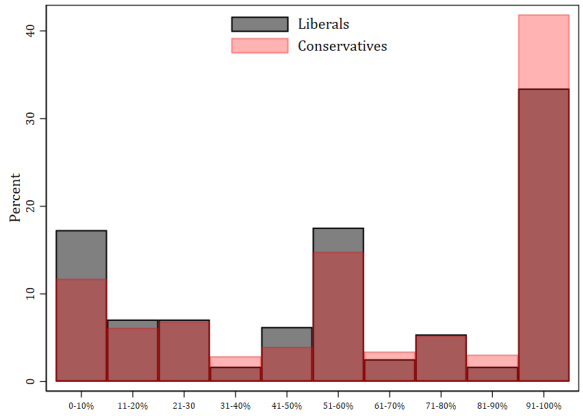
Figure A2: Distribution of Tax Rates in Easy and Hard Task



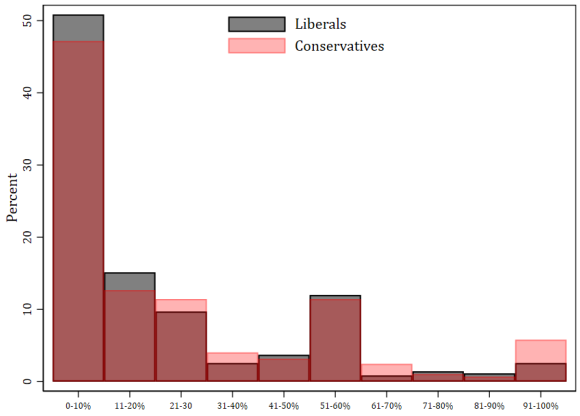
Notes: Histograms showing the distribution of tax decision in steps of 10% separated by *Easy* and *Hard task* (*N=1,825*).

Figure A3: Distribution of Tax Rates by Political Orientation

a. Hard Task



b. Easy Task



Notes: Histograms showing the distribution of tax decision in steps of 10% separated by Political Orientation. The left panel shows the distribution for the *Hard* Task and the right panel for the *Easy* Task.

A4. Additional Tables

Table A1. Regression: Dropout on Easy Task

	(1)	(2)
Dep. Variable	Dropout	Dropout
Easy task	-0.016	-0.013
	(0.013)	(0.009)
Constant	-0.014***	0.044***
	(0.010)	(0.007)
Observations	2,027	1,987
Controls	No	No
R-squared	0.001	0.001

Notes: OLS-Regression with robust standard errors in parentheses. "*Easy task*" is an indicator for respondents randomly assigned to the *easy task* (treatment). Column 2 only considers dropouts after participants learned about the bonus assignment.

*** p<0.01, ** p<0.05, * p<0.1

Table A2. Balance between No-Dropouts and Dropouts

	No-Dropouts (n=1825)		Dropouts (n = 202)		
Variables	**Mean**	**S.D.**	**Mean**	**S.D.**	p-value
L-o-C-Index	20.95	9.16	21.88	9.94	0.17
Age (in years)	39.17	12.41	37.65	11.66	0.09
Female (in %)	52.44	49.95	45.05	49.88	0.05
White (in %)	76.66	42.31	73.27	44.37	0.28
Married (in %)	45.21	49.78	41.58	49.41	0.33
People in Household	2.66	1.42	2.70	1.37	0.72
Full-Time Employed (in %)	61.37	48.70	67.82	46.83	0.07
Part-Time Employed (in %)	11.34	31.72	11.39	31.84	0.99
Self-Employed (in %)	11.12	31.45	8.91	28.56	0.34
Not-in-Labor-Force (in %)	9.75	29.67	5.94	23.70	0.08
Income (in $)	64,784	42,589	62,203	40,993	0.41
Strongly Liberal (in %)	18.14	38.54	15.84	36.60	0.71
Moderately Liberal (in %)	22.30	41.64	24.75	43.26	0.43
Slightly Liberal (in %)	21.04	40.77	21.29	41.04	0.94
Slightly Conservative (in %)	20.27	40.22	19.31	39.57	0.75
Moderately Conservative (in %)	12.66	33.26	13.86	34.64	0.63
Strongly Conservative (in %)	5.59	22.98	4.95	21.75	0.38
Democrats (in %)	52.88	49.93	54.46	49.92	0.67
Republicans (in %)	28.27	45.05	25.74	43.83	0.45
No/ Other Political Party (in %)	18.85	39.12	19.80	39.95	0.74
Northeast Region (in %)	19.04	39.28	21.78	41.38	0.35
South Region (in %)	38.36	48.64	37.62	48.56	0.84
Midwest Region (in %)	20.75	40.56	18.81	39.18	0.52
West Region (in %)	21.84	41.33	21.78	41.38	0.98
Only High school Degree (in %)	8.98	28.61	7.43	26.28	0.46
Only Some College (in %)	24.27	42.89	21.29	41.04	0.35
2-Year College Degree (in %)	12.22	32.76	12.38	33.01	0.95
4-Year College Degree (in %)	38.36	48.64	37.38	45.05	0.06
Master Degree (in %)	12.22	32.76	11.39	31.84	0.73
Doctoral/ Professional Degree (in %)	3.67	18.81	1.98	13.97	0.22

Notes: The L-o-C-Index is a measure for locus of control (for details see main text or Appendix). The last column presents p-values from separate OLS regressions of the form $y_i = \beta_0 + \beta_0 * covariate + \varepsilon_i$, where y_i is a treatment indicator. The F-statistic from a joint significance test of all covariates is 0.83 (p-value =0.727).

Table A3. Summary Statistics and Balance between *Easy* and *Hard task*

	All (n=1825)		***Hard task***		***Easy task***		
Variables	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	p-value
L-o-C-Index	20.95	9.16	21.16	9.12	20.74	9.20	0.34
Age (in years)	39.17	12.41	39.30	12.40	39.04	12.42	0.66
Female (in %)	52.44	49.95	53.14	49.91	51.74	50.00	0.55
White (in %)	76.66	42.31	75.74	42.88	77.56	41.74	0.36
Married (in %)	45.21	49.78	46.97	49.94	43.46	49.60	0.13
People in Household	2.66	1.42	2.63	1.37	2.69	1.48	0.33
Full-Time Employed (in %)	61.37	48.70	61.63	48.66	61.11	48.78	0.82
Part-Time Employed (in %)	11.34	31.72	10.80	31.06	11.87	32.37	0.47
Self-Employed (in %)	11.12	31.45	12.57	33.17	9.69	29.61	0.05
Not-in-Labor-Force (in %)	9.75	29.67	9.59	29.46	9.91	29.90	0.82
Income (in $)	64,784	42,589	64,388	41,709	65,089	43,241	0.73
Strongly Liberal (in %)	18.14	38.54	17.64	38.14	18.63	38.95	0.58
Moderately Liberal (in %)	22.30	41.64	21.50	41.10	23.09	42.17	0.41
Slightly Liberal (in %)	21.04	40.77	21.94	41.41	20.15	40.14	0.35
Slightly Conservative (in %)	20.27	40.22	19.96	39.99	20.59	40.46	0.74
Moderately Conservative (in %)	12.66	33.26	12.90	33.54	12.42	33.00	0.76
Strongly Conservative (in %)	5.59	22.98	6.06	23.88	5.12	22.05	0.38
Democrats (in %)	52.88	49.93	52.70	49.95	53.05	49.93	0.88
Republicans (in %)	28.27	45.05	28.34	45.09	28.21	45.03	0.95
No/ Other Political Party (in %)	18.85	39.12	18.96	39.22	18.74	39.04	0.90
Northeast Region (in %)	19.04	39.28	20.40	40.32	17.65	38.14	0.13
South Region (in %)	38.36	48.64	38.04	48.57	38.56	48.70	0.82
Midwest Region (in %)	20.75	40.56	20.18	40.15	21.24	40.92	0.57
West Region (in %)	21.84	41.33	21.28	40.95	22.33	41.67	0.59
Only High school Degree (in %)	8.98	28.61	9.59	28.61	8.39	27.74	0.37
Only Some College (in %)	24.27	42.89	23.70	42.55	24.84	43.23	0.57
2-Year College Degree (in %)	12.22	32.76	12.90	33.53	11.55	31.98	0.38
4-Year College Degree (in %)	38.36	48.64	37.38	48.41	39.32	48.87	0.39
Master Degree (in %)	12.22	32.76	12.23	32.79	12.20	32.75	0.98
Doctoral/ Professional Degree (in %)	3.67	18.81	4.19	20.04	3.16	17.50	0.24

Notes: The L-o-C-Index is a measure for locus of control (for details see main text or Appendix). The last column presents p-values from separate OLS regressions of the form $y_i = \beta_0 + \beta_0 * covariate + \varepsilon_i$, where y_i is a treatment indicator. The F-statistic from a joint significance test of all covariates is 1.09 (p-value =0.348).

Table A4. Comparison between Selected Experiment Demographics and U.S. Population

Variables	Experiment	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: The U.S. Population data was taken from the U.S. Census Bureau: Median age (2018)[1], Female (2019)[2], White (not Hispanic or Latino, 2018)[3], Married (2018)[4], People in Household (2019)[5], Median Household Income (2018)[6] , Bachelor's degree or higher (25 years age or over)(2018)[7], Region (Northeast, Midwest, West, South)(2019)[8]

[1] https://data.census.gov/cedsci/table?q=female&tid=ACSST1Y2018.S0101&vintage=2018&hidePreview=true(03.04.2020)
[2] https://www.census.gov/quickfacts/fact/table/US/LFE046218 (03.04.2020)
[3] https://www.census.gov/quickfacts/fact/table/US/LFE046218 (03.04.2020)
[4] https://data.census.gov/cedsci/table?q=S1201%3A%20MARITAL%20STATUS&tid=ACSST1Y2018.S1201&vintage=2018&hidePreview=true (03.04.2020)
[5] https://www.statista.com/statistics/183648/average-size-of-households-in-the-us/ (03.04.2020)
[6] https://data.census.gov/cedsci/table?q=median%20income&tid=ACSST1Y2018.S1903&t=Income%20%28Households,%20Families,%20Individuals%29&hidePreview=true&vintage=2018 (03.04.2020)
[7] https://data.census.gov/cedsci/table?q=education&tid=ACSST1Y2018.S1501&t=Education&vintage=2018&hidePreview=true (03.04.2020)
[8] https://www.census.gov/popclock/data_tables.php?component=growth (03.04.2020)

A5. IV-Estimates: Effect of the treatment on the treated

We identify the causal impact of economic success on meritocratic beliefs and redistributive taxes through the random assignment of participants to the *easy* and *hard task*. Recall that we calibrated the two tasks such that completing the *easy task* should result in a better performance than completing the *hard task*. Consequently, economic success should 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. In the following we present the effects of treatment on treated (i.e. the effect of receiving the bonus – economic success – on meritocratic beliefs and redistributive taxes) 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. This exercise reveals that the magnitude of the IV estimates presented here is very similar to the ITT estimates reported in the paper.

Table A5. IV-Regression: Change in Beliefs (Posterior - Prior)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable	Δ - Belief Task Difficulty			Δ-Belief Deserving Bonus			Δ - Belief Relative Performance			Effort Determines Success		
Economic Success	-1.579	-0.788	-0.832	12.31***/#	12.80***/#	12.47***/#	11.61***/#	9.610***/#	9.394***/#	18.23***/#	18.34***/#	18.42***/#
	(1.049)	(1.539)	(1.533)	(0.983)	(1.648)	(1.637)	(0.793)	(1.277)	(1.282)	(1.509)	(2.369)	(2.364)
Liberal		-0.195	0.0623		0.652	0.297		-1.096	-1.057		-3.918*	-3.965*
		(1.435)	(1.451)		(1.585)	(1.577)		(1.127)	(1.140)		(2.294)	(2.315)
Liberal x Economic Sucess		-1.277	-1.147		-0.813	-0.570		3.250**	3.485**		-0.120	0.285
		(2.086)	(2.090)		(2.049)	(2.045)		(1.630)	(1.640)		(3.067)	(3.054)
Constant	3.629***	3.747***	-12.76*	-6.517***	-6.915***	-8.479	-4.888***	-4.219***	-0.923	53.01***	55.40***	40.59***
	(0.727)	(1.042)	(7.400)	(0.753)	(1.295)	(6.987)	(0.559)	(0.855)	(5.728)	(1.127)	(1.774)	(10.37)
F-statistic first stage	6891.64	1057.52	1057.52	6891.64	1057.52	1057.52	6891.64	1057.52	1057.52	6891.64	1057.52	1057.52
Observations	1,825	1,825	1,822	1,825	1,825	1,822	1,825	1,825	1,822	1,825	1,825	1,822
Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
R-squared	0.002	0.003	0.011	0.095	0.095	0.100	0.122	0.120	0.128	0.089	0.093	0.107

Notes: Two-stage least squares (2SLS) regression with robust standard errors in parentheses. "Economic Success" is an indicator for the bonus payment and is instrumented by random treatment assignment. "Δ" is the difference between the posterior and the prior belief. The 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, Deserving Bonus": deserving the $2-bonus payment in %; "Prior Belief, Relative Performance": perceived number of participants performing the same task with a lower score; "Effort Determines Success": likelihood that the $2-bonus payment depends on her exerted effort in %. "Liberal" is an indicator for respondents 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, Midwest, West). Columns without interaction report the F-statistic of the first instrument ("*Easy task*") and columns with interaction report the F-Statistics of the second instrument ("Liberal x *Easy task*").

*** p<0.01, ** p<0.05, * p<0.1, # indicates significance, when using the adaptive 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 A6. IV-Regression: Tax Rate and Political Views

	(1)	(2)	(3)	(4)
Dep. Variable	Tax Rate			
Economic Success	-44.52***	-44.45***	-41.44***	-41.08***
	(1.645)	(1.651)	(2.581)	(2.604)
Liberal	5.540***	5.425***	8.064***	8.169***
	(1.490)	(1.543)	(2.597)	(2.612)
Liberal x Economic Success			-5.016	-5.492
			(3.347)	(3.366)
Constant	59.33***	66.23***	57.79***	64.97***
	(1.570)	(11.52)	(2.042)	(11.54)
F-statistic first stage	3444.26	660.40	1057.52	1057.52
Observations	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes
R-squared	0.323	0.328	0.323	0.328

Notes: Two-stage least squares (2SLS) regression with robust standard errors in parentheses. "Tax Rate" is the redistribution rate of the $2-bonus payment in percent (0-100 percent). "Liberal" is an indicator for respondents who self-identified as strongly liberal, moderately liberal and slightly liberal. "Economic Success" is an indicator for the bonus payment and is instrumented by random treatment assignment. 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 without interactions report the F-statistic of the first instrument ("*Easy task*") and columns with interaction report the F-Statistics of the second instrument ("Liberal x *Easy task*").

*** p<0.01, ** p<0.05, * p<0.1

Table A7. IV-Regression: Tax Rate and Beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Variable				Tax Rate			
Economic Success	-44.47***	-44.41***	-50.05***	-29.32***	-17.06***	-7.670	5.173
	(1.652)	(1.657)	(4.185)	(4.098)	(4.279)	(5.202)	(6.059)
Prior Belief, Task Difficulty				0.217***			0.152***/#
				(0.0489)			(0.0490)
Economic Success x Prior Belief, Task Difficulty				-0.219***			-0.121*
				(0.0647)			(0.0643)
Prior Belief, Relative Performance					0.345***		0.205***/#
					(0.0508)		(0.0655)
Economic Success x Prior Belief, Relative Performance					-0.521***		-0.294***/#
					(0.0717)		(0.0878)
Prior Belief, Deserving Bonus						0.273***	0.147***/#
						(0.0425)	(0.0543)
Economic Success x Prior Belief, Deserving bonus						-0.499***	-0.334***/#
						(0.0653)	(0.0797)
Effort Determines Success			-0.174***				
			(0.0400)				
Economic Success x Effort Determines Success			0.125**				
			(0.0605)				
Constant	62.71***	70.99***	79.06***	56.47***	52.99***	49.03***	38.19***
	(1.268)	(11.49)	(11.65)	(11.70)	(11.54)	(11.52)	(11.68)
F-statistic first stage	6891.64	6891.64	1253.30	789.67	1106.47	1106.47	961.49
Observations	1,825	1,822	1,822	1,822	1,822	1,822	1,822
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.318	0.323	0.335	0.334	0.355	0.357	0.371

Notes: Two-stage least squares (2SLS) regression with robust standard errors in parentheses. "Economic Success" is an indicator for the bonus payment and is instrumented by random treatment assignment. All beliefs elicited before the bonus announcement and measured on a scale from 0 – 100 and defined as in other Tables. "Liberal," defined as in other Tables. Standard set of controls. Columns without interaction report the F-statistic of the first instrument ("*Easy task*") and columns with interaction report the F-Statistics of the second instrument ("Liberal x *Easy task*").

*** p<0.01, ** p<0.05, * p<0.1, # indicates significance, using adaptive linear step-up procedure by Benjamini, Krieger, and Yekutieli (2006) controlling for a false discovery rate (q=0.05) for the treatment.

A6. Locus-of-Control: Estimates

Table A8. Regression: Locus of Control Index on Political Orientation

	(1)	(2)
Dep. Variables	L-o-C-Index	
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	0.005	0.073

Notes: OLS-Regression with robust standard errors in parentheses. "L-o-C-Index" is bounded between 7 (internal) and 49 (external). "Liberal" is an indicator for respondents 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, Midwest, West).

*** p<0.01, ** p<0.05, * p<0.1

Table A9. Regression: Tax Rate and Locus-of-Control Index

	(1)	(2)	(3)	(4)
Dep. Variable		Tax Rate		
L-o-C-Index	0.060	0.061	-0.035	-0.033
	(0.084)	(0.087)	(0.139)	(0.141)
Easy task	-39.518***	-39.416***	-43.455***	-43.330***
	(1.521)	(1.531)	(3.884)	(3.888)
L-o-C-Index x *Easy task*			0.188	0.187
			(0.170)	(0.170)
Constant	58.889***	68.990***	60.906***	70.322***
	(2.202)	(12.242)	(3.244)	(12.350)
Observations	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes
R-squared	0.277	0.272	0.273	0.277

Notes: OLS-Regression with robust standard errors in parentheses. "Tax Rate" is the redistribution rate of the $2-bonus payment in percent (0-100 percent). "L-o-C-Index" is bounded between 7 (internal) and 49 (external). "*Easy task*" is an indicator for respondents randomly assigned to the *easy task* (treatment). 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<0.01, ** p<0.05, * p<0.1

Table A10. Regression: Change in Beliefs (Posterior – Prior) and Locus-of-Control Index

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	Δ - Belief Task Difficulty		Δ-Belief Deserving Bonus		Δ - Belief Relative Performance		Effort Determines Success	
L-o-C-Index	-0.046	-0.032	0.086	0.076	-0.122*	-0.121*	-0.289**	-0.260**
	(0.075)	(0.076)	(0.078)	(0.079)	(0.067)	(0.0694)	(0.121)	(0.123)
Easy task	-0.457	-0.813	10.261***	10.152***	6.661***	6.701***	16.866***	16.681***
	(2.311)	(2.320)	(2.198)	(2.179)	(1.838)	(1.839)	(3.489)	(3.492)
L-o-C-Index x *Easy task*	-0.047	-0.028	0.0346	0.0316	0.174**	0.169**	-0.037	-0.015
	(0.105)	(0.104)	(0.098)	(0.098)	(0.084)	(0.085)	(0.154)	(0.155)
Constant	4.515***	-10.782	-7.622***	-13.112*	-1.651	1.632	60.165***	50.286***
	(1.644)	(7.905)	(1.789)	(7.695)	(1.435)	(6.657)	(2.799)	(11.016)
Observations	1,825	1,822	1,825	1,822	1,825	1,822	1,825	1,822
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.002	0.010	0.080	0.086	0.106	0.113	0.082	0.093

Notes: OLS-Regression with robust standard errors in parentheses. "Δ" is the difference between posterior and prior beliefs. The beliefs are elicited before the bonus assignment (prior) and after the bonus assignment (posterior). All beliefs are measured on a scale from 0 – 100: "Belief Task Difficulty": likelihood of performing in the *hard task* in %; "Belief Deserving Bonus": deserving the $2-bonus payment in %; "Belief Relative Performance": perceived number of participants performing the same task with a lower score; "Effort Determines Success": likelihood that the $2-bonus payment depends on her exerted effort in %. "L-o-C-Index" is bounded between 7 (internal) and 49 (external). "*Easy task*" is an indicator for random assignment to the *easy task*. "Liberal" is an indicator for respondents 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<0.01, ** p<0.05, * p<0.1

Table A11. Regression: Willingness to Pay and Locus-of-Control Index

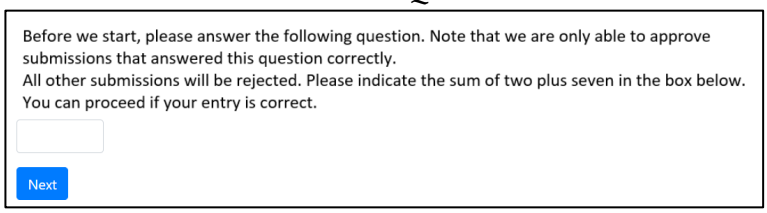
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	WTP		Revised Tax Rate			
L-o-C-Index	0.022	0.045	-0.103	-0.063	-0.176	-0.114
	(0.042)	(0.043)	(0.176)	(0.178)	(0.242)	(0.248)
Easy task	-0.801	-0.916	-49.037***	-48.991***	-51.08***	-50.594***
	(1.281)	(1.268)	(4.924)	(4.949)	(6.468)	(6.524)
L-o-C-Index	-0.009	-0.008	0.381*	0.382*	0.493*	0.476
x *Easy task*	(0.057)	(0.056)	(0.219)	(0.220)	(0.294)	(0.296)
Receive Info					2.321	3.365
					(8.112)	(8.123)
L-o-C- Index					0.115	0.0560
x Receive Info					(0.352)	(0.354)
Receive Info					6.227	5.291
x *Easy task*					(10.02)	(10.02)
L-o-C-Index x Receive					-0.266	-0.234
Info x *Easy task*					(0.443)	(0.442)
Constant	6.911***	-3.034	65.819***	59.377***	65.135***	60.008***
	(0.971)	(4.173)	(4.039)	(16.721)	(5.416)	(17.260)
Observations	1,776	1,773	1,130	1,128	1,130	1,128
Controls	No	Yes	No	Yes	No	Yes
R-squared			0.284	0.295	0.289	0.299

Notes: Interval-Regressions in column (1) and (2) and OLS-Regressions columns (3)-(6). Robust standard errors in parentheses. "WTP" is the willingness to pay for seeing information about the task difficulty and score of the other participant or receiving extra money. The variable is categorized in 9 intervals [0¢,1¢]; [1¢,3¢]; [3¢,5¢]; [5¢,7¢]; [7¢,10¢]; [10¢,20¢]; [20¢,35¢]; [35¢,50¢]; [50¢,inf). We dropped 49 participants with multiple switching points, since they could not be assigned to a category. "Revised Tax Rate" is the redistribution rate of the $2-bonus payment in percent (0-100 percent) after participants decide to receive or not receive additional information about their assigned treatment. All participants who received the information and half of the participants who did not receive the additional information could revise their previous tax rate. "L-o-C-Index" is bounded between 7 (internal) and 49 (external). "*Easy task*" is an indicator for respondents randomly assigned to the *easy task* (treatment). "Receive Info" indicates a dummy variable for having received information about the task difficulty and the performance of the other participant. 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).

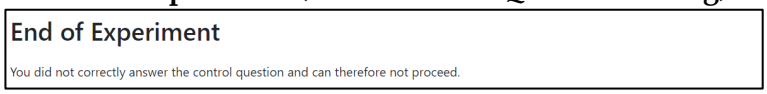
*** p<0.01, ** p<0.05, * p<0.1

A7. Screenshots of Survey and Experimental Tasks

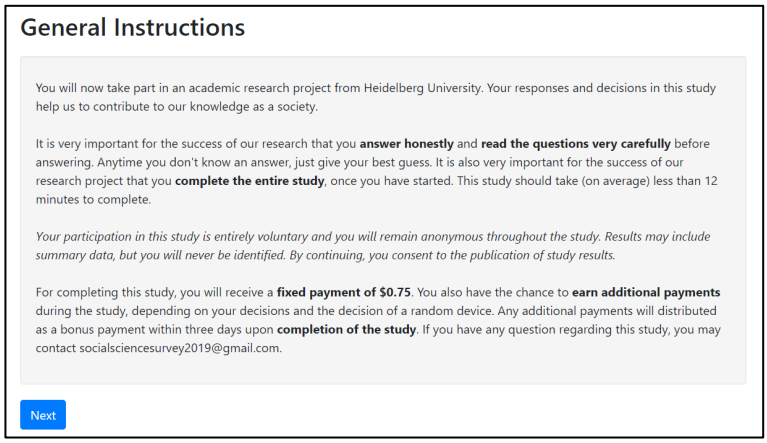
Bot Control-Question



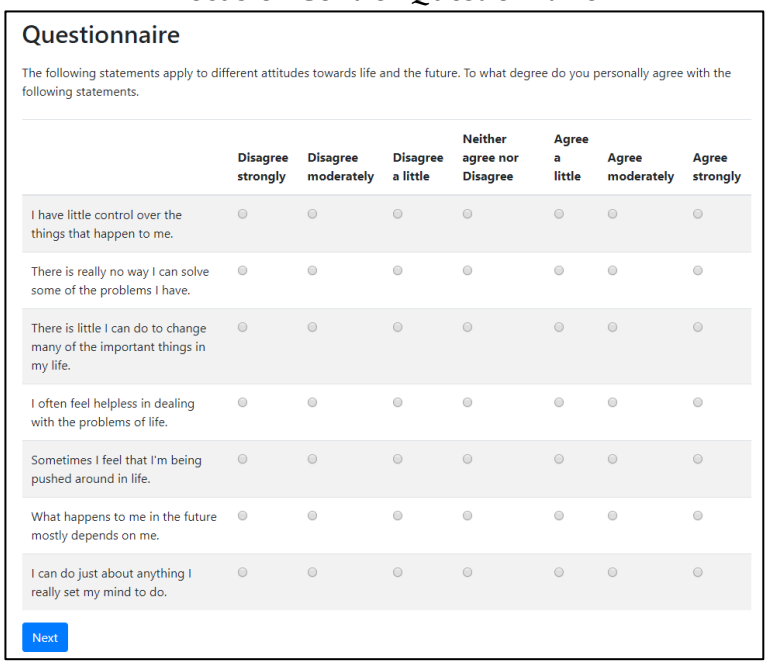
End of Experiment (if Bot Control-Question wrong)



General Instructions



Locus-of-Control Questionnaire



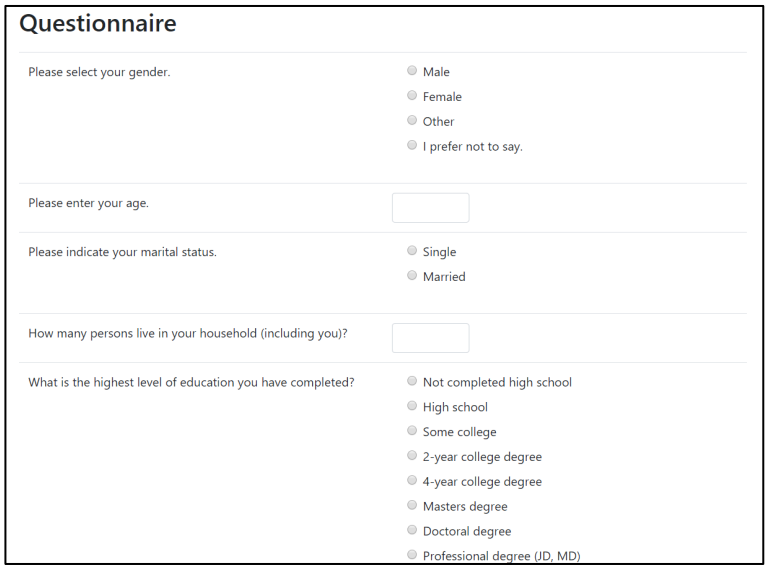
Questionnaire

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

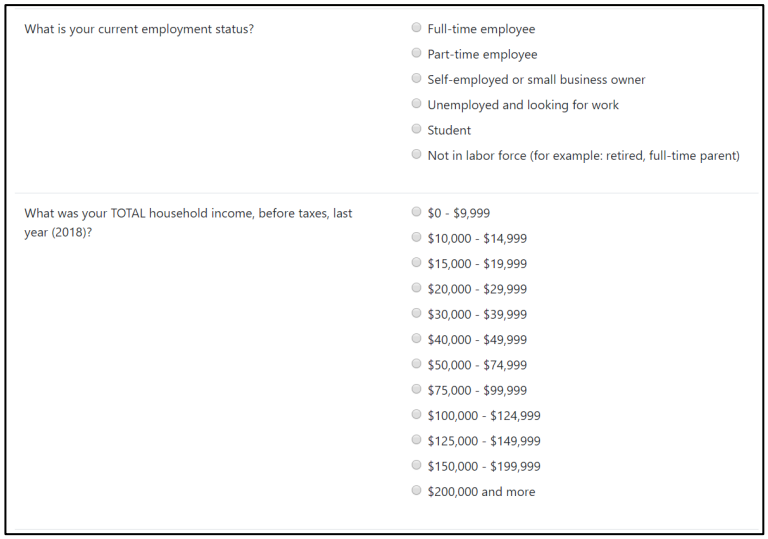
Please select your gender.
- Male
- Female
- Other
- I prefer not to say.

Please enter your age.

Please indicate your marital status.
- Single
- Married

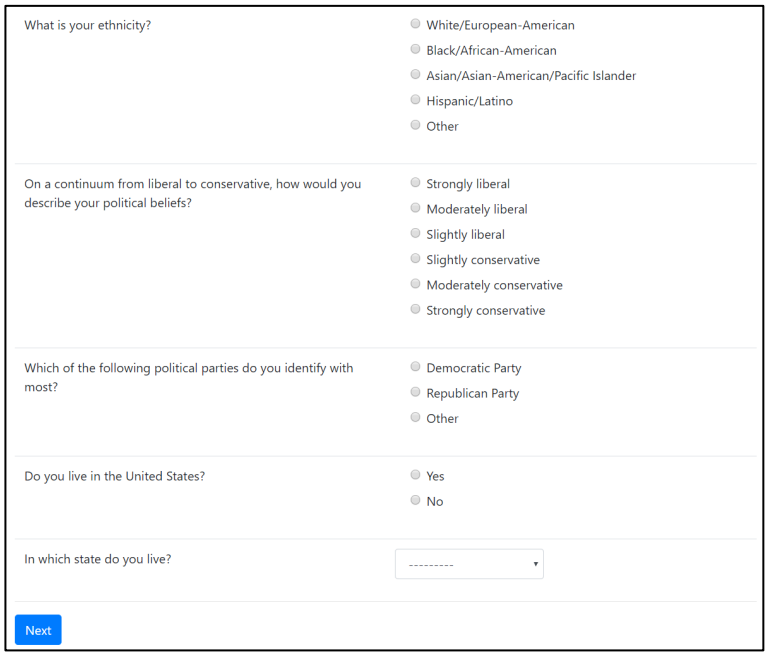
How many persons live in your household (including you)?

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)



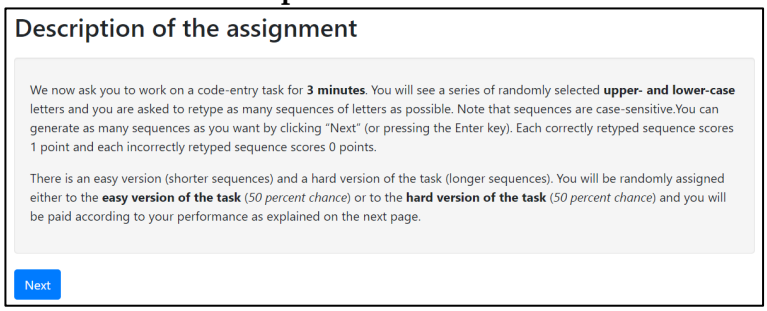
What is your current employment status?
- Full-time employee
- Part-time employee
- Self-employed or small business owner
- Unemployed and looking for work
- Student
- Not in labor force (for example: retired, full-time parent)

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

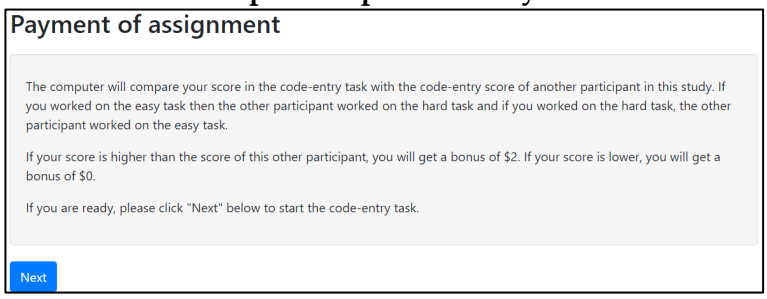

What is your ethnicity?
White/European-American
Black/African-American
Asian/Asian-American/Pacific Islander
Hispanic/Latino
Other
On a continuum from liberal to conservative, how would you describe your political beliefs?
Strongly liberal
Moderately liberal
Slightly liberal
Slightly conservative
Moderately 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

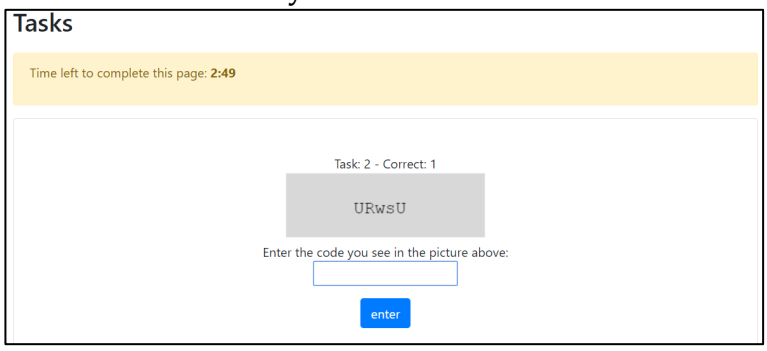


Description Experiment Payment



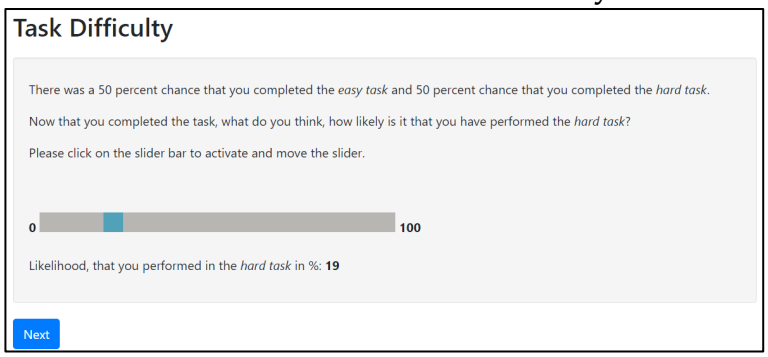
Hard Real Effort Task

Easy Real Effort Task

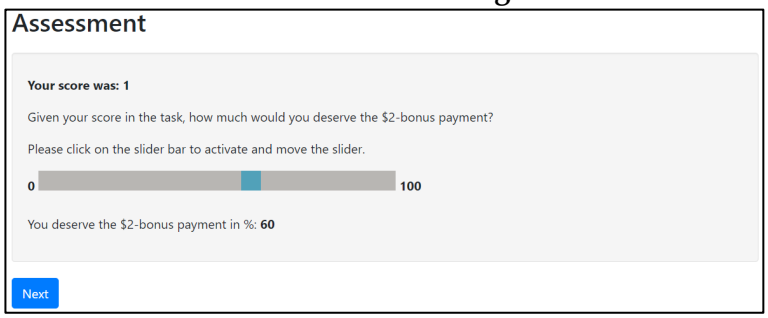


Information Real Effort Task Finished

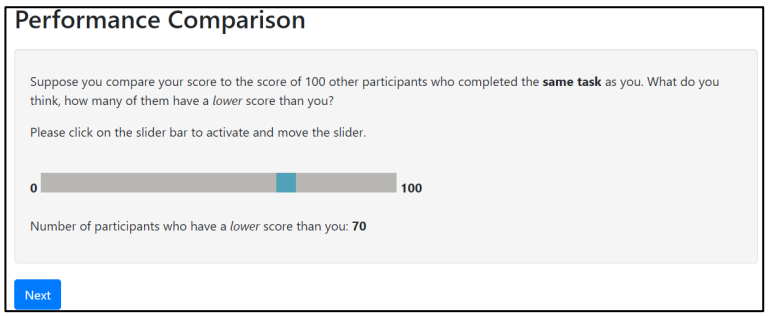
Prior-Belief about Task Difficulty



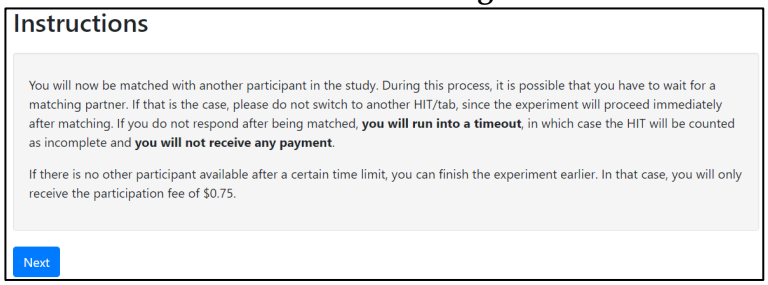
Prior- Belief about Deserving the Bonus



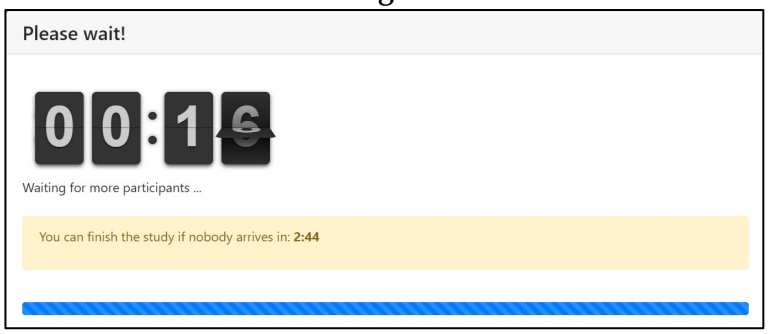
Prior-Belief about Relative Performance



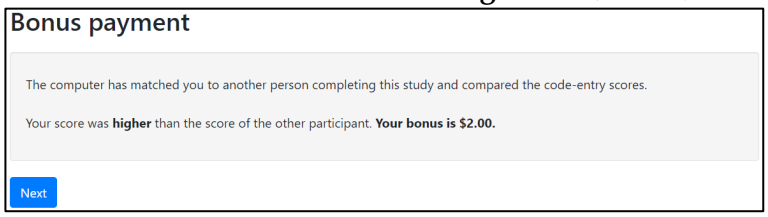
Instructions about Matching Mechanism



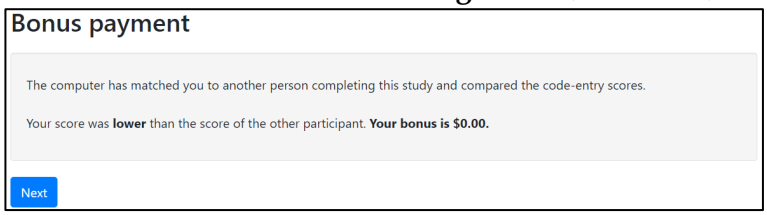
Waiting Room



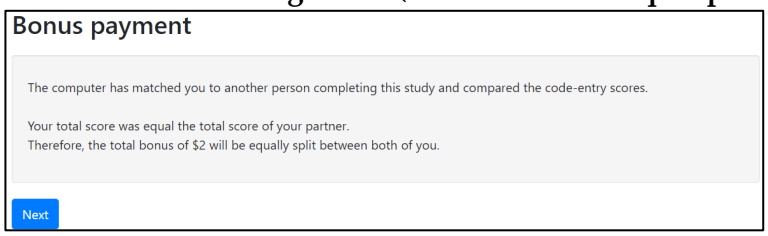
Information about Bonus Assignment (Bonus)



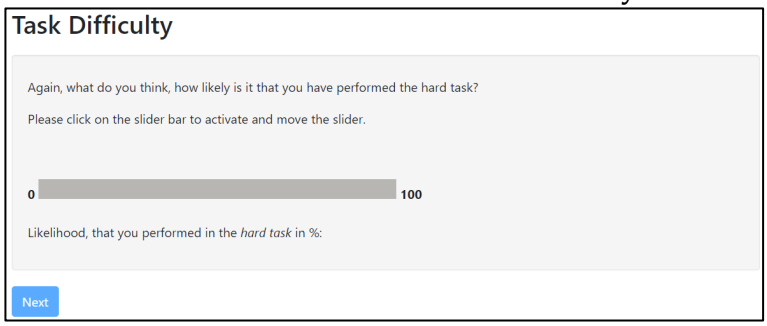
Information about Bonus Assignment (No Bonus)



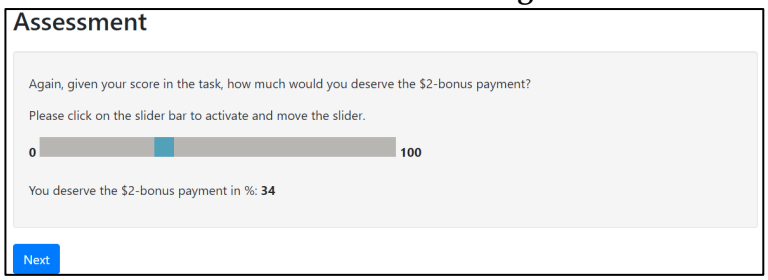
Information about Bonus Assignment (Bonus shared if equal performance)



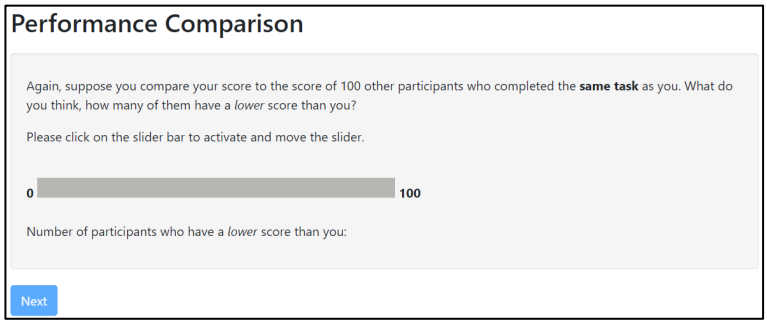
Posterior-Belief about Task Difficulty



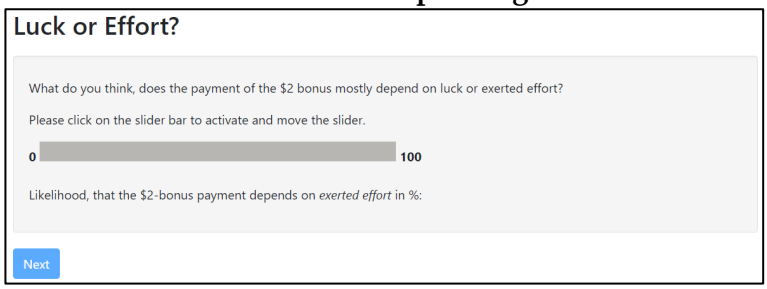
Posterior-Belief about Deserving the Bonus



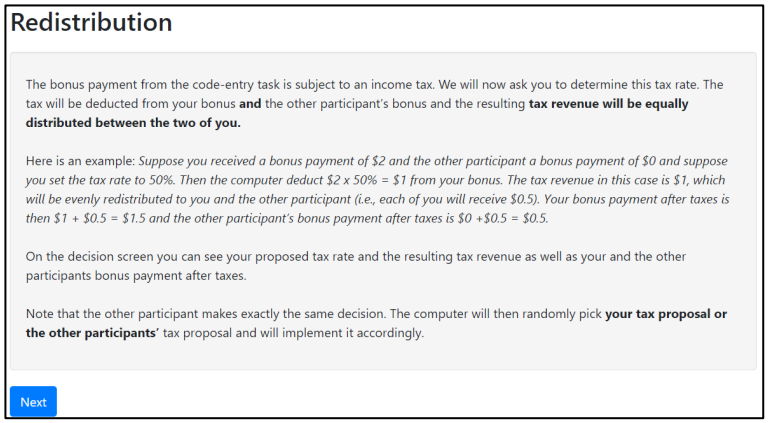
Posterior-Belief about Relative Performance



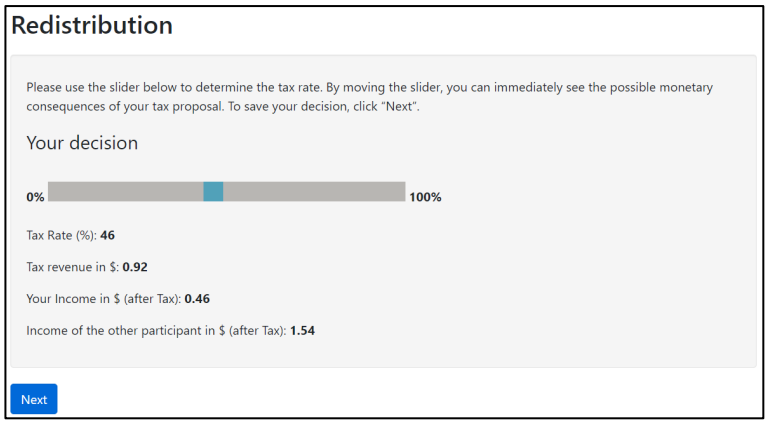
Belief about Bonus Depending on Effort



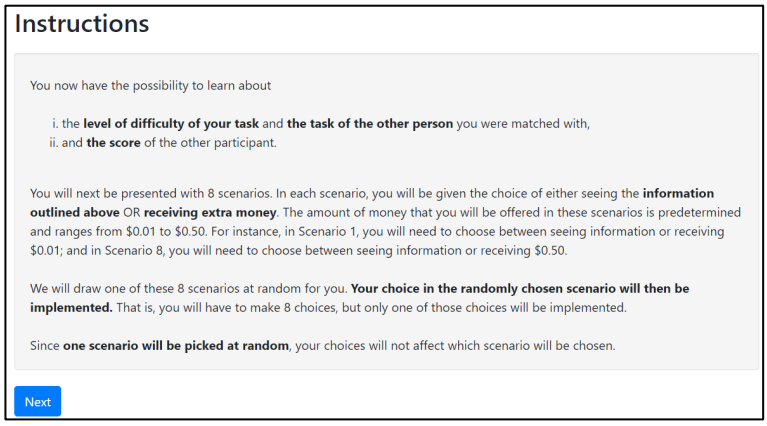
Information about Redistribution Mechanism



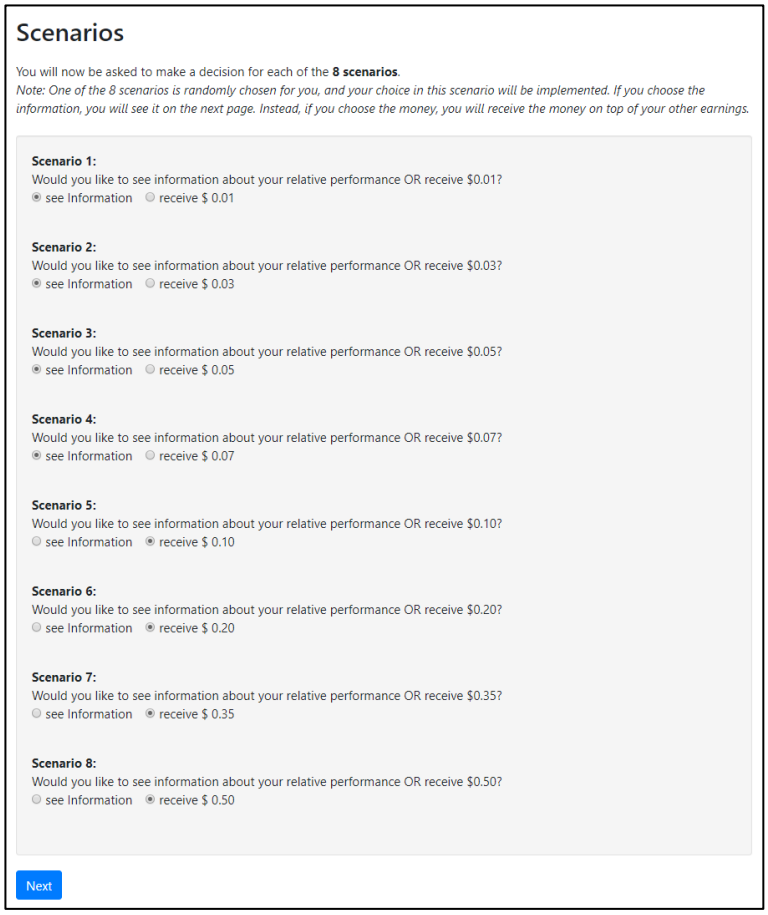
Redistribution First Time



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

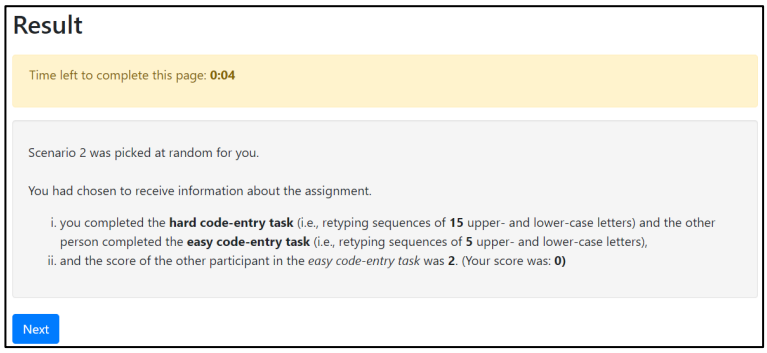
Scenario 6:
Would you like to see information about your relative performance OR receive $0.20?
○ see Information ◉ receive $ 0.20

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**

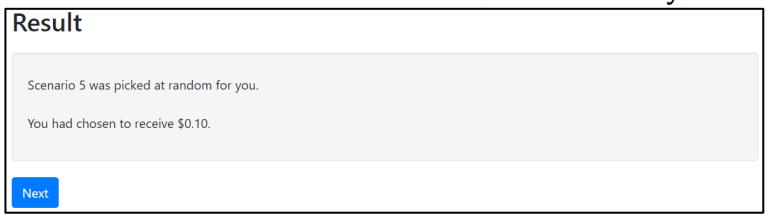
Scenario 2 was picked at random for you.

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)



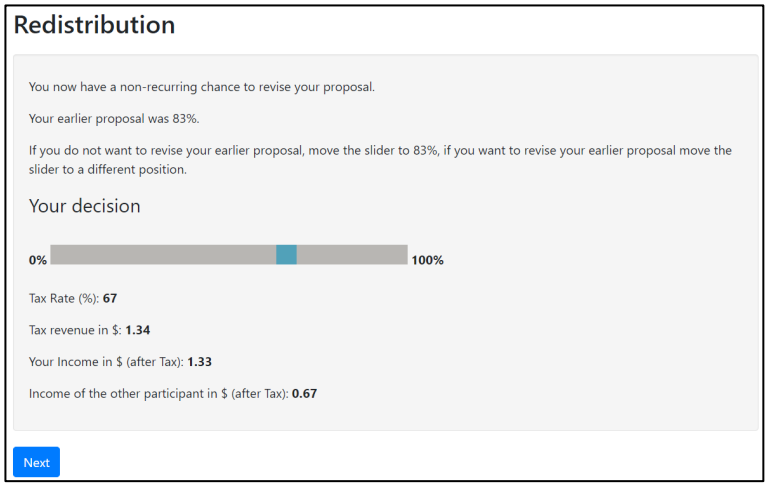
Result

Scenario 5 was picked at random for you.

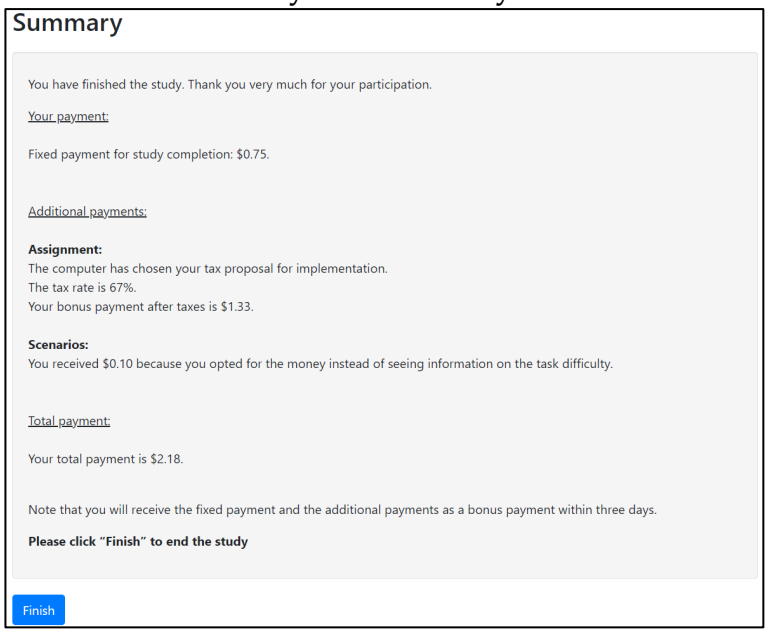
You had chosen to receive $0.10.

Next

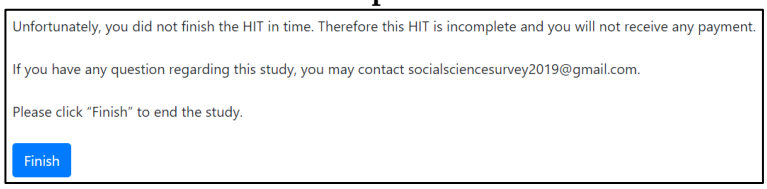
Redistribution Second Time



Payment Summary



Information if Participants run into Timeout



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.

References

Almås, Ingvild, Alexander Cappelen, and Bertil Tungodden, 2019. "Cutthroat Capitalism Versus Cuddly Socialism. Are Americans More Meritocratic and Efficiency-Seeking than Scandinavians?" *Journal of Political Economy (forthcoming).*

Anderson, Michael L., 2012. "Multiple Inference and Gender Differences in the Effects of Early Intervention. A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association* 103(484), 1481–95.

Bartling, Björn, Alexander W. Cappelen, Mathias Ekström, Erik Sorensen, and Bertil Tungodden, 2018. "Fairness in Winner-Take-All Markets." *NHH Dept. of Economics Discussion Paper*(08).

Benjamini, Yoav, Abba M. Krieger, and Daniel Yekutieli, 2006. "Adaptive linear step-up procedures that control the false discovery rate." *Biometrika* 93(3), 491–507.

Berinsky, Adam J., Gregory A. Huber, and Gabriel S. Lenz, 2012. "Evaluating Online Labor Markets for Experimental Research. Amazon.com's Mechanical Turk." *Political Analysis* 20(3), 351–68.

Cappelen, Alexander W., Karl O. Moene, Siv-Elisabeth Skjelbred, and Bertil Tungodden, 2017. "The Merit Primacy Effect." *SSRN Electronic Journal*.

Chen, Daniel L., Martin Schonger, and Chris Wickens, 2016. "oTree—An open-source platform for laboratory, online, and field experiments." *Journal of Behavioral and Experimental Finance* 9, 88–97.

Cobb-Clark, Deborah A. and Stefanie Schurer, 2013. "Two Economists' Musings on the Stability of Locus of Control." *The Economic Journal* 123(570), F358-F400.

Durante, Ruben, Louis Putterman, and Joël van der Weele, 2014. "Preferences for Redistribution and Perception of Fairness: An Experimental Study." *Journal of the European Economic Association* 12(4), 1059–86.

Paolacci, Gabriele, Jesse Chandler, and Panagiotis G. Ipeirotis, 2010. *Running Experiments on Amazon Mechanical Turk*.

Appendix to PAP

Figure 5 Real Effort Task (hard)

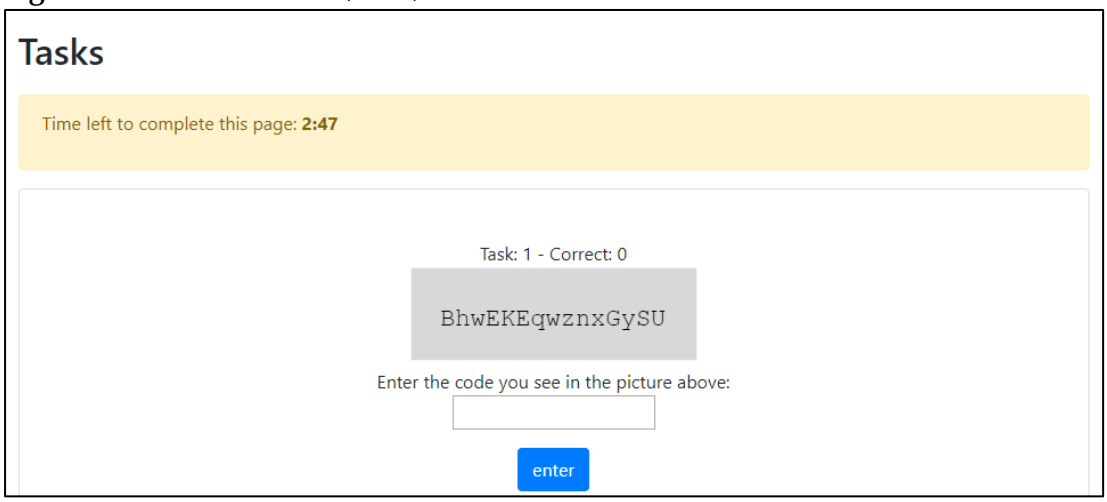


Figure 6 Redistribution Decision

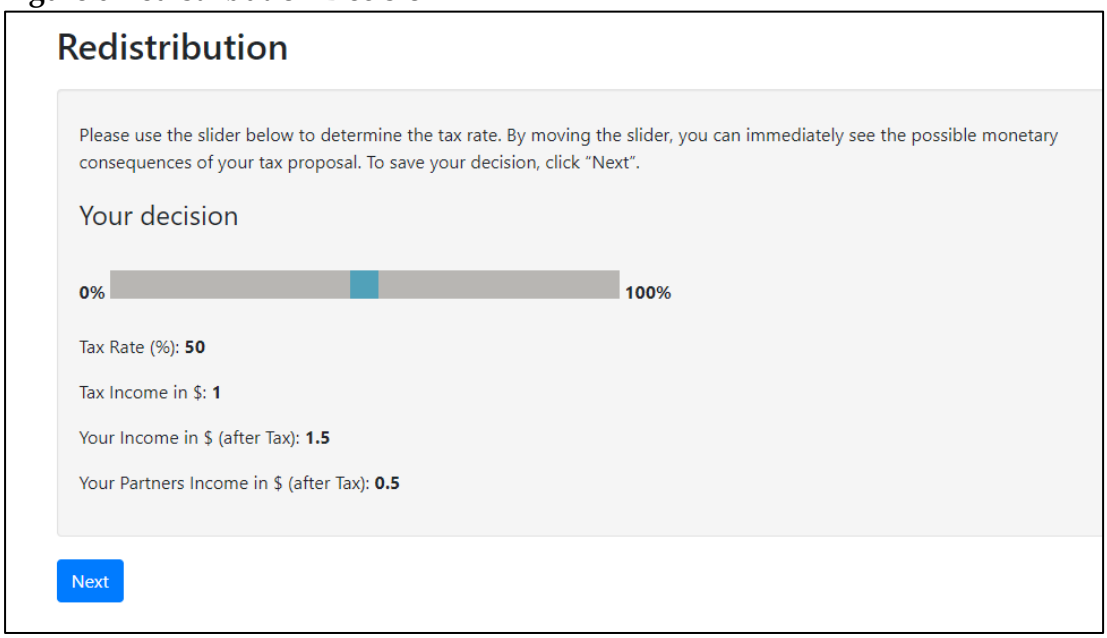


Figure 7 Price List Scenarios

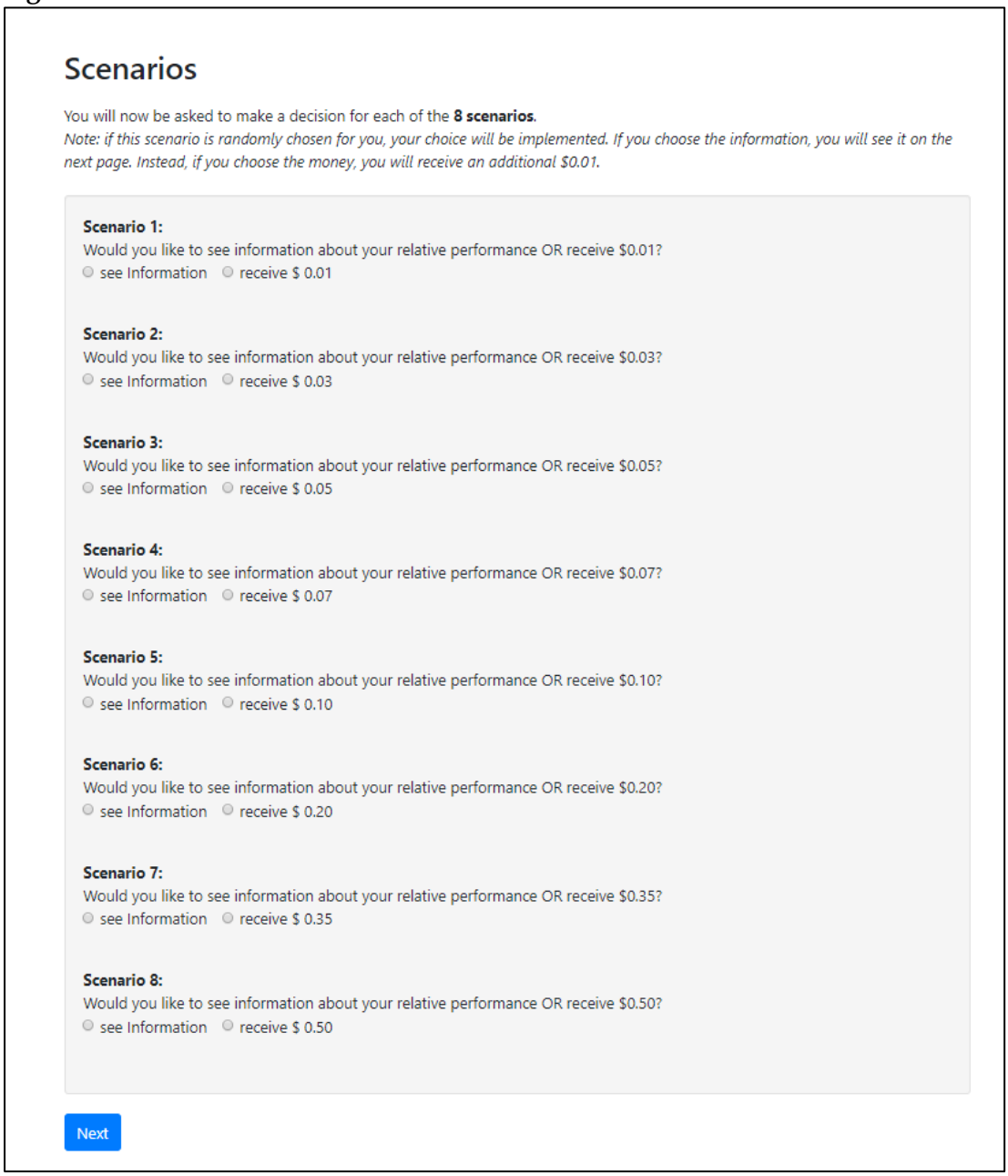


Figure 8 Result Summary