# Personal Relative Position, Attribution and Social Trust. *

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**Abstract**

Across the social sciences researchers have debated the impact income inequality has on people's perceptions, specifically on attribution and social trust. In this paper we use a combination of surveys and behavioral lab experiments to identify a causal impact of inequality on attribution and social trust. We find that higher relative position has a positive impact on belief in meritocracy and social trust, which we causally identify both using a novel incentivized lab task as well as standard survey measures. These results are in line with correlational associations we find using larger general surveys. They speak to why inequality can be so socially and economically corrosive while at the same time remaining largely unaddressed.

*Keywords: Income Inequality, Attribution, Belief in Meritocracy, Social Trust, Experiments.*

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# 1 Introduction

Economic inequality is on the rise in many countries across the globe, even as global poverty rates reached all-time lows in 2019 (Alvaredo, Atkinson, Piketty, & Saez, 2013; Atkinson, 2015; Gould, 2017; Morris & Western, 1999). Philosophical arguments about the justifiability of high inequality aside, empirical research from across the social sciences has implicated inequality in a host of negative social outcomes including a decline in public health and the health of the environment, an erosion of social cohesion and increase in crime, and a suppression of social mobility as relative advantage and disadvantage become entrenched (Atkinson, 2015; Currie, 2011; Pickett & Wilkinson, 2010; Stiglitz, 2012). To understand how inequality can be so socially and economically corrosive while remaining largely unaddressed, it is important to understand its impact on people's psychological perceptions including social trust and attributional beliefs like a belief in meritocracy.

People have a strong motivation to believe that the world is a just place. Such 'just world' beliefs (Lerner, 1980) are a form of motivated social cognition that can help to offset the stress and uncertainty inherent in a world that seems indifferent to human suffering (Furnham, 2003). Research spanning several distinct literatures from psychology, economics, and political science illustrates how such beliefs can serve palliative functions for both the relatively advantaged and disadvantaged (Bullock, 2008; Jost, Banaji, & Nosek, 2004). When applied to the economic domain these beliefs often take the form of meritocratic beliefs, which incorporate the related set of beliefs that economic status — poverty or affluence — are earned, as the result of hard work or ability and not due to other factors such as luck, circumstance, or preexisting personal advantage or connections (Ross & Nisbett, 1991). Thus, when exposed to inequality meritocratic beliefs can resolve potential feelings of guilt for the advantaged. Because of this palliative function such processes of attribution can be crucial for people's acceptance of the (unequal) status quo. They can even deepen inequality by making those with a poor relative position more pessimistic about their chances to move ahead.[1]

While inequality can lead people to adopt meritocratic beliefs in order to avoid questioning the functioning of institutions, inequality has also been robustly associated with declines in generalized social trust (Alesina & Ferrara, 2002; Bjornskov, 2008; Delhey & Newton, 2005; Putnam, 2000; Uslaner, 2002). Social trust, or a belief in the kindness and fairness of others, is part of a broader syndrome of personality characteristics that includes optimism and a belief in cooperation, but also elements that relate to just world beliefs, such as trust that we will receive from others what we deserve (Galeotti, Kline, & Orsini, 2017; Uslaner, 2002). Like belief in meritocracy, social trust is crucial for people's willingness to engage in social and economic interactions and to invest in collective activities (Coleman, 1990).

Because of the importance of these outcomes, there is a huge interest in the Social Sciences in understanding the relationship between inequality, attribution and social trust. Much of the scholarly attention paid to these relationships within economics and political science is

[1] Beyond the palliative functions they serve, meritocratic beliefs are also important because of their connection with broader socio-economic attitudes, such as trust in institutions (McCoy & Major, 2007) and policy preferences, especially support for redistribution (Alesina & Ferrara, 2005; Benabou & Tirole, 2006; Fong, 2001; Gilens, 1999; Hasenfeld & Rafferty, 1989). Perceptions of systemic unfairness, like low belief in meritocracy can lead to frustration and have been linked to corruption (Charron, 2017) and political radicalization (van den Bos, 2020).

observational and examines the impact of the Gini coefficient and other contextual indicators on public attitudes (Kelly & Enns, 2010; Newman, Johnston, & Lown, 2015; Wolak & Peterson, 2020). As levels of inequality fluctuate across time and geography co-varying with a host of other factors, it is difficult to establish a strong causal link.

The goal of the present study is to provide (i) *causal* evidence for the relationship between inequality, belief in meritocracy and social trust while[2] (ii) carefully decomposing the overall effect of inequality into the effect of inequality exposure per se and that of relative position. Our design also allows us (iii), for the first time, to measure belief in meritocracy both in an incentivized lab task as well as using the more common survey measure.

We use a combination of surveys and lab experiments to identify the causal effect of income inequality on meritocratic attributions and social trust. First, using non-incentivized survey responses from a 2019 survey of British youth called Next Steps 8, we find a consistent impact of relative economic position. Higher relative position is associated with increased belief in meritocracy and social trust. Inequality measured by the Gini coefficient has a moderating effect on belief in meritocracy among wealthy youths living in highly unequal contexts, but does not impact social trust.

We then designed novel incentivized experiments to provide causal evidence for the effects of inequality. We exogenously assign participants to information treatments comparing participant data to local economic context in one of two randomly assigned boroughs in England, one a great deal wealthier than the other. Hence participants were randomly placed into the position of upward or downward economic comparison. In order to assess belief in meritocracy, participants completed a real effort task in which their total score was a function of effort, ability and luck. Then - after seeing their overall rank among their fellow participants - participants were asked how much effort and ability contributed to their overall position. Guesses were incentivized to be accurate using the interval scoring rule (Schlag & van der Weele, 2015). Our results show a causal impact of inequality on belief in meritocracy. We can show that this effect is neither due to anchoring nor due to changes in confidence. We also find that personal relative position is much more important than inequality exposure by itself in determining belief in meritocracy. While inequality exposure by itself has no effect on belief in meritocracy, when combined with information on personal relative position there is a strong positive effect. A higher relative position leads to increased belief in meritocracy, while a lower relative position leads to rejection of meritocracy. This is true both for our novel incentivized lab task as well as for the un-incentivized survey measure. Social trust is positively affected by a higher relative position. A lower relative position leads to substantially lower social trust using both an incentivized lab task and standard survey measures. We also find evidence of a negative impact of inequality exposure by itself on social trust using the non-incentivized survey measure, but not using the lab task.

Taken as a whole, our results show that relative position has a causal impact on belief in meritocracy and social trust. Participants made to feel their economic position was higher were both more likely to affirm a belief in meritocracy and to trust others. As both trust

[2]There are a handful of papers providing a causal link between some forms of inequality and either belief in meritocracy or social trust (see e.g. McCall, Burk, Laperriere, and Richerson (2017)). We discuss these in detail in the Literature Review in Section 2.

and belief in meritocracy are related to a person's ability to use the opportunities the system provides for them, the results also establish a direct link between outcome equality and equality of opportunities. As such the results underline the promise of interventions aimed at increasing social trust or belief in meritocracy in poorer communities.

The paper is organized as follows. In Section 2 we discuss related literature across different subfields in Social Sciences in more detail. Section 3 discusses evidence from general social surveys. In Section 4 we present our experimental and survey designs. Sections 5 and 6 contain our main results on attribution and social trust, respectively. Section 7 concludes. A series of online appendices contains details and materials from the experiments as well as additional tables and figures.

## 2 Literature

Research on inequality, attribution and social trust spans various fields including Psychology, Political Science and Economics.

There is a large literature in social psychology on how people perceive the world around them (Furnham, 2003; Jost et al., 2004; Ross & Nisbett, 1991). Social psychologists use the term "Belief in a just world" (BJW) to refer to a set of more or less articulated beliefs which underlie the way people orient themselves to their environment. They include the belief that others can be trusted as well as a general belief that we live in a just world where everyone receives what they earn and consequently earns what they receive (Lerner, 1980). Just world beliefs reflect an innate human psychological tendency arising from our attempts to attribute causality to the events that involve us as well as other people. Such causal attribution processes provide a foundation upon which social interaction can take place. There can be no trust and reputation without the ability to update such tallies against the actions of others. BJW can serve a palliative function if people overestimate to which extent their successes are due to merit rather than luck (Davidai & Gilovich, 2016; Langer, 1975).[3]

Political Scientists and Economists have mostly been interested in economic manifestations of just world beliefs, such as belief in meritocracy (Alesina, Stantcheva, & Teso, 2018; Mo & Conn, 2018; Newman, 2016; Newman et al., 2015; Wolak & Peterson, 2020). Most research in these areas documents a negative relationship between inequality and belief in meritocracy either cross-sectionally (Newman et al., 2015) or across time (Wolak & Peterson, 2020). There is, however, disagreement about the interpretation of these effects, largely due to the difficulty of causal identification. It is unclear, for example, whether inequality indeed impacts beliefs or whether beliefs allow inequality to persist or both. Further, because most of this work is observational, we do not know whether the effect of inequality might be due to inequality of the distribution itself or due to relative position. There are reasons to believe that relative position might play a more important role, as people often only have a tenuous grasp on inequality and are unable to appreciate the scale of it (Trump, 2017; Xu & Garand, 2010). There is further evidence that people's perceptions of inequality and other economic indicators are politically

[3]Ross and Nisbett (1991) describe this the "fundamental attribution error", an excessive tendency to explain the behaviour and outcomes of others and oneself by underlying "dispositions" (personal attributes) rather than external circumstances or luck.

malleable (Alesina et al., 2018; Bartels, 2016; Kuziemko, Norton, Saez, & Stantcheva, 2015). Newman et al. (2015) show that in unequal contexts, low-income people are more likely to identify as 'have nots', also suggesting a role for relative position. On the other hand there is also literature supporting sociotropic concerns over individualistic ones (Smith & Pettigrew, 2015).

Much of the research on the relationship between inequality and social trust has encountered similar methodological issues. The cross-country correlation between social trust and national income equality is well documented. It is often assumed (but not shown) that inequality leads to lower trust (Alesina & Ferrara, 2002; Bjornskov, 2008; Delhey & Newton, 2005; Uslaner, 2002) and the theoretical mechanisms involved are still subject to debate (Gustavsson & Jordahl, 2008; Nannestad, 2008). It is also unclear from this literature whether inequality exposure per se affects social trust or whether it is mainly personal relative position that affects trust.

Behavioural Economists have studied various other causal impacts of inequality using lab experiments. In this research inequality is usually manipulated within the lab e.g., by giving participants different endowments. A number of papers in this area have studied the effect of exogenous income inequality (created by giving participants different endowments) on public good contributions with mixed results (Chan, Mestelman, Moir, & Muller, 1996; Ostrom, Gardner, & Walker, 1994; Reuben & Riedl, 2013; Sadrieh & Verbon, 2006; van Dijk, Sonnemans, & van Winden, 2002). Gaechter, Mengel, Tsakas, and Vostroknutov (2017) found a negative impact of endogenous inequality (created over time by differing past contributions) on contributions. Nishi, Shirado, Rand, and Christakis (2015) found that inequality per se only has a small negative effect on welfare, but a poor relative position (visible wealth differences) has a much more negative effect.[4]

A number of authors have related inequality and trust within a lab experiment (Holm & Danielson, 2005; Xiao & Bicchieri, 2010). Greiner, Ockenfels, and Werner (2011) find that both exogenous and endogenous variation in income affect behaviour in a trust game and Xiao and Bicchieri (2010) find that inequality concerns can crowd out trustworthiness. Last, Fehr, Rau, Trautmann, and Xu (2020) find that inequality harms both trust and trustworthiness when it is perceived as unjust. They main difference between these papers and our research is the measure of social trust, which in our case is a belief that others will reward us fairly for our effort. Compared to trustworthiness measured in a trust game our measure is designed to pick up aspects of social trust which relate more closely to meritocratic beliefs.

To our knowledge there is very little prior research measuring the causal impact of inequality on belief in meritocracy. McCall et al. (2017) show that when people are exposed to information about rising economic inequality in the United States they afterwards display lower belief in meritocracy. There are a few differences between this work and our work. First McCall et al. (2017) do not measure the impact of personal relative positions. Second they give people

[4]There is also a substantial literature on the impact of inequality on pro-social behaviour usually focusing on the effect of relative position as opposed to inequality *per se* (Andreoni, Nikiforakis, & Stoop, 2017; Cote, House, & Willer, 2015; Horvath, Kovarik, & Mengel, 2012; Korndoerfer, Egloff, & Schmukle, 2015; Piff, Stancato, Cote, Mendoza-Denton, & Keltner, 2012; Schmukle, Korndoerfer, & Egloff, 2019; Smeets, Bauer, & Gneezy, 2015; Trautmann, van de Kuilen, & Zeckhauser, 2013) find that this difference is more pronounced if there is a high degree of inequality in the area where the rich or poor person lives. This literature is summarized in detail in Appendix D where we document a positive impact of relative position and a negative impact of inequality per se on pro-social behaviour in our data.

information about changes in inequality as opposed to levels of inequality as we do. Third, their measure of belief in meritocracy is not incentivized. Combining their and our results the evidence on the impact of inequality on belief in meritocracy can be summarized as follows. Providing information about different levels of inequality does not affect belief in meritocracy (our paper), information about increases in inequality decreases belief in meritocracy (McCall et al., 2017) and information about a good personal relative position increases belief in meritocracy (our paper).

Beyond this work, there is an important and active line of experimental research studying the impact of belief of meritocracy and other fairness views on redistribution (Almas, Cappelen, Sorensen, & Tungodden, 2022; Almas, Cappelen, & Tungodden, 2019; Mollerstrom, Reme, & Sorensen, 2015). Mollerstrom et al. (2015) ask participants in the role of spectators to redistribute income between others who had been allocated unequal earnings either due to luck or due to merit. They found that spectators do not always compensate for uncontrollable luck. Almas et al. (2019) compare spectators from the United States and Norway. They find that Norwegians in the role of spectators implement less unequal distributions on average and are less accepting than Americans of unfairness purely due to luck. Almas et al. (2022) show that where the fortunes of the rich are perceived to be the result of selfish behavior, inequality is viewed as unfair, and there is stronger support for income redistribution.

There is also literature on how inequality exposure and relative position impacts policy preferences and in particular preferences for redistribution (Alesina & Ferrara, 2005; Benabou & Tirole, 2006; Cappelen, Mollerstrom, Reme, & Tungodden, 2022; Fong, 2001; Gilens, 1999; Hasenfeld & Rafferty, 1989). Cruces, Perez-Truglia, and Tetaz (2013) and Karadja, Mollerstrom, and Seim (2017) find that most people hold biased beliefs about the income distribution that when informed of their true relative position, individuals who are richer than they initially thought demand less redistribution. By contrast Hoy and Mager (2021) find that people who are told they are relatively poorer than they thought are less concerned about inequality and are not more supportive of redistribution. Fehr, Mollerstrom, and Perez-Truglia (2019) compare the demand for national and global redistribution and find that, while nationally demand for redistribution decreases with income, there is no such relationship for global redistribution. Other research has focused on the impact of inequality on the demand for redistribution with mixed results (Jimenez-Jimenez, Molis, & Solano-Garcia, 2020; Magni, 2020; Roth & Wohlfarth, 2018). Several authors suggest that the effect of inequality on preferences for redistribution might operate via respondents' fairness views and in particular via their belief in meritocracy (Fehr et al., 2019; Karadja et al., 2017; Roth & Wohlfarth, 2018). By establishing a causal link between personal relative position and belief in meritocracy our research provides support for such a mechanism.

## 3 Correlational Evidence from General Social Surveys

We briefly study correlational evidence from general social surveys before moving on to causal identification of the effects of inequality on attribution and social trust. We use data from the Next Steps 8 (Longitudinal Study of Young People in England) survey (UCL, 2018) to see if we can identify a relationship between inequality and belief in meritocracy and/or social trust. Next

Steps 8 is ideally suited for our purposes as its respondents are young adults in the UK, a similar population to our lab experimental participants. Determining the effects of inequality on young adults also seems particularly relevant as they are at a stage of life where belief in meritocracy and social trust can affect many crucial decisions in terms of education and careers among others. Appendix Table A.1 contains some summary statistics for this sample. Our empirical approach in this Section follows closely the existing survey literature on these questions (see e.g Newman et al. (2015)).

### 3.1 Belief in Meritocracy

Next Steps 8 contains three questions that are often used to measure belief in meritocracy[5]

A *If someone is not a success in life, it is usually their own fault.*

B *How well you get on in this world is mostly a matter of luck.*

C *If you work hard at something you'll usually succeed.*

Respondents indicated agreement with these statements on four levels (strongly agree, agree, disagree, strongly disagree). We create a binary variable indicating agreement ("strongly agree" or "agree") whereby we reverse-code statement B. Following Newman et al. (2015) we measure belief in meritocracy with a dummy taking the value 1 if there is agreement to all three statements A, B and C. The dummy identifies 31 percent of respondents as having high belief in meritocracy. Income takes three values ("low", "middle", "high") based on annual household (HH) income of less than 25K, 25-45K and greater than 45K. Those are the same cutoffs used in our lab experiment, which are calibrated to induce about equally big income categories in our lab sample. The Gini coefficient is derived based on the respondent's residence at the level of the government office region using data from the ONS (Office for National Statistics).[6]

Table 1 shows the results. As in Newman et al. (2015) higher income is correlated with higher belief in meritocracy. The Gini coefficient does not have a statistically significant effect on those in the lowest income category, but it does have a large negative effect for those in the middle and higher income categories.[7]

### 3.2 Social Trust

Next Steps 8 also contains a question measuring social trust, more precisely agreement to the statement "Most people in life can be trusted" using an 11-point Likert Scale. Table 2 shows the results of regressions where the endogenous variable measures the extent of agreement to this statement. The table shows a positive and statistically significant relationship between

[5]These questions are used in a module relating to "locus of control". The difference is that belief in meritocracy refers specifically to the relationship between hard work (effort) and good outcomes or one's position in society, whereas locus of control refers to a broader sense of being able to control one's fate and is not restricted to economic outcomes.

[6]We use the "Income and tax, by gender, region and county, 2015-2016" table provided by the ONS.

[7]Hence compared to Newman et al. (2015) different income groups seem to be affected by inequality in Next Steps 8. Several differences between the surveys (apart from the UK-US country difference) should be noted, though. First, Next Steps 8 considers young people while Newman et al. (2015)'s sample is representative in terms of age of the US population. Second, the Gini level is available only at a much coarser level of aggregation in the UK, making it less clear whether people react to "local" inequality here.

| | *Belief in Meritocracy Next Steps 8* | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| medium income | 0.403*** | 0.396*** | 0.390*** | 0.398*** | 0.446*** |
| | (0.107) | (0.107) | (0.107) | (0.107) | (0.139) |
| high income | 0.700*** | 0.684*** | 0.687*** | 0.675*** | 0.813*** |
| | (0.193) | (0.193) | (0.193) | (0.193) | (0.296) |
| Gini | -0.148 | -0.083 | 0.256 | 0.344 | 0.337 |
| | (0.241) | (0.241) | (0.369) | (0.370) | (0.469) |
| Gini × med income | -1.119*** | -1.095*** | -1.076*** | -1.103*** | -1.298*** |
| | (0.380) | (0.379) | (0.378) | (0.380) | (0.489) |
| Gini × high income | -1.965*** | -1.898*** | -1.911*** | -1.865*** | -2.435** |
| | (0.676) | (0.675) | (0.675) | (0.676) | (1.037) |
| Constant | 0.336*** | 0.263*** | -0.024 | -0.025 | -0.095 |
| | (0.067) | (0.070) | (0.236) | (0.238) | (0.313) |
| Individual Controls | YES | YES$^+$ | NO | YES$^+$ | YES$^+$ |
| Region Controls | NO | NO | YES | YES | YES |
| Observations | 6,906 | 6,899 | 6,962 | 6,899 | 4,143 |
| R-squared | 0.022 | 0.025 | 0.016 | 0.025 | 0.021 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1: LPM regression of Belief in Meritocracy dummy on income categories, local Gini coefficient and interactions. Individual controls are gender, religion and ethnicity fixed effects. The larger set of individual controls (YES$^+$) also includes an indicator for whether the respondent is unemployed, their level of interest in politics and whether they have a higher education degree. The region controls are population size, ethnic diversity (share of white population) and the share of the population living in an urban area. Column (5) is a restricted sample of people who haven't moved in the last 2 years.

income and social trust. There are also substantial interaction effects with the Gini coefficient, which are, however, not statistically significant. These results can be replicated in the UK part of the European Value Survey, a much smaller sample, which contains the same question. As Appendix Table E.1 shows also here there is a positive and statistically significant relationship between income and social trust and also here there are substantial interaction effects with the Gini coefficient, which are, however, not statistically significant.

To summarize, we have seen evidence for a possible link from income, relative position and inequality to belief in meritocracy as well as social trust. Importantly these relationships can only be interpreted as correlational and there is a strong possibility of endogeneity, for example, as those with higher belief in meritocracy might be expected to work harder and hence achieve higher income. This should affect the income distribution and hence the Gini coefficient as well. High social trust can also lead to higher income or people with high belief in meritocracy might move to areas where the Gini coefficient is lower.[8] Those are the type of endogeneity issues that make causal interpretation of these findings difficult. An additional problem with these type of findings is that it is difficult to disentangle the effect of inequality from the effect of relative position. The reason is that - conditional on income - relative position changes as the Gini coefficient changes.

The aim of our experiments discussed in the next Sections is (i) to provide *causal* evidence on these relationships and (ii) to disentangle the effect of inequality exposure per se from that of relative position.

[8]Specification (5) in Table 1 tries to partially address this particular issue.

| | *Social Trust Next Steps 8* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| medium income | 0.423*** | 0.669 | 0.514*** | 0.423*** | 0.652 | 0.375 |
| | (0.0597) | (0.533) | (0.0583) | (0.0597) | (0.534) | (0.716) |
| high income | 0.672*** | -0.115 | 0.797*** | 0.670*** | -0.105 | 0.083 |
| | (0.107) | (0.961) | (0.105) | (0.107) | (0.962) | (1.521) |
| Gini | 0.419 | 0.494 | -1.926 | -1.263 | -1.164 | -0.249 |
| | (0.932) | (1.202) | (1.668) | (1.670) | (1.844) | (2.415) |
| Gini × med income | | -0.874 | | | -0.816 | 0.0255 |
| | | (1.889) | | | (1.891) | (2.513) |
| Gini × high income | | 2.764 | | | 2.723 | 2.507 |
| | | (3.364) | | | (3.368) | (5.333) |
| Constant | 6.285*** | 6.261*** | 6.565*** | 6.862*** | 6.829*** | 4.879*** |
| | (0.279) | (0.349) | (1.149) | (1.160) | (1.185) | (1.609) |
| Individual Controls | YES | YES$^+$ | NO | YES$^+$ | YES$^+$ | YES$^+$ |
| Region Controls | NO | NO | YES | YES | YES | YES |
| Observations | 6,899 | 6,899 | 6,927 | 6,899 | 6,899 | 4,143 |
| R-squared | 0.028 | 0.029 | 0.016 | 0.029 | 0.029 | 0.034 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: OLS regression of Social Trust in Next Steps 8 survey ("0=not at all agree",...,"10=extremely strongly agree"). Individual controls are gender, religion and ethnicity fixed effects. The larger set of individual controls (YES$^+$) also includes an indicator for whether the respondent is unemployed, their level of interest in politics and whether they have a higher education degree. The region controls are population size, ethnic diversity (share of white population) and the share of the population living in an urban area. Column (6) is a restricted sample of people who haven't moved in the last 2 years.

# 4 Experimental Design

Our experiments are designed to identify the causal impact of inequality exposure and personal relative position on attribution, specifically belief in meritocracy and blame, and social trust. We now describe the experimental design starting with the treatment structure, then describing in detail the information treatments, the outcomes, the sample and the correlation among our main outcomes.

## 4.1 Design and Procedures

Figure 1 shows the general structure of our experiments. First participants fill in an income questionnaire. Our first treatment dimension then concerns the type of information provided. In treatments **REL** we show participants an income distribution *and* their own relative position within the distribution. In treatments **INEQ** we only show them a distribution. The second treatment dimension concerns the main outcome elicited. In the online experiments we elicit only one main outcome per treatment. In the lab experiments we elicit all three main outcomes but with changing order. In treatments **MTB** the order is "Belief in **M**eritocracy, Social **T**rust, **B**lame". In treatments **BMT** it is "**B**lame, Belief in **M**eritocracy, Social **T**rust". Changing the order of tasks allows us to have - for each of these outcomes - one treatment that cleanly measures the impact of the information provided on that outcome, while at the same time allowing us to study the cross-correlation among the different outcomes.[9]

[9]Note that we do not have all possible orders of treatments. The reason is that our social trust measure is best elicited directly after the meritocracy task as will become clear below. Eliciting social trust always after belief in meritocracy could have two undesirable consequences. First, a possible causal effect on social trust might not be detectable as it is diluted by the longer time that passes between the information treatment and the task. Second, any observed effect might not be causal but instead be triggered by the differential effect the information

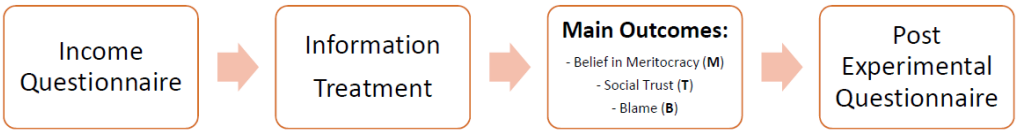



Figure 1: Stages of the experiment. First participants fill in an income questionnaire, then the information treatment happens. In the online experiments we elicit only one main outcome per treatment. In the lab experiments we elicit all three main outcomes but with changing order. In the post experimental questionnaire we elicited risk attitude, competitiveness, demographic covariates and survey based measures of aspirations.

Table 3 shows the number of participants both in the online and the lab experiments. We targeted a sample size of $\approx 100$ participants for the **MTB** treatments and $\approx 200$ for the **BMT** treatments and proceeded by conducting sessions until this threshold was reached. We conducted a power-analysis to determine these sample sizes and pre-registered the sample size and procedure (see below). This resulted in five sessions for each **MTB** treatment, eleven sessions for **REL-BMT** and ten sessions for **INEQ-BMT** and the number of participants shown in Table 3. The reason we collected more observations for the **BMT** treatments is the fact, that will become clear below, that inclination to blame can only be measured for around half the participants in each session.

| | *Online Experiment* | | *Lab Experiment* | | |
|---|---|---|---|---|---|
| | INEQ | REL | | INEQ | REL |
| Belief in Meritocracy ( **M**) | 185 | 194 | **MTB** | 114 | 114 |
| Inclination to Blame (**B**) | 109 | 107 | **BMT** | 219 | 221 |
| Social Trust (**T**) | 318 | 322 | | 333 | 335 |

Table 3: Number of participants in different experiments. In the lab experiments **MTB** measure the effect on belief in meritocracy, treatments **BMT** on blame and for social trust we pool both lab treatments. In each online experiment we measure only one outcome. The total number of participants is $N$ =**1903**.

We now describe in turn first the details of the process to provide information and then our different outcomes and how they were elicited.

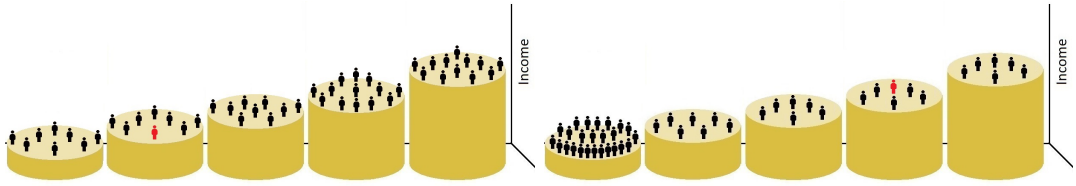
### 4.1.1 Information treatments

To study the causal effect of inequality exposure and personal relative position we exogenously assign participants to information treatments using income distributions of differing degrees of inequality.[10] In the **REL** treatments we also show participants their own position within a distribution. In order to do so we first need to elicit some information about their income and social class. Our **income questionnaire** elicits information about (i) self-reported social class, (ii) own or (for students) parents' annual gross income, (iii) monthly rent paid by (parents') household, (iv) size of (parents') household, (v) which grocery store the household does their monthly shopping in, (vi) if and where they go for holidays abroad, (vii) how much (parents')

treatment has on prior tasks. We are not worried too much about the first effect as by aggregating both orders we have substantial power to detect even a small effect on social trust. We address the second concern in Section 4.3. Last, Appendix Table E.2 shows the time elapsed between the information treatment and the start of the task.

[10]Providing this information to participants that is not of immediate instrumental value could be though of as a "priming technique". While common in Psychology, priming techniques are not often used in Economics. There are well known pros and cons of using primes (Cohn & Marechal, 2016). We discuss some specific considerations regarding the "primes" used in this study in Section 5.2.

household spends on eating out every week and (viii) the type of school (comprehensive, grammar, private, boarding) they attended. Appendix B.2 shows the exact questions and answer categories for all of these questions.




(a) Low (downward) info (b) High (upward) info

Figure 2: The pictures show the image used to induce downward and upward comparisons in the REL treatments for those in the medium income category. For those in the low (high) income category the red person was one bar lower (higher). In the treatments without relative position the figures were shown without the red person. Appendix Figure F.1 shows all the eight different pictures used.

Based on the answers to question (ii) we then sort participants into three income categories (low, medium or high) corresponding to an annual HH income of less than 25K, 25-45K and greater than 45K. Those cutoffs are calibrated to induce about equally big income categories in our lab sample.

We then randomly assign participants to receiving either downward or upward information regarding their position in terms of relative position using images like the ones shown in Figure 2. The bars on the figure correspond to income categories which match the mean income of the three categories low, medium, high in the three leftmost bars of distribution (a) used for the downward comparison and in the three rightmost bars of distribution (b) used for the upward comparison. Participants are told that the picture represents the income distribution of a borough in England and that "*based on your answers in the initial questionnaire, we have computed a rough estimate of your position in the income distribution of the borough*". Their own position was highlighted by showing one person in red as in Figure 2. The UK boroughs the two distributions represent are Chelsea and Kensington (mean annual income 178K GBP) and Norwich (mean annual income 26K GBP).[11] Those are the richest and poorest borough on average and hence will present a downward shock or upward shock to beliefs about the own relative position for most residents of the UK.[12] Upward and downward comparisons are randomly assigned allowing causal identification of the joint effect of inequality exposure and relative position on our outcomes. In the **INEQ** treatments we show them only an income distribution. The figures used in these treatments are identical to those used in the **REL** treatments with the only difference being that own position is not highlighted by a red person (see Appendix Figure F.1). Comparing these two treatments hence allows us to distinguish the effect of relative position from the effect of inequality exposure in itself.

We pretested the understanding of these pictures in two separate online surveys. In the first online survey ($n = 176$) we compared participants' understanding of these images with other

[11] Images are based on 2015-2016 data from the ONS (Office for National Statistics).

[12] Of course there is some variation within our income categories. For someone earning 44K in category "25-45K" being placed in the distribution of Norwich will not be as much of an upward shock as for someone in the same category who has income 25K. To place income categories on the different bars we computed the mean income within each category for our lab population which was elicited in a prior experiment.

representations of an income distribution (including e.g. a representation using quintiles). We chose the representations shown in Figure 2 as participants understood them well and much better than the other representations. The second online survey ($n = 108$) asked a different set of respondents which of the two distributions they perceive shows a more unequal income distribution. 84.26% of respondents found the distribution shown in Panel (b) more unequal and 7.41% found them "about the same". This is important for the interpretation of possible behaviour differences between those assigned to either distribution. Appendix A contains details about both of these surveys.

#### 4.1.2 Outcomes

After information was exogenously assigned we elicited our main outcomes. We measure each outcome in two ways: (i) using a non-incentivized survey question in the online experiment and (ii) using an incentivized task in the lab experiment. We now describe how we elicited these different outcomes in turn.

**"Belief in Meritocracy"** To elicit belief in meritocracy we first had participants complete a task with three components: an ability component (consisting of four questions from an IQ test), an effort task (counting the number of "1" entries in four different 20×20 binary matrices) and a luck task (coin toss). The total score in the task is $S = A + B + C$, where $A$ is the number of correct answers in the ability task (ranging from 0 to 4), $B$ the number of correct answers in the effort task (ranging from 0 to 4) and $C = 2$ if the coin falls on "tails" and 0 otherwise. How $S$ is determined is known to participants and described both in the paper instructions and on the screen. After completing the task participants are informed about their total score $S$, but not about the individual components. Participants receive $S$ GBP if this part is selected for payment (see paragraph "Other Details").

Afterwards participants are randomly matched in groups of ten participants and ranked by their overall score $S$, where 1 is the best rank (highest score) and 10 the worst rank (lowest score). Ties are broken randomly. Then participants are asked to guess their rank $R$. Guesses are incentivized using the interval scoring rule (Schlag & van der Weele, 2015). Participants specify a range $\left[\underline{R}, \overline{R}\right]$ using a slider. They are paid $9 - (\overline{R} - \underline{R}) * 2$ if the true rank $R \in \left[\underline{R}, \overline{R}\right]$ and zero otherwise. Hence participants face a trade-off between making sure the interval is large enough to contain the true answer, but also small to increase payments. We denote the mean of the interval $\left[\underline{R}, \overline{R}\right]$ by $\hat{R}$.

Next information is progressively revealed to participants. In Step 1 they are told their true rank $R$ and asked to guess their rank in the ability task $R_A$ and in the effort task $R_E$ as well as to indicate whether they believe they were lucky with the coin toss. The former two are incentivized in the same way as guesses about $R$. A correct guess on the luck component is rewarded by 18 GBP.[13] In Step 2 they are told their ability rank $R_A$ and asked to guess again their rank in the effort task $R_E$ and whether they were lucky. In Step 3 they are told whether they were lucky and asked again to guess their effort rank $R_E$.

To measure belief in meritocracy we focus on Step 1 where participants know their true rank

[13] 18 GBP is the maximum payment that can be obtained for the effort/ability rank guesses.

and are asked to guess their rank in the ability and effort tasks. Denote by $x^i := |\frac{\hat{R}^i_A + \hat{R}^i_E}{2} - R^i|$ the absolute difference between participant $i$'s average guess of their ability and effort rank and their true rank $R^i$. If $x^i$ is close to zero, then participant $i$ believes that their ability and effort rank explain their overall position well. The larger $x^i$ the larger is the gap between $i$'s belief in their ability/effort rank and their known overall rank $R^i$.

We denote by $\mathcal{F}(x)$ the distribution of this statistic among participants in the same treatment and denote by $x_{50}$ the 50th percentile of this distribution. We define belief in meritocracy (BIM) as follows

$$BIM^i = \begin{cases} 1 & \text{if } x^i < x_{50} \\ 0 & \text{else.} \end{cases}$$

Hence BIM is a dummy variable taking the value 1 for those participants who perceive a tighter association between their estimated rank in the effort and ability task ($\frac{\hat{R}^i_A + \hat{R}^i_E}{2}$) and their true overall rank $R^i$. In other words it takes the value 1 for participants who believe that effort and ability explain their overall rank well. The reason we decided to use a relative measure, i.e. focus on those who have high BIM *compared to others* in the experiment, is that (i) there is no natural cutoff for what it would mean to have "high" belief in meritocracy in the experiment and (ii) there is no absolute sense in which the task presented above is or is not meritocratic. We can analyze, though, whether those with "high" or "low" $BIM$ as characterized by our procedure do have more accurate beliefs and we do so below. We use a dummy as this is the standard way to measure belief in meritocracy in surveys (Newman et al., 2015).

It is important to note that the weight of the three components (ability, effort and luck) in determining the score (and hence also $R^i$) is known to all participants. If this was not the case then our measure would mostly pick up what participants believe about this specific lab task. This is not what we are interested in. Instead we think of $BIM^i$ as picking up a general attitude or mental state. In the online experiment we use the same measure as in Next Steps 8 (see Section 3). In Section 5.2 we show that the incentivized measure described here and the more common survey based measure yield consistent results.

**"Social Trust"** To measure social trust we randomly match participants in groups of three players. In a random dictator setting they are then (i) shown the sum of scores $\sum_i S_i$ of the three group members and asked to distribute it among themselves. Each group member makes this allocation independently.[14] Afterwards (ii) they are asked how much they believe each of the others allocated to the group members. The second part is the basis of our measure of social trust. Specifically, we measure social trust as the mean answer to part (ii). If part (i) is drawn for payment one of the decisions of the three group members is randomly chosen and implemented. If part (ii) is chosen for payment participants simply receive 2 GBP for each correct guess.[15] While trust is often measured by economists using trust games (Berg, Dickhaut, & McCabe, 1995), we are interested in capturing the aspect of social trust that is most closely related to

[14] Asking them to distribute $\sum_i S_i$ instead of an arbitrarily chosen "pie from the sky" seems the correct choice in our context as we are interested in whether people believe they are treated fairly by others and rewarded accordingly for their efforts (their contribution to $S_i$.)

[15] The reason that we chose not to use the interval scoring rule for this part is (i) for simplicity and to save time and (ii) as we are only interested here in how amounts rank across conditions and *not* in cardinal differences.

"belief in a just world" and in particular capture the belief that one is treated by others in a fair way. A random dictator game preceded by a production stage is one way to capture these beliefs (Cappelen, Drange, Soerensen, & Tungodden, 2007). In the online experiment we use the same measure as in Next Steps 8. Unlike the measure used in the lab this measure is not incentivized, but again we see consistent results across the two measures.

**Other measures** We also elicited a measure of peoples' inclination to blame which was pre-registered as a main outcome. The evidence we find regarding this outcome is inconclusive (despite increasing sample size via an amendment to our pre-registration). We hence discuss this measure only briefly, but all regression tables for this outcome can be found in the Appendix. Further, a measure of risk aversion and a measure of competitiveness will be used as control variables in our regressions. See Appendix B for details of how they were elicited. In a post-experimental questionnaire we also elicited participants' aspirations (beliefs about future earnings, GPA etc.) as well as some other covariates (such as gender, age etc). See Appendix B for details. In Appendix D we discuss how some of these outcomes are affected by the exogenously assigned information.

**Other Details** Lab Experiments were conducted at Essex Lab at the University of Essex (`https://www.essex.ac.uk/research/essexlab`). The study was originally pre-registered on EGAP, but after their site shut down it was transferred to OSF and can now be found under `https://osf.io/9mhvg` and `https://osf.io/muh8s/`. Participants in the lab experiments are paid for two randomly selected tasks in addition to a show up fee of 4 GBP and a flat fee for filling in the questionnaire (2 GBP). Average earnings were 14.86 GBP with a range between 7 GBP and 39 GBP. In the online experiments we paid a flat fee of 1.50 GBP to all participants. 668 people participated in the lab experiment and 1235 people participated in our online experiments. Ethical approval was obtained by the University of Essex (Faculty of Social Sciences subcommittee) in October 2018.

### 4.2 Sample Characteristics

Table 4 shows summary statistics for some of the characteristics of our participants in our different experiments. The vast majority of our participants in the lab are students, but there is a substantial minority of 13-20 % non-students. The share of female participants ranges between 40-48% across treatments. The average age ranges between 23.7-27.1 years. We restricted the sample to consist of UK nationals only. In terms of their self-reported social class about an equal amount of participants classify themselves as working or middle class. A much smaller fraction (ranging from 8-15%) classify themselves as "upper class". We designed income categories ("low", "middle", "high") in such a way that - based on our expectations from previous experiments in the same lab - we would have around a third of participants in each category. Table 4 shows that this was successful. In each treatment there is about a third of participants in each income category in the lab.

In the online experiments participants are somewhat older (mean age ranges between 33.4-35.9 years) and only a minority (17-25%) here are students. The share of women ranges between

| Type of Prime | Lab Experiments | | | | Online Experiments | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Outcomes | **REL MTB** | **INEQ MTB** | **REL BMT** | **INEQ BMT** | **REL M** | **INEQ M** | **REL B** | **INEQ B** | **REL T** | **INEQ T** |
| mean age | 27.1 | 26.6 | 24.6 | 23.7 | 33.4 | 35.9 | 35.2 | 35.6 | 34.0 | 35.1 |
| share female | 0.48 | 0.40 | 0.44 | 0.48 | 0.69 | 0.65 | 0.64 | 0.75 | 0.66 | 0.63 |
| share students | 0.80 | 0.82 | 0.87 | 0.86 | 0.19 | 0.17 | 0.21 | 0.18 | 0.19 | 0.25 |
| share working class | 0.49 | 0.32 | 0.32 | 0.41 | 0.40 | 0.39 | 0.44 | 0.40 | 0.41 | 0.40 |
| share middle class | 0.40 | 0.58 | 0.52 | 0.48 | 0.55 | 0.57 | 0.50 | 0.54 | 0.54 | 0.55 |
| share upper class | 0.09 | 0.09 | 0.15 | 0.08 | 0.05 | 0.04 | 0.06 | 0.06 | 0.05 | 0.05 |
| share lower income | 0.39 | 0.30 | 0.32 | 0.33 | 0.56 | 0.61 | 0.64 | 0.58 | 0.51 | 0.54 |
| share middle income | 0.32 | 0.38 | 0.36 | 0.37 | 0.30 | 0.31 | 0.29 | 0.32 | 0.35 | 0.32 |
| share higher income | 0.29 | 0.32 | 0.31 | 0.30 | 0.14 | 0.08 | 0.07 | 0.10 | 0.14 | 0.14 |
| N | 114 | 114 | 219 | 221 | 194 | 185 | 107 | 109 | 322 | 318 |

Table 4: Summary Statistics for participant characteristics across different treatments of the lab and online experiments.

63-75%. Participants in the online experiment are also less likely to identify as upper class and to belong to the high income bracket compared to the lab sample.

### 4.3 Correlation among outcome measures

| | *Raw Correlation* | | |
|---|---|---|---|
| | BIM | Social Trust | Blame |
| BIM | - | -0.0318 | 0.1470** |
| Social Trust | - | - | -0.1536*** |
| Blame | - | - | - |

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

Table 5: Correlation among outcome measures.

Having elicited all three outcomes in the lab experiment allows us to study how our main outcome measures correlate among each other. We find two correlations that are substantial and statistically significant. Those who have higher belief in meritocracy also have a substantially higher inclination to blame. This is intuitive as both are measures of people's inclination to ascribe responsibility for outcomes to a persons' actions - in the case of *BIM* themselves and in the case of blame others - as opposed to outside forces like luck or the design of the economic and social system. The second correlation we find is between blame and social trust. Those with lower social trust are substantially more likely to blame. There is only a very small and not statistically significant correlation between social trust and belief in meritocracy.[16] Appendix Table E.3 shows that these correlations also appear in a regression where other covariates are controlled for.

## 5 Results: Attribution

In this section we present our results on attribution. We start with belief in meritocracy measures in the lab (Section 5.1), then move to the online experiment on belief in meritocracy (Section 5.2). We then discuss the inequality prime more extensively (Section 5.3) and last briefly discuss the inconclusive results on blame (Section 5.4).

[16] This is true for both orders **MTB** ($\rho = -0.0019$) and **BMT** ($\rho = -0.0470$).

## 5.1 Lab Experiment: Belief in Meritocracy

We start by discussing some descriptives and covariates of belief in meritocracy in our lab experiment (Section 5.1.1). We then discuss the causal effect of inequality exposure and relative position on belief in meritocracy (Section 5.1.2) before discussing some alternative mechanism and additional results (Section 5.1.3). In Section 5.2 we discuss the results from our online experiments.

### 5.1.1 Descriptives and Covariates of Belief in Meritocracy

The procedure described in Section 2.1 classifies 42% of our participants in both **REL-MTB** and **INEQ-MTB** as having high belief in meritocracy.[17] Those with high belief in meritocracy believe that the absolute difference between their ability/effort rank and their overall rank $x^i$ is on average 0.6 (median 0.5, range [0,1.25]) in **REL-MTB** and 0.6 (0.5, [0,1.5]) in **INEQ-MTB**. For those with low belief in meritocracy, by contrast, these numbers are 2.41 (2.5, [1.5,8]) in **REL-MTB** and 2.48 (2.5, [1.75,6]) in **INEQ-MTB**.

We first ask whether high belief in meritocracy is justified in our experiment. Appendix Figure F.3 shows the difference in the average ability/effort rank and the overall rank across the rank distribution. The figure shows that high belief in meritocracy is justified in our experiment. The average difference $x^i$ is almost always below 1.5 across the rank distribution and hence not enough to justify "low belief in meritocracy".

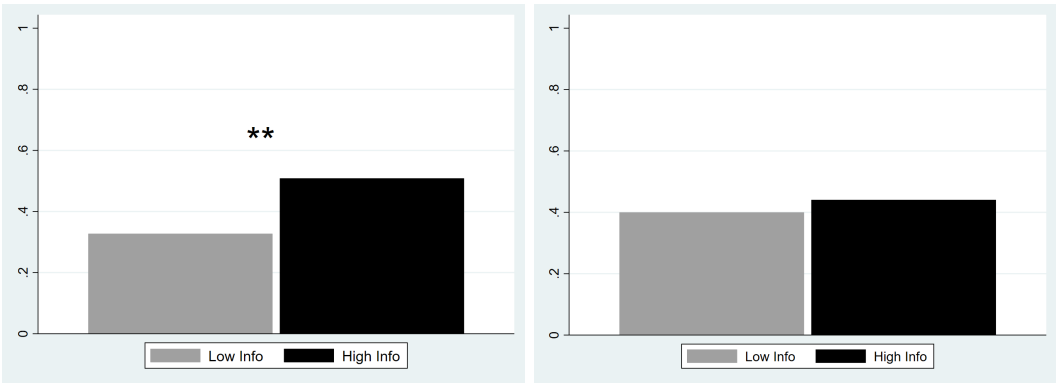
We now study demographic as well as experiment based covariates of high belief in meritocracy in these two treatments. We consider four demographic covariates: age, income, gender and class. Appendix Table E.3 shows that across both **REL-MTB** and **INEQ-MTB** and in line with the correlational evidence found by Newman et al. (2015) and in the Next Steps 8 survey those with higher income have higher belief in meritocracy. There is no statistically significant impact of age, gender or self-reported social class. Appendix Figure F.2 shows that high belief in meritocracy is present across all ranks 1-10 and there are no statistically significant differences in the proportions of those classified as "high belief in meritocracy" across the rank distribution.

### 5.1.2 The Causal Effect of Inequality Exposure and Relative Position

We are interested in the causal effect of inequality exposure and relative position on belief in meritocracy. Based on the correlational evidence from the Next Steps 8 survey we would expect that being assigned to a high relative position should increase belief in meritocracy, while being assigned to higher inequality should decrease belief in meritocracy.

Figure 3 shows the share of participants who have high belief in meritocracy ($BIM = 1$, see Section 4.1.2) depending on whether they were assigned to the low or high relative position (**REL-MTB**, Panel (a)) and depending on whether they were assigned to low or high inequality using only the distribution (**INEQ-MTB**, Panel (b)). The figure shows that among those exogenously assigned to information suggesting a high relative position a substantially bigger

[17] The reason that fewer than 50 percent of participants are classified as high belief in meritocracy is that our definition requires them to be strictly below the 50th percentile. Our treatment effects are robust to slight changes in this cutoff.




(a) REL-MTB (b) INEQ-MTB

Figure 3: Belief in Meritocracy by whether participants were assigned to high or low info regarding relative position (Panel (a)) and by whether they were shown high or low inequality (Panel (b)). Stars are from t-test based on regression in Tables 6 and 7. *** p<0.01, ** p<0.05, * p<0.1

share display high belief in meritocracy than among those assigned to a low relative position. There seems to be no difference based on inequality exposure alone.

| | *Belief in Meritocracy (BIM)* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.181** | 0.180** | 0.178** | 0.173* | 0.224** | 0.222* |
| | (0.041) | (0.041) | (0.045) | (0.070) | (0.076) | (0.084) |
| med income | | 0.075 | 0.088 | 0.119 | 0.078 | -0.047 |
| | | (0.041) | (0.055) | (0.070) | (0.062) | (0.191) |
| high income | | 0.338** | 0.331** | 0.324* | 0.314 | 0.048 |
| | | (0.094) | (0.118) | (0.119) | (0.156) | (0.163) |
| Constant | 0.327*** | 0.206** | 0.231 | 0.0514 | 0.227 | -0.089 |
| | (0.055) | (0.062) | (0.154) | (0.261) | (0.365) | (0.322) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 114 | 114 | 114 | 113 | 113 | 113 |
| R-squared | 0.034 | 0.116 | 0.151 | 0.180 | 0.256 | 0.434 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Belief in Meritocracy (*BIM* dummy) in treatment **REL-MTB**. Extra Income Controls are fixed effects for answers from income questionnaire. The smaller set includes questions 1 and 4, the larger set (YES$^+$) all questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy and overall score S. Standard errors clustered at the session level.

Table 6 shows regression analysis for treatment **REL-MTB** where we regress *BIM* on a dummy indicating whether the participant was assigned to a high relative position as well as two income fixed effects. The coefficient $\beta$ on "high info" shows the causal effect of relative position, while the income coefficients show a correlational effect similar to what we saw in Next Steps 8. The table shows that those who are assigned to a high relative position are 56% more likely to express high belief in meritocracy. The effect is robust when other demographic as well as additional income controls are included (columns (3)-(6)).

**Heterogeneity and Persistence of the Effect** We consider a number of sample splits using always regressions analogous to specification (1) in Table 6. We find that the effect is particularly strong for those in the highest income category ($\beta = 0.365^{***}$) and for those who

self-identify as upper class ($\beta = 0.692^{**}$).[18] The information treatment has a similar effect for men ($\beta = 0.154^{**}$) and women ($\beta = 0.214^{**}$). We can also ask how persistent the effect is. Recall that after having guessed their rank in ability and effort participants are told their rank in the ability task and asked to guess again their rank in the effort task. The effect of information persists when we define *BIM* based on this second guess with $\beta = 0.179^{**}$. At the third step, however, after participants have also been told whether they were lucky or not, the effect disappears ($\beta = 0.056$). Hence, when all information has been revealed and there is no longer any uncertainty about the rank in other components, then the type of information provided does no longer have an effect.

| | *Belief in Meritocracy (BIM)* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.040 | 0.037 | 0.025 | 0.014 | -0.069 | -0.114 |
| | (0.049) | (0.049) | (0.040) | (0.039) | (0.066) | (0.064) |
| med income | | 0.128 | 0.128 | 0.128 | 0.198 | 0.067 |
| | | (0.118) | (0.119) | (0.120) | (0.102) | (0.054) |
| high income | | -0.031 | -0.021 | -0.025 | -0.000 | -0.080 |
| | | (0.125) | (0.129) | (0.162) | (0.175) | (0.207) |
| Constant | 0.400*** | 0.364*** | 0.140 | 0.074 | 0.197 | 0.332 |
| | (0.050) | (0.030) | (0.278) | (0.286) | (0.595) | (0.725) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^{+}$ |
| Other Controls | NO | NO | YES | YES$^{+}$ | YES$^{+}$ | YES$^{+}$ |
| Observations | 114 | 114 | 114 | 114 | 114 | 114 |
| R-squared | 0.002 | 0.022 | 0.049 | 0.107 | 0.244 | 0.407 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Belief in Meritocracy (*BIM* dummy) in treatment **INEQ-MTB**. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1 and 4, the larger set (YES$^{+}$) all questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy and overall score S. Standard errors clustered at session level.

Table 7 shows regression analysis for treatment **INEQ-MTB** where participants were only shown the income distribution and were *not* shown their personal position in the distribution. In this case the information seems to have little effect. The coefficient $\beta$ is substantially smaller compared to **REL-MTB**, changing in sign and not statistically different from zero. Hence without information on one's own relative position inequality exposure in itself do not seem to affect belief in meritocracy. We now discuss potential mechanisms behind these results.

### 5.1.3 Alternative Mechanisms

We start by discussing two possible alternative mechanisms which might drive the causal effect identified in treatment **REL-MTB**. Specifically, as we introduce a novel measure of belief in meritocracy, we first ask whether the information treatment might affect some other outcome which is picked up by our *BIM* measure.

#### Optimism and (Over-) Confidence

The first possibility we explore is whether being assigned to a high relative position increases confidence and makes participants more optimistic about their performance in terms of the ability and effort tasks. If that was the case, then our measure of belief in meritocracy might be

[18] For those in the lowest income category $\beta = 0.128^{**}$ and for those in the middle income category $\beta = 0.072^{*}$.

picking up some of this effect. We hence, in analogy to our BIM definition, define `confidence` using a dummy taking the value "1" for those who believe they are in the better half of the distribution.

| | *Confidence* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | -0.068 | -0.073 | -0.083 | -0.084 | -0.048 | 0.005 |
| | (0.066) | (0.066) | (0.074) | (0.041) | (0.056) | (0.038) |
| med income | | 0.170 | 0.151 | 0.039 | -0.003 | -0.132 |
| | | (0.101) | (0.074) | (0.100) | (0.123) | (0.150) |
| high income | | -0.120* | -0.120 | -0.041 | 0.008 | -0.085 |
| | | (0.047) | (0.070) | (0.057) | (0.084) | (0.188) |
| Constant | 0.509*** | 0.491*** | 0.105 | 0.906** | 1.349*** | 1.411*** |
| | (0.052) | (0.044) | (0.334) | (0.292) | (0.061) | (0.214) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 114 | 114 | 114 | 113 | 113 | 113 |
| R-squared | 0.005 | 0.057 | 0.073 | 0.368 | 0.446 | 0.559 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Confidence in treatment **REL-MTB**. Extra Income Controls are fixed effects for answers from income questionnaire. The smaller set includes questions 1 and 4, the larger set all questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy and overall score S. Standard errors clustered at the session level.

We would like to know whether the information affects confidence defined in this way. Table 8 reproduces Table 6 using `confidence` as outcome instead of *BIM*. The table shows that there is no effect of the information on confidence. The coefficient $\beta$ is small, changing in sign and not statistically significant. It also has the "wrong" sign in five out of six specifications suggesting that being assigned to a higher relative position would lower confidence. Being assigned to a high relative position hence does not seem to make people more confident on their task performance.

### Anchoring

The second possibility we explore is that priming people to a "good position" in society increases their belief that they have a "good" rank in the task. To evaluate the possibility of such an anchoring effect we focus on the average guessed rank in ability and effort $\frac{\hat{R}^i_A+\hat{R}^i_E}{2}$ and regress it on the same exogenous variables as in Table 6.

Table 9 shows the results. There is no statistically significant effect of the information provided on the average guessed rank. Further, the sign of the coefficient $\beta$ is not in line with the anchoring story, as it would imply that those being assigned to a "good" position in society believe that they are ranked worse in the experimental task. Anchoring does not seem to drive the results.

In sum, being assigned to a high relative position neither makes people more confident in their task performance, nor does it make them believe they have a better rank in these tasks. It does, however, lead them to perceive a tighter association between their performance and their overall rank as we have seen above. We interpret this as increased belief in meritocracy.

| | *Anchoring* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.313 | 0.298 | 0.317 | 0.371 | 0.317 | 0.008 |
| | (0.344) | (0.335) | (0.293) | (0.234) | (0.294) | (0.298) |
| med income | | 0.403 | 0.411 | 0.185 | 0.210 | 0.467 |
| | | (0.422) | (0.429) | (0.354) | (0.390) | (0.468) |
| high income | | -0.802* | -0.740* | -0.232 | -0.449 | -0.204 |
| | | (0.373) | (0.332) | (0.219) | (0.466) | (0.764) |
| Constant | 5.077*** | 5.186*** | 6.071*** | 8.844*** | 7.918** | 7.449*** |
| | (0.243) | (0.405) | (0.790) | (1.180) | (1.816) | (1.172) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 114 | 114 | 114 | 113 | 113 | 113 |
| R-squared | 0.007 | 0.068 | 0.083 | 0.446 | 0.496 | 0.625 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Anchoring treatment **REL-MTB**. Extra Income Controls are fixed effects for answers from income questionnaire. The smaller set includes questions 1 and 4, the larger set all questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy and overall score S. Standard errors clustered at the session level.

## 5.2 Online Experiment: Belief in Meritocracy

In this subsection we ask whether the same information also affects belief in meritocracy when measured using the standard survey measure of belief in meritocracy. In our online experiment we use the same questionnaire and information as in the lab and the same outcomes (measures of belief in meritocracy) as in the Next Steps 8 survey (see Section 3). Hence, as in the lab, we can make *causal* inference on belief in meritocracy, but unlike in the lab the outcome measures here are not incentivized. Appendix Table A.1 compares the characteristics of the different samples: our lab sample, our online sample and the Next Steps 8 sample.

| | *Belief in Meritocracy: Survey Measure* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.175** | 0.176** | 0.191*** | 0.177** | 0.158** | 0.180** |
| | (0.069) | (0.069) | (0.070) | (0.069) | (0.068) | (0.080) |
| med income | | 0.180** | 0.169** | 0.113 | 0.107 | 0.091 |
| | | (0.079) | (0.080) | (0.079) | (0.080) | (0.091) |
| high income | | 0.043 | 0.011 | 0.006 | 0.061 | 0.100 |
| | | (0.102) | (0.105) | (0.102) | (0.114) | (0.139) |
| Constant | 0.505*** | 0.447*** | 0.266* | -0.0961 | -0.184 | -0.398 |
| | (0.0493) | (0.0579) | (0.147) | (0.186) | (0.229) | (0.294) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 194 | 194 | 190 | 189 | 188 | 187 |
| R-squared | 0.032 | 0.058 | 0.098 | 0.152 | 0.238 | 0.302 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Belief in Meritocracy using survey measures and the **REL** information. Extra Income Controls are fixed effects for answers from income questionnaire. The smaller set includes questions 1 and 4, the larger set all questions. Other Controls are age, gender and student status. The larger set also includes self-reported measures of risk aversion and competitiveness.

We then redo the analysis presented in Tables 6 and 7 but this time using the survey measures of belief in meritocracy as outcome variable. Table 10 shows the results for the surveys where people are assigned to their relative position. The table again shows a clear effect of the information treatment on belief in meritocracy. Participants assigned to a high relative position

| | *Belief in Meritocracy: Survey measure* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | -0.037 | -0.038 | -0.034 | -0.035 | -0.048 | -0.043 |
| | (0.073) | (0.073) | (0.076) | (0.077) | (0.082) | (0.091) |
| med income | | -0.008 | 0.012 | 0.016 | -0.036 | -0.072 |
| | | (0.081) | (0.084) | (0.085) | (0.089) | (0.105) |
| high income | | 0.245* | 0.239* | 0.257* | 0.213 | 0.240 |
| | | (0.134) | (0.136) | (0.138) | (0.146) | (0.169) |
| Constant | 0.543*** | 0.525*** | 0.404** | 0.335 | 0.109 | -0.178 |
| | (0.0518) | (0.0611) | (0.159) | (0.208) | (0.267) | (0.320) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 185 | 185 | 181 | 177 | 176 | 174 |
| R-squared | 0.001 | 0.021 | 0.027 | 0.032 | 0.081 | 0.201 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Belief in Meritocracy using survey measures and the **INEQ** information. Extra Income Controls are fixed effects for answers from income questionnaire. The smaller set includes questions 1 and 4, the larger set all questions. Other Controls are age, gender and student status. The larger set also includes self-reported measures of risk aversion and competitiveness.

are 53% more likely to express high belief in meritocracy. By contrast if participants are only shown the inequality information there is no discernible effect (Table 11). Hence we obtain very similar results using the established survey based measure of belief in meritocracy as we do with our novel incentivized measure in the lab.

### 5.3 Providing Information about Inequality

We have seen that providing information about inequality only does not cause changes in belief in meritocracy neither when measured by using our incentivized lab task nor when measured using the standard social survey question. On the other hand we have seen that there is a correlational effect in large social surveys where inequality is measured by the local Gini coefficient.[19] There could be several reasons for this difference. First, it could be that inequality per se does not affect belief in meritocracy but that the local Gini coefficient correlates with something else that does. It is also difficult in the field to disentangle relative position from inequality exposure per se and variation in the Gini coefficient will induce variation in both. Second, it could be that there is indeed an effect but that our images of distributions don't lead people to perceive inequality. Or it could be that the two distributions used in the experiment display too similar degrees of inequality to induce substantial enough differences in belief in meritocracy for us to detect.

To address in particular the latter concern we ran an online survey ($n = 331$) where we use ten different (hypothetical) societies with levels of inequality ranging from complete equality to very high inequality (see Appendix Figures A.2). We again inform participants using one of these distributions (randomly selected) and ask them to indicate belief in meritocracy using the same measure as in the survey discussed in Section 5.2 and in Next Steps 8. At the end of the survey we show them (a different) distribution and ask them to indicate on a scale from 0,...,10 how unequal they believe this society is.

Figure 4 shows the share of respondents for who the belief in meritocracy dummy takes

[19] In Next Steps 8 the average effect of "Gini" is $-0.69^{***}$ in specification (1) of Table 1.

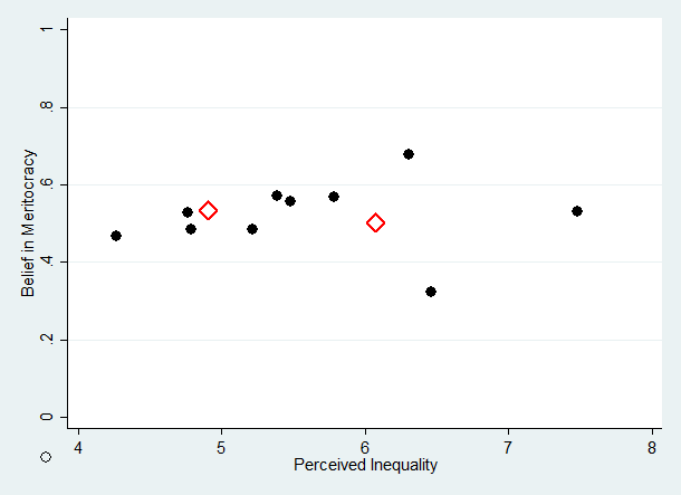



Figure 4: Belief in Meritocracy depending on how unequal society is perceived (on a scale from 0,...,10) by respondents. The red diamonds indicate the two distributions used in the lab experiment and in the online survey discussed in Section 5.2.

the value 1 as a function of how unequal the society they were shown with is perceived. The figure shows that there are substantial differences in how unequal societies are perceived on average with the measure of perceived inequality ranging from 4.2 to 7.6. Also the distributions used in the lab experiment (indicated by red diamonds) differ in terms of how unequal they are perceived. However, even the more substantial differences in terms of perceived inequality do not translate into differences in belief in meritocracy.

This evidence suggests that there may not be a direct causal link between inequality exposure in itself (distribution only) and belief in meritocracy. There are also intuitive reasons to believe that inequality *per se* should not have an unambiguous effect on belief in meritocracy. On the one hand fully equal societies, where everyone has the same income, are unlikely to be meritocracies as it seems not possible to get ahead of others by providing effort. On the other hand societies with extreme levels of inequality are also unlikely to be meritocracies as it is unlikely that extreme differences in earnings are caused by differential effort or ability within one generation. Hence it seems entirely plausible that societies at both extremes of the equality spectrum would be associated with a lack of meritocracy by participants. It seems then likely that the correlational effects found in surveys relate to other factors associated with inequality that go beyond the income distribution alone, which includes the possibility that they are entirely driven by relative position. We cannot rule out, on the other hand, that there is a causal effect and that the information provided is not strong enough to trigger an impact of inequality *per se* (distinct from personal relative position). There are several reasons why this might be the case. Inequality perceptions and perceptions of economic indicators more generally are more abstract and arguably less personal or ego-relevant and hence less likely to trigger an effect. It could also be the inequality matters more when it is more local, e.g. applying to the borough that participants live in.[20] In sum, we have identified a clear and robust positive influence of relative position on belief in meritocracy. Based on the analysis in this Section it is doubtful to us that there is an additional distinct effect of inequality exposure *per se*.

[20]The inequality information does affect social trust (see Section 6), though.

### 5.4 Blame

To elicit attribution of blame we use a task previously used by Gurdal, Miller, and Rustichini (2013). There is no prior empirical research relating inequality exposure and inclination to blame, but based on the psychological mechanisms involved in attribution we might expect inclination to blame to increase with relative position (Brooks, Hoff, & Pandey, 2018; Magni, 2020). Appendix Table E.6 shows regression results. There is a small positive, but not statistically significant effect of relative position on blame. There is also a positive correlational effect of income which is very imprecisely estimated, though. Note also that the $R^2$ increases substantially each time we add income controls (columns (5) and (6)) from the initial income questionnaire. Hence, while we are unable to detect a statistically significant causal effect of relative position on blame, additional controls from the income questionnaire seem to be able to explain a substantial share of the variation in blame.[21] Table E.7 shows the results for the inequality information. The table shows that there is no statistically significant effect of the inequality information on blame. The coefficient $\beta$ is very close to zero but also not very precisely estimated. As with belief in meritocracy we also collected a non-incentivized survey measure of inclination to blame. Appendix Tables E.8 and E.9 show the results. They again show no statistically significant effect of the information on inclination to blame, though we do again see a positive coefficient when participants are assigned to relative position and a positive correlational effect of income on inclination to blame. Overall, our results on blame remain inconclusive.

## 6 Results: Social Trust

We now study social trust. As before we first discuss the lab experiment on social trust (Section 6.1) . In Section 6.2 we discuss the results from our online experiment on social trust.

### 6.1 Lab Experiment: Social Trust

#### 6.1.1 Descriptives and Covariates of Social Trust

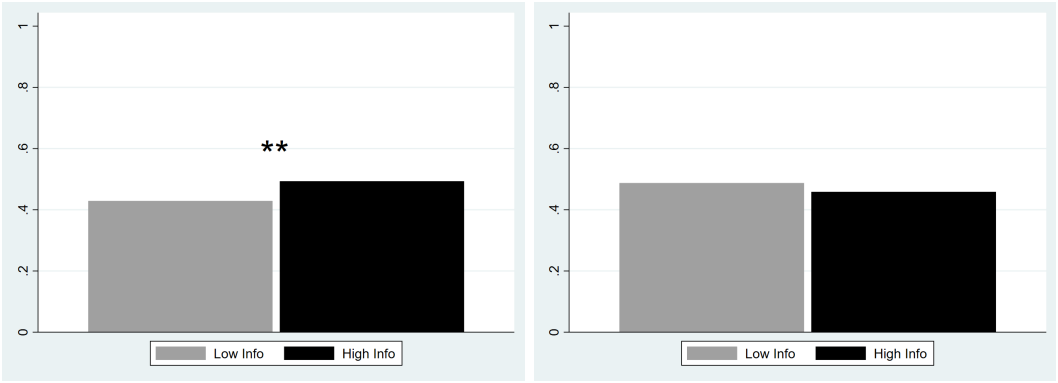
On average participants in both the **REL** and **INEQ** treatments believe that dictators will share 46% of the pie with others. There are no statistically significant differences neither by age, income nor gender. Upper class participants seem to have lower social trust compared to middle and working class participants.[22] In terms of experiment based covariates we find no statistically significant associations except for the negative relationship with inclination to blame discussed already above (Appendix Table E.3).

[21] We note that the causal effect is imprecisely estimated. We did do a power analysis after collecting half our sample size which suggested that we should detect an effect of the size found in those data ($\beta = 0.331$) with 80 percent probability `https://osf.io/muh8s/`. Note also that we do get statistical significance in the pooled data from the **REL** treatments ($\beta = 0.313^{*}$, $p = 0.061$), however as there is positive correlation between blame and belief in meritocracy we do not want to over-interpret these results.

[22] One reason why upper class participants might have lower social trust in our sample is that they are the minority among participants. They hence express low social trust towards a population of participants mostly coming from a working or lower middle class background. It should also be kept in mind that the sample of participants self-identifying as upper class is relatively small.

### 6.1.2 The Causal Effect of Inequality Exposure and Relative Position

We now ask whether being assigned to a high relative position or a high degree of inequality affects social trust. If the survey evidence discussed in Section 3 can be interpreted as causal, then we would expect a positive effect of relative position on social trust.




(a) REL (b) INEQ

Figure 5: Social Trust by whether participants were shown high relative position (Panel (a)) and by whether they were shown the income distribution only (Panel (b)). Stars are from t-test based on regression in Table 12. *** p<0.01, ** p<0.05, * p<0.1

Panel (a) in Figure 5 shows that social trust indeed increases on average when people are assigned to a high relative position. By contrast priming participants using only the income distributions with varying degrees of inequality does not induce changes in our measure of social trust. Table 12 shows regression results for the **REL** treatments. Participants who are assigned to a high relative position display about 15 percent higher levels of social trust compared to those assigned to a low relative position. They expect dictators to share around 49 percent of the pie while those assigned to a low relative position expect them to share around 43 percent (column (1)). The effect is robust to including additional demographic, experiment based and income controls across columns (2)-(6). As what is deemed fair or "what one deserves" is likely to depend on score, columns (3)-(6) also control for participants' scores as well as the size of the total pie. Table 13 shows the effect of the inequality information. Those assigned to a high degree of inequality display about 6% less social trust than others. The effect is not statistically significant, though, in four out of six specifications and only significant at the 10% level in the remaining two.

**Heterogeneity** The positive effect of own relative position is similarly strong for both genders ($\beta = 0.072$ for men and $\beta = 0.056^{**}$ for women). It is also similarly strong for high ($\beta = 0.089^{**}$) and low income earners ($\beta = 0.095^{**}$), though it is smaller for the middle income category ($\beta = 0.010$). We cannot say whether this is a fundamental effect or due to the fact that the information shock is stronger for the former categories compared to the latter. In terms of social class we find a similar pattern with a strong effect for upper ($\beta = 0.166$) and working class ($\beta = 0.087^{*}$) participants and a smaller effect for middle class participants ($\beta = 0.044^{*}$). Across the two different orders effect sizes are virtually identical (**REL-MTB**: $\beta = 0.0632^{**}$; **INEQ-MTB**: $\beta = 0.0639^{**}$ for specification (1)).

| | *Social Trust* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.064** | 0.064** | 0.065** | 0.067** | 0.067** | 0.071** |
| | (0.023) | (0.023) | (0.023) | (0.025) | (0.026) | (0.024) |
| medium income | | 0.021 | 0.020 | 0.032 | 0.018 | 0.032 |
| | | (0.036) | (0.036) | (0.035) | (0.058) | (0.057) |
| high income | | 0.008 | 0.005 | 0.006 | 0.034 | 0.057 |
| | | (0.038) | (0.039) | (0.038) | (0.075) | (0.075) |
| Constant | 0.429*** | 0.419*** | 0.448*** | 0.332*** | 0.225 | 0.245 |
| | (0.016) | (0.028) | (0.095) | (0.091) | (0.155) | (0.174) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 335 | 335 | 335 | 334 | 334 | 334 |
| R-squared | 0.020 | 0.021 | 0.024 | 0.041 | 0.095 | 0.150 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Social trust **REL** treatments. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy, the size of the total pie and overall score S.

| | *Social Trust* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | -0.028 | -0.028 | -0.029 | -0.029 | -0.033* | -0.039* |
| | (0.021) | (0.021) | (0.020) | (0.020) | (0.019) | (0.020) |
| medium income | | 0.014 | 0.012 | 0.012 | 0.025 | 0.027 |
| | | (0.032) | (0.032) | (0.033) | (0.039) | (0.036) |
| high income | | 0.006 | 0.006 | 0.005 | 0.021 | 0.041 |
| | | (0.041) | (0.040) | (0.041) | (0.068) | (0.061) |
| Constant | 0.487*** | 0.483*** | 0.487*** | 0.486*** | 0.425*** | 0.409*** |
| | (0.016) | (0.024) | (0.083) | (0.101) | (0.091) | (0.103) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 333 | 333 | 333 | 333 | 333 | 333 |
| R-squared | 0.004 | 0.005 | 0.010 | 0.012 | 0.098 | 0.141 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Social trust **INEQ** treatments. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy, the size of the total pie and overall score S.

## 6.2 Online Experiment: Social Trust

We also used the same income questionnaire and information provision in an online experiment where we measured their effect on answers to the standard survey question "Most people in life can be trusted". This question is used in Next Steps 8, the European Value Survey and other general survey measures of social trust. We aimed for a similar sample size as in the lab experiment where we did detect a statistically significant effect of relative position on our lab based measure of social trust, but we had a small percentage of drop-outs (fewer than 5%). Appendix Table A.3 compares sample characteristics of participants in Next Steps 8, the lab and the online experiment.

Table 14 shows the results for the **REL** information treatment. Those assigned to a higher relative position express around 10% higher levels of social trust using the survey measure than those assigned to a lower relative position. The effect size corresponds to about three standard

| | *Social Trust: Survey Measure* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.501** | 0.504** | 0.529** | 0.537** | 0.514** | 0.521** |
| | (0.233) | (0.230) | (0.227) | (0.227) | (0.229) | (0.232) |
| medium income | | 0.704*** | 0.638** | 0.671*** | 0.649** | 0.492* |
| | | (0.253) | (0.257) | (0.258) | (0.265) | (0.281) |
| high income | | 0.887*** | 0.867** | 0.860** | 0.715* | 0.539 |
| | | (0.336) | (0.343) | (0.342) | (0.372) | (0.393) |
| Constant | 4.956*** | 4.570*** | 3.087** | 2.511 | 2.178 | 2.777 |
| | (0.165) | (0.200) | (1.423) | (1.575) | (1.657) | (1.765) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 322 | 322 | 321 | 321 | 321 | 320 |
| R-squared | 0.014 | 0.048 | 0.079 | 0.091 | 0.104 | 0.188 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 14: Survey measure of social trust **REL** treatments. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion and a self reported competitiveness measure.

| | *Social Trust: Survey Measure* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | -0.461** | -0.542** | -0.595*** | -0.548** | -0.670*** | -0.798*** |
| | (0.232) | (0.230) | (0.222) | (0.219) | (0.223) | (0.232) |
| medium income | | 0.561** | 0.274 | 0.203 | 0.086 | 0.015 |
| | | (0.256) | (0.254) | (0.251) | (0.256) | (0.268) |
| high income | | 1.026*** | 0.973*** | 0.922*** | 0.791** | 0.918** |
| | | (0.346) | (0.336) | (0.330) | (0.369) | (0.390) |
| Constant | 5.790*** | 5.511*** | 2.968** | 0.714 | 0.883 | -0.337 |
| | (0.165) | (0.188) | (1.352) | (1.470) | (1.574) | (1.685) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 318 | 318 | 317 | 317 | 317 | 312 |
| R-squared | 0.012 | 0.045 | 0.119 | 0.157 | 0.191 | 0.274 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 15: Survey measure of social trust **INEQ** treatments. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion and a competitiveness measure.

deviations. The table also shows a substantial correlational effect of income with those with higher income displaying higher levels of social trust. Table 15 shows the results for the **INEQ** treatment. Respondents assigned to a higher degree of inequality subsequently show lower levels of trust. The effect size here is also substantial corresponding to about 2.8 standard deviations.

To sum up we have identified a positive effect of relative position on social trust and a negative effect of inequality exposure per se. The effects show up both using our incentivized lab experimental measure as well as the standard survey measure of social trust.

# 7 Conclusions

We provide causal evidence of non-negligible effects of inequality exposure and personal relative position on attribution and social trust. Our design allows us not only to establish causality but also to distinguish between the impact of personal relative position and inequality exposure per se. We found that a higher personal relative position leads to higher belief in meritocracy and

higher levels of social trust. Inequality exposure by itself decreases social trust. These results are evidence for both the palliative and corrosive effect of inequality.

Those results have important implications for our understanding of the medium and long run impacts of inequality. A high relative position leads to an increased belief in meritocracy and increased social trust, while a low relative position leads to rejection of meritocracy and low levels of social trust. For the advantaged, meritocratic beliefs can resolve potential feelings of guilt when exposed to inequality (Bullock, 2008; Jost & Hunyady, 2003). Wealth in this case is viewed as the result of virtuous traits of the wealthy while poverty is the result of the shortcomings of the poor (Ross & Nisbett, 1991). When people are exposed to inequality, such processes of attribution can be crucial for people's acceptance of the (unequal) status quo. They can even deepen inequality by making those with a poor relative position more pessimistic about their chances to move ahead.

More broadly the results also speak to the question of how economic and social contexts shape people's beliefs and preferences and they can help us understand, for example, why different fairness views persist in different societies (Almas et al., 2019). The results also have implications for the design of institutions and policies. When designing policies it is important to have in mind the belief system of those the policy is defined for, which may not be the same as the belief system of those who design the policy especially if they are from different social classes. In this respect one important question is how such beliefs translate into policy preferences and how they aggregate in the political process.

This paper has identified robust causal effects of inequality on attribution and social trust. We were also able to distinguish the effect of relative position from that of inequality exposure *per se*. We have seen that in terms of attribution inequality exposure in itself has little or no effect unless it is accompanied with information about relative position in which case we detect strong and robust effects on belief in meritocracy. An important question is how the effect of relative position interacts with inequality in the income distribution. Empirically it is hard to identify the effect of relative position separately from inequality as relative position cannot be communicated without information on the distribution.[23] In terms of policy implications we know, however, that differences in relative position will increase with inequality as long as a cardinal interpretation is given to relative position. Digging deeper into these interactions seems one avenue for future research. Further, while we were able to rule out some mechanisms, a fully fledged analysis of the mechanisms driving the co-evolution of social context, beliefs and preferences is outside the scope of this paper. Clearly, though, this is a very important avenue for future research. We believe that the new experimental measures introduced in this paper will be helpful in executing this research agenda.

[23]There is research in psychology showing that rank of income matters more than absolute income in determining happiness and life satisfaction (Boyce, Brown, & Moore, 2010) and that there is an interaction between inequality and effects of income on life satisfaction (Quispe-Torreblanca, Brown, Boyce, M.Wood, & Neve, 2020).

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# Online Appendix "The Causal Effect of Income Inequality on Attribution and Social Trust"

**A. Albertazzi, P. Lown and F.Mengel**

**For Online Publication**

## Contents

# A Additional Details Online Studies

## A.1 Pre-test

We pre-tested a general population's understanding of a number of different ways to illustrate income distributions. Participants ($n = 176$) were randomly shown either one of the three income distributions depicted in Figure A.1. Subjects where told the picture represented the income distribution in a borough in England and were asked to pretend to be the individual highlighted in red. We asked three questions in order to test participants' understanding of the income distribution and their relative position within the borough.


(a) Version 1 (b) Version 2 (c) Version 3

Figure A.1: The distributions used in Survey I.

The first question asked "Compared to the richest person in the borough, how high is your income?" Answer options were (i) equally high, (ii) more than half as high, (iii) less than half as high but more than a third, (iv) less than a third as high but more than a fourth, (v) at most a fourth as high and (vi) none of the above.

The second question asked "What is your relative position with respect to the population in this borough?" with answer options (i) most of the population has a much higher income than me, (ii) most of the population has a lower income than me, (iii) most of the population is poorer than me (iv) most of the population has a slightly higher income than me and (v) none of the above.

The third question asked "Which of the following statements best describes the image above?" with answer options (i) Only a small fraction of the population in the borough has a high income. Most of the population has a low income level and I belong to this part; (ii) Only a small fraction of the population in the borough has a low income. Most of the population has a high income level and I belong to this part; (iii) A high fraction of the population in the borough has a high income. Only a small portion of the population has a low income level and I belong to this part; (iv) A high fraction of the population in the borough has a low income. Only a small portion of the population has a high income level and I belong to this part and (v) none of the above.

We accepted answers (iii)-(v) as correct in the first question, answers (iv) and (v) in the second question and answer (i) in the third question. We found that respondents did not understand Version 3 at all and they understood Version 1 somewhat better than Version 2.

## A.2 Online Experiment: Belief in Meritocracy

This online experiment measured the effect of the information on typical survey-based measures of belief in meritocracy. Specifically, we conduct an online survey where we use the exact same

questionnaire and information provision as in the lab and the exact same outcomes (measures of belief in meritocracy) as in the Next Steps 8 survey. At the end of the survey we also ask participants to indicate how risk averse and how competitive they are on a scale from 0-10.

Hence, as in the lab, we can make *causal* inference on belief in meritocracy, but unlike the lab the outcome measures here are not incentivized. We fielded the survey online using a large UK survey provider and restricted the sample to UK national (just as in the lab). We have 194 respondents for the **REL** condition and 185 respondents for the **INEQ** condition. No participants were dropped from the sample. Table A.1 shows some properties of this sample and compares them to our lab samples and the Next Steps 8 samples.

| | NS 8 | Lab | Lab | Online | Online |
|---|---|---|---|---|---|
| *sample* | | | | | |
| age | 25.3 | 27.1 | 26.6 | 33.4 | 35.9 |
| female | 0.55 | 0.48 | 0.40 | 0.69 | 0.74 |
| student | 0.07 | 0.80 | 0.82 | 0.19 | 0.17 |
| low income | 0.57 | 0.39 | 0.30 | 0.56 | 0.61 |
| high income | 0.08 | 0.29 | 0.32 | 0.14 | 0.08 |
| *measurement* | | | | | |
| causal | NO | YES | YES | YES | YES |
| incentivized | NO | YES | YES | NO | NO |
| type of info | - | REL | INEQ | REL | INEQ |
| N | 6906 | 114 | 114 | 194 | 185 |

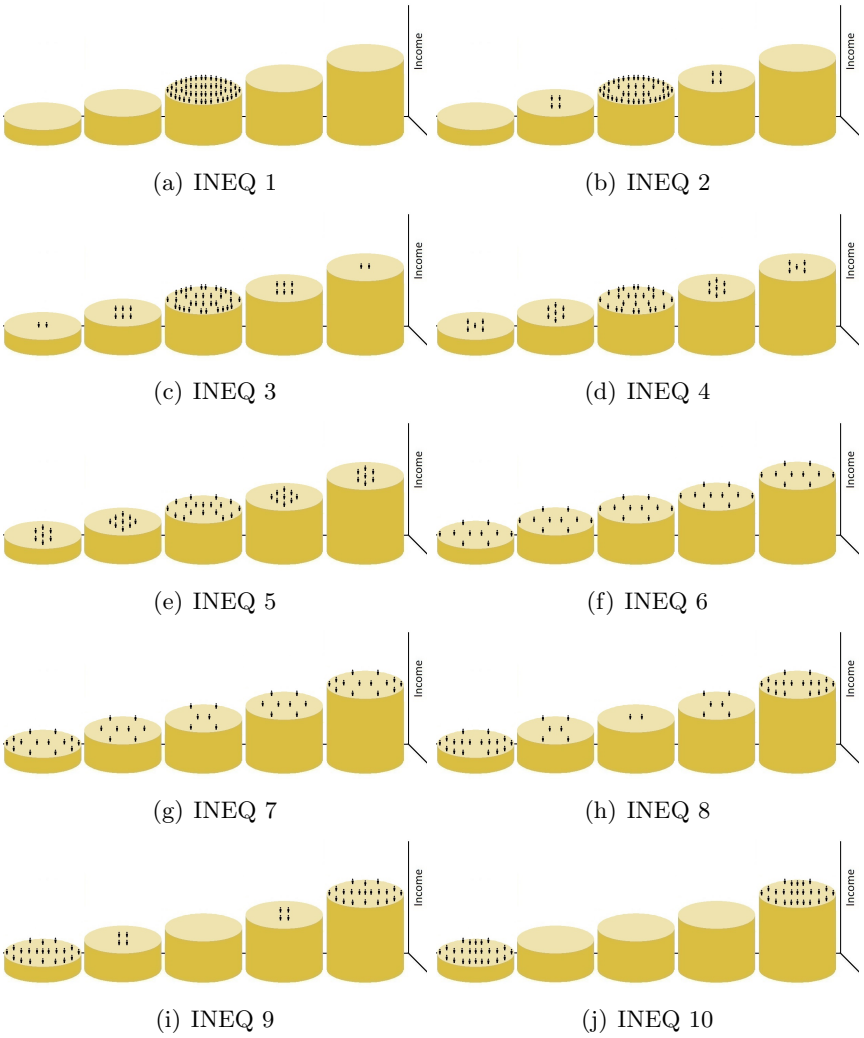
Table A.1: Characteristics of Next Steps 8, Lab and online experiment participants as well as types of measurement of belief in meritocracy.

## A.3 Online Experiment: Inequality Information

This online experiment investigated in more detail the effectiveness of the inequality information. In this survey we use ten different income distributions with levels of inequality ranging from complete equality to very high inequality (see Figure A.2). We again show participants one of these distributions (randomly selected) and ask them to indicate belief in meritocracy using the same measure as in Next Steps 8. As these income distributions do not really exist in the UK we emphasize that they are income distributions of a *"hypothetical"* borough in the UK. At the end of the survey we show them (a different) distribution, again randomly selected, and ask them to indicate on a scale from 0,...,10 how unequal they believe this society is. Again the survey was conducted online with a large UK survey provider and the sample was restricted to UK nationals. We had 331 respondents. The mean age was 38.14 years (range 19,72), 66 percent were women, 11.5 percent were students, 49.24% fall into the low income category and 14.5% in the high income category.

## A.4 Online Experiment: Blame

This online experiment measured the effect of the information on a non-incentivized measure of blame. We used the exact same questionnaire and information provision as in the lab. Afterwards we describe to the participants hypothetical choices of player A and asked them how they would distribute 15GBP between players A and B hypothetically. Hence the blame task is also the same as in the lab with the difference that it was not incentivized. The reason we chose this task is that it there is no established measure of blame used in general surveys. At the end




(a) INEQ 1 (b) INEQ 2

(c) INEQ 3 (d) INEQ 4

(e) INEQ 5 (f) INEQ 6

(g) INEQ 7 (h) INEQ 8

(i) INEQ 9 (j) INEQ 10

Figure A.2: The distributions used in Survey III.

of the survey we also ask participants to indicate how risk averse and how competitive they are on a scale from 0-10.

Hence, as in the lab, we can make *causal* inference on blame, but unlike the lab the outcome measures here are not incentivized. We fielded the experiment online using a large UK survey provider and restricted the sample to UK national (just as in the lab). Power analysis based on specification (1) and means and standard errors observed in the lab (**REL** treatment) suggested to pick a sample size of 106 to have 80% power to detect an effect of this size in the survey. We invited as many participants and over-recruited slightly. We have 107 respondents for the **REL** condition and 109 respondents for the **INEQ** condition. No participants were dropped from the sample. Table A.2 shows some properties of this sample and compares them to our lab samples and the Next Steps 8 samples.

## A.5 Online Experiment: Social Trust

This online experiment measured the effect of the information on typical survey-based measures of social trust. Specifically, we conduct an online survey where we use the exact same questionnaire and information provision as in the lab and the exact same outcomes (measures of

| | NS 8 | Lab | Lab | Online | Online |
|---|---|---|---|---|---|
| *sample* | | | | | |
| age | 25.3 | 24.6 | 23.7 | 35.2 | 35.6 |
| female | 0.55 | 0.44 | 0.48 | 0.64 | 0.75 |
| student | 0.07 | 0.87 | 0.86 | 0.21 | 0.18 |
| low income | 0.57 | 0.32 | 0.33 | 0.64 | 0.58 |
| high income | 0.08 | 0.31 | 0.30 | 0.07 | 0.10 |
| *measurement* | | | | | |
| causal | NO | YES | YES | YES | YES |
| incentivized | NO | YES | YES | NO | NO |
| type of info | - | REL | INEQ | REL | INEQ |
| N | 6906 | 219 | 221 | 107 | 109 |

Table A.2: Characteristics of Next Steps 8, Lab and online experiment participants as well as types of measurement for Blame.

social trust) as in the Next Steps 8 survey. At the end of the survey we also ask participants to indicate how risk averse and how competitive they are on a scale from 0-10.

Hence, as in the lab, we can make *causal* inference on social trust, but unlike the lab the outcome measures here are not incentivized. We fielded the survey online using a large UK survey provider and restricted the sample to UK national (just as in the lab). We have 292 respondents for the **REL** condition and 216 respondents for the **INEQ** condition. No participants were dropped from the sample. Table A.3 shows some properties of this sample and compares them to our lab samples and the Next Steps 8 samples.

| | NS 8 | Lab | Lab | Online | Online |
|---|---|---|---|---|---|
| *sample* | | | | | |
| age | 25.3 | 27.1 | 26.6 | 34.0 | 35.1 |
| female | 0.55 | 0.48 | 0.40 | 0.66 | 0.63 |
| student | 0.07 | 0.80 | 0.82 | 0.19 | 0.25 |
| low income | 0.57 | 0.39 | 0.30 | 0.51 | 0.54 |
| high income | 0.08 | 0.29 | 0.32 | 0.14 | 0.14 |
| *measurement* | | | | | |
| causal | NO | YES | YES | YES | YES |
| incentivized | NO | YES | YES | NO | NO |
| type of info | - | REL | INEQ | REL | INEQ |
| N | 6906 | 335 | 333 | 322 | 318 |

Table A.3: Characteristics of Next Steps 8, Lab and online experiment participants as well as types of measurement for Social Trust.

# B Additional Details Lab Experiment

## B.1 Experimental Instructions

Participants were provided with a paper sheet reporting the general information about the experiment. Instructions for each part, were instead displayed on subjects screens prior the beginning of the corresponding part.

### General Information

Welcome and thanks for participating in this experiment. Please, read these instructions carefully. These are identical for all the participants. Should you have any question, please

raise your hand. An experimenter will come to you and answer your questions. From now on communication with other participants is not allowed. If you do not conform to these rules we will have to exclude you from the experiment. Please do also switch off, or set to off line mode, your mobile phone at this moment.

At the beginning of the experiment we will ask you some questions about yourself (e.g. age, gender, etc.). These data will be used for the purpose of this experiment only, and will be completely anonymous.
You will receive 1 GBP for filling in the initial questionnaire and 4 GBP for showing up today. During the experiment you can earn more. All payments and payoffs will be expressed in british pounds (GBP).
All your answers and decisions will be treated confidentially.

**The Experiment** The main experiment consists of six parts in each of which you can earn some money. How much depends on your decisions and those of other participants. Detailed instructions for each part will be shown on your computer screen as the experiment proceeds. The order of the parts will be randomized

**Your earnings** At the end of the experiment one part will be randomly selected for each participant. You will receive the amount of money you earned in this part. In addition, you will be paid 1 GBP for completing the initial questionnaire and 4 GBP for showing up today.

**Participation** Your participation to this study is completely voluntary. Choosing not to take part will not disadvantage you in any way. You can withdraw from the experiment at any time without consequences.

**Confidentiality** All your answers will be treated confidentially and only used for research purposes only.

If you have any questions about these instructions or the experiment, then please raise your hand now and someone will come and answer them.
Once everyone has finished reading the instructions and questions have been answered, the experiment will start. At the beginning of each part you will receive detailed instructions, and some control questions will appear on your screen that will allow you to test your understanding of the instructions.

## Part 1a

In this part, you will first perform three tasks:

- You will complete a short test consisting of 4 questions. For every correct answer you earn 2 GBP. Your score ($A$) from this task is determined as follows:
  $A =$ number of correct answers.

- You will perform a task in which you will have to count the number of 0's in four tables containing only 0's and 1's. For every table for which you report the correct number of 0's, you earn 2 GBP. Your score ($B$) from this task is determined as follows:
  $B =$ number of correct answers.
- You will toss a fair coin. Your will earn 0 GBP if "head" comes up and 10 GBP if "tail" comes up. Your score ($C$) from this task is determined as follows:
  $C = 0$ "if head" and $C = 2$ "if tail".

Your overall score ($S$) will be calculated as a combination of the scores you earned in each task, as follows:

$$S = A + B + C$$

Afterwards, we will randomly sort people in groups of ten and rank all participants by their score S, where the highest score is ranked 1 and the lowest score 10.

Before knowing the results, we will ask you to guess your rank. The guesses are made by specifying a range (between X and Y) in which you believe your rank belongs.

For this, you will be paid according to the accuracy of your guesses. A wrong guess (your actual rank falls outside the specified range) yields nothing. A correct guess (your actual rank lies within the specified range) yields the following:

$$(9 - (Y - X)) \cdot 2$$

Therefore, the smaller the specified range, the higher the earnings if the guess is correct, i.e the true rank is within the specified range. However, a smaller range also increases the risk that the guess is not correct, in which case you earn nothing.

*Example*
Suppose your overall rank is 3, i.e. you scored the third-best performance $S$ among the ten people in your group.
If you guess $X = 4$ and $Y = 6$, your specified range is [4, 6]. Since your rank falls outside the specified range you earn zero.
If you guess $X = 1$ and $Y = 10$, your specified range is [1, 10], the biggest possible range. Since your rank lies in the specified range you earn $(9 - 9) \cdot 2 = 0$ GBP.
If you guess $X = 3$ and $Y = 8$, your specified range is [3, 8]. Since your rank lies in the specified range you earn $(9 - 5) \cdot 2 = 8$ GBP.
If you guess $X = 3$ and $Y = 5$, your specified range is [3, 5]. Since your rank lies in the specified range you earn $(9 - 2) \cdot 2 = 14$ GBP.
If you guess $X = 3$ and $Y = 3$, your specified range is [3, 3], the smallest possible range. Since your rank lies in the specified range you earn $(9 - 0) \cdot 2 = 18$ GBP.

Your total payoff from this part will be determined with 50 percent chance by the performance in the three tasks ($S$) and with 50 percent chance by the correctness of your guesses.

## Part 1b

In this part, each of you will be randomly matched in groups of three participants. You will not be told who these persons are either during or after the experiment nor will they be told who the others are.
The three participants, including you, will be referred to later as player A, B, and C. Each of you will be assigned one player type only. Thus, you can be either player A, B or C.
At the beginning of the task, the individual score ($S$) of each group member from Part 1a will be combined together. Thus, the total amount will be the sum of the $S$ scores of each of the group members. Each player will then be asked to allocate this total amount among the group members.
For example, if you are player A you will have to decide how much to keep for yourself and how much to allocate to player B and player C. Only one of your allocation decisions will be selected at random with equal probability and implemented.
After the allocation decisions, each of you will be asked to guess how much the other group members allocated to themselves. A correct guess will yield a bonus of 2 GBP.

## Part 2

*(The following instructions where provided on paper)*
In this part, each of you will be randomly matched in groups of three participants. You will not be told who these persons are either during or after the experiment nor will they be told who the others are.
The three group members will be referred to later as A, B and C. Agent B does not make any choice in this part and thus, is passive. Each of you will be assigned to one type only. Thus, you can be either A or B or C.
At the first stage, A is asked to make an investment decision. In particular, A has to choose, without costs, between a risky lottery or a safe alternative. The lottery and the certain amount of the safe alternative are known to all players.
At the second stage, the decision of agent A and the outcome of the lottery are revealed to all players. Further, the outcome of the investment of A will constitute the payoff of C.

After the outcome of the investment is observed, C is asked to divide 15 GBP between A and B. The allocations to each agent can be between 0 and 15 GBP and together have to total to 15 GBP or less. Note that money not allocated to the agents will *not* be kept by C.
Thus, the payoff for C will be determined by the outcome of the investment while for A and B they will be given by the allocation of the 15 GBP decided by C.
Next, A will observe the allocation made by C while B will learn about this payment at the end of the experimental session.
Finally, both A and C will have to rate the other's decision on a 1-10 scale ranging from very bad to very good.
In total there will be ten choices made by players A and C, each time with a different investment

decision for player A. Only one of those ten choices will be randomly selected for the payment.

*(More detailed instructions where provided on subjects screens following Gurdal et al. (2013))*

## Part 3

In this part, each of you will be randomly matched in groups of three participants. You will not be told who these persons are either during or after the experiment nor will they be told who the others are.
Each of you has to perform the same task as in Part 1. Thus, you have to count the number of 0's in five different tables that contain only 0's and 1's. However, this time you will not be paid for every correct answer you provide. Instead, the person in your group who provides the correct answer most often will be paid 10 GBP. If more than one group member has the most correct answers then we will throw a coin to determine who wins the 10 GBP. The other group members will receive 0 GBP.

## Part 4

In this part we will ask you questions which require you to make choices involving wheels of fortune. In every question you will be asked to choose between two different wheels, each of which can deliver two monetary outcomes. From this part, you will earn the amount of money you win from one of the wheels you choose. More precisely, at the end of this part we will randomly draw one of your choices. The outcome of the selected wheel of fortune will constitute your payment from this part.

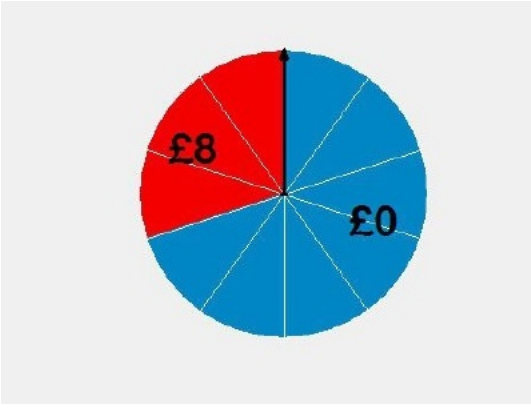



Figure B.1: Example of a wheel of fortune

This is an example of a wheel of fortune with 10 equal sized coloured zones. The wheel is spun and equally likely to stop with the arrow in one of the zones. In this wheel, there are 3 red zones and 7 blue zones. If the arrow ends in any of the red zones you receive 8 GBP. If it ends in any of the blue zones you receive 0 GBP.

## Part 5

In this part we will ask you some questions about yourselves. You will receive 2 GBP for completing all these questions.

1. How old are you?
2. Are you a student?
   Yes:

   - At what stage of your studies are you?
   - What is your field of study?
   - What is your expected grade at graduation?
   - Are you planning to continue with your studies? If yes, which options do you plan to choose next term?
   - What plans do you have for your career?
   - What is your annual income expectation (in GBP) in ten years from now?

   No:

   - What is your field of work?
   - What is your annual income expectation (in GBP) in ten years from now?
   - How satisfactory are these different aspects of your life? Indicate the option which best suits your situation.
     - Life as a whole is
     - My ability to manage my self-care (dressing, hygiene, transfers, etc.) is
     - My leisure situation is
     - My vocational situation is
     - My financial situation is
     - My sexual life is
     - My partnership relation is
     - My family life is
     - My contacts with friends and acquaintances are

### B.2 Income Questionnaire

Before commencing the main experiment, we will ask you some questions about yourself. Please answer to these questions truthfully. Your answers will be used for the purpose of this experiment only and will be treated confidentially.

1. How would you primarily characterize your social class?

   - Working class
   - Lower middle class
   - Middle class

- Upper middle class
- Upper class

2. What is your annual gross (parents') household income?

   - Less than 15.000 GBP
   - Between 15.000 - 25.000 GBP
   - Between 25.000 - 35.000 GBP
   - Between 35.000 - 45.000 GBP
   - Between 45.000 - 55.000 GBP
   - More than 55.000 GBP

3. How much rent does your (parents') household currently pay?

   - Less than 400 GBP per month
   - Between 400-600 GBP per month
   - Between 600-800 GBP per month
   - Between 800-1000 GBP per month
   - Between 1000-1200 GBP per month
   - More than 1200 GBP per month
   - My (parents') household lives in owned property

4. Including yourself, how many members does your (parents') household have?

   - 1
   - 2
   - 3
   - 4
   - 5
   - More than 5

5. At which grocery store does your (parents') household do their weekly shopping?

   - Aldi
   - Asda
   - Lidl
   - Mark and Spencer
   - Sainsbury's
   - Tesco
   - Waitrose
   - Other

6. If you have to buy a new mobile phone, which price are you usually willing to pay?

   - Less than 200 GBP
   - Between 200-400 GBP
   - Between 400-600 GBP
   - Between 600-800 GBP
   - More than 800 GBP

7. If you go on holidays abroad where are you most likely to go?

    - I never go to holidays abroad.
    - Spain, Portugal or Greece.
    - Spain, Portugal, Greece, Italy or France.
    - Anywhere in Europe, and some non-European countries.
    - Anywhere in the world.

8. About how much does your (parents') household spend eating out every week?

    - Less than 25 GBP per week
    - Between 25-50 GBP per week
    - Between 50-100 GBP per week
    - Between 100-200 GBP per week
    - More than 200 GBP per week

9. Where were you educated?

    - At a comprehensive
    - A grammar school
    - Private school, not boarding
    - Private school, boarding

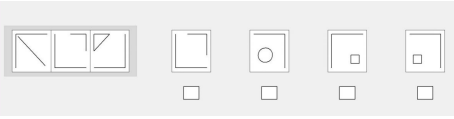
### B.3 Outcomes

**Task 1a** IQ-test: 4 questions

1. Which number logically follows this series? 4 6 9 6 14 6 ...

    - 6
    - 17
    - 19
    - 21

2. Which image logically follows next?



3. Which conclusion follows from the statements with absolute certainty? (i) None of the stamp collectors is an architect; (ii) All the drones are stamp collectors.

    - all stamp collectors are architects
    - architects are not drones
    - no stamp collectors are drones
    - some drones are architects

4. Tina who is 16 years old is four times as old as her brother. How old will she be when she is twice as old as him?

   - 24
   - 30
   - 32
   - 42

**Task 1b and Task 3** Figure B.2 shows an example of a matrix used in Task 1b and Figure B.3 shows how the coin toss was illustrated on the screen.

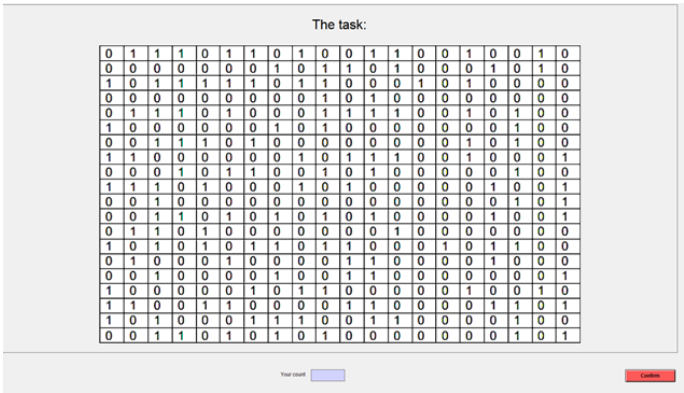



Figure B.2: Example of a Matrix for Task 1b and 3


Figure B.3: Screenshot: Coin Toss.

# C Sample Characteristics

| | Age | Gender | Student | A | B | C | S | Risk | Compet |
|---|---|---|---|---|---|---|---|---|---|
| high info | 2.149 | 0.018 | -0.038 | -0.235 | 0.039 | 0.210 | 0.013 | 0.085 | -0.058 |
| | (2.717) | (0.094) | (0.075) | (0.219) | (0.192) | (0.188) | (0.345) | (0.399) | (0.092) |
| Constant | 26.05*** | 0.473*** | 0.818*** | 2.473*** | 1.164*** | 0.909*** | 4.545*** | 4.673*** | 0.618*** |
| | (1.955) | (0.0680) | (0.0545) | (0.157) | (0.138) | (0.135) | (0.248) | (0.286) | (0.0668) |
| Observations | 114 | 114 | 114 | 114 | 114 | 114 | 114 | 113 | 114 |
| R-squared | 0.006 | 0.000 | 0.002 | 0.010 | 0.000 | 0.011 | 0.000 | 0.000 | 0.004 |
| Income Questionnaire | Q1 | Q2 | Q3 | Q4 | Q5 | Q6 | Q7 | Q8 | |
| high info | -0.230 | 0.121 | -0.167 | -0.180 | -0.368 | -0.051 | -0.238 | 0.028 | |
| | (0.195) | (0.327) | (0.408) | (0.260) | (0.412) | (0.311) | (0.167) | (0.132) | |
| Constant | 2.145*** | 3.218*** | 4.964*** | 3.909*** | 4.673*** | 3.255*** | 1.764*** | 1.327*** | |
| | (0.140) | (0.235) | (0.294) | (0.187) | (0.296) | (0.224) | (0.120) | (0.095) | |
| Observations | 114 | 114 | 114 | 114 | 114 | 114 | 114 | 114 | |
| R-squared | 0.012 | 0.001 | 0.001 | 0.004 | 0.007 | 0.000 | 0.018 | 0.000 | |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.1: Balancing tests **REL-MTB**. Gender= 1 is female, student is a dummy indicating whether the participant is a University student, A, B and C are sub-scores in ability, effort and luck task, respectively. S is the overall score in the task. Risk is our measure of risk aversion and Compet our measure of competitiveness. Q1-Q8 are the questions of the income questionnaire.

| | Age | Gender | Student | A | B | C | S | Risk | Compet |
|---|---|---|---|---|---|---|---|---|---|
| high info | 1.725 | -0.088 | 0.003 | 0.045 | -0.175 | -0.082 | -0.212 | 0.359 | -0.006 |
| | (1.506) | (0.066) | (0.045) | (0.146) | (0.155) | (0.135) | (0.266) | (0.255) | (0.0673) |
| Constant | 23.65*** | 0.487*** | 0.867*** | 2.372*** | 1.425*** | 1.027*** | 4.823*** | 4.761*** | 0.460*** |
| | (1.053) | (0.046) | (0.031) | (0.102) | (0.109) | (0.094) | (0.186) | (0.178) | (0.047) |
| Observations | 221 | 221 | 221 | 221 | 221 | 221 | 221 | 221 | 221 |
| R-squared | 0.006 | 0.008 | 0.000 | 0.000 | 0.006 | 0.002 | 0.003 | 0.009 | 0.000 |
| Income Questionnaire | Q1 | Q2 | Q3 | Q4 | Q5 | Q6 | Q7 | Q8 | |
| high info | -0.157 | -0.052 | -0.205 | 0.059 | -0.082 | -0.114 | 0.010 | -0.061 | |
| | (0.145) | (0.227) | (0.290) | (0.175) | (0.319) | (0.210) | (0.129) | (0.109) | |
| Constant | 2.407*** | 3.664*** | 5.363*** | 3.681*** | 4.619*** | 3.327*** | 1.832*** | 1.478*** | |
| | (0.102) | (0.159) | (0.203) | (0.122) | (0.223) | (0.146) | (0.0899) | (0.0759) | |
| Observations | 221 | 221 | 221 | 221 | 221 | 221 | 221 | 221 | |
| R-squared | 0.005 | 0.000 | 0.002 | 0.001 | 0.000 | 0.001 | 0.000 | 0.001 | |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.2: Balancing tests **REL-BMT**. Gender= 1 is female, student is a dummy indicating whether the participant is a University student, A, B and C are sub-scores in ability, effort and luck task, respectively. S is the overall score in the task. Risk is our measure of risk aversion and Compet our measure of competitiveness. Q1-Q8 are the questions of the income questionnaire.

| | Age | Gender | Student | A | B | C | S | Risk | Compet |
|---|---|---|---|---|---|---|---|---|---|
| high info | 0.340 | -0.098 | -0.022 | 0.082 | 0.510** | 0.384** | 0.976*** | 0.098 | -0.151* |
| | (2.551) | (0.092) | (0.071) | (0.225) | (0.206) | (0.185) | (0.356) | (0.379) | (0.089) |
| Constant | 26.49*** | 0.455*** | 0.836*** | 2.291*** | 0.982*** | 0.836*** | 4.109*** | 4.800*** | 0.727*** |
| | (1.835) | (0.066) | (0.051) | (0.162) | (0.148) | (0.133) | (0.256) | (0.273) | (0.064) |
| Observations | 114 | 114 | 114 | 114 | 114 | 114 | 114 | 113 | 114 |
| R-squared | 0.000 | 0.010 | 0.001 | 0.001 | 0.052 | 0.037 | 0.063 | 0.001 | 0.025 |
| Income Questionnaire | Q1 | Q2 | Q3 | Q4 | Q5 | Q6 | Q7 | Q8 | |
| high info | -0.015 | 0.080 | 0.485 | -0.157 | -0.477 | 0.137 | -0.229 | 0.061 | |
| | (0.202) | (0.315) | (0.400) | (0.259) | (0.458) | (0.292) | (0.185) | (0.174) | |
| Constant | 2.473*** | 3.564*** | 5.091*** | 3.818*** | 5.291*** | 3.473*** | 1.873*** | 1.600*** | |
| | (0.145) | (0.227) | (0.288) | (0.186) | (0.329) | (0.210) | (0.133) | (0.125) | |
| Observations | 114 | 114 | 114 | 114 | 114 | 114 | 114 | 114 | |
| R-squared | 0.000 | 0.001 | 0.013 | 0.003 | 0.010 | 0.002 | 0.013 | 0.001 | |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.3: Balancing tests **INEQ-MTB**. Gender= 1 is female, student is a dummy indicating whether the participant is a University student, A, B and C are sub-scores in ability, effort and luck task, respectively. S is the overall score in the task. Risk is our measure of risk aversion and Compet our measure of competitiveness. Q1-Q8 are the questions of the income questionnaire.

| | Age | Gender | Student | A | B | C | S | Risk | Compet |
|---|---|---|---|---|---|---|---|---|---|
| high info | -0.894 | 0.094 | 0.000 | -0.091 | 0.171 | -0.300** | -0.221 | 0.044 | -0.102 |
| | (0.941) | (0.070) | (0.046) | (0.143) | (0.157) | (0.134) | (0.254) | (0.254) | (0.066) |
| Constant | 24.03*** | 0.434*** | 0.867*** | 2.478*** | 1.442*** | 1.168*** | 5.088*** | 5.097*** | 0.460*** |
| | (0.655) | (0.048) | (0.032) | (0.099) | (0.109) | (0.093) | (0.177) | (0.176) | (0.046) |
| Observations | 219 | 219 | 219 | 219 | 219 | 219 | 219 | 219 | 219 |
| R-squared | 0.004 | 0.008 | 0.000 | 0.002 | 0.005 | 0.023 | 0.003 | 0.000 | 0.011 |
| Income Questionnaire | Q1 | Q2 | Q3 | Q4 | Q5 | Q6 | Q7 | Q8 | |
| high info | 0.023 | -0.015 | 0.074 | -0.030 | 0.040 | -0.112 | 0.108 | 0.072 | |
| | (0.143) | (0.235) | (0.294) | (0.185) | (0.329) | (0.222) | (0.129) | (0.110) | |
| Constant | 2.071*** | 3.478*** | 4.991*** | 3.823*** | 4.611*** | 3.168*** | 1.779*** | 1.381*** | |
| | (0.099) | (0.164) | (0.205) | (0.129) | (0.229) | (0.154) | (0.089) | (0.076) | |
| Observations | 219 | 219 | 219 | 219 | 219 | 219 | 219 | 219 | |
| R-squared | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.001 | 0.003 | 0.002 | |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.4: Balancing tests **INEQ-BMT**. Gender= 1 is female, student is a dummy indicating whether the participant is a University student, A, B and C are sub-scores in ability, effort and luck task, respectively. S is the overall score in the task. Risk is our measure of risk aversion and Compet our measure of competitiveness. Q1-Q8 are the questions of the income questionnaire.

# D Additional Results and Discussion

In this section we will discuss additional results, in particular the effect of the information on secondary outcomes that but might nevertheless be of independent interest. We start by studying pro-social behaviour and then move to aspirations.

## D.1 Pro-social behaviour

Is the increased level of social trust for those assigned to a high relative position accompanied by an increase in pro-social behaviour by the same group? There is an active literature discussing how people's relative position in society affects how pro-social they are. The results in this literature are pretty mixed. Psychology literature working with highly contextualized situations has found that a higher relative position tends to decrease pro-social behaviour (Piff et al., 2012). Cote et al. (2015) find that this difference is more pronounced if there is a high degree of inequality in the area where the rich or poor person lives. This effect is not found by Schmukle et al. (2019). Smeets et al. (2015) find a non-monotonic effect with both millionaires as well as poor people being more pro-social than those in the middle. Both Korndoerfer et al. (2015) and Andreoni et al. (2017) find a positive effect which they argue is driven by the different marginal utility of money rather than fundamental differences in preferences. Trautmann et al. (2013) emphasize the important role of contextual factors and suggest there is no simple answer to this question. Given this intense debate it is interesting to briefly study differences in pro-social behaviour in our sample, especially since we, unlike most studies above, can make causal inference on the role of relative position on pro-social behaviour.

| | Pro-Social Behavior | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info | 0.071*** | 0.070*** | 0.071*** | 0.070*** | 0.062** | 0.058** |
| | (0.022) | (0.022) | (0.022) | (0.020) | (0.022) | (0.022) |
| medium income | | -0.016 | -0.014 | -0.015 | 0.006 | 0.023 |
| | | (0.024) | (0.023) | (0.023) | (0.041) | (0.046) |
| high income | | -0.053* | -0.046 | -0.043 | 0.014 | 0.045 |
| | | (0.026) | (0.027) | (0.028) | (0.054) | (0.056) |
| Constant | 0.463*** | 0.485*** | 0.607*** | 0.610*** | 0.426** | 0.439** |
| | (0.015) | (0.013) | (0.075) | (0.080) | (0.148) | (0.158) |
| Observations | 335 | 335 | 335 | 334 | 334 | 334 |
| R-squared | 0.026 | 0.035 | 0.047 | 0.053 | 0.129 | 0.184 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.1: Pro-social behaviour REL treatments. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy, the size of the total pie and overall score S.

Appendix Table D.1 shows regression results where we regress the share of the pie allocated to others on the information, income category and controls in the same format as above. We find that those assigned to a high relative position are indeed more pro-social. They share on average 53 percent of the pie compared to 46 percent for those who are assigned to a low relative position, a 15 percent increase. This difference is highly statistically significant ($p < 0.0001$) and robust to including additional controls. Hence, despite using priming techniques like some of the literature identifying negative effects in very contextualized situations (Piff et al., 2012)

| | Pro-Social Behavior | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info | 0.033* | 0.033* | 0.033* | 0.034* | 0.025 | 0.025 |
| | (0.018) | (0.018) | (0.018) | (0.016) | (0.014) | (0.018) |
| medium income | | 0.007 | 0.006 | 0.013 | 0.013 | 0.007 |
| | | (0.032) | (0.032) | (0.032) | (0.045) | (0.042) |
| high income | | 0.009 | 0.011 | 0.008 | 0.028 | 0.037 |
| | | (0.031) | (0.030) | (0.030) | (0.044) | (0.046) |
| Constant | 0.516*** | 0.516*** | 0.387*** | 0.452*** | 0.493*** | 0.462*** |
| | (0.016) | (0.027) | (0.051) | (0.055) | (0.075) | (0.094) |
| Observations | 333 | 333 | 333 | 333 | 333 | 333 |
| R-squared | 0.007 | 0.009 | 0.024 | 0.056 | 0.111 | 0.180 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.2: Pro-social behaviour INEQ treatments. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy, the size of the total pie and overall score S.

our results are in line withe positive effects identified in some of the Economics literature, for example Andreoni et al. (2017) or Korndoerfer et al. (2015). Appendix Table D.2 shows the effects of the inequality information on pro-social behaviour. Being assigned to higher levels of inequality seems to make participants less pro-social. The effect is, however, small statistically significant only at the 10% level.

## D.2 Aspirations

We also collected data on aspirations. We asked participants about their expected income in ten years from now and, if they were students, about whether they believe they will get a good degree (2:1 or above in the UK)[1], whether they want to continue further studies after the BA and in which occupation they plan to pursue a career. These measures were not incentivized. However, as the time elapsed between the information provision and these questions was relatively long we wouldn't expect big effects. Indeed we find no effect of the information on any of the aspirations elicited in the lab. However we do find correlational evidence of a negative association between income and the aspiration to get a good degree as well as a positive association between income and expected income as well as the aspiration to do a career in finance. These associations motivate us to dig a bit deeper into a possible causal relationship.

As these measures are not incentivized, they can be elicited relatively easily in online surveys. We hence conduct an online survey ($n = 240$) where after eliciting income using our standard income questionnaire and then priming participants to a high or low relative position using the exact same procedure as in our lab experiment, we immediately elicit the following aspirations. The mean age of respondents in the survey was 34.58 years, the share of women 65% and 22.3% were students. 49.39% fall in the low income category and 15.51% fall in the high income category.

For students we elicit their expected income in ten years from now, whether they believe

[1]In UK universities the following degree classification is widely used. First-Class Honours (70% and above): a first class degree, usually referred to as a 'first' or 1st, is the highest honours degree one can achieve. Upper Second-Class Honours (60-70%), known as a 2:1 or two-one. Lower Second-Class Honours (50-60%), a 2.2 or two-two. Third-Class Honours (40-50%) is the lowest honours degree achievable

they will get a good degree, whether they want to continue further studies after the BA and in which occupation they plan to pursue a career, exactly as in the lab. For non-students we also elicit their expected income in ten years from now and we ask whether they expect their personal economic situation will improve over the next 5 years and whether they expect to get a promotion in their job in the next 5 years. For full details see the questionnaire in Appendix D.3.

Appendix Table D.3 shows that being assigned to a high relative position has a positive effect on future income expectations both for students and non-students. There are no other statistically significant effects of the information for non-students, but students assigned to a high relative position are more likely to believe they will get a good degree and are more likely to indicate that they plan to continue further studies. Being assigned to higher inequality does not per se have an effect on aspirations (Appendix Table D.4). We also observe several correlational effects with income. As expected, those with higher current income (parents' income) expect higher income in the future. They are also more likely to plan further studies and a career in finance. Last, they are more likely to expect a promotion in their current job.

| | *Students* | | | | | | *Non-Students* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | degree | study | exp_inc | c_finance | c_edu | c_NGO | future | promo | exp_inc |
| high info | 0.266** | 0.405*** | 0.437*** | 0.073 | -0.045 | -0.020 | -0.008 | -0.022 | 0.131* |
| | (0.126) | (0.128) | (0.150) | (0.105) | (0.085) | (0.094) | (0.065) | (0.063) | (0.071) |
| medium income | 0.229 | 0.466*** | 0.391** | 0.145 | 0.053 | -0.185* | 0.021 | 0.225*** | 0.629*** |
| | (0.140) | (0.143) | (0.167) | (0.117) | (0.095) | (0.105) | (0.073) | (0.070) | (0.079) |
| high income | 0.190 | 0.155 | 0.863*** | 0.291** | -0.094 | -0.123 | -0.061 | 0.290*** | 1.084*** |
| | (0.157) | (0.160) | (0.187) | (0.131) | (0.106) | (0.118) | (0.101) | (0.097) | (0.110) |
| Constant | -1.075** | 0.179 | 0.611 | -0.470 | -0.521 | 0.0406 | 0.819*** | 0.397* | 2.020*** |
| | (0.513) | (0.522) | (0.611) | (0.429) | (0.347) | (0.385) | (0.218) | (0.209) | (0.237) |
| Observations | 53 | 53 | 53 | 53 | 53 | 53 | 187 | 187 | 187 |
| R-squared | 0.301 | 0.394 | 0.404 | 0.134 | 0.354 | 0.204 | 0.201 | 0.223 | 0.464 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.3: Aspirations depending on income and whether participants are assigned to high relative position. Controls are age, gender, risk attitude and self-reported degree of competitiveness.

| | *Students* | | | | | | *Non-Students* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | degree | study | exp_inc | c_finance | c_edu | c_NGO | future | promo | exp_inc |
| high info | -0.147 | 0.043 | -0.048 | 0.035 | 0.341 | -0.166 | -0.001 | -0.013 | -0.069 |
| | (0.267) | (0.279) | (0.308) | (0.150) | (0.218) | (0.240) | (0.0730) | (0.061) | (0.082) |
| medium income | -0.247 | 0.141 | 0.365 | -0.009 | 0.101 | -0.029 | 0.111 | 0.365*** | 0.628*** |
| | (0.236) | (0.247) | (0.273) | (0.132) | (0.192) | (0.212) | (0.0790) | (0.066) | (0.0894) |
| high income | 0.224 | -0.368 | 0.984*** | 0.217 | -0.167 | 0.264 | 0.123 | 0.365*** | 1.035*** |
| | (0.300) | (0.315) | (0.346) | (0.168) | (0.245) | (0.270) | (0.121) | (0.101) | (0.137) |
| Constant | 1.321 | -0.431 | 2.806*** | 0.198 | -1.400** | 0.337 | 0.834*** | 0.496*** | 1.966*** |
| | (0.825) | (0.884) | (0.951) | (0.462) | (0.672) | (0.742) | (0.181) | (0.151) | (0.205) |
| Observations | 30 | 29 | 30 | 30 | 30 | 30 | 183 | 183 | 183 |
| R-squared | 0.267 | 0.216 | 0.361 | 0.115 | 0.404 | 0.101 | 0.084 | 0.243 | 0.367 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.4: Aspirations depending on income and whether participants are assigned to high inequality. Controls are age, gender, risk attitude and self-reported degree of competitiveness.

### D.2.1 Blame

In this task each participant is randomly assigned a role A, B or C. We then randomly match three participants (one A, B and one C) to play together. Player A then first chooses between a lottery and a safe asset. The lottery pays 0 with probability $p$ and $Z$ with probability $(1-p)$. The money earned from the choice goes to player C. Afterwards player C decides how much of 15 GBP to allocate between agent A and a passive agent B. The process is ten times repeated for different lotteries and safe outcomes. Blame is measured for player C. Following Gurdal et al. (2013) we measure blame by the difference between the amount allocated to player A when the lottery was won and when the lottery was lost conditional on A having chosen the lottery. Hence the question is whether $C$ blames A for realizations of a random draw which A has no control over. As blame is measured only for player C we needed to have a higher overall sample size for this outcome (see above).[2]

We now study inclination to blame. Conditional on having chosen the lottery players A are rewarded by 0.67 (0.56) cents if the lottery outcome was lucky in **REL-BMT** (**INEQ-BMT**). The range of blame is substantial, though, with the minimum amount of blame being $-0.30$ cents (where players are "punished" for good lottery outcomes) and the maximum 6 GBP. Around 25 percent of participants do not blame and the vast majority display levels of blame between 0 and 2 GBP. This is roughly in line with the amount of blame found by Gurdal et al. (2013), who find an average effect of $\approx 1.2$ US-dollars or $\approx 0.9$ GBP. Appendix Table E.3 reports demographic covariates of (standardized) blame. There is no systematic relationship between age, gender, income or class and blame. In terms of experiment-based co-variates we see a positive relationship between blame and belief in meritocracy and a negative relationship between inclination to blame and social trust.

[2] An alternative would have been to use the strategy method and ask all participants - how would you decide if you were selected as player C. This has two downsides in our context. First imagining yourself in other roles than the one ultimately realized can generate empathy which would not be present with fixed roles, which in turn can affect blame. Second, making decisions in a "hot" situation can be quite different from a "cold" situation for outcomes like blame, where emotions are likely to be quite relevant. Using the same method as Gurdal et al. (2013) also allows us to benchmark our results against theirs, which is maybe particularly relevant for an outcome that has not been measured yet very often.

# E Additional Tables

## E.1 Additional Tables for Section 3

| | *Social Trust EVS* | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| medium income | 0.153*** | 0.148*** | 0.113 |
| | (0.0487) | (0.0488) | (0.561) |
| high income | 0.215*** | 0.206*** | -0.527 |
| | (0.0601) | (0.0604) | (0.667) |
| Gini | | 1.244 | 0.840 |
| | | (0.839) | (1.034) |
| Gini × med income | | | 0.136 |
| | | | (2.066) |
| Gini × high income | | | 2.685 |
| | | | (2.436) |
| Constant | 0.291*** | -0.037 | 0.074 |
| | (0.055) | (0.228) | (0.282) |
| Individual Controls | YES | YES | YES |
| Region Controls | NO | NO | NO |
| Observations | 607 | 607 | 607 |
| R-squared | 0.044 | 0.047 | 0.049 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.1: Social Trust in the European Value Survey. Individual controls are age, gender and religion fixed effects. The region controls are population size, ethnic diversity (share of white population) and the share of the population living in an urban area.

## E.2 Additional Tables for Section 4

| | -MTB | -BMT |
|---|---|---|
| Belief in Meritocracy | **3** min | 78 min |
| Social Trust | **15** min | 90 min |
| Inclination to Blame | 30 min | **8** min |

Table E.2: Approximate time between information provision and elicitation of different outcomes. The measure includes the time until the actual start of the task, i.e. includes time spent reading task-specific instructions and answering control questions.

| | BIM | | Social Trust | | Blame | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| age | 0.003 | 0.003 | -0.000 | -0.000 | -0.003 | 0.000 | -0.002 |
| | (0.004) | (0.004) | (0.001) | (0.001) | (0.009) | (0.009) | (0.009) |
| gender | 0.040 | 0.032 | -0.001 | -0.002 | 0.153 | 0.124 | 0.139 |
| | (0.066) | (0.066) | (0.017) | (0.017) | (0.137) | (0.137) | (0.137) |
| student | 0.123 | 0.115 | 0.011 | 0.014 | 0.207 | 0.216 | 0.192 |
| | (0.159) | (0.160) | (0.037) | (0.037) | (0.255) | (0.254) | (0.254) |
| middle class | -0.021 | -0.024 | -0.005 | -0.005 | -0.039 | -0.048 | -0.070 |
| | (0.105) | (0.105) | (0.028) | (0.029) | (0.217) | (0.217) | (0.217) |
| upper class | 0.164 | 0.194 | -0.248*** | -0.245*** | -0.714 | -0.694 | -1.074 |
| | (0.297) | (0.302) | (0.094) | (0.094) | (0.967) | (0.968) | (0.972) |
| medium income | 0.126 | 0.149 | 0.0212 | 0.023 | 0.134 | 0.200 | 0.194 |
| | (0.113) | (0.113) | (0.031) | (0.031) | (0.244) | (0.245) | (0.245) |
| high income | 0.145* | 0.139* | -0.000 | 0.000 | 0.032 | 0.112 | 0.113 |
| | (0.083) | (0.085) | (0.022) | (0.022) | (0.175) | (0.177) | (0.177) |
| S | | 0.042** | | 0.008 | | -0.065* | -0.063* |
| | | (0.021) | | (0.005) | | (0.037) | (0.037) |
| luck | | 0.001 | | -0.012 | | | |
| | | (0.039) | | (0.010) | | | |
| risk | | -0.013 | | -0.003 | | -0.037 | -0.042 |
| | | (0.016) | | (0.004) | | (0.039) | (0.039) |
| competitiveness | | 0.120* | | 0.024 | | -0.113 | -0.141 |
| | | (0.072) | | (0.019) | | (0.153) | (0.152) |
| Social Trust | | 0.014 | | | | | -0.549* |
| | | (0.153) | | | | | (0.286) |
| Belief in Meritocracy | | | | -0.019 | | 0.264* | |
| | | | | (0.018) | | (0.145) | |
| Constant | 0.121 | -0.071 | 0.474*** | 0.438*** | -0.254 | 0.065 | 0.532 |
| | (0.242) | (0.293) | (0.060) | (0.073) | (0.451) | (0.526) | (0.533) |
| Observations | 228 | 227 | 668 | 667 | 193 | 193 | 193 |
| R-squared | 0.024 | 0.059 | 0.014 | 0.021 | 0.025 | 0.063 | 0.065 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.3: Demographic and Experiment-based covariates of main outcomes in lab experiment.

## E.3 Additional Tables for Section 5

| | *Belief in Meritocracy* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info | 0.179* | 0.181** | 0.191** | 0.180** | 0.190* | 0.210* |
| | (0.092) | (0.089) | (0.088) | (0.088) | (0.103) | (0.122) |
| medium income | | -0.042 | -0.047 | -0.061 | -0.267 | -0.338 |
| | | (0.107) | (0.106) | (0.110) | (0.216) | (0.239) |
| high income | | 0.278** | 0.203* | 0.147 | 0.0624 | -0.118 |
| | | (0.110) | (0.113) | (0.115) | (0.254) | (0.300) |
| Constant | 0.364*** | 0.296*** | -0.00190 | -0.156 | -0.0890 | 0.212 |
| | (0.066) | (0.085) | (0.361) | (0.403) | (0.550) | (0.680) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 114 | 114 | 114 | 114 | 114 | 114 |
| R-squared | 0.032 | 0.107 | 0.159 | 0.196 | 0.280 | 0.374 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.4: Persistence of Effect on Belief in Meritocracy at Step 2. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy and overall score S.

| | *Belief in Meritocracy* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info | 0.056 | 0.053 | 0.056 | 0.048 | 0.156 | 0.193 |
| | (0.093) | (0.093) | (0.094) | (0.095) | (0.102) | (0.124) |
| medium income | | 0.116 | 0.119 | 0.103 | -0.073 | -0.119 |
| | | (0.111) | (0.113) | (0.118) | (0.214) | (0.242) |
| high income | | 0.234** | 0.215* | 0.185 | -0.071 | -0.117 |
| | | (0.114) | (0.120) | (0.124) | (0.252) | (0.303) |
| Constant | 0.418*** | 0.314*** | 0.272 | 0.321 | 0.537 | 0.525 |
| | (0.067) | (0.088) | (0.384) | (0.435) | (0.545) | (0.688) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 114 | 114 | 114 | 114 | 114 | 114 |
| R-squared | 0.003 | 0.040 | 0.045 | 0.062 | 0.289 | 0.357 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.5: Persistence of Effect on Belief in Meritocracy at Step 3. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion, a competitiveness dummy and overall score S.

| | *Blame* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.221 | 0.226 | 0.296 | 0.282 | 0.274 | 0.246 |
| | (0.160) | (0.160) | (0.192) | (0.210) | (0.218) | (0.320) |
| medium income | | 0.183 | 0.169 | 0.116 | -0.014 | 0.237 |
| | | (0.222) | (0.208) | (0.207) | (0.201) | (0.314) |
| high income | | 0.191 | 0.094 | 0.083 | 0.018 | 0.041 |
| | | (0.165) | (0.179) | (0.191) | (0.270) | (0.364) |
| Constant | -0.089 | -0.224 | -0.784 | -0.526 | -0.561 | -1.117 |
| | (0.111) | (0.146) | (0.541) | (0.871) | (0.527) | (1.315) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 96 | 96 | 96 | 96 | 96 | 96 |
| R-squared | 0.012 | 0.019 | 0.084 | 0.102 | 0.263 | 0.425 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.6: Blame Treatment **REL-BMT**. Extra Income Controls are fixed effects for answers from income questionnaire. The smaller set includes questions 1 and 4, the larger set all questions. Other Controls are age, gender and student status. The larger set also includes risk aversion and a competitiveness dummy. Standard errors clustered at the session level.

| | *Blame* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.047 | 0.052 | 0.051 | -0.003 | -0.072 | 0.040 |
| | (0.245) | (0.248) | (0.226) | (0.215) | (0.251) | (0.261) |
| medium income | | 0.076 | 0.093 | 0.134 | 0.065 | 0.177 |
| | | (0.250) | (0.261) | (0.274) | (0.327) | (0.387) |
| high income | | 0.154 | 0.155 | 0.052 | 0.359 | 0.245 |
| | | (0.192) | (0.200) | (0.199) | (0.322) | (0.454) |
| Constant | -0.024 | -0.012 | 0.022 | 0.686 | 0.037 | 1.079 |
| | (0.148) | (0.183) | (0.923) | (0.980) | (1.281) | (1.672) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 97 | 97 | 97 | 97 | 97 | 97 |
| R-squared | 0.001 | 0.009 | 0.013 | 0.077 | 0.228 | 0.379 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.7: Blame Treatment **INEQ-BMT**. Extra Income Controls are fixed effects for answers from income questionnaire. The smaller set includes questions 1 and 4, the larger set all questions. Other Controls are age, gender and student status. The larger set also includes risk aversion and a competitiveness dummy. Standard errors clustered at the session level.

| | *Blame* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | 0.122 | 0.145 | 0.153 | 0.224 | 0.211 | 0.136 |
| | (0.180) | (0.180) | (0.179) | (0.183) | (0.183) | (0.222) |
| medium income | | 0.081 | -0.064 | -0.119 | -0.142 | -0.058 |
| | | (0.201) | (0.213) | (0.219) | (0.239) | (0.315) |
| high income | | 0.499 | 0.586* | 0.565 | 0.338 | 0.458 |
| | | (0.347) | (0.350) | (0.357) | (0.382) | (0.464) |
| Constant | -0.060 | -0.058 | -0.245 | -0.338 | -0.326 | -1.255 |
| | (0.126) | (0.144) | (0.348) | (0.509) | (0.543) | (0.782) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 107 | 107 | 107 | 105 | 105 | 102 |
| R-squared | 0.004 | 0.028 | 0.098 | 0.123 | 0.212 | 0.385 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

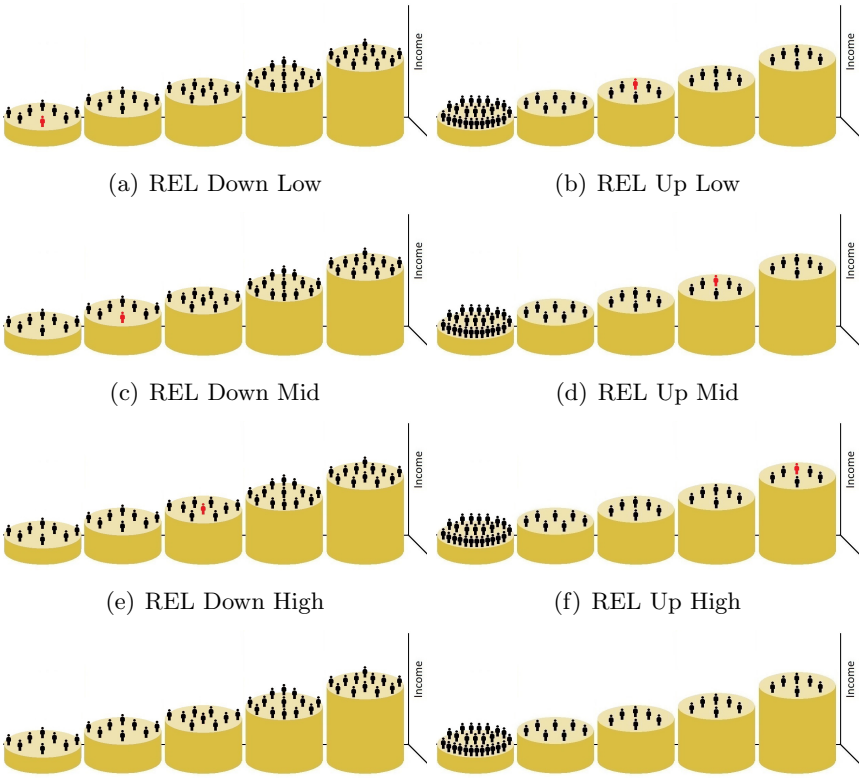
Table E.8: Survey measure of blame **REL** treatments. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion and a self reported competitiveness measure.

| | *Blame* | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| high info ($\beta$) | -0.102 | -0.206 | -0.189 | -0.224 | -0.234 | 0.007 |
| | (0.212) | (0.215) | (0.221) | (0.227) | (0.235) | (0.298) |
| medium income | | 0.470** | 0.418* | 0.432* | 0.405 | 0.354 |
| | | (0.220) | (0.231) | (0.232) | (0.245) | (0.284) |
| high income | | 0.250 | 0.310 | 0.292 | 0.320 | 0.161 |
| | | (0.370) | (0.376) | (0.385) | (0.437) | (0.544) |
| Constant | -0.039 | -0.184 | 0.203 | 0.160 | 0.064 | 0.997 |
| | (0.124) | (0.142) | (0.413) | (0.528) | (0.646) | (0.934) |
| Extra Income Controls | NO | NO | NO | NO | YES | YES$^+$ |
| Other Controls | NO | NO | YES | YES$^+$ | YES$^+$ | YES$^+$ |
| Observations | 109 | 109 | 109 | 108 | 108 | 107 |
| R-squared | 0.002 | 0.044 | 0.061 | 0.070 | 0.096 | 0.263 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E.9: Survey measure of blame **INEQ** treatments. Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion and a self reported competitiveness measure.

# F Additional Figures



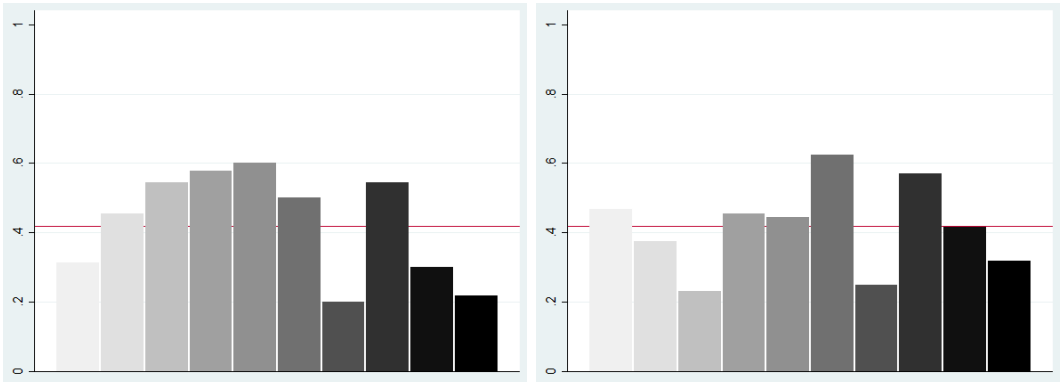

(a) REL Down Low (b) REL Up Low

(c) REL Down Mid (d) REL Up Mid

(e) REL Down High (f) REL Up High

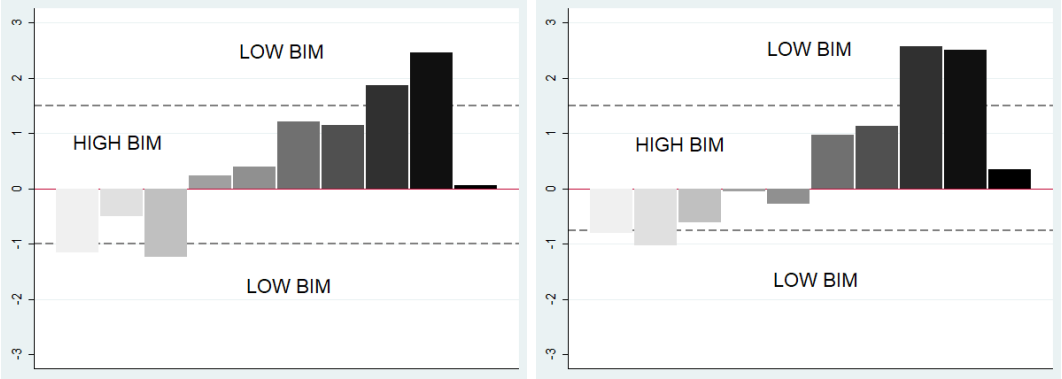
(g) INEQ Down (h) INEQ Up

Figure F.1: The pictures show the upwards and downwards information pictures for the different income categories as well as the information shown when relative position is *not* communicated.




(a) REL-MTB (b) INEQ-MTB

Figure F.2: Belief in meritocracy depending on rank. Best rank (= 1) on the left and worst rank (= 10) on the right.




(a) REL-MTB (b) INEQ-MTB

Figure F.3: How much does a participants rank depend on luck? Difference between actual overall rank and average rank in ability and effort (y-axis) depending on participants actual rank (x-axis). Three regions defined by cutoffs in how much beliefs on average effort and ability rank differ from actual overall rank which split people into those with low and high belief in meritocracy.