ECONOMIC EXPERIMENTS ON TRUTH-TELLING, INFORMATION AGGREGATION, AND INCOME INEQUALITY

By

Andrea Albertazzi

Dissertation

Submitted to the Department of Economics

University of Essex

for the degree of

DOCTOR OF PHILOSOPHY

in

Economics

March 2021

Declaration of Authorship

I, Andrea Albertazzi, declare that this thesis titled, "Economic Experiments on truth-telling, information aggregation, and income inequality" and the work presented in it are my own and a collaboration with others. In detail:

- Chapter 2 is jointly written with Prof. Friederike Mengel and Prof. Ronald Peeters.
- Chapter 3 is jointly written with Dr. Patrick Lown and Prof. Friederike Mengel.

I confirm that:

- This work was done wholly or mainly while in candidature for a research degree at this University.
- Where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated.
- Where I have consulted the published work of others, this is always clearly attributed.
- Where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work.
- I have acknowledged all main sources of help.
- Where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself

Abstract

This thesis contains three chapters employing controlled economic experiments. The first chapter studies to what extent laboratory measures of cheating generalize to the field. I present a novel measure that allows for individual level observations of cheating, and I relate decisions made in this laboratory task with individual choices taken in the field, where subjects can lie by mis-reporting their experimental earnings. According to this new measure, no correlation of behaviour between the laboratory and the field is found. The second chapter contributes to the literature on the ability of financial markets to perfectly aggregate private information into asset prices. Along with my co-authors, I conduct an experiment designed to benchmark information aggregation in markets, by randomly assigning subjects to different institutional environments, either a market or a BDM (Becker-DeGroot-Marschak) mechanism. We find a difference between the two environments that seems to be driven by price-insensitive traders, who appear to be unable to learn from market prices. In the third chapter, my co-author and I provide a causal identification of the impact of income inequality on attribution and social trust. We do so by using a combination of surveys and behavioral lab experiments. Using positional primes we find that a 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

ACKNOWLEDGMENTS

I would like to thank my supervisor, Friederike Mengel, for providing guidance and encouragement throughout the doctoral degree. I express sincere gratitude to the EssexLab for the incredible support given to my experiments. I thank my colleagues Elisabetta, Federico, Ludovica, and Patrik for our precious conversations. Thanks to Ennio Bilancini for his encouragement to pursue a Ph.D. and an academic career. Last but not least, a special thanks to Elisa, for her constant support and love.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1

Individual Cheating in the Lab: A New Measure and External Validity

1.1 Introduction

Cheating permeates many social and economic interactions of our daily life (DePaulo et al., 1996; Ariely, 2012). Examples range from corporate scandals (e.g., Dieselgate, Facebook-Cambridge Analytica), to tax evasion (Slemrod, 2007), and consumers misbehaviour (Mazar and Ariely, 2006). To make things worse, endevours to study cheating in natural contexts are hindered by its secretive nature. Therefore, controlled experiments represent an attractive instrument to study individual attitudes toward cheating.

The die-roll paradigm (Fischbacher and Föllmi-Heusi, 2013) represents the most popular measure of cheating used in the laboratory. Participants are asked to roll a die in private and to report the result to the experimenter. Because the true outcome is observed by subjects only, there is a monetary incentive to lie by reporting those outcomes associated with higher rewards. Despite its simplicity, this type of task presents a considerable limitation: cheating can only be inferred at the aggregate level by comparing the empirical distribution of actual reports with its theoretical prediction. Hence, it is not possible to know, by design, if a particular subject actually lied or not.[1]

Whether laboratory measures of cheating extend to non-controlled environments, is still under investigation. For instance, the experimenter scrutiny or the artificiality of the lab environment might trigger different ethical norms. If this is the case, then laboratory results on cheating might not generalize to the field (Levitt and List, 2007). Our paper aims to address these two limitations.

First, we design a novel task that, in contrast to the existing literature, allows to observe

[1] Other existing laboratory tasks that do allow individual level observation of cheating are sender-receiver games (Gneezy, 2005), variations of the die-roll task (e.g., Gneezy et al., 2018), and the matrix task (Mazar et al., 2008). However, sender-receiver games involve strategic interaction and, as the variations of the die-roll task, require observability of lies to be common knowledge, with obvious consequences on dishonest behaviour. The matrix task, instead, requires participants to be explicitly deceived in order to collect individual level observations of cheating.

cheating at the individual level. In our task subjects have five seconds to choose, in their mind, one out of sixty colours (e.g. *Yellow*) from a list displayed on their screen. Once this list disappears, three new different lists, containing four colours each (e.g. *White, Beige, Milk, Plum*), are displayed. Every new list is associated with a different positive payoff. If subjects claim their chosen colour to be in one of the three new lists, they receive the payoff associated with that list, otherwise they receive zero. We know that the participant has cheated if they pick a list of colours on the second screen that does not contain any colour that was already present in the first larger list.

Second, we use the fact that in our task cheating is observable at the individual level and ask to what extent cheating in the lab predicts cheating in the field within the *same* population. Participants are not paid immediately after the experiment. Instead, after a few days they have the opportunity to cheat in the field by self-reporting their earnings. Subjects are paid according to the amount of money they claim to have earned in the laboratory. We use two field variations that differ in the degree of anonymity of the field decision. In the first one, the self-reporting procedure is completely anonymous while the second field variation requires participants to meet in person with the experimenter.

The main contribution of this paper is twofold: (i) it develops a new laboratory task that allows for individual level observations of cheating, and (ii) it allows to compare both the extensive and intensive margin of cheating between the laboratory and a non-controlled environment.[2]

In line with previous findings on individual dishonesty, we find that a considerable fraction of subjects cheats in our laboratory task, but some of them do not cheat to the full extent. However, no significant correlation of dishonest behaviour between lab and field is observed. Although more than half of the subjects cheat to some extent in our new task, most of them refrain from over-reporting their experimental earnings. Moreover, for those who do so, we find no difference in the extent of cheating between subjects that are honest

[2]The extensive margin corresponds to the fraction of people who lie; the intensive margin corresponds to the extent of cheating for people who choose to do so.

in the laboratory and those who are not.

To the best of our knowledge, only few other studies examine the correlation between dishonest behaviour in the lab and cheating in the field within the *same* population.[3] Dai et al. (2018) perform an artefactual field experiment where passengers of public transportation are asked to play a modified version of the die-roll task. As a main result, the study finds that fare dodgers, on average, are more likely to report the most profitable outcome than ticket holders.

Similarly to our study, Potters and Stoop (2016) use a student subject pool to correlate self-reported performance in a mind game implemented in the lab, with a field measure of cheating. After the experiment, payments are issued via bank transfer and some subjects are deliberately overpaid by an amount of €5. A significant correlation of 0.31 between performance in the mind game and not reporting the overpayment is found. In contrast to Potters and Stoop (2016), our study allows to observe cheating at the individual level, measures cheating at both the extensive and intensive margin, provides full anonimity in the lab and in one of the field tasks, and requires active misreporting in both the lab and the field. These new features allow to gain a deeper understanding of whether lab measures of cheating are reliable predictors of dishonesty in other environments.

The extent to which laboratory results on cheating can be generalized to other environments remains unclear.[4] Laboratory evidence shows persistent patterns on dishonesty across subjects. Some individuals are completely honest, while others either lie to the maximum extent possible, or forfeit part of the monetary gains when they do cheat (Gneezy et al., 2018; Abeler et al., 2019; Gerlach et al., 2019). Instead, studies that focus on dishonesty in the field provide mixed results. While some find substantial cheating among subjects (e.g., Drupp et al., 2019; Bucciol and Piovesan, 2011), other studies report different findings. For

[3] Other papers focus on the correlation between a lab measure of cheating with the broader concept of rule violation in the field: in-prison offences (Cohn et al., 2015), school misconduct (Cohn and Maréchal, 2018) and work absenteeism (Hanna and Wang, 2017).

[4] For a broad discussion on the generalizability of experimental results in economics see Levitt and List (2007); Al-Ubaydli and List (2013); Falk and Heckman (2009); Camerer (2015); Kessler and Vesterlund (2015); Al-Ubaydli et al. (2017).

example, Abeler et al. (2014) report no evidence of lying in a randomized field experiment where subjects are called at home and have a monetary incentive to misreport the outcome of a privately tossed coin. Similarly, Cohn et al. (2014) show that bankers cheat in a coin-flip task when they are reminded about their professional identity. However, when such cue is not emphasized, reported outocmes do not differ from their truthful distribution.

The remainder of this paper is organized as follows: Section 1.2 describes the experimental design, Section 1.3 presents the main results of the paper, Section 1.4 discusses about the main findings, and Section 1.5 concludes.

1.2 Method

1.2.1 Experimental Procedure

The experiment was conducted between November 2017 and July 2019 at EssexLab at the University of Essex. In total, 249 participants were recruited using hroot (Bock et al., 2014). Laboratory sessions (twelve in total) lasted about 43 minutes and average total earnings (inclusive of a £4 show-up fee) were £12.62 (s.d. £4.60). The experiment was programmed using z-Tree (Fischbacher, 2007a).

Before the laboratory session, participants acknowledged that the experimental proceedings were paid after few days (see Figure A.1 in appendix). Any further detail about the payment procedure was omitted. Subjects entered the lab anonymously and were randomly allocated to the terminals so that it was impossible to link their identity to a particular workstation. At the beginning of the experiment each subject was informed that the session consisted of five parts and a short final questionnaire. Detailed instructions about each part were displayed on subjects' screens only upon completion of the previous one (all instructions are reproduced in Appendix A.1). Where needed, control questions where elicited before the actual choices were made. Participants were informed that at the end of the experiment two of the five parts were randomly selected for payment.

1.2.2 Laboratory Experiment

The laboratory experiment consisted of five different parts, whose order was randomized at the session level.

Part 1. In the first part of the experiment subjects faced our new so-called mind game (hereinafter *list game*).[5] The *list game* consists in a simple decison problem. First, a random list of 60 colours names (e.g., *Yellow*) appears on the computer screen and is displayed for five seconds only. This ensures that no subject can read all of the colours in the given time. Before the timer expires, each participant must choose, in their mind, a colour from the list. After five seconds, the list disappears and three new random lists containing four colours each are displayed on the screen – e.g., one of the three lists might be *White, Beige, Milk, Plum*. Subjects are then asked whether the colour they have in mind appears in one of the three new lists, each of which is associated with a specific payoff: £1, £3 and £5 respectively. If yes, then they must select the list that contains the colour they thought of, otherwise they must select the alternative option ("Not in the lists"). Participants who claim to have found their colour by selecting one of the three lists, earn the corresponding payoff. Instead, subjects who choose the alternative option earn £0. By design, the colours displayed in the three new lists are never present in the list where subjects actually choose from. Hence, every positive payoff reported by participants can be classified as a lie.[6] Because the colour choice is made in subject's mind, individual cheating appears to be undetectable.[7] This is verified via a control question. After the decision on whether to cheat or not is made, participants answer to the following question:

[5]Usually, in mind games, subjects have to "predict", in their mind, the outcome of a random device (e.g. die-roll). Then, they are asked to report whether their prediction was correct or not. They receive a positive reward if the answer is yes, zero otherwise. See Jiang (2013), Potters and Stoop (2016) and Kajackaite and Gneezy (2017) for examples.

[6]It is unlikely that subjects forget they colour. Even in that case, we would expect participants to randomize between the four options but we do not find evidence of this.

[7]We designed our instructions carefully (see Appendix A.1). Participants are never told neither that the colours in the three lists are present in the first one, nor otherwise. They simply receive no information on this matter. Our design is similar in this regard to other laboratory (e.g., Andreoni, 1988; Gächter and Thöni, 2005) and field (e.g., Bertrand and Mullainathan, 2004; Das et al., 2016) studies that withhold information to participants.

"Out of 100 participants, how many do you think successfully choose a colour in the first list that is also present in one of the three lists?"

Subjects earn an additional £1 if their answer is within five points from the true value – i.e., zero. As a consequence, any answer below or equal to five indicates that subjects believe the colours in the three lists are not present in the first one. Thus, they realize that cheating could be detected with certainty.[8]

Part 2. This part consists in a computerized variation of the mind game used in Kajackaite and Gneezy (2017). Subjects have to roll a virtual five-sided die where each side is associated to a colour. First, participants must choose one of the five colours in their mind. Then, the outcome of the die roll is revealed and subjects must report whether the colour they have in mind corresponds to the actual outcome of the die roll. If the answer is yes they earn £5, otherwise £0. This task resembles the *list game* because the decision is made in subjects' mind with the difference that cheating cannot be detected at the individual level.

Part 3. In this part subjects are randomly paired and play a dictator game. Each member of the pair is endowed with £6 and decides how much money to transfer, in steps of £1, to the other group member. After both decisions are made, one of the two choices is implemented with equal probability. The dictator game is used as a measure of greed and is elicited as a proxy for pro-social behaviour.

Part 4. Part four consists in a trust game similar to Burks et al. (2003), where each participant knows in advance that they will play both as a sender and as a receiver. Subjects are randomly paired and after being endowed with £3 they choose whether to send £0, £1, £2 or £3 to their counterpart. Any amount sent is tripled. Without knowing the decision of the other player, both subjects decide how much to return for any possible transfer they could receive. After all decisions are made, the computer assigns the roles with equal probabilities and the corresponding decisions are implemented. We measure trust as a control for

[8] The aim of the question is not to accurately measure subjects' beliefs. Instead, it represents a rough measure that verifies whether participants understood lying could be detected and thus, if our new laboratory task can be interpreted as a mind game. A different and more accurate scoring rule might have emphasized cheating as the matter of the study undermining subsequent behaviour.

social preferences. This measure allows us to investigates whether subjects that put more trust in others (or are more trustworthy) are also less likely to lie.

Part 5. In the last part, risk preferences are elicited using a slightly modified version of the lottery choice task implemented in Eckel and Grossman (2008). Participants must choose one out of five virtual boxes. Every box contains two payoffs that are realized with equal probability (see Table A.1). Starting from a risk free lottery that yields £2, the expected payoffs of the subsequent lotteries increase so as their variance. Hence, the higher the expected payoff, the higher the risk. The main advantage of this task resides in its simplicity and thus, can be easily understood by participants. Nonetheless, it can identify enough heterogeneity in risk attitudes. It is important to elicit risk attitudes as the decision to cheat also depends on the risk of being caught lying. Understanding the relation between individual preferences for honesty and risk attitudes might unveil important insights on one's decision to cheat.

Upon completion of the five parts, subjects answer to an incentivized questionnaire collecting socio-demographic information and to a 20-item measure of Big five (Donnellan et al., 2006). Once participants complete the questionnaire, their own experimental earnings are calculated and displayed on their screen. Subjects are then asked to note their earnings on a piece of paper ("reminder card"), to fold this into an envelope, and to conceal their rewards by clicking a button on their screen.[9] At this point, participants are the only ones knowing the amount of money they have earned.[10]

At the very end of the session, each subject is provided with a paper sheet named "Payment form" which contains detailed instructions about the payment procedure.[11] Note that every form contains a hidden code that allows it to be associated with the corresponding workstation.[12] Hence, it is possible to uniquely identify behaviour in the lab – but not individual's

[9] The role of the "reminder card" is to ensure that subjects do not forget the amount of money they earned in the experiment.

[10] Of course earnings where stored in the data, but they could not be linked to a subject's identity.

[11] This prevents behaviour in the lab to be affected by the subsequent field task.

[12] Note that, as in the *list game*, participants were never told that it was possible to link lab-field choices. Simply, they received no information on this regard.

identity – with subsequent choices in the field.

Subjects are then asked to leave the lab without filling in the payment form.

1.2.3 Field Experiment

The field experiment is designed to resemble a variation of the standard payment procedure. Participants are not paid immediately after the laboratory session. Instead, after few days they can self-report their earnings using the Payment form they were provided with. Payments are provided, in cash, upon provision of this paper sheet. Subjects are free to self-report any integer number between the minimum and the maximum possible payoff, £5 and £26 respectively.[13] Thus, there is a monetary incentive to cheat by claiming a higher payment than the amount of money actually earned in the lab. Note that, at this stage, detection of lies is not possible. Cheating in the field can only be inferred later on after decoding each payment form and then, by comparing the self-reported payment with the actual experimental earnings. Moreover, apart from self-reported earnings and the payment date, no other personal information is contained on the forms. Hence, it is not possible to link the Payment forms to individuals' identities.

We employ two treatment variations so as to investigate possible factors that might influence cheating outside the laboratory. The first treatment (*NoFtF*) involves no face-to-face interaction with the experimenter, resembling the full anonymity condition available in the lab. In more detail, at the end of the experiment each participant is randomly assigned to a locker located in a university campus building, and is endowed with the corresponding key. Subjects must leave the Payment forms, containing their self-reported earnings, into their assigned locker. The sheets are then collected by the experimenter and replaced with cash corresponding to the money claimed by subjects. After all payments have been pro-

[13]The purpose of this interval is twofold: (i) to bound the maximal payoff that a dishonest person could claim, and (ii) to minimize possible confoundings due to strategic behaviour. For example, a person that earns £12 in the lab and is tempted to report £15, might question whether this payoff was actually earned by some other participant. If not, the lie would be caught immediately undermining the decision to cheat. Knowing that payoffs are bounded and that the subject pool is at least of 100 participants, should minimize this issue.

vided, participants can then collect their cash earnings.[14]

In contrast, the second treatment requires participants to meet face-to-face (*FtF*) with the experimenter in an office room. Instead of leaving the payment form into a locker, subjects hand the paper sheet to the experimenter and are paid immediately.[15] Besides the personal interaction, a degree of anonimity is also assured in this phase of the experiment as no personal information is collected.

1.2.4 Design considerations

The main contribution of this experiment is to allow for individual level observation of cheating. Moreover, it makes possible to measure both the extensive and intensive margin of cheating in the lab and in the field.

Despite the fact that the *list game* and the field tasks differ in their intrinsic nature, the experimental design still allows to compare behaviour between two similar decision problems. It is true that the field experiment differs in many aspects from the *list game*. The aim of this exercise, however, is to relate a laboratory measure of cheating to dishonesty in a task that might reflect a real-life situation and thus, not too artificial.

First, it must be noted that both in the lab and in the field participants can only cheat by commission. This is in contrast with Potters and Stoop (2016) – the study most closest to our design – where subjects can cheat by just not reporting the payment error to the experimenter. The difference between cheating by commission and omission might lead to differences in behaviour indeed. As one might expect, lying by commission is less tempting when compared to a situation where cheating requires no active choice (Pittarello et al., 2016).

Another important variable that is kept constant between the two environments is anonymity.

[14] Upon payment collection, subjects complete the receipt form left in their locker and leave this, along with the keys, in a separated letterbox along with those of other participants. This procedure allows to maintain complete anonymity even after subjects are paid for their participation.

[15] Immediately after a payment is collected, and without supervision, a subject has to complete the receipt form and leave it in a box along with those of other participants. This procedure guarantees that the payment forms cannot be linked to participants' identities afterwards.

As Gneezy et al. (2018) suggests, the probability of being caught lying highly affects dishonesty. In this experiment, despite cheating can be detected at the individual level, subjects' identity can never be linked to their choices. This feature allows to generate conditions similar to those real-life situations where dishonest actions cannot be associated to one's identity, e.g., not returning a lost wallet.[16]

Finally, the design allows to control for possible confounding variables caused by social preferences. The consequences that lying might have on other people is known to affect dishonesty (Gneezy, 2005; Erat and Gneezy, 2012). For this reason, in the lab as well as in the field, the victim of the lie is always the experimenter.

1.3 Main Results

1.3.1 Laboratory results

The main results presented in this section focus on choices made in the *list game* and also on how these correlate with other co-variates elicited in the laboratory. Because the treatment variation pertains to the field only, laboratory observations are pooled to increase the power of the analysis.

Figure 1.1 shows the choices made in the *list game* where each bar represents one of the options that subjects could choose. The three rightmost bars (£1, £3, £5) represent the fractions of participants that dishonestly reported to have found the colour they had in mind in one of the three subsequent lists. Instead, the first column (£0) corresponds to the percentage of subjects that have been honest in the *list game*. The figure highlights significant heterogeneity in lying preferences. In contrast to standard economic predicitons, 41% of the subjects choose to not cheat at all by selecting the option that pays nothing. Interestingly, although 40% of participants cheat to the maximum extent possible (£5), a substantial proportion of them forfeits the maximal gains from lying choosing the lists associated with either the £1 or £3 payoff, 4% and 15% respectively. Hence, dishonest

[16] As Cohn et al. (2019b) show, returning a wallet is perceived as a civic honest act.

behaviour seems to be driven by heterogeneity in lying preferences. Some participants are either always honests or unconditional liars, whilst the remaining subjects fall in between these two categories depending on the relative gains from lying.

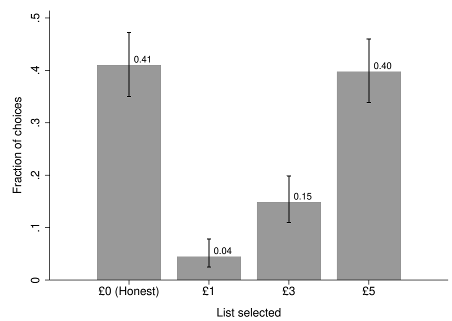


Figure 1.1: Proportions for each choice made in the *list game*. Vertical lines represent 95% confidence intervals (N=249).

Result 1: The highest fraction of cheaters in the *list game* report the payoff-maximizing lie. A significant proportion of liars do not cheat to the maximum extent possible.

Statistical support: When restricting the data to two options, one-sided binomial tests reject the null hypothesis that these two options occur with probability equal to 0.5. For the pairs (£1,£3), (£3,£5) and (£1,£5), the conditional probability for the option with a higher payoff is significantly above 0.5 at 1% level for all pairs.[17]

Looking at participants' beliefs, Figure 1.2 presents the answers to the control question elicited after the *list game*. This question allows to verify whether participants think their lies cannot be detected. As the figure shows, only about 6% of the subjects reported a belief lower or equal to five.[18] Thus, almost all of the participants made their decisions as if it was not possible to detect cheating at the individual level.

One might question whether the new task herein introduced can be related to some other laboratory measures of cheating that do not allow for individual level observations.

[17] In detail, $N = 48$, $p < 0.001$ for pair (£1,£3), $N = 136$, $p < 0.001$ for pair (£3,£5), and $N = 110$, $p < 0.001$ for pair (£1,£5).

[18] We acknowledge that some subjects might have misunderstood the question and reported their belief of how many participants actually cheated. Because we did not want to emphasize cheating as the matter of the study, such question was not elicited.

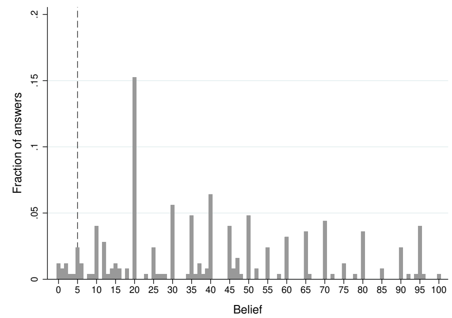


Figure 1.2: Beliefs elicited in the control question. Participants earned £1 if their answer was within 5 points from the correct value (zero). Hence, the vertical dashed line represents the upper bound for which a subject is thought to believe the colours in the three lists were not present in the first one. Notably, the highest fraction of answers corresponds to 20. This is consistent with the belief that the 12 colours in the three lists were randomly drawn, with equal probability, from the first list containing 60 colours (N=249).

To corroborate our new measure we look at how choices in the *list game* are correlated with choices made in the mind game with the die-roll (Part 2). In the latter task the fraction of positive claims amounts to about 60%, which is very distant from its expected value (20%). Hence, about 40% of participants cheated in the die-roll task by reporting a "Yes". If the two measures are related, then we should expect participants that are dishonest in the *list game* to be more likely to report a "Yes" after rolling the die. As can be seen in Figure 1.3, this seems to be the case. Participants who cheat in the *list game* (right panel), are more likely to obtain a positive payoff in the other mind game (two-sided Fisher's exact test: $p = 0.027, N = 225$).[19] This result is also confirmed in Table 1.1.

As described in Section 1.2, other individual attitudes as risk preferences, individual greed, and trust were further elicited during the laboratory sessions. Table 1.1 shows how behaviour in these tasks correlates with cheating. The first three regressions represent linear average effects on choices in the *list game* while specifications 4-6, show marginal effects on a dichotomic variable that takes value one if a subject lied, to some extent, in the same task. Variable *Yes* represents the report made in the die-roll game as seen

[19]Due to a fault of some computers (after playing the *list game*), in one session choices in the trust game were not recorded for some subjects. Thus, the observations from that session have been removed when looking at the correlation between choices in the *list game* and the other laboratory tasks.

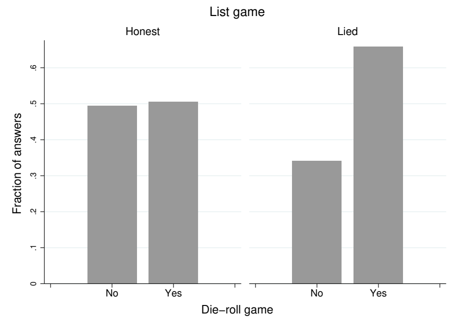


Figure 1.3: Correlation between cheating in the *list game* and choices in the die-roll game. The left panel represents choices in the latter task for those that have been honest in the *list game*. The right panel shows choices in die-roll game for those participants that lied in the *list game* (N=225).

previously. The variable *Risk* corresponds to the lottery chosen in Part 5 and can take integer values starting from one, which corresponds to the safe option, to five, where higher numbers are associated with higher risk. The two following variables, *Transfer dictator* and *Transfer trust*, correspond to the money sent to the receiver in the dictator and trust game respectively.[20]

Similar to what is found in Hübler et al. (2018), it seems that participants who are more

	OLS			Probit (dy/dx)		
	(1)	(2)	(3)	(4)	(5)	(6)
Yes (=1)	0.563*	0.576*	0.574*	0.153**	0.154**	0.155**
	(0.306)	(0.299)	(0.301)	(0.064)	(0.063)	(0.064)
Risk	0.198*	0.181*	0.179*	0.051**	0.049**	0.049**
	(0.101)	(0.099)	(0.101)	(0.020)	(0.020)	(0.021)
Transfer dictator	−0.236**	−0.219*	−0.219*	−0.028	−0.026	−0.026
	(0.115)	(0.113)	(0.113)	(0.023)	(0.023)	(0.023)
Transfer trust	−0.391**	−0.343*	−0.346*	−0.091**	−0.085**	−0.085**
	(0.189)	(0.187)	(0.192)	(0.039)	(0.039)	(0.040)
Constant	1.929***	1.527***	1.560**			
	(0.560)	(0.566)	(0.667)			
Controls	YES	YES^{+}	YES^{++}	YES	YES^{+}	YES^{++}
Observations	225	225	225	225	225	225

Table 1.1: Cheating in the *list game* and other laboratory choices.
Note: Specifications 1-3 represent least square estimations on choices made in the *list game*. Specifications 4-6 represent marginal effects on a dummy variable indicating whether a subject lied in the *list game* or not. Specifications (1) and (4) control whether the *list game* was played after the other cheating task involving the virtual die. Specifications (2) and (5) include a dummy for the *NoFtF* treatment and regressions (3) and (6) additionally control for gender effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

[20] Table A.2 in Appendix A.2 presents a similar analysis. Instead of using the transfer in the trust game, the money returned is used as a regressor. This variable is not significant at any conventional statistical level.

willing to choose risky lotteries are also more likely to lie. As dishonesty highly depends on the perceived risk of being exposed as a liar (Gneezy et al., 2018), it is reasonable that individuals more prone to cheat are also more willing to bear the risk associated with it.

Focusing on the variables *Transfer dictator* and *Transfer trust*, it is possible to note that both of them are inversely related with cheating, though the evidence for the dictator game is not significant in the probability model. This correlation translates into the relation between social preferences and dishonesty. Participants that are more generous or more trusting cheat, on average, by a lower amount and less frequently. This suggests that individuals who value social preferences the most are also those who attribute high value to social norms or, in this particular case, honesty.

On what concerns how cheating relates to demographic co-variates elicited in the final questionnaire, no particular effect is found. Tables A.3 and A.4 (Appendix A.2) show no robust and significant pattern for any of the individual demographics or personality traits.

1.3.2 Field results

In this section, we present results for both the field treatments and their correlation with laboratory behaviour.[21]

Because in the payment procedure the maximum amount of money a subject can claim depends on their actual experimental earnings, cheating in the field is standardized as follows

$$\textit{Cheat field} = \frac{\text{self-reported earnings} - \text{actual earnings}}{26 - \text{actual earnings}}$$

[21]Note that the total number of observations used for the lab-field comparison is lower then the one used for the laboratory analysis. This is due to the fact that in *FtF* treatment 15 subjects either forgot to collect the payment or were not able to participate in the field experiment. In *NoFtF* instead, because during the trust game (after playing the *list game*) some answers were not recorded, participants whose lab payment was determined by this task, have been removed from the lab-field analysis. Conclusions presented in Section 1.3.1 do not change if these observations are fully removed from the whole analysis.

Hence, such variable can take values in the interval of $[0,1]$.[22] In other words, it measures how many pounds (£) are over-reported, relative to the maximum amount of money a subject could claim.

Figure 1.4 presents the results for both field treatments and their relation with choices made in the *list game*. The vertical axis measures cheating outside the laboratory as defined in the previous equation. Thus, any observation above zero represents the extent of cheating in the field for a particular subject. The horizontal axis, instead, summarizes the choices made by participants in the laboratory. Thus, from this graph it is possible to relate both the extensive and the intensive margin of cheating between the *list game* and over-reporting of experimental earnings.

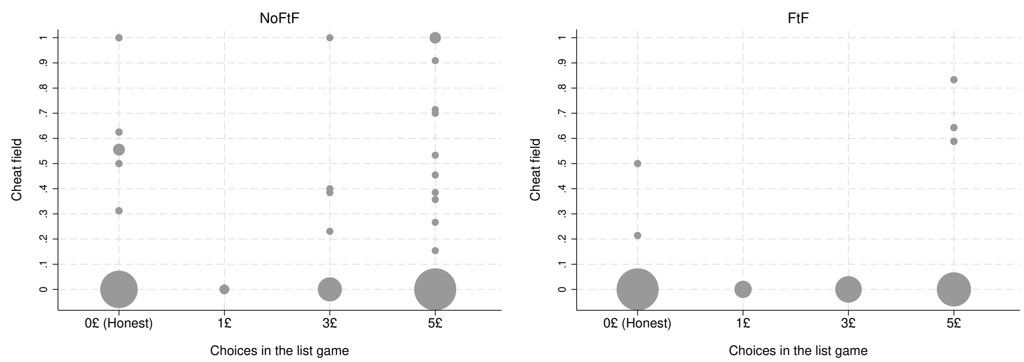


Figure 1.4: Comparison of cheating between the lab and the field for the *NoFtF* (left panel, n=123) and *FtF* (right panel, n=103) treatments with weighted markers. The smallest circles represent one single participant. The y-axis indicates the extent of cheating in the field. The x-axis represents the choices made in the *list game*.

As the figure shows, the data do not support generalizability of laboratory results on cheating in either of the two field variations. First, in both cases, most of the participants refrain from over-reporting their experimental earnings. The percentage of cheaters drops from about 66% (54%) in the lab, to slightly below than 19% (5%) in the field in the *NoFtF* (*FtF*) treatment.[23] As expected, in the field variation with a weaker degree of anonymity

[22] Actual lab earnings range between £5 and £19 included. Thus, the variable *Cheat field* is always defined. Further, no subject under-reported their earnings. On average, subjects actually earned £11.84 (SD 3.35) and £11.86 (SD 3.47) in the *NoFtF* and *FtF* treatments respectively. A Mann-Whitney U test does not reject the hypothesis of equality ($p = 0.897, N = 226$).

[23] Similar to this result, Gerlach et al. (2019) show in a meta-analysis that dishonesty is significantly more prevalent in lab experiments than in field studies.

(*FtF*), the fraction of participants that do cheat is significantly lower (two-sided Fisher's exact test: $N = 226$, $p = 0.002$). The face-to-face interaction appears to trigger higher costs associated with lying and thus, to reduce dishonest behaviour. A similar result is also found in Conrads and Lotz (2015).

Moreover, it appears there is no significant difference on the extent of cheating in the field between who cheated in the *list game* and those who did not. The mean value of *Cheat field* is 0.59 for both honest and dishonest participants in the *NoFtF* treatment. In the *FtF* variation this value is 0.37 and 0.69 for honests and cheaters respectively but the low number of observations does not allow to make any reliable inference.

	OLS			Probit (dy/dx)		
	(1)	(2)	(3)	(4)	(5)	(6)
list game:						
1£	−0.035	−0.036	−0.040			
	(0.023)	(0.023)	(0.024)			
3£	−0.023	−0.020	−0.020			
	(0.025)	(0.025)	(0.025)			
5£	0.040	0.044	0.043			
	(0.033)	(0.032)	(0.032)			
Cheater (= 1)				0.018	0.028	0.023
				(0.047)	(0.050)	(0.050)
Risk	0.009	0.010	0.008	0.011	0.012	0.009
	(0.011)	(0.011)	(0.011)	(0.016)	(0.016)	(0.017)
Transfer dictator	0.021	0.021	0.021	0.028	0.028	0.029
	(0.014)	(0.014)	(0.014)	(0.021)	(0.020)	(0.020)
Transfer trust	-0.036^{**}	-0.036^{**}	-0.039^{**}	-0.060^{**}	-0.060^{**}	-0.064^{**}
	(0.016)	(0.016)	(0.017)	(0.029)	(0.029)	(0.028)
Constant	−0.004	0.015	0.037			
	(0.051)	(0.060)	(0.062)			
Controls	YES	YES^{+}	YES^{++}	YES	YES^{+}	YES^{++}
Observations	209	209	209	209	209	209

Table 1.2: Laboratory behaviour and cheating in the field with *Cheat field* as a dependent variable.

Note: Specifications 1-3 represent least square estimations on the variable *Cheat field*. Dummies 1£, 3£, and 5£ represent choices made in the *list game* (honests are the excluded category). Specifications 4-6 represent marginal effects of cheating in the *list game* on a dummy variable indicating whether a subject lied in the field or not. Specifications (1) and (4) include a dummy for the *NoFtF* treatment, regressions (2) and (5) further control for actual laboratory earnings and, specifications (3) and (6) additionally control for gender effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Result 2: There is no significant correlation of cheating betweeen choices in the *list game* and in the field.

Statistical support: Cheaters in the lab are not more likely to cheat in the field in both treatment variations (two-sided Fisher's exact test: $N = 123$, $p = 0.809$ (*NoFtF*); $N = 103$, $p = 1.000$ (*FtF*)). The Spearman correlation between choices in the *list game* and *Cheat field* is 0.04 and 0.08 in *NoFtF* and *FtF* respectively, and not statistically significant in either of the two field variations (two-sided test: $N = 123$, $p = 0.658$ (*NoFtF*); $N = 103$, $p = 0.375$ (*FtF*)).

Table 1.2 confirms the results.[24] Interestingly, if the above analysis is replicated using choices in the die-roll task, hence not individual level observations of cheating, different conclusions are reached. Labelling as cheaters all subjects that answered "Yes" in this task, we find a weakly significant correlation of cheating in the *NoFtF* treatment. Participants who answer "Yes" are more likely to cheat in the field (two-sided Fisher's exact test: $N = 123$, $p = 0.088$). Furthermore, the Spearman correlation coefficient between *Cheat field* and *Yes* is 0.16 and weakly significant (two-sided test: $N = 123$, $p = 0.070$).[25] These numbers are distant from the ones shown in *Result 2*, and might lead to the opposite conclusion.

Hence, individual level observations of cheating appear to be of paramount importance in understanding such secretive and subtle behaviour. These type of data might then provide new insights that cannot be inferred using aggregate statistics.

1.4 Discussion

Dishonesty can be very sensitive to personal factors (Rosenbaum et al., 2014; Jacobsen et al., 2018), and this in turn translates into heterogeneity in lying preferences (Gibson et al., 2013). The data show that cheating within and across the two environments is sensitive to individual preferences. Moreover, while in the *list game* both risk and social preferences are correlated with individual dishonesty (Table 1.1), this seems not to be the case for cheating in the field. Table A.6 and Table 1.2 (Appendix A.2) show that only choices

[24] A similar analysis is carried out in Table A.5 (Appendix A.2). The OLS estimates are generated using the amount of over-reported money as a dependent variable and draw the same conclusions.

[25] In the *FtF*, obviously, these two variables do not correlate. The Fisher's exact test delivers $p = 0.649$ while the Spearman correlation coefficient is 0.08 with $p = 0.417$, $N = 103$.

in the trust game are significantly correlated with dishonesty in the payment procedure.

Apart from heterogeneity in preferences, differences in dishonest behaviour might also hinge on the experimental paradigm (Gerlach et al., 2019). For example, while Gächter and Schulz (2016) find a positive correlation between the corruption index on the country level and reports in die-roll tasks, such effect is not found using coin-flip tasks (Pascual-Ezama et al., 2015). Hence, another possible source of variability in cheating can be generated by differences between the laboratory and the field tasks.

First, it should be noted that, although in the lab all decisions are computerized, the self-reporting procedure adopted in the field requires participants to lie to the experimenter. Cohn et al. (2019a) indeed find that interacting with a human induces significantly less cheating when compared to interacting with a machine. Hence, this difference in the communication channel might concur in explaining the results presented in Section 1.3. However, the data cannot explain why subjects that have been either honest or dishonest in the *list game*, are equally likely to lie and to cheat to the same extent (on average), in the field. Hence, the communication channel, per se, does not seem to fully explain the main findings.

Another difference between the lab and field tasks might rest on the moral costs associated with cheating. While participants can lie about a random event in the *list game*, the self-reporting procedure forces them to cheat in the field by claiming a higher payment, i.e., by "stealing" money. In the latter case, it is possible that cheating triggers higher moral costs compared to lying about an artificial outcome, and this would result in more honest reports. Hermann and Mußhoff (2019) indeed find that individuals are less willing to steal than lying in a die-roll experiment. Hence, higher moral costs implied by stealing would partially explain the low number of subjects that over-reported their experimental earnings. However, this effect alone cannot fully explain the lack of correlation between the lab and the field presented in *Result 2*.

It is also possible that differences in dishonest behaviour depend on the time available to make a decision. While in the laboratory choices are made within few minutes, in the field this is not the case. Subjects can spend few days to think on whether to claim a higher payment or not. If reflecting more time on the possibility to lie reduces dishonest behaviour, this might explain why only few subjects lied in the field. To the best of my knowledge, only Andersen et al. (2018) study the effect of time on cheating within the die-roll paradigm and find no difference in dishonesty when participants are given an extra day to decide. In light of this finding, it seems unlikely for *Result 2* to be driven by the difference in the time available to make the decision.

Apart from individual preferences for honesty or differences between experimental paradigms, another explanation for *Result 2* might rest on the experimental design as a whole.

As the reader might have noted, one's willingness to claim a higher payment could depend on their actual laboratory choices. Subjects who cheat in the *list game* are more likely to obtain higher earnings and in turn, they might refrain from self-reporting a higher payment because of an income effect. By a similar argument, participants that remain honest in the lab might be more tempted to cheat in the field due to the higher stakes involved. Thus, we should expect a negative relation between laboratory earnings and over-reporting in the payment procedure.[26] Althought only two randomly drawn parts where used to determine each subject's payment, if the argument above is true, it could explain why no correlation is found between the two environments.

However, field behaviour seems to not depend on actual laboratory earnings. The coefficient of actual laboratory earnings in Table A.6 and Table 1.2 (Appendix A.2) is not statistically significant at any conventional level. Although cheaters in the *list game* have actually earned, on average, £2.8 (£1.3) more than honests participants in the *NoFtF* (*FtF*) treatment, these differences are relatively small. Therefore, the relative difference in potential gains from over-reporting between honests and liars is little. Moreover, two recent

[26]Moral licensing or conscious accounting might generate the same effect but are less likely to play a role in explaining the main results. First, their effect might have been washed out by the dictator and the trust games. Second, the correlation found between the *list game* and the dice game works in the opposite direction.

meta-analysis find a weak (if none) effect of rewards on dishonesty (Abeler et al., 2019; Gerlach et al., 2019). Hence, although the lab and field tasks are not perfectly independent, income effect and stakes size do not seem to explain results found in Section 1.3.2.

This section examined some factors that might have concurred in determining *Result 2*. Although some of them can partially account for the main findings, none of them, alone, can fully explain the lack of correlation of cheating presented in this study.

1.5 Conclusions

Even though laboratory experiments on cheating abound in the economic literature, only few studies explore their generalizability to the field. This paper aims to relate a laboratory measure of cheating with dishonesty in a non-controlled environment within the *same* population. To this purpose, we provide a laboratory experiment that employs a new measure of cheating. As a main novelty, this task allows for individual level observations of cheating. Behaviour in the lab, is then compared to choices in the field, where subjects have the possibility to cheat by over-reporting their experimental earnings. Payments are not issued immediately after the laboratory experiment. Instead, after few days participants are allowed to self-report their earnings to the experimenter. Subjects are paid according to the amount of money they claim to have earned in the laboratory.

As shown by the laboratory data, established results as lying aversion and non-payoff-maximizing lies are replicated. However, no correlation of cheating between the lab and the field is observed. While more than half of the subjects cheat to some extent in the laboratory, most of the participants truthfully report their experimental earnings. Moreover, we find no difference in the extent of cheating in the field between those who are honest in the lab and those who are not.

Although it is not possible to pinpoint the drivers of these results, it appears that only an interaction between individual preferences and contextual factors can account for the difference in cheating between the lab and the field.

Toghether, the findings of this study underline the importance to be very cautious when extending laboratory results on dishonesty outside a controlled environment.

CHAPTER 2

Benchmarking Information Aggregation in Experimental Markets

2.1 Introduction

One of the properties of efficient markets that economists have been most fascinated by is their ability to aggregate private information held by market participants which is revealed in prices. Sometimes markets are even created with the sole purpose of aggregating information. Such prediction markets have been shown to outperform opinion polls in predicting the outcome of elections (Forsythe et al., 1992; Berg et al., 2008), expert forecasts in sports (Spann and Skiera, 2009), or sales forecasts in business (Plott and Chen, 2002). In other contexts, however, like in financial markets, the evidence on successful information aggregation is more mixed. While early empirical literature found support for the efficient market hypothesis (Fama, 1965, 1970; Scholes, 1972), subsequent research produced opposite evidence (De Bondt and Thaler, 1985; Jegadeesh and Titman, 1993; Ito et al., 1998). These mixed results extend to lab experimental studies where some have found evidence of "good" information aggregation (Plott and Sunder, 1988; Forsythe and Lundholm, 1990; Forsythe et al., 1992; Camerer and Weigelt, 1991) and some evidence of substantial divergence between market prices and underlying fundamentals (O'Brien and Srivastava, 1991; Corgnet et al., 2018; Page and Siemroth, 2018; Corgnet et al., 2019). One difficulty in understanding how well markets aggregate information is that there is no natural alternative institution to which we can compare the market's performance. Our paper attempts to provide one such benchmark.

To this purpose we design a lab experiment where we randomly assign subjects to different artificial institutional environments.[1] In treatments with market interaction (the market treatments) two assets are in parallel traded via a call auction mechanism (Plott and

[1] Using a lab experiment allows us to determine exactly the relevant public and private information held by traders and to assess to what extent, all information is embodied in market prices. It also allows us to compare our results to existing evidence in directly comparable markets.

Smith, 2008a). In the non-market treatments we remove the strategic interaction among traders. Here, prices of assets are determined via a Becker-DeGroot-Marschak (hereafter BDM) mechanism (Becker et al., 1964). Both institutional environments are tested under two information conditions. Treatments with public information present no information aggregation problem, while in their counterparts information about asset returns is private. All treatments are designed in such a way that the information available to participants across the market and non-market variations is exactly identical. The only difference is how prices are determined.

We assess information aggregation using two measures. First, we ask whether first-order stochastic dominance of assets is reflected in the way assets are ranked by their prices. This is a minimal measure of correct aggregation. Second, we compare prices in the treatments with private information to prices in treatments that have public information about asset returns, but are otherwise identical. If information aggregation is perfect, then prices under the private and public information treatments should be the same. Further, any difference between the two institutions (market and non-market) that is *not* related to information aggregation should appear in the public information treatments as well. This differences-in-differences design hence allows us to cleanly identify differences in information aggregation across the two institutions.

The market and the BDM mechanism rank assets correctly with 95% and 93% probability, respectively, under public information and with 65% and 73% probability, respectively, under private information. Neither of these differences between institutions are statistically different. However, with respect to our second measure we find that markets do significantly worse compared to the non-market institution.[2] Prices are further away from the public information benchmark in the market compared to the BDM mechanism. Hence, across the two measures, we find that information aggregation is worse in the market than

[2]We do not find differences in asset prices between the market and the non-market institution in the case of public information, which is in line with Crockett et al. (2020) where behaviour is found to be invariant to prices being form exogenous or endogenous.

in our non-market benchmark.

We then explore several factors that could explain these results and find that the difference is driven by price-insensitive traders who seem unable to learn from market prices. Because of this we assume they perceive a wedge between their subjective beliefs and the market price, which they cannot rationalize by their priors.[3] As the fictitious market price (labelled "group value") is purely informational and not directly payoff relevant under the BDM mechanism, it seems intuitive that such traders would ignore it and simply follow their subjective beliefs. In the market by contrast, the price is harder to ignore (as it has to be paid) and the wedge between the price and the subjective prior would then lead participants to perceive ambiguity and to act accordingly. This is what worsens the market's performance in terms of information aggregation. We also show that, in contrast to price-insensitive traders, price-sensitive traders learn equally well in the market as they do under the BDM mechanism.

Our research contributes to a long tradition of experimental research on information aggregation in markets dating back to the 1980s. Plott and Sunder (1982) studied five experimental markets and found that in all but one prices promptly adjusted to near their rational-expectation values. Similar results are found in Friedman et al. (1984). Plott and Sunder (1988) studied the information aggregation properties of an oral double-auction where, in contrast to the previous literature, the state of nature is unknown to every trader. They found that in markets where only one asset is traded the rational expectations (RE) model performs poorly. In contrast, in markets with uniform dividends among traders or with a complete set of state-contingent assets, the RE model outperforms other competing models in predicting market prices. Recently, though, Corgnet et al. (2019) failed to replicate the results by Plott and Sunder (1988).

Forsythe and Lundholm (1990) investigated the role of trading experience and common

[3] While we do not directly elicit neither priors nor posteriors from traders and hence cannot prove this point, it seems very unlikely to us that a trader whose bids and asks do not react to the market price would be making inference on the return distribution from the price. As the price changes over time there must be a wedge between fixed priors and the changing price.

knowledge of the set of payoffs. They found that both conditions are jointly, but not separately, sufficient for prices to converge to the RE equilibrium. Similar results are found in Copeland and Friedman (1991) where traders receive information either sequentially or simultaneously in a computerized double-auction. While a model of partial revelation of information better predicts the allocation of assets in their study, market prices are consistent with the RE predictions. O'Brien and Srivastava (1991) analyze more complex markets with experienced traders and without common knowledge about the distribution of private information. In contrast to the results from simpler environments, it is found that markets are on average inefficient in aggregating all the available information.[4] In a meta-study on experimental double auctions Page and Siemroth (2018) find that while publicly announced information tends to be well reflected in prices, this is not the case for private information.

There are also some experimental studies on prediction markets. Healy et al. (2010) test the performance of double-auction prediction markets for different information structures. Although the double-auction market, when compared with other mechanisms, performs relatively well with a simple information structure, it performs the worst when the information structure becomes more complex. Ledyard et al. (2009) report that double-auction markets do not always generate more accurate predictions than other mechanisms (see also Hanson et al. (2006)). Page and Siemroth (2017) conduct a prediction market experiment with the possibility of information acquisition and conclude that bidders tendency to over-acquire information might be part of the explanation why prediction markets tend to aggregate information well.

The main difference between our work and existing literature is how market performance is assessed. Previous literature studied markets in isolation and contrasted outcomes to theoretical predictions. This approach has the downside that when theoretical

[4] Plott et al. (2003) study the ability of parimutuel betting systems in aggregating information under two specific environments. The difference lies on the "precision" of the private information hence, on the difficulty to learn the state of the world. While the simpler environment advocates for the RE equilibrium, in the more complex situation the most accurate model predicts that individuals decide according to their private information.

predictions and market outcomes differ, it is not clear whether this difference is due to the market failing to aggregate information or to the model using the "wrong" assumptions on (e.g. risk) preferences. Even if the difference between theory and empirical outcomes can be unambiguously attributed to an information aggregation failure, it is usually not possible to assess the extent of failure, as there are no natural benchmarks to assess whether a mis-pricing is "small" or "large". In our paper, by contrast, we benchmark information aggregation in markets against a comparable non-market institution. This approach allows us to net out the effect of market interaction and to obtain a benchmark against which to assess the quantitative importance of deviations from perfect aggregation.

To our knowledge there is only one previous paper comparing the Becker-DeGroot-Marschak mechanism with a market institution, albeit in a different context. Bohm et al. (1997) examine the sensitivity of the BDM mechanism to the choice of the upper bound of the randomly generated price and thus, its ability of eliciting reservation prices. They report that when the upper bound is close to an expected real maximum buying price, the BDM mechanism generates individual evaluations comparable to a double-auction market. The experimental market they use is, however, designed such that traders are unable to influence transaction prices. Unlike us and the literature cited above Bohm et al. (1997) are not interested in aggregation of private information in the market.

Our paper is organized as follows. In Section 2.2.1 we describe the experimental design. Section 2.3 contains our main results, Section 2.4 provides a discussion of mechanisms and Section 2.5 concludes. Experimental instructions, information about the sample as well as additional tables and figures can be found in Appendix B.

2.2 Experimental Design

2.2.1 Method

In all treatments of our experiment, groups of five participants trade two separate assets for three *repetitions* of ten *trading periods* each. Starting out with one unit of each asset at the beginning of every trading period, participants independently and simultaneously submit buying and selling prices – i.e., participants indicate for each asset the prices at which they are willing to sell their unit and they are willing to buy an additional unit. Both assets have a return of either 50, 100 or 150 and the probability distributions over these three outcomes are 3/5, 1/5 and 1/5 for one asset (asset *L*) and 1/5, 1/5 and 3/5 for the other one (asset *H*) respectively. Thus, asset *H* first-order stochastically dominates asset *L*. Having two assets allows us to focus on differences in how these two assets are valued on market level and to test whether they are correctly ranked according to the market price (i.e., whether the stochastic dominance relation is reflected in the ranking).

Our experiment consists of a $2 \times 2 \times 2$ between-subjects design, and each participant is exposed to only one of the eight different treatments as summarised in Table 2.1. Treatments differ according to (i) whether assets are traded in a market or not, (ii) whether there is public or private information, and (iii) whether feedback on individual bids and asks is provided via order books. We continue with a detailed description of these treatment dimensions and variations.

Information condition		*Public information*		*Private information*	
Institution		*BDM*	*Market*	*BDM*	*Market*
Bid–Ask	*without*	Pub–BDM–NoBAF	Pub–Mkt–NoBAF	Priv–BDM–NoBAF	Priv–Mkt–NoBAF
feedback	*with*	Pub–BDM–BAF	Pub–Mkt–BAF	Priv–BDM–BAF	Priv–Mkt–BAF

Table 2.1: Overview of treatments.

Institution (Market vs. BDM).

In order to isolate the effect of market incentives and analyse their implication on information aggregation, we implement two different institutional environments. Both have equal decision frameworks and information conditions. They only differ whether participants interact via a market mechanism.

In treatments with market interaction (*Market*), participants trade via a call auction mechanism (Plott and Smith, 2008b). Assets are traded every time some participant's buying price is above another participant's selling price. The market price of each asset is determined to allow all possible simultaneous trades of this asset and is made public after every trading period. If market clearing can be achieved with a range of prices, then the midpoint of this range is adopted as the market price (see Appendix B.1 for further details). In case trade is not feasible – that is, when the lowest selling price is above the highest buying price – every participant keeps her initial stocks endowment and no market price is determined.

In treatments without market interaction, participants buy and sell assets via a Becker-DeGroot-Marschak mechanism (*BDM*). Every trading period, transactions are determined according to a price that is a randomly drawn number from an uniform distribution between 50 and 150. Participants with a buying price above this number purchase a stock unit while those with a selling price below the random number sell their asset. Hence, in the *BDM* treatments individual trades do not depend on market prices that result from aggregated buying and selling prices. In order to make the two institutional environments comparable in the information made available to the participants, a simulated hypothetical market price (determined from bids and asks in the same way as the market price in the *Market* treatment), labelled *group value*, is communicated after each trading period. This treatment variation allows us to compare the behaviour induced by the double-auction market with an institution where strategic interactions are absent but which is informationally equivalent to the market setting.

Information condition (Public vs. Private information).

We further vary the information available to participants. In the *Public information* treatments, all participants are publicly informed about both assets' probability distributions over return values. Hence, there is no information aggregation problem. By contrast, in the *Private information* treatments, participants receive for each asset a *private signal* that provides a hint about the assets' probability distributions over return values. Signals are chosen in such a manner that perfect information (on which asset leads to higher expected returns) is available at the group level.

At the beginning of each repetition participants received two signals: one for each asset. In some repetitions the distribution of signals over participants was according to

$$\rho_1 = \{\,(150,50),(50,150),(100,50),(150,50),(150,100)\,\},$$

in other repetitions according to

$$\rho_2 = \{\,(150,50),(50,100),(100,150),(150,50),(150,50)\,\}.$$

For instance, if the set of signals was ρ_2, there would be three participants who would receive the signal $(150,50)$ (that is, signal 150 for asset H and signal 50 for asset L); one participant would receive the signal $(50,100)$ and one participant the signal $(100,150)$.

These two signal distributions were carefully designed in order to have some, but not all, participants start out with signals that are in agreement with the true ranking. They ensure that private information needs to be aggregated in order for the market to price correctly, but also that all information relevant to a correct pricing was available on market level. For instance, for the first asset (in this case asset H), three participants see the value 150, one the value 100 and one the value 50, which perfectly reflects the $(3/5,1/5,1/5)$ probability distribution.

Participants did not know the signal distributions, so that the information on their signals would not reveal any information on the signals that others received. Use of the two signal distributions was varied across repetitions and groups (see Table B.2 for details).

Comparing prices between the *Public information* and *Private information* variations allows us to cleanly identify differences in information aggregation across the institutions, as any difference between the two institutions that is *not* related to information aggregation should appear in the *Public information* treatments as well.[5]

Bid-Ask feedback (with or without).

Our last treatment variation concerns the feedback given to participants after each trading period. In the first variation (*without Bid-Ask feedback*), participants can only exploit their private information about asset prices in order to unveil the state of nature. This setting mirrors markets in which little information about other traders' choices and outcomes are provided and the only available information are the market prices. Under the second feedback variation (*with Bid-Ask feedback*), participants further observe other traders' bids and asks (after the trading period). This type of markets resembles more transparent markets where other traders' outcomes and information can be inferred from their behaviour. These two variations allow for comparisons of market and non-market settings in more and less information-rich environments. They also manipulate the salience of social comparisons, which is one potential channel through which markets could differentially affect bidding behaviour.

Procedures.

The experiments were conducted in the experimental laboratory at Maastricht University between March 2014 and February 2017.[6] In total, we recruited 320 undergraduate students to participate in the experiment using ORSEE (Greiner, 2015). Students were evenly allocated over treatments, such that we have 40 students participating in each of the eight treatments. Table B.3 provides basic randomisation checks and shows that treatments were balanced with respect to key variables.

For each treatment, we collected buying and asking prices over three repetitions of ten trading periods using z-Tree (Fischbacher, 2007b). Since the participants were operating in

[5]The impact of information being public or private is also addressed in some of the experimental common-value auction literature (Brocas et al., 2015; Grosskopf et al., 2018), though there are many differences between these and our settings.

[6]We conducted the private information sessions in 2014 and the public information sessions between late 2016 and early 2017.

fixed groups of five, this gives us eight independent observations per treatment. In order to avoid income effects and eliminating hedging opportunities between the two markets, final payments in the experiment were based on the earnings in one randomly chosen market in one randomly chosen trading period. Since participants were not given a cash budget during the trading phase, in the event that a trader made a loss on trade (resulting from buying an asset for a price that exceeded the drawn return value), this loss was covered by the show-up fee.[7]

In a post-experimental questionnaire, we elicited information on participants' characteristics and personalities (see Appendix B.2). A typical session lasted about two hours and average earnings were about 16.06 Euros, including a 5 Euros show-up fee.

2.2.2 Theoretical Predictions

Before we discuss the results we briefly describe the theoretical predictions concerning information aggregation properties of our setting. While the purpose of the experiment is *not* to test these predictions, it can be useful to have them in mind as a benchmark for how information aggregation might work in theory in this setting. It is well known that double auctions with sufficiently many buyers and sellers, who can bid using a sufficiently fine discrete set of prices, do have an equilibrium that is arbitrarily close to the fully revealing rational expectations equilibrium (Reny and Perry, 2006). Our setting does not quite fit this well studied case, but it is easy to show that also in our environment the state of nature is eventually revealed by the equilibrium price.

For the sake of exposition, let us denote the state space by $\Omega = \Omega_H \times \Omega_L$ with[8]

$$\Omega_i = \left\{ \left(\tfrac{1}{5},\tfrac{1}{5},\tfrac{3}{5}\right), \left(\tfrac{1}{5},\tfrac{3}{5},\tfrac{1}{5}\right), \left(\tfrac{3}{5},\tfrac{1}{5},\tfrac{1}{5}\right), \left(\tfrac{2}{5},\tfrac{2}{5},\tfrac{1}{5}\right), \left(\tfrac{2}{5},\tfrac{1}{5},\tfrac{2}{5}\right), \left(\tfrac{1}{5},\tfrac{2}{5},\tfrac{2}{5}\right) \right\},\; i = H, L.$$

[7] Hence, technically, the show-up served as an endowment; though, this was not explicitly presented as such to the participants.

[8] In Appendix B.3 we derive predictions for the case where agents consider a more general state space and in particular where they deem it possible that probabilities are defined on a grid finer than $\frac{1}{5}$.

Hence, consistent with the assets in our experiment, states are probability distributions over the three return values 50, 100 and 150.

We focus on the *Market* treatment with *Private information* and without bid-ask feedback for signal distribution ρ_1. Assume agents are risk-neutral and have prior beliefs uniformly distributed on all the states contained in Ω.[9] It is straightforward to show (Appendix B.3) that the agent with signal $(50, 150)$ will have posterior beliefs that imply an expected value of 95 for asset H and 105 for asset L, with these values reversed for the two agents with signal $(150, 50)$. The agent with signal $(100, 50)$ will have an expected value for asset H of 100 and an expected value for asset L of 95 and the agents with signal $(150, 100)$ will have an expected value of 105 and 100 for asset H and L respectively. In the first trading period the ordered bids for asset H will be $(105, 105, 105, 100, 95)$ and the ordered asks will be $(95, 100, 105, 105, 105)$, which means that asset H will trade at a price of 105 (below its expected value of 120). Analogously, asset L will trade at a price of 95, above its expected value of 80. From these prices agents recognize that at least three agents have received a signal of 150 for asset H and a signal of 50 for asset L. Thus all private information will be revealed already in the first period. Under these theoretical assumptions, hence, information aggregation is relatively straightforward in our setting and prices should reflect all private information early on in the experiment.[10] We consider a more general setting in Appendix B.3.1 and a case where traders are strategic in Appendix B.3.2.

2.3 Main Results

The main results presented in this section are focused on comparing how well markets aggregate information compared to an institution which shares all the same features in terms of outcomes and information flows, except for the fact that trade is not bilateral (our

[9] Note that in the experiment we did not provide a prior to participants.

[10] Under the more general setting presented in Appendix B.3, we find that more than one period of trading is needed to reveal all private information, but that one period is sufficient for all agents to learn to rank the assets correctly. Further, note that this results is not particular to the chosen signal distribution ρ_1, and would hold for all signal distributions (including ρ_2) that reflect the exact probability distribution over the asset outcomes.

BDM treatments).

Figure 2.1 shows the average market prices over time for both assets and both institutional environments under the two information treatments. Both assets are on average priced below their expected values (120 and 80 for asset *H* and *L* respectively) in the *Public information* treatment where there is no information aggregation problem, and there does not seem to be a substantial difference between *BDM* and *Market* in this information condition. Under *Private information* prices differ from their *Public information* counterparts in both treatments. Note first that in both the *BDM* and the *Market* treatment, the prices of the two assets move closer together than when returns are public information. This is intuitive, since under *Private information* there is ambiguity regarding the identity of the high and low value assets.

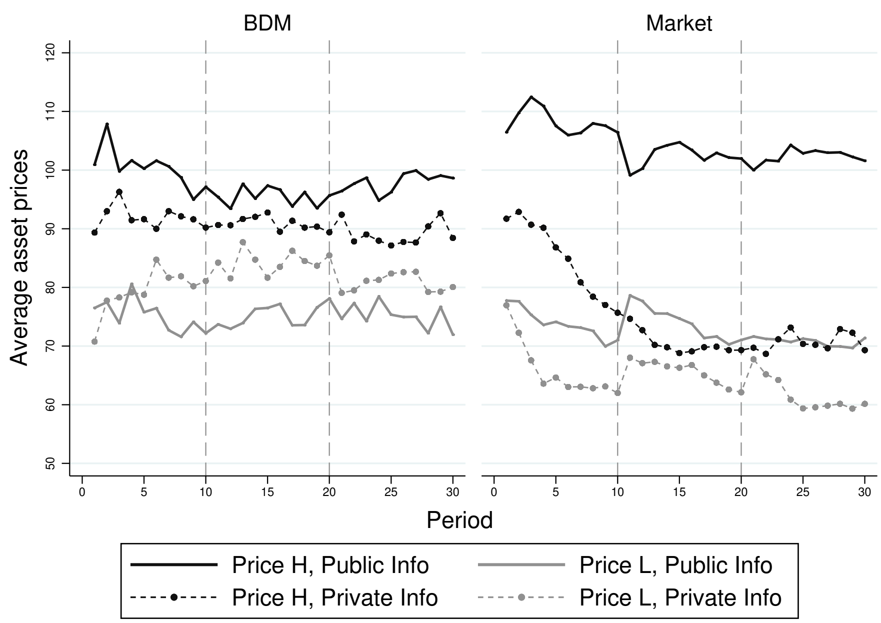


Figure 2.1: Average prices of both assets by institution.
Note: Treatments are pooled with respect to the bid-ask feedback dimension. All observations from all markets and groups are included as long as trade was feasible and a market price (group value) determined. The solid and dashed lines represent stock prices in *Public information* and *Private information* treatments respectively. The left panel shows fictitious prices under the *BDM* mechanism (*group value*) and the right panel *Market* prices.

More specifically, in the *BDM* treatments asset *H* is underpriced compared to the public information case, while the opposite holds for asset *L*. In the *Market* treatments both assets are substantially undervalued and the difference to the public information case seems bigger than it is in the *BDM* treatments. Note also that prices don't start lower in the *Market* treatments compared to the *BDM*. Hence these lower prices are learned. This suggests that market interaction is detrimental for information aggregation. We will now investigate this possibility more formally.

In our statistical analysis, we will use two measures of information aggregation. The first measure (*Correct Ranking*) examines whether assets H and L are ranked correctly by market prices. This is a weak measure of information aggregation simply asking whether the stochastic dominance relation between assets is correctly reflected by how their market prices are ranked. Our second measure (*Perfect Aggregation*) is more ambitious and compares, for each institution, asset prices in the *Private information* treatments with their counterparts in the *Public information* treatments where there is no information aggregation problem. If stock prices under private information are the same as under public information, then all private information is revealed in the price. If markets successfully aggregate information, we should find no differences between the public information and private information conditions using either of these measures. As in Figure 2.1 we pool data from the variations with and without bid/ask feedback. Appendices B.4 and B.5 contain tables and figures where we split them out.

Correct Ranking.

We start with the less demanding measure of information aggregation (Correct Ranking), which asks how frequently the price for asset H exceeds the price for asset L. Table 2.2 shows results from LPM and Probit estimates of the probability that assets are correctly ranked depending on our treatment dimensions:

$$Pr(p_H > p_L)_{it} = \alpha + \beta \; Private\ info_i + \gamma \; Market_i + \delta \; Private\ info_i \times Market_i + X_{it} + \varepsilon_{it}, \tag{2.1}$$

where $Pr(p_H > p_L)_{it}$ is the probability that the market price of asset H exceeds that of asset L in group i in period t, *Private info* is a dummy for the treatments with private information and *Market* is a dummy variable for the *Market* treatments. X_{it} represents other covariates such as the signal distribution (ρ_1) or the repetition.

	LPM			Probit (dy/dx)		
Prob(Price H > Price L)	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.933***	0.933***	0.910***			
	(0.025)	(0.025)	(0.034)			
Private info (β)	−0.204***	−0.237***	−0.236***	−0.236***	−0.256***	−0.258***
	(0.048)	(0.067)	(0.066)	(0.063)	(0.067)	(0.066)
Market (γ)	0.018	0.017	0.023	0.086	0.086	0.093
	(0.046)	(0.046)	(0.045)	(0.086)	(0.086)	(0.085)
Private info × Market (δ)	−0.101	−0.099	−0.101	−0.136	−0.133	−0.135
	(0.082)	(0.117)	(0.117)	(0.100)	(0.113)	(0.112)
ρ_1		0.069	0.073		0.048	0.061
		(0.106)	(0.101)		(0.074)	(0.066)
ρ_1 × Market		−0.007	−0.015		−0.008	−0.021
		(0.162)	(0.159)		(0.110)	(0.105)
Repetition 1			0.101**			0.103**
			(0.045)			(0.047)
Repetition 2			−0.035			−0.032
			(0.049)			(0.044)
$\beta+\delta$	−0.305	−0.336	−0.337	−0.372	−0.389	−0.393
p-value test $\beta+\delta=0$	0.000	0.001	0.000	0.000	0.000	0.000
p-value test $\|\beta+\delta\| \leq \|\beta\|$	0.109	0.200	0.194	0.088	0.119	0.113
$\gamma+\delta$	−0.084	−0.081	−0.078	−0.050	−0.047	−0.042
p-value test $\gamma+\delta=0$	0.219	0.451	0.468	0.322	0.512	0.554
Observations	1572	1572	1572	1572	1572	1572

Table 2.2: Correct ranking.
Note: LPM (columns (1)-(3)) and probit (columns (4)-(6)) estimates of equation (2.1). Robust standard errors (clustered at the group level) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The smaller number of observations is due to the fact that in some rounds at most one asset is traded, such that a price is not properly specified for at least one of the assets.

The *Market* and the *BDM* mechanism rank assets correctly with 95% and 93% probability, respectively, under public information and with 65% and 73% probability, respectively, under private information. We find no significant difference between the two institutions in the likelihood of ranking assets correctly under *Public information*: the coefficient γ is small and not statistically significant in any of the specifications. This result persists also under *Private information*. While in both the *BDM* and the *Market* private information decreases the probability to rank correctly – the coefficients β and $\beta+\delta$ are significantly negative ($p<0.01$) –, we find no difference between the two institutions according to this criterion: the coefficient $\gamma+\delta$, although negative, is never significantly different from zero ($p>0.219$). This result is robust when we control for additional covariates (columns (2), (3), (5) and (6)) and is true under both bid-ask feedback variations (see Table B.4).

Result 1 (Correct Ranking): There is no significant difference between the *Market* and the *BDM* mechanism in terms of the likelihood that assets are ranked correctly.

Perfect Aggregation.

Next, we turn to the more ambitious measure to examine information aggregation. We estimate for each asset the following model:

$$MP_{it} = \alpha + \beta \ Private\ info_i + \gamma \ Market_i + \delta \ Private\ info_i \times Market_i + X_{it} + \varepsilon_{it}, \qquad (2.2)$$

where MP_{it} is the market price in group i in period t, and other variables as introduced earlier. Table 2.3 reports the results.

	Price asset H			Price asset L		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	98.328***	98.328***	96.472***	75.834***	75.833***	74.419***
	(2.676)	(2.677)	(2.824)	(1.514)	(1.515)	(1.580)
Private info (β)	−8.130**	−9.558***	−9.556***	5.634**	5.641*	5.638*
	(3.346)	(3.684)	(3.550)	(2.809)	(3.108)	(2.976)
Market (γ)	6.369	6.369	6.407	−2.355	−2.356	−2.389
	(4.292)	(4.295)	(4.268)	(2.450)	(2.451)	(2.434)
Private info × Market (δ)	−21.359***	−20.683***	−20.727***	−14.738***	−14.615***	−14.605***
	(5.576)	(5.878)	(5.720)	(4.227)	(4.782)	(4.730)
ρ_1		2.783	3.013		−0.010	−0.052
		(2.641)	(2.457)		(2.050)	(1.853)
$\rho_1 \times$ Market		−1.293	−1.581		−0.246	−0.158
		(4.274)	(3.986)		(2.941)	(2.746)
Repetition 1			5.999***			1.549
			(1.771)			(1.162)
Repetition 2			−0.246			2.719**
			(1.225)			(1.061)
$\beta+\delta$	−29.489	−30.241	−30.283	−9.104	−8.974	−8.967
p-value test $\beta+\delta=0$	0.000	0.000	0.000	0.004	0.014	0.015
p-value test $\lvert\beta+\delta\rvert \leq \lvert\beta\rvert$	0.000	0.000	0.000	0.206	0.243	0.241
$\gamma+\delta$	−14.990	−14.314	−14.319	−17.093	−16.971	−16.993
p-value test $\gamma+\delta=0$	0.000	0.000	0.000	0.000	0.000	0.000
Observations	1761	1761	1761	1653	1653	1653

Table 2.3: Perfect aggregation.
Note: GLS regression of equation (2.2). Robust standard errors (clustered at the group level) in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

In the absence of an aggregation problem (*Public information* treatments), prices of both assets are below their expected values. The intercept, representing *BDM* treatments under *Public information*, in columns (1) and (4) is around 98 and 76 for assets H and

L respectively. The coefficient γ, which measures the impact of market interactions under public information, is never statistically different from zero at any conventional significance level. This implies that we find no statistical difference, in terms of asset prices, between the two institutions when there is no aggregation problem.

Turning the analysis to the *Private information* treatments, neither the *BDM* nor the *Market* aggregate information perfectly. The coefficients β and $\beta+\delta$, representing the effect of private information in the *BDM* and *Market* institutions, respectively, are both significantly different from zero in all specifications. Prices under private information are, hence, different from prices under public information. As illustrated in Figure 2.1, while private information induces a decrease in prices for asset *H* and an increase for asset *L* in *BDM* (β), in the *Market* treatments both assets are undervalued: the coefficient $\beta+\delta$ is significantly negative in all specifications ($p < 0.05$).

When we compare the effect of *Private information* between the two institutions, we find that in *Market* treatments the price for asset *H* presents larger negative departures from its *Public information* counterpart – i.e., we do reject the null hypothesis of $|\beta+\delta| \leq |\beta|$. For asset *L* we cannot reject this hypothesis ($p > 0.2$). All these results are robust when controlling for additional covariates (columns (2), (3), (5) and (6)) and are true for both bid-ask feedback conditions (see Table B.5).

Result 2 (Perfect Aggregation): Under private information prices for asset *H* are further away from the public information benchmark in the *Market* compared to the *BDM* mechanism.

Does the worse performance in terms of perfect aggregation in the market decrease or increase prices compared to the *BDM*? The coefficient $\gamma+\delta$ measures the impact of market incentives under *Private information*. We find that market interaction significantly decreases prices of both assets when compared to the *BDM* ($p < 0.01$) but this effect is not there when there is no information aggregation problem (γ). Hence, there is no difference between the prices in the *Market* and *BDM* treatments in the absence of an information aggregation problem.[11] With private information, however, we find different results. Markets as an institution do *not* perform better than the *BDM* in aggregating information. While there is no significant difference on the likelihood of ranking assets correctly, prices in the market differ more strongly from the public information counterparts compared to the *BDM*. In some cases markets lead to considerable mis-pricing.

2.4 Discussion and Additional Results

In this section we discuss possible mechanisms leading to Results 1 and 2. The first thing to notice is that in the absence of an information aggregation problem, i.e. in the *Public information* treatments, there is no difference between the *Market* and *BDM* institutions neither in terms of correct ranking nor in terms of average prices (Tables 2.2 and 2.3). This suggests that mechanisms where markets affect preferences or beliefs *per se* are not likely to be the main driver of our results. In other words any explanation of the differences identified above must be directly or indirectly linked to the informational structure. In the following we discuss several such potential mechanisms.

2.4.1 Learning: Price-sensitive and -insensitive Traders

In this section we outline our main explanation for why prices for asset *H* are further away from the public information benchmark in the *Market* compared to the *BDM* with private information (Result 2). We should first emphasize that our explanation does not rely on fundamentally different preferences, nor on different strategies. Such explanations are inconsistent with our experimental evidence as we will demonstrate in Section 2.4.2. Instead we start from the observation, demonstrated in this section, that the difference between the two institutions is driven by price-insensitive traders, who are apparently not able to learn

[11] This differs from the results obtained in Bohm et al. (1997)'s artificial market, suggesting that the design of experimental markets is important.

from the market price.[12] We then argue that participants who are not able to learn from the price are more likely to ignore it and follow their subjective prior in the *BDM* where the price is purely informational, but does not actually have to be paid. By contrast, in the *Market* they are more likely to perceive ambiguity, which – in the presence of ambiguity aversion – will lead to lower bids as we formally demonstrate in Appendix B.3.3.

We now outline this explanation in more detail. To empirically classify traders into price-sensitive and -insensitive we follow the methodology by Asparouhova et al. (2015) who classify participants based on the slope in an OLS regression where period-by-period changes in asset holdings are regressed on the difference between the actual asset price in the experiment and the expected value of the asset using correct (updated) probabilities. A negative slope means participants decrease their asset holdings when the asset is overpriced, i.e. they are price-sensitive. A zero slope indicates price-insensitivity and a positive slope indicates what Asparouhova et al. (2015) call "perverse" price-sensitivity, i.e. participants increasing their holdings of overpriced assets. Following Asparouhova et al. (2015) we use cutoffs for the t-statistic of -1.6 and 1.9 to indicate price sensitivity in either direction and classify participants as price-insensitive whose t-statistic falls in between these cutoff values.[13]

This procedure classifies a total of 85 participants (53%) in *Market* treatments as price-insensitive to both assets (39 in *Public information* and 46 in *Private information*); the remaining 75 participants (47%) are price-sensitive to at least one asset (41 in *Public information* and 34 in *Private information*). This is in line with Asparouhova et al. (2015) who find that 69 participants (58%) are price-sensitive and 51 (42%) are price-insensitive. In the *BDM* treatments there are 59 price-sensitive and 101 price-insensitive traders (respectively, 35 and 24 in *Public information* and 45 and 56 in *Private information*). Given

[12]In theory it could also be that they are price-insensitive because they have very special preferences. Any such explanation is essentially ruled out by the evidence from our public information treatments, see Section 2.3.

[13]The reason Asparouhova et al. (2015) use asymmetric cutoffs is a well known simultaneous-equation bias in estimating price-sensitivity. Because total changes in holdings of assets must balance out across participants, slope coefficients must sum to zero and OLS estimates will be biased upwards.

the large fraction of price-insensitive traders it is important to understand their role in the information aggregation process.

	Asset H				Asset L			
	Sensitive to H		Not sensitive		Sensitive to L		Not sensitive	
	Ask	Bid	Ask	Bid	Ask	Bid	Ask	Bid
Constant	111.895***	86.141***	113.250***	83.974***	85.981***	69.879***	88.243***	64.348***
	(5.261)	(5.048)	(3.011)	(2.481)	(5.060)	(3.662)	(1.742)	(1.292)
Private info (β)	−14.722**	−14.839***	−6.168*	−6.423**	15.477**	4.169	4.206	4.070**
	(6.618)	(5.180)	(3.714)	(3.244)	(6.472)	(6.081)	(2.723)	(1.919)
Market (γ)	−9.070	0.353	5.501	13.467***	−5.263	−7.933*	−8.087***	2.357
	(7.068)	(7.050)	(3.976)	(3.927)	(6.245)	(4.196)	(2.346)	(1.936)
Private info × Market (δ)	5.833	−7.797	−25.357***	−25.134***	−12.635	−6.051	−9.671**	−11.952***
	(9.292)	(7.958)	(5.787)	(4.899)	(8.865)	(6.769)	(4.387)	(2.738)
ρ_1	4.085	3.999*	2.745	1.755	3.598	1.905	−2.843	−1.988*
	(3.783)	(2.337)	(2.229)	(1.430)	(3.855)	(2.496)	(2.116)	(1.121)
Repetition 1	13.354***	7.196***	5.691***	0.270	2.720	−1.557	5.558***	0.153
	(2.898)	(2.420)	(1.618)	(1.459)	(3.345)	(2.122)	(1.207)	(0.652)
Repetition 2	0.487	1.421	−1.219	−1.336*	1.114	−0.679	3.408***	1.565**
	(2.586)	(1.927)	(1.056)	(0.798)	(2.990)	(2.115)	(1.219)	(0.744)
$\beta+\delta$	−8.889	−22.635	−31.525	−31.557	2.842	−1.882	−5.464	−7.881
p-value test $\beta+\delta=0$	0.229	0.000	0.000	0.000	0.680	0.632	0.195	0.001
p-value test $\lvert\beta+\delta\rvert \leq \lvert\beta\rvert$	0.735	0.164	0.000	0.000	0.923	0.617	0.411	0.118
$\gamma+\delta$	−3.237	−7.443	−19.856	−11.667	−17.897	−13.985	−17.758	−9.594
p-value test $\gamma+\delta=0$	0.591	0.044	0.000	0.000	0.004	0.008	0.000	0.000
Observations	2310	2310	7080	7080	2130	2130	7290	7290

Table 2.4: Bids and asks of price-sensitive and price-insensitive traders for both assets.
Note 1: For asset H the table uses only participants who are price sensitive for asset H. Those are 42 participants for asset H. Hence the "insensitive" category here includes (i) the 75 participants who are price-insensitive to both assets, but also (ii) 43 participants who are price-insensitive to H, but price-sensitive to L. Analogously for asset L.
Note 2: Data of 7 (6) participants are missing for asset H (L), since it was not possible to classify their sensitivity for the respective asset due to either lack of variability in their behaviour/holdings or market prices were missing.
Note 3: Clustered standard errors at the group level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 2.4 shows the results of regressions comparing bids and asks of price-sensitive and price-insensitive traders across the different settings.[14] The table shows that for asset H information aggregation is not perfect for either type of trader and either type of institution with both coefficients β and $\beta+\delta$ significantly different from zero.[15] When it comes to our differences-in-differences analysis comparing public and private information in BDM and the market, we find that there is no statistically significant difference in the public-private information gap in bids and asks for price-sensitive traders, while there is a substantial

[14]Reported results do not change if we would classify participants as price-sensitive or price-insensitive based on their behaviour in Repetition 1, and restrict the regression to their bids and asks in Repetitions 2 and 3.

[15]Note that it is entirely plausible that some traders who are classified as "price-sensitive" do perceive some amount of ambiguity and act accordingly.

gap for price-insensitive traders, which is highly statistically significant. For asset H, in particular, the coefficient δ is sizeable and the hypothesis $|\beta+\delta| \leq |\beta|$ summarily rejected for price-insensitive traders. All these results are robust when controlling for additional covariates. The treatment difference we observed in Section 2.3, and in particular the massive drop in prices for this asset seen in Figure 2.1, seems driven by price-insensitive traders. For asset L price-sensitive traders' bids do not differ significantly between the public and private information cases with particularly the value of $\beta+\delta$ being very close to zero. For price-insensitive traders there are statistically significant differences between public and private information also for asset L and also here we reject the hypothesis $|\beta+\delta| \leq |\beta|$.[16]

Why do price-insensitive traders behave differently across the two institutions? As the price (*group value*) is not directly payoff relevant for participants in the *BDM* condition (it affects payoffs only via beliefs), it seems intuitive that traders in the *BDM* who do not learn from the price decide to ignore it, i.e. use their subjective priors to determine their bids and asks. This is also in line with the pattern of roughly constant prices over time seen in Figure 2.1. In the market, by contrast, it is harder for participants to ignore the price they have to pay for an asset. Hence, here, when confronted with the dissonance between subjective priors and the price, it seems intuitive that traders who do not learn correctly from the price do not ignore it, but instead perceive ambiguity. In Appendix B.3.3 we show that under weak assumptions agents who perceive ambiguity will place lower bids for an asset than those who do not.[17] This would explain the downward trend in prices in the *Market* treatment particularly for asset H (see Figure 2.1).

There is a considerable and diverse body of literature broadly showing that ambiguity (typically generated exogenously) might affect market outcomes, though the design as well

[16]Appendix Figure B.2 shows a simulated price path for price-sensitive traders only. The figure shows that for these traders the difference between prices under public and private information is much smaller compared to the full sample.

[17]Essentially these assumptions are that agents who perceive ambiguity entertain at least one posterior that implies a worse expected asset return than the correct posterior of an agent who does not perceive ambiguity.

as the direction and the size of the effects differs across studies with some finding negative effects on prices and some finding no effects (Sarin and Weber, 1993; Bossaerts et al., 2010; Corgnet et al., 2013; Huber et al., 2014; Ngangoué, 2017).[18] In our data we find that asks/bids for both assets are shifted upwards/downwards for price-insensitive traders (see Table 2.4) in line with ambiguity aversion. In the next section we consider other possible mechanisms.

2.4.2 Other Potential Mechanisms

In this subsection we discuss other potential mechanisms and provide evidence why we believe that they are unlikely to be a key driver of our results. We first study differences in preferences, then differences in strategic behaviour, and last differences in cognitive strain between the two environments. It is important to note that we are not denying that differences in preferences or strategic behaviour may exist. We argue, however, that they are not the main underlying reason behind the results discussed in Section 2.3.

Differences in Preferences

If the differences observed across institutions would be driven by different preferences over outcomes across the two institutions, then these differences should also be observed within the *Public information* treatments. Table 2.3 shows that – if such differences exist – they are not translated into market prices. We can hence rule out explanations based on simple differences between preferences over outcomes.

Still, preferences (even if the same across environments) might play a role in other ways. One notable difference between the *Market* treatments and the *BDM* treatments is that, because in the *Market* treatment assets are traded, risks between traders are negatively correlated in the sense that a favourable realization for a traded asset is benefiting the agent who bought the asset while it is an implicit loss for the agent who sold the asset. If agents

[18] Theoretically, in the presence of ambiguity-averse agents equilibrium prices may fail to reflect all the available information (Caskey, 2009; Condie and Ganguli, 2017). See Epstein and Schneider (2010) and Guidolin and Rinaldi (2013) for a review of the theoretical literature.

care about social comparisons this negative correlation could affect their behaviour. More formally, sensitivity to such implicit losses can be captured by a model of reference dependent preferences (Kőszegi and Rabin, 2006) incorporating a social comparison reference point (Schmidt et al., 2015).

Consider an agent i facing a lottery with K outcomes x_k and associated probabilities p_k ($k = 1, \ldots, K$), and a lottery with L (social comparison) reference points r_ℓ ($\ell = 1, \ldots, L$) with $q_{k\ell}$ being the probability distribution over pairs (x_k, r_ℓ). In the state (x_k, r_ℓ), agent i receives outcome x_k while his reference point is r_ℓ. We define the agent's utility V on the domain of outcomes x and reference points r,

$$V(x,r) = \eta \sum_k p_k u(x_k) + \psi \sum_{k,\ell} q_{k\ell} v(u(x_k) - u(r_\ell)).$$

The first term is the expected (consumption) utility of the gamble (asset) held, weighted by factor η. The parameter ψ in the second term controls the sensitivity to social comparison. We assume that $v(0) = 0$ and $v' > 0$. In order to understand how social comparison affects trading prices, it suffices to consider a simple swap between assets H and L between agent i and j, taking the other's payoff as 'reference point'. We show in Appendix B.3.4 that the more sensitive agent i is to social comparison (everything else equal), the more reluctant is she to swap assets when she is owning the H asset. That is, the more sensitive she is to social comparison, the more valuable asset H is relative to asset L. Hence with social comparison sensitive traders, the difference between the (hypothetical) prices of assets H and L is expected to increase. For our experiment this means that price differences should be larger in *Market* versus *BDM*. Comparing the mean price difference between *Market* (20.26) and *BDM* (14.09) we do indeed find evidence in line with the former prediction (Mann-Whitney U test, $p < 0.001$).

To the extent that bid-ask feedback facilitates social comparisons we should also see a difference between prices across these variations. However, within the *Market* treatments we do not find a significant effect of bid-ask feedback on mean price differences (NoBAF: 20.43, BAF: 20.08; Mann-Whitney U test: $p = 0.596$). Figure B.1 splits Figure 2.1 by the bid-ask feedback dimension and illustrates that such feedback seems to make little difference, and the regressions reported in Table B.5 confirm this.[19] Hence, despite the fact that adding social comparison information should make the type of considerations discussed above more salient we do not find significant treatment differences. Based on this evidence it seems unlikely individuals' sensitivity to social comparisons is a key driver of our results.

Differences in Strategic Behaviour

A second class of alternative explanations we discuss are based on differences in strategies. As we do not see price differences with public information, any explanation based on differences in strategic behaviour needs to rely on some assumption as to why strategies are different enough to cause price differences with private but not public information.

How could strategic behavior affect prices, though? Note first that in the *BDM*, subjects have no influence on the price they pay or receive for the asset. If traders are risk neutral this will lead (as we show in Appendix B.3.1) to an initial price in the interval $[103.1, 112.5)$ for asset H and in the interval $(87.5, 96.9]$ for asset L. Further, for each asset, three units will be traded (hypothetically given bids and asks). Unlike in the *BDM*, in the call market traders can influence the price at which they are buying or selling: buyers like to lower the price and sellers like to increase the price. Subjects in the market, therefore, have an incentive to shade their bids and asks by bidding a bit lower and asking a bit more than the expected value. If we assume participants shade their bids and asks, but not by "too much", this will lead (as we show in Appendix B.3.2) to a price in the interval $[114.0625, 118.75)$ for asset H and in the interval $(93.75, 98.4375]$ for asset L. For both assets, two units will

[19] For the situation without (with) bid-ask feedback, the differences between the market and the BDM are in Table B.5 captured by γ (τ) for public information and by $\gamma + \delta$ (ϕ) for private information. For both situations, there is no significant difference with public information and are for both assets prices lower in the market with private information. Hence, there is no sign of the results reported in Section 2.3 being driven by one of the bid-ask feedback conditions only.

be traded.

Based on this, strategic behaviour should lead to higher prices for asset H in the market relative to the *BDM*, and fewer units to be traded. Table 2.3 shows our findings in this regard. In the public information setting we find, consistent with this, a slightly higher price for asset H, but a slightly lower price for asset L; however, both difference are not statistical significant (see coefficient β). For the private information setting we find for both assets significantly lower prices in the market (see coefficient $\gamma + \delta$), which does not point to strategic behaviour being a dominant factor. In terms of units traded we find that actually fewer units are traded under the *BDM* compared to the market treatment (see Table B.6).[20] With strategic behaviour we would expect the opposite result. Taken together these pieces of evidence suggest to us that it is unlikely that strategic trading causes the difference between the *BDM* and market treatment.

Differences in Cognitive Strain

One difference between the *BDM* setting and the *Market* setting is that, while in the *BDM* the price (*group value*) has a purely informational role, in the *Market* it also enters participants' payoff calculations directly. This double role of the price is one way in which differences in cognitive strain between the two treatments could come about. Similarly, there could be differences in cognitive strain between the treatments differing in bid-ask feedback, since in the treatments with bid-ask feedback there is more information available to process for participants. There are several factors which suggest to us that differences in cognitive strain are not a key driver of our results. First, note that there is no difference between the *BDM* and *Market* settings in the public information case (see coefficient γ in Tables 2 and 3). Hence if differences in cognitive strain are behind the differences observed with private information, they must come from an interaction between the informational role of the price (which is identical) and the price mechanism. Second, we can

[20] Of course under the *BDM* no units are actually traded at all. In Table B.6 we compare the amount of units that would be traded hypothetically under the *BDM* given bids and asks with those actually traded in the market.

compare noise levels across the two institutions. If cognitive strain is higher under the *Market* condition, this could be reflected in higher levels of noise in the *Market* compared to the *BDM*. We do not find evidence for this in the data. In fact, the coefficient of variation ($\frac{\sigma}{\mu}$ ratio) for market prices is, if at all, higher in the *BDM* compared to the *Market* (0.32 vs. 0.30 for asset H and 0.39 vs. 0.27 for asset L). Third, for those participants who are price-sensitive there is no difference between the two institutions, suggesting that if cognitive strain matters, it matters only for some participants. Fourth, we do not see a difference between the treatments with bid-ask feedback and those without, which should arguably also differ in terms of cognitive strain. While none of these elements by itself constitutes proof, their combination suggests to us that differences in cognitive strain are not of key importance in driving our results.

2.5 Conclusions

We conducted a lab experiment to study information aggregation in markets. The innovative aspect of our work is that we assess the quality of information aggregation in the market relative to a comparable non-market institution. To this purpose participants in our lab experiment are randomly assigned to different institutional environments. In treatments with market interaction (the market treatments) assets are traded via a call auction mechanism. In the non-market treatments prices of assets are determined via a Becker-DeGroot-Marschak (BDM) mechanism. Treatments are designed in such a way that the information available to participants across the market and non-market variations is exactly identical. The only difference is how prices are determined.

We find that markets do worse compared to the non-market institution. In particular prices are further away from the public information benchmark in the market compared to the BDM mechanism. The difference is driven by price-insensitive traders who seem unable to learn from market prices. Because of this, they perceive a wedge between their subjective prior and the market price, which they cannot rationalize. This leads them to perceive ambiguity in the market which then affects their bids and asks. Price-sensitive traders, by contrast, learn equally well in the market as they do under the BDM mechanism. They seem, hence, able to extract information from the price despite the noise generated by price-insensitive traders.

There are two obvious directions for future research. First, the robustness of our findings could be studied more extensively including for settings with more traders and more assets with positively correlated risk. While we have documented robustness across some information conditions, more could be done in this direction as well, both in terms of feedback structures and initial information available (including asymmetries with informed and uninformed agents). Second, while our treatments and explorative analysis have suggested one possible mechanism (and ruled out some others), more research needs to be done to understand the role of price-insensitive traders and how they learn across different settings.

CHAPTER 3

The Causal Effect of Income Inequality on Attribution and Social Trust.

3.1 Introduction

Economic inequality is on the rise in many countries across the globe, even as global poverty rates reached all-time lows in 2019 (Morris and Western, 1999; Alvaredo et al., 2013; Atkinson, 2015; Gould, 2017). 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 (Stiglitz, 2002; Pickett and Wilkinson, 2010; Currie, 2011; Atkinson, 2015). 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 is 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 (Jost et al., 2004; Bullock, 2008). 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. Thus, economic status is seen as deserved and therefore not subject to recrimination or correction by state intervention.

Meritocratic beliefs offer up a set of well-worn attributions for wealth and poverty to assuage negative psychological states. For the advantaged, meritocratic beliefs can resolve potential feelings of guilt when exposed to inequality (Jost and Hunyady, 2003; Bullock, 2008). 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 and Nisbett, 1991).[1] For the disadvantaged, a belief in meritocracy is a psychological road-map for success — namely working harder — to the exclusion of other avenues such as collective action, as each individual is seen as responsible for their inability to improve their own situation. Routine experiences of failure, often the result of systemic injustice and the psychological weight of poverty, can lead to passivity and hence an inability to learn that providing effort is effective (Seligman, 1972).[2] In sum, 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.

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 and Major, 2007) and policy preferences, especially support for redistribution (Hasenfeld and Rafferty, 1989; Gilens, 1999; Fong, 2001; Alesina and Ferrara, 2005; Benabou and Tirole, 2006). 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).

Inequality has also been associated with declines in generalized social trust (Alesina

[1]Higher relative socio-economic position can additionally increase inclination to blame if it is linked to entitlement. Brooks et al. (2018) show, for example, that high-caste men in India are more likely to retaliate than low-caste men after what they perceive as a "slight".

[2]A great deal of medical literature as well as that from health psychology shows that even in developed societies the poor show worse mental health outcomes and a higher morbidity of a variety of conditions, including heart disease, and that much of this can be attributed to stress (Muramatsu, 2003; Mitchell and Popham, 2008; Buttrick and Oishi, 2017). In development economics there is an active research agenda studying the relation between poverty and cognition, quality of decision-making and worker productivity (Mani et al., 2013; Haushofer and Fehr, 2014; Kaur et al., 2019).

and Ferrara, 2002; Uslaner, 2002; Delhey and Newton, 2005; Bjornskov, 2008). (Putnam, 2000) claims that "in virtually all societies have-nots are less trusting than haves". 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 (Uslaner, 2002). Social trust is widely seen as a massively important factor in social and economic interactions (Uslaner, 2018).

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 observational and examines the impact of the Gini coefficient and other contextual indicators on public attitudes (Kelly and Enns, 2010; Newman et al., 2015). 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. Additionally, it is impossible to vary the causal variable of inequality in natural settings and difficult to do it in a lab setting without deception or conflating inequality with relative position. The goal of the present study is to provide strong *causal* evidence for the relationship between inequality, the attributional processes of meritocratic belief and blame, and social trust while 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, for the first time, to measure belief in meritocracy in an incentivized task.

We focus on three key outcomes contributing to the broader concept of belief in a just world: (i) belief in meritocracy, (ii) inclination to blame and (iii) social trust. 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, which we operationalized as a prime prior to the incentivized tasks. The prime compared participant data to local economic context in two boroughs in England, one a great deal wealthier than the other, such that 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. Our results show a causal impact of inequality on belief in meritocracy. 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. We do not find a statistically significant effect of inequality exposure on blame measured using a design by Gurdal et al. (2013). In line with Putnam (2000)'s argument 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 even a subtle positional prime can have 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, though we find little evidence for a hypothesized increase in likelihood to blame others. As both trust 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. They also provide one additional reason for the need to go beyond equality of opportunities and to focus on reducing inequality of outcomes. Inequality may lead not only to negative institutional aspects but also a general psychological fraying and loss of faith, at least among large portions of society.

The paper is organized as follows. In Section 3.2 we discuss related literature across different subfields in Social Sciences. Section 3.3 discusses evidence from general social surveys. In Section 3.4 we present our experimental and survey designs. Sections 3.5 and 3.6 contain our main results on attribution and social trust, respectively. Section 3.7 concludes. Appendix C contains details and materials from the experiments as well as additional tables and figures.

3.2 Literature

Being neither the strongest nor most biologically well-adapted to their environments, human beings have required social cooperation to survive. Yet cooperation, particularly over the long-term is dependent on being able to trust that our fellow humans will cooperate in return. Interpersonal trust is seen as a "social lubricant", and "an important factor in economic and social exchange" (Galeotti et al., 2017; Uslaner, 2018). Norms of trust are closely related to norms of fairness: we cooperate with others, but when we do so successfully, we also expect to be rewarded fairly in proportion as a "result of skill plus effort" (Galeotti et al., 2017). Such meritocratic beliefs are closely related to the psychological concepts of personal efficacy, agency, and locus of control; our ability to control our own lives, the events around us, and to improve our circumstances over the long haul. At a broader scale, they feed into our perceptions of distributional justice in the world around us - a ‘belief in a just world’.

There is a large literature in social psychology on how people perceive the world around them (Ross and Nisbett, 1991; Furnham, 2003; Jost et al., 2004). 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 and are not seeking to harm others 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. Indeed personal BJW has been linked to interpersonal trust (Zuckerman and Gerbasi, 1977; Bgue, 2002) and to work ethic and is sometimes thought of as reflecting variance in the extent that people are motivated to justify the economic and social system as fair and legitimate (Jost et al., 2004; Jost and Hunyady, 2005).[3] BJW can serve a palliative function if people overestimate to which extent their successes are due to merit rather than luck (Langer, 1975; Davidai and Gilovich, 2016).[4]

Political Scientists and Economists have mostly been interested in economic manifestations of just world beliefs, such as belief in meritocracy (Newman et al., 2015; Newman, 2016; Alesina et al., 2018; Mo and Conn, 2018; Wolak and 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 and Peterson,

[3] Personal BJW is often measured by psychologist by using agreement to statements like “I am convinced that in the long run, people will be compensated for injustices” (general BJW) or “I believe that, by and large, I deserve what happens to me” (personal BJW). See Furnham (2003) for a survey discussing these and other measures of BJW.

[4] 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.

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 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 (Xu and Garand, 2010; Trump, 2017). There is further evidence that people's perceptions of inequality and other economic indicators are politically malleable (Kuziemko et al., 2015; Bartels, 2016; Alesina et al., 2018). 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 and Pettigrew, 2015).

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 and Ferrara, 2002; Uslaner, 2002; Delhey and Newton, 2005; Bjornskov, 2008) and the theoretical mechanisms involved are still subject to debate (Gustavsson and 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.

Our study contributes to this literature in three ways. First, we provide *causal* evidence for a link between inequality, attribution and social trust. Second, we are able to disentangle the effect of inequality exposure from the effect of personal relative position.[5] Third, we measure the main outcomes both using standard survey measures as well as in an in-

[5] There is research in psychology showing that rank of income matters more than absolute income in determining happiness and life satisfaction (Boyce et al., 2010) and that there is an interaction between inequality and effects of income on life satisfaction (Quispe-Torreblanca et al., 2020).

centivized way in the lab and we introduce a novel incentivized task to measure belief in meritocracy.

Behavioural Economists have studied various 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 et al., 1996; van Dijk et al., 2002; Ostrom et al., 1994; Sadrieh and Verbon, 2006; Reuben and Riedl, 2013). Gaechter et al. (2017) found a negative impact of endogenous inequality (created over time by differing past contributions) on contributions. Nishi et al. (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.[6] A number of authors have related inequality and trust within a lab experiment (Holm and Danielson, 2005; Xiao and Bicchieri, 2010). Greiner et al. (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.

To our knowledge there are no lab experimental studies measuring belief in meritocracy. Possibly this is in part due to the absence of incentivized measures of belief in meritocracy prior to our research. Some researchers study attitudes towards fairness and meritocracy by allowing people to redistribute earnings in online experiments (Mollerstrom et al., 2015; Almas et al., 2019). 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 uncontrol-

[6]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* (Piff et al., 2012; Horvath et al., 2012; Trautmann et al., 2013; Cote et al., 2015; Korndoerfer et al., 2015; Smeets et al., 2015; Andreoni et al., 2017; Schmukle et al., 2019) 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 C.4 where we document a positive impact of relative position and a negative impact of inequality per se on pro-social behaviour in our data.

lable 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.

There is some literature on how inequality exposure and relative position impacts policy preferences (Hasenfeld and Rafferty, 1989; Gilens, 1999; Fong, 2001; Alesina and Ferrara, 2005; Benabou and Tirole, 2006). Karadja et al. (2017) find that most people believe they are poorer than they actually are and that when informed of their true relative position, individuals who are richer than they initially thought demand less redistribution. Fehr et al. (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 et al., 2020; Roth and Wohlfarth, 2018; Magni, 2020). Several authors suggest that the effect of inequality might operate via respondents' fairness views (Karadja et al., 2017; Fehr et al., 2019; Roth and Wohlfarth, 2018). By establishing a causal link between personal relative position and belief in meritocracy our research provides support for such a mechanism.

3.3 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 C.1 contains some summary statistics for this sample.

3.3.1 Belief in Meritocracy

Next Steps 8 contains three questions that are often used to measure belief in meritocracy[7]

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 HH income of less than 25K, 25-45K and greater than 45K. Those are the same cutoffs as 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).[8]

Table 3.1 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

[7]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.

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

	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 3.1: Belief in Meritocracy dummy regressed on income categories, local Gini coefficient and interactions.

Note: 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.

for those in the middle and higher income categories.[9]

3.3.2 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 3.2 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 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

[9]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.

contains the same question. As Appendix Table C.12 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.

	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 $\times$ med income		-0.874			-0.816	0.0255
		(1.889)			(1.891)	(2.513)
Gini $\times$ 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 3.2: Social Trust in Next Steps 8 survey (“0=not at all agree”,...,“10=extremely strongly agree”).
Note: 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.

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.[10] 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

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

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.

3.4 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 primes, the outcomes, the sample and the correlation among our main outcomes.

3.4.1 Design and Procedures

The lab experiment consists of a $2 \times 2 \times 2$ between subjects design where we vary two dimensions across sessions and one dimension within sessions. Our first treatment dimension concerns the type of prime. There are two types of primes. 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 when the prime took place. In treatments **MTB** ("**M**erit, **T**rust, **B**lame") the prime occurs right before we elicit belief in meritocracy to maximize the potential effect of the prime on that task, whereas in **BMT** it occurs right before the blame task, again to maximize the potential effect on that task. Changing the order of tasks allows us to have - for each of these outcomes - one treatment that cleanly measures the impact of the prime on that outcome, while at the same time allowing us to study the cross-correlation among the different outcomes.[11] Figure 3.1 illustrates the timing of the different tasks in the lab experiment.[12] Our third treatment dimension takes places within sessions where participants are randomly assigned to either a “low” or “high” prime.

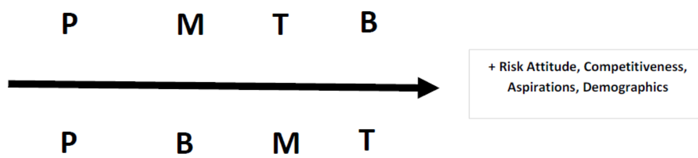


Figure 3.1: Timeline of the experiment. Treatments differ in order of the **M**erit task, Social **T**rust task and the **B**lame task. In each treatment we also elicited risk attitude, competitiveness, demographic covariates and survey based measures of aspirations.

Table 3.3 shows the number of participants in each of the four treatments. 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. 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 3.3. 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 a subset of participants in each session. Table 3.3 also shows the number of respondents across six different online experiments we fielded that will be discussed in more detail below. In each online experiment we measured only one outcome.

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

[11] Note that for the Social Trust outcome we do not have a treatment where the prime occurs directly before the relevant task. This could have two undesirable consequences. First, a possible causal effect might not be detectable as it is diluted by the longer time that passes between the prime and the task. Second, any observed effect might not be causal but instead be triggered by the differential effect the prime 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 need to address the second concern and will do so in Section 3.4.3.

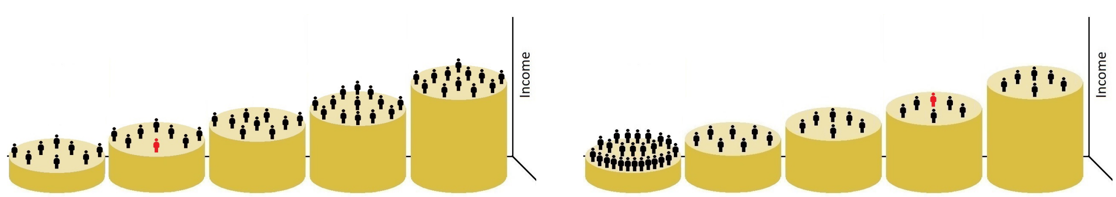
[12] Appendix Table C.13 shows the time elapsed between the prime and the start of the task.

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

Table 3.3: Number of participants in different treatments and online surveys.
Note: In the lab treatments **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.

3.4.1.1 Priming

To study the causal effect of inequality exposure and personal relative position we prime participants using income distributions of differing degrees of inequality. 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') household spends on eating out every week and (viii) the type of school (comprehensive, grammar, private, boarding) they attended. Appendix C.2.2 shows the exact questions and answer categories for all of these questions.



(a) (b)

Figure 3.2: (a) Downward Prime (b) Upward Prime.The pictures shows the downward and upward prime used 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 C.7 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 prime participants either downward or upward regarding their position in terms of relative position using images like the ones shown in Figure 3.2. 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 prime and in the three rightmost bars of distribution (b) used for the upward prime. 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 3.2. The UK boroughs the two distributions represent are Chelsea and Kensington (mean annual income 178K GBP) and Norwich (mean annual income 26K GBP).[13] Upward and downward primes 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 that own position is not highlighted by a red person (see Appendix Figure C.7). 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 representations of an income distribution (including e.g. a representation using quintiles). We chose the representations shown in Figure 3.2 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

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

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 primed to either distribution. Appendix C.1 contains details about both of these surveys.

3.4.1.2 Outcomes

After priming participants we elicited the following incentivized outcomes in the lab: (i) belief in meritocracy, (ii) social trust and (iii) inclination to blame. 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 and 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.[14] 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 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 $\mathscr{F}(x)$ 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.[15] We should emphasize that the weight of

[14] 18 GBP is the maximum payment that can be obtained for the effort/ability rank guesses.

[15] As there is no natural cutoff for what it means to explain overall rank well, we decided to use a relative measure, i.e. focus on those who have high BIM *compared to others* in the experiment. We use a dummy as this is the standard way to measure belief in meritocracy in surveys (Newman et al., 2015).

the three components (ability, effort and luck) in determining the score (and hence also R^i) is known to all participants. We hence think of BIM^i *not* as an evaluation of this specific task, but rather as a general attitude or mental state.

"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.[16] 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.[17] While trust is often measured by economists using trust games (Berg et al., 1995), we are interested in capturing the aspect of social trust that is most closely related to "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 et al., 2007).

"Attribution of Blame"

To elicit attribution of blame we use a task previously used by Gurdal et al. (2013). 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

[16] 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.)

[17] 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.

$(1-p)$. 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).[18]

Other measures

We also elicited a measure of risk aversion and a measure of competitiveness that we will use as control variables in our regressions. See Appendix C.2 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 C.2 for details. In Appendix C.4 we discuss how some of these outcomes are affected by positional primes.

Measures in Online Experiments

In our online experiments we use the same measures used in Next Steps 8 to measure belief in meritocracy and social trust (see Section 3.3). As there is no established survey measure of blame we use a hypothetical Gurdal et al. (2013) task. Unlike the measures elicited in the lab experiment, the measures elicited in the online experiment are not incentivized.

[18]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.

Other Details

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.

3.4.2 Sample Characteristics

Table 3.4 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 3.4 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 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.

Appendix C.3 contains balancing tests where we compare those receiving a "high" and those receiving a "low" prime based on these and other characteristics. Out of 68

Type of Prime Outcomes	Lab Experiments **REL MTB**	**INEQ MTB**	**REL BMT**	**INEQ BMT**	Online Experiments **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 3.4: Summary Statistics for participant characteristics across different treatments of the lab and online experiments.

comparisons 4 are significant at the 5% level. The balancing tests reveal no statistically significant differences between the two groups in almost all treatments. In **INEQ-MTB** those receiving a "high" prime had provided more effort in the counting task and had been more often lucky in the coin toss. As a result they also had a higher score.

3.4.3 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 3.5: Correlation among outcome measures.

In this section we 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.[19] Appendix Table C.14 shows that these correlations also appear in a regression where other covariates are controlled for.

3.5 Results: Attribution

In this section we present our results on belief in meritocracy and blame.

3.5.1 Belief in Meritocracy

We start by discussing some descriptives and covariates of belief in meritocracy in our lab experiment (Section 3.5.1.1). We then discuss the causal effect of inequality exposure and relative position on belief in meritocracy (Section 3.5.1.2) before discussing some alternative mechanism and additional results (Section 3.5.1.3). In Section 3.5.1.4 we discuss the results from our online experiments.

3.5.1.1 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.[20] 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 C.9 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 ex-

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

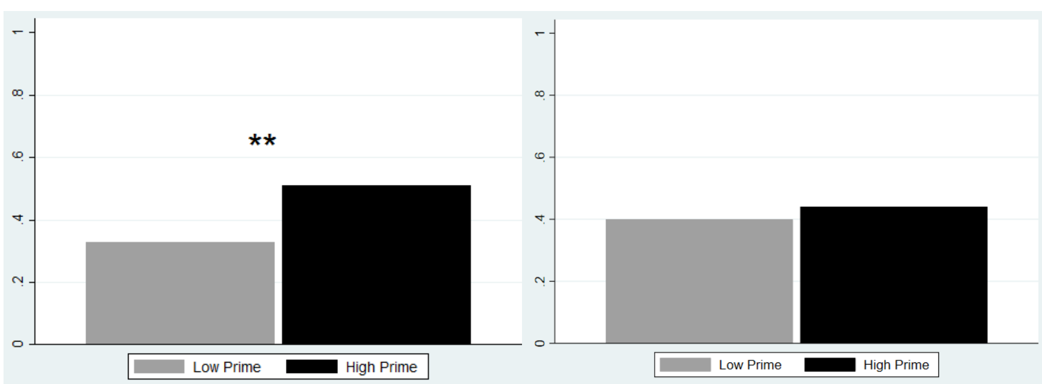
[20]The reason that fewer than 50 percent of participants are classified as high belief in meritocracy is that our definition requires them to be strictly below the 50th percentile. Our treatment effects are robust to slight changes in this cutoff.

periment. 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 C.14 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 C.8 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.

3.5.1.2 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 primed to a high relative position should increase belief in meritocracy, while being primed to higher inequality should decrease belief in meritocracy.



(a) (b)

Figure 3.3: (a) REL-MTB, (b) INEQ-MTB. Belief in Meritocracy by whether participants were primed with high relative position (Panel (a)) and by whether they were primed with the income distribution only (Panel (b)). Stars are from t-test based on regression in Tables 3.6 and 3.7. *** p<0.01, ** p<0.05, * p<0.1

Figure 3.3 shows the share of participants who have high belief in meritocracy ($BIM =$ 1, see Section 3.4.1.2) depending on whether they were primed with a low or high relative position (**REL-MTB**, Panel (a)) and depending on whether they were primed to low or high inequality using only the distribution (**INEQ-MTB**, Panel (b)). The figure shows that among those primed to a high relative position a substantially bigger share display high belief in meritocracy than among those primed 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 prime (β)	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 3.6: Belief in Meritocracy (*BIM* dummy) in treatment **REL-MTB**.
Note: 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 3.6 shows regression analysis for treatment **REL-MTB** where we regress *BIM* on a dummy indicating whether the participant was primed to a high relative position as well as two income fixed effects. The coefficient of the prime 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 primed 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)).

3.5.1.2.1 Heterogeneity and Persistence of the Effect

We consider a number of sample splits using always regressions analogous to specification (1) in Table 3.6. We find that the effect of the prime is particularly strong for those in the highest income category ($\beta = 0.365^{***}$) and for those who self-identify as upper class ($\beta = 0.692^{**}$).[21] The prime 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 the prime 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 prime does no longer have an effect.

	Belief in Meritocracy (BIM)					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	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 3.7: Belief in Meritocracy (*BIM* dummy) in treatment **INEQ-MTB**.
Note 1: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 3.7 shows regression analysis for treatment **INEQ-MTB** where participants were only primed by the income distribution and were *not* shown their personal position in the distribution. In this case the prime seems to have little effect. The coefficient β is sub-

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

stantially 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.

3.5.1.3 Alternative Mechanisms

We start by discussing two possible alternative mechanisms which might drive the causal effect of the prime identified in treatment **REL-MTB**. Specifically, as we introduce a novel measure of belief in meritocracy, we first ask whether the prime 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 primed 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 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.

We would like to know whether the prime affects confidence defined in this way. Table 3.8 reproduces Table 3.6 using `confidence` as outcome instead of *BIM*. The table shows that there is no effect of the prime on confidence. The coefficient β is small, changing in sign and not statistically significant. It also has the "wrong" sign in five out of six specifications suggesting that being primed to a higher relative position would lower confidence. Being primed to a high relative position hence does not seem to make people more confident on their task performance.

	Confidence					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	-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 3.8: Confidence in treatment **REL-MTB**.
Note: 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.

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 $\frac{\hat{R}_A^i+\hat{R}_E^i}{2}$ and regress it on the same exogenous variables as in Table 3.6.

	Anchoring					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	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 3.9: Anchoring treatment **REL-MTB**.
Note: 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.

Table 3.9 shows the results. There is no statistically significant effect of the prime on the average guessed rank. Further, the sign of the coefficient β is not in line with the anchoring story, as it would imply that those being primed 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 primed 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.

3.5.1.4 Online Experiment: Belief in Meritocracy

In this subsection we ask whether the same prime 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 prime as in the lab and the same outcomes (measures of belief in meritocracy) as in the Next Steps 8 survey (see Section 3.3). 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 C.1 compares the characteristics of the different samples: our lab sample, our online sample and the Next Steps 8 sample.

We then redo the analysis presented in Tables 3.6 and 3.7 but this time using the survey measures of belief in meritocracy as outcome variable. Table 3.10 shows the results for the surveys where people are primed to their relative position. The table again shows a clear effect of the prime on belief in meritocracy in this case. Participants primed to a high relative position are 53% more likely to express high belief in meritocracy. By contrast if participants are only primed using the inequality prime there is no discernible effect (Table 3.11). Hence we obtain very similar results using the established survey based measure of

	Belief in Meritocracy: Survey Measure					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	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 3.10: Belief in Meritocracy using survey measures and the **REL** prime.
Note: 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.

	Belief in Meritocracy: Survey measure					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	-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 3.11: Belief in Meritocracy using survey measures and the **INEQ** prime.
Note: 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.

belief in meritocracy as we do with our novel incentivized measure in the lab.

The Inequality Prime

We have seen that the inequality prime 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.[22] 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 C.2). We again prime 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 3.5.1.4 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 3.4 shows the share of respondents for who the belief in meritocracy dummy takes the value 1 as a function of how unequal the society they were primed 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

[22] In Next Steps 8 the average effect of "Gini" is -0.69^{***} in specification (1) of Table 3.1.

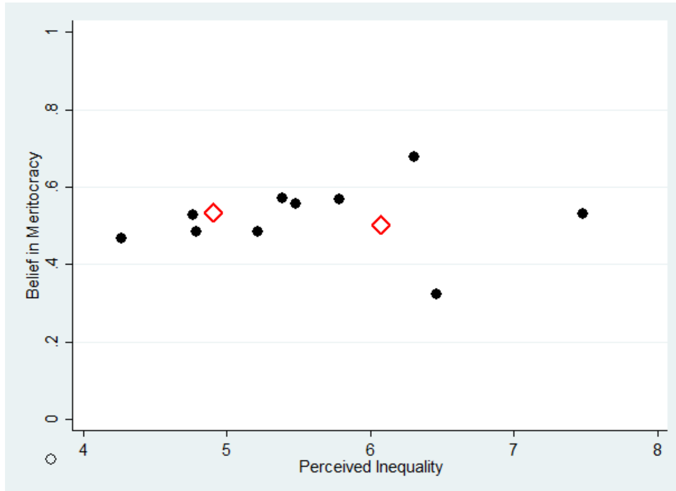


Figure 3.4: Belief in Meritocracy depending on how unequal society is perceived (on a scale from 0,...,10) by respondents. The red diamonds indicate the two distributions used in the lab experiment and in the online survey discussed in Section 3.5.1.4.

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 priming techniques do not work well enough to capture the impact of inequality *per se* (distinct from personal relative position) as it is a more abstract and arguably less ego-relevant measure.[23] 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*.

[23]The inequality prime does affect social trust (see Section 3.6), though.

3.5.2 Blame

We now study inclination to blame. As before we first discuss some descriptives and co-variates of inclination to blame (Section 3.5.2.1) and then move to the causal effect of inequality and relative position on inclination to blame (Section 3.5.2.2).

3.5.2.1 Descriptives and Covariates of 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. Appendix Figure C.10 shows the cumulative distribution of blame. The figure shows that 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 ≈ 1.2 US-dollars or ≈ 0.9 GBP. As there is no natural conversion of these monetary amounts into "levels of blame" we standardize these values (to mean zero and standard deviation of one) for the remainder of this section. Appendix Table C.14 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. We now study the causal effect of inequality exposure and relative position on inclination to blame.

3.5.2.2 The Causal Effect of Inequality Exposure and Relative Position

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 et al., 2018; Magni, 2020). Panel (a) in Figure 3.3 shows indeed somewhat higher inclination to blame for those primed to a higher relative position, but the difference is not statistically significant.

	Blame					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	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 3.12: Blame Treatment **REL-BMT**.
Note: 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.

Table 3.12 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 R2 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.[24]

Table 3.13 shows the results for the inequality prime. The table shows that there is no statistically significant effect of the inequality prime on blame. The coefficient β is very close to zero but also not very precisely estimated.

[24]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. 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.

	Blame					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	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 3.13: Blame Treatment **INEQ-BMT**.
Note: 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.

3.5.2.3 Online Experiment: Blame

As with belief in meritocracy we also collected a non-incentivized survey measure of inclination to blame. We described the experimental scenario to participants in the online survey and then asked them to make hypothetical choices. The income questionnaire and primes used were again identical to those used 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. More details on the survey procedures and sample characteristics can be found in Appendix C.1 and C.3. Appendix Tables C.17 and C.18 show the results. They again show no statistically significant effect of the prime on inclination to blame, though we do again see a positive coefficient when participants are primed to relative position and a positive correlational effect of income on inclination to blame.

3.6 Results: Social Trust

We now study social trust. As before we first discuss some descriptives and covariates of social trust (3.6.1) and then move to the causal effect of inequality and relative position on social trust (Section 3.6.2). In Section 3.6.3 we discuss the results from our online experiment on social trust.

3.6.1 Descriptives and Covariates of Social Trust

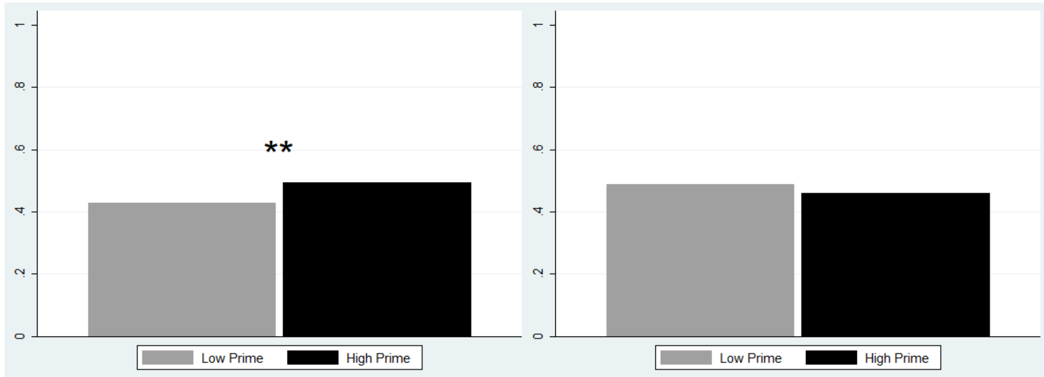
On average participants in both the **REL** and **INEQ** treatments believe that dictators will share 46% of the pie with others. There are no statistically significant differences neither by age, income nor gender. Upper class participants seem to have lower social trust compared to middle and working class participants.[25] 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 C.14).

3.6.2 The Causal Effect of Inequality Exposure and Relative Position

We now ask whether being primed to a high relative position or a high degree of inequality affects social trust. If the survey evidence discussed in Section 3.3 can be interpreted as causal, then we would expect a positive effect of relative position on social trust.

Panel (a) in Figure 3.5 shows that social trust indeed increases on average when people are primed 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 3.14 shows regression results for the **REL** treatments. Participants who are primed to a high relative position display about 15 percent higher levels of social trust compared to those primed to a low relative position. They expect dictators to share around 49 percent of the pie while those primed to a low relative position expect

[25]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.



(a) (b)

Figure 3.5: (a) REL, (b) INEQ. Social Trust by whether participants were primed with high relative position (Panel (a)) and by whether they were primed with the income distribution only (Panel (b)). Stars are from t-test based on regression in Table 3.14. *** p<0.01, ** p<0.05, * p<0.1

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 3.15 shows the effect of the inequality prime. Those primed 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.

			Social Trust			
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	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 3.14: Social trust **REL** treatments.
Note: 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 prime (β)	-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 3.15: Social trust **INEQ** treatments.
Note: 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.

3.6.2.0.1 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 prime 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)).

3.6.3 Online Experiment: Social Trust

We also used the same income questionnaire and primes 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 C.3 compares sample characteristics of participants in Next Steps 8, the lab and the online experiment.

	Social Trust: Survey Measure					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	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 3.16: Survey measure of social trust **REL** treatments.
Note: 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 prime (β)	-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 3.17: Survey measure of social trust **INEQ** treatments.
Note: 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.

Table 3.16 shows the results for the **REL** prime. Those primed to a higher relative position express around 10% higher levels of social trust using the survey measure than those primed to a lower relative position. The effect size corresponds to about three standard deviations. The table also shows a substantial correlational effect of income with those with higher income displaying higher levels of social trust. Table 3.17 shows the results for the **INEQ** prime. Respondents primed 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.

3.7 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. 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.

Those results have important implications for our understanding of the medium and long run impacts of inequality. They 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. 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.

Appendix A

Appendix for "Individual Cheating in the Lab: A New Measure and External Validity"

A.1 Experimental Instructions

This section provides the experimental instructions for both the laboratory and the field.

A.1.1 General instructions

Welcome!

You are about to take part in a decision-making experiment. It is important that you do not talk to any of the other participants during the experiment. If you have a question at any time, raise your hand and an assistant will come to your desk to answer it. This experiment consists of five different parts and you will play each of them only once. You will receive detailed instructions for each part on your computer screen as the experiment progresses. In each part you will be asked to make one or more decisions. Decisions made in one part of the experiment will bear no consequences for the other parts of the experiment. During the experiment your earnings will be calculated in pounds and you will have the chance to earn an amount of money that can range from 5 to 26. At the end of the main experiment you will have to complete a brief questionnaire. At the end of the experiment the computer will randomly select two parts for each participant. The sum of the earnings in these two selected parts will constitute your payment for this experiment. In addition to this money we will pay you 4 for showing up today and 1 for completing the questionnaire. Your cash earnings will not be immediately paid. Instead, payments will be issued within few days (from 23rd to 30th of November). You will receive further instructions about the payment procedure at the end of the experiment. If you have a question now, please raise your hand and a lab assistant will come to your workstation.

A.1.2 Instructions for the *list game*

You are about to play an easy game. In the next screen you will see a list of **60 colour names** (e.g. tamarind). Once the list appears, a countdown of **5 seconds** will start. This list will be displayed until the countdown reaches zero. Before the list disappears, you will have to choose one of the colour names in the list and keep it in your mind. Then, three random lists containing 4 colour names each (for a total of **12 colours names**) will appear. If the colour you have in mind is in one of the lists, you will win the amount of money associated to that list, otherwise 0. After you click the OK button the first list containing 60 colour names will be shown and the 5 seconds timer will start. Choose a colour in your mind before the timer reaches zero. Click the OK button to start.

A.1.3 Instructions for the dice game

In this part you will have to roll a fair die with 5 sides. Every side corresponds to a colour. Hence, every colour has probability of 1/5 to come up. This means that, in expectation, out of 100 rolls every colour will come up 20 times. Before rolling the die, you have to choose a colour in your mind from the ones displayed below. If the outcome of the roll is the same colour you though of, you will earn 5, otherwise 0.

A.1.4 Instructions for dictator game

In this part the computer will randomly pair you with another participant. You will remain paired with this person for the whole duration of this part. Once the decisions are made, the pair will be dissolved. You, as well as the person you are paired with, will never learn the identity of each other. In this part, both you and the participant you are paired with, will have to split the same amount of money among you. Each of you simultaneously decides the amount to transfer to the other participant. Hence, the decision of one subject is not observable by the other participant. The computer will then choose with equal probability which one of the two actions will be implemented. Your earnings from this part correspond to the money that you keep for yourself (in case your choice is implemented) or to the money the other participant decides to transfer to you (if his/her choice is implemented).

A.1.5 Instructions for the lottery choice

In this part you will have to choose between five options. You will be paid based on which option you choose. Each option involves a simple lottery with two possible outcomes that are equally likely to occur. Hence, every lottery will return each of the two numbers with 50% probability.

A.1.6 Instructions for the trust game

In this part the computer will randomly pair you with another participant. You will remain paired with this person for the whole duration of this part. Once the decisons are made, the pair will be dissolved. You, as well as the person you are paired with, will never learn the identity of each other. There are two types of player in this part, a sender and a receiver. You will play **both roles**: at first as a sender and then as a receiver. Each person will be allocated with the same amount of X. Firstly, each of you will simultaneously decides as if you were the sender. As a sender you will have the opportunity to send some of the X to the other person (receiver). Each pound sent to the receiver will be tripled. Thus, if the sender sends **x**, the other player will receive **3x**. Then, without observing the choice of the other sender, you will be asked to choose as if you were the receiver. You will have to decide how much money to send back to the sender for any possible amount of money that you can receive. Once the decisions are made, the computer will choose with equal probability which member of the pair is the sender and who is the receiver, implementing the corresponding choices. The earnings of the sender from this part will correspond to the amount of the endowment of X he/she keeps for his/herself plus the money returned by the receiver. The earnings of the receiver from this part will correspond to the endowment of X, plus three times the transfer from the sender, minus the money returned to the sender.

A.1.7 Documents

A.1.7.1 Consent forms

Informed Consent for Study at ESSEXLab

Please tick the appropriate boxes	**Yes**	**No**
1. Taking part in the study		
I have read and understood the study information, or it has been read to me. I have been able to ask questions about the study and my questions have been answered to my satisfaction.	☐	☐
I consent voluntarily to be a participant in this study and understand that I can refuse to answer questions and I can withdraw from the study at any time, without having to give a reason.	☐	☐
I understand that the study will take approximately 60 minutes and that I am free to leave if I no longer wish to participate.	☐	☐
I understand that for my participation I will receive at least 5£. Any additional money will depend on my answers and the ones from others.	☐	☐
I understand that the payment will not be issued immediately after the experiment	☐	☐
I understand that, in order to collect my payments, I need to come to campus two times between the Day1 and the Day2 of Month Year.	☐	☐
2. Use of the information in the study		
I understand that information I provide will be used for academic research purposes.	☐	☐
I understand that personal information collected about me that can identify me, such as my name, will not be shared beyond the study team.	☐	☐
3. Future use and reuse of the information by others		
I give permission for the anonymized data that I provide to be deposited in the principal investigator's personal computer so it can be used for future research and learning.	☐	☐

4. Signatures

______________________ ______________________ ______________________
Name of participant [IN CAPITALS] Signature Date

I have accurately read out the information sheet to the potential participant and, to the best of my ability, ensured that the participant understands to what they are freely consenting.

_ANDREA ALBERTAZZI______________ ______________________ ______________________
Name of researcher [IN CAPITALS] Signature Date

5. Study contact details for further information

Andrea Albertazzi, Dept. of Economics, University of Essex, email: aalber@essex.ac.uk

INFORMED CONSENT FORM PRODUCED BY UK DATA SERVICE

University of Essex

ESSEXLab

Consent form

The purpose of this study is to get an indication on how people take decisions. In this study, you will be asked to complete different tasks. Your participation in this study will take about one hour. If you have any questions about the study, they will be answered for you.

For your participation in the study, you will receive a £4 show up payment, £1 for completing a short survey plus additional earnings depending on your choices. Your experimental earnings will be paid in cash but not immediately the end of the session. Instead, **payments will be issued between the 23rd and the 30th of November**. You will receive further instructions about the payment procedure at the end of the experiment.

Your participation in this study is purely voluntary, and you may withdraw your participation or your data at any time without any penalty to you.

Your data will be kept completely confidential by the researcher. Your personal information will not be stored with the data. Your responses will be stored in a computer database and used anonymously only for research purposes.

If you have any questions, you can contact: _aalber@essex.ac.uk_

I have read the description of this study, my questions have been answered, and I give my consent to participate.

Participant Signature: …………………………………………………………………

Name (printed):…………………………………………………………………

Date:……………………………

Signature of Individual Obtaining Consent: ……………………………………………………

Name (printed): …………………………………………………………………

Date: ……………………………

Figure A.1: Consent forms for the *NoFtF* (left) and *FtF* (right) treatments. Subjects completed and handed the forms before entering the lab. The change in the format is due to a change in EssexLab policy happened between the two field treatments.

A.1.7.2 Payment forms

ESSEXLab

Payment form

This experiment will involve more than 100 participants and the experimental earnings can range from a minimum of £5 to a maximum of £26 (i.e., every integer number between 5 and 26 is possible).

In order to increase your anonymity, payments will not be provided in the lab. Instead, the payment procedure has been modified in the following stages:

Stage 1
While leaving the lab you have to draw one key. Every key corresponds to a locker. Lockers are located at ground floor of the LTB building (to the left after the main entrance).

Stage 2
From 9:00 of Tuesday 25^{th} to 18:00 of Wednesday 26^{th} leave this form, already filled in (date and earnings), in the locker with the number corresponding to the key that you have drawn. Keep the key with you.

Stage 3
After the above dates, this form will be collected by the experimenter and cash, corresponding to the reported total earnings at the bottom of this form, will be left into the locker.

Stage 4
From 9:00 of Thursday 27^{th} to 17:00 of Saturday 29^{th}:
You can collect your payment from the locker with your key. After collection, please close the locker and **leave the keys in the letterbox** that will be installed just next to the lockers.

Note: Please follow the payment procedure carefully. Remember to **fill in this form** otherwise it would not be possible to issue the payment. Please **do not lose the key** and remember to **leave it into the letterbox**.

In case you have any question please contact: aalber@essex.ac.uk

Date (stage 2):...............

Total earnings: £...............

ESSEXLab

Payment form

This experiment will involve more than 100 participants and the experimental earnings can range from a minimum of £5 to a maximum of £26 (i.e., every integer number between 5 and 26 is possible).

In order to increase your anonymity, the payments will be issued only when all observations will be collected. This will take about four business days and for this reason your payment will not be immediately issued.

You can collect your experimental earnings bringing this form filled with all the relevant information (date and earnings) to:

Office: 5B.149 (Department of Economics)

Dates: from 23^{rd} to 30^{th} of November

Hours: 9:00-12:00 and 14:00-17:00

Your payment will be immediately issued in cash upon this form is handed to the experimenter.

Note: Failure of providing this form will result in a payment of the £4 show-up fee only.

In case you have any question please contact: aalber@essex.ac.uk

Payment date:...............

Total earnings: £...............

Figure A.2: Payment forms for the *NoFtF* (left) and *FtF* (right) treatments. The unique and hidden code that characterizes each form is given by the combination of the number of dots in the "Payment date" and "Total earnings" fields (subject id), and the lenght of the line below the email address (session id). In order to prevent copies, an university logo was stamped in the bottom right corner of the paper sheets.

A.2 Additional tables

	Outcome (£)			
	A (50%)	B (50%)	*Expected value*(£)	*Standard deviation*
Lottery 1	2	2	2	0
Lottery 2	1.5	3.5	2.5	1
Lottery 3	1	5	3	2
Lottery 4	0.5	6.5	3.5	3
Lottery 5	0	8	4	4

Table A.1: Part five - Lottery task
Participants did not receive information regarding lottery's expected value and standard deviations.

	OLS			Probit (dy/dx)		
	(1)	(2)	(3)	(4)	(5)	(6)
Yes (=1)	0.662**	0.663**	0.666**	0.175***	0.176***	0.177***
	(0.305)	(0.297)	(0.300)	(0.063)	(0.063)	(0.063)
Risk	0.185*	0.169*	0.173*	0.050**	0.047**	0.049**
	(0.101)	(0.099)	(0.102)	(0.021)	(0.020)	(0.021)
Transfer dictator	−0.309***	−0.284**	−0.284**	−0.051**	−0.047**	−0.047**
	(0.116)	(0.114)	(0.115)	(0.024)	(0.024)	(0.023)
Amount returned (trust)	0.010	0.017	0.018	0.020	0.021	0.020
	(0.130)	(0.131)	(0.131)	(0.027)	(0.027)	(0.027)
Constant	1.576***	1.181**	1.131*			
	(0.548)	(0.545)	(0.630)			
Controls	YES	YES^{+}	YES^{++}	YES	YES^{+}	YES^{++}
Observations	225	225	225	225	225	225

Table A.2: Cheating and other individial attitudes.

Note: Specifications 1-3 represent least square estimations on choices made in the *list game*. Specifications 4-6 represent marginal effects on a dummy variable indicating whether a subject lied in the *list game* or not. Specifications (1) and (3) control whether the *list game* was played after the other cheating task involving the virtual die. Specifications (2) and (5) include a dummy for the *NoFtF* treatment and regressions (3) and (6) additionally control for gender effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	OLS				Probit (dy/dx)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	−0.001	−0.002	−0.001	−0.002	−0.003	−0.004	−0.003	−0.004
	(0.020)	(0.021)	(0.020)	(0.020)	(0.004)	(0.005)	(0.004)	(0.004)
Female (= 1)	−0.405	−0.391	−0.172	−0.209	−0.062	−0.061	−0.015	−0.025
	(0.308)	(0.310)	(0.341)	(0.346)	(0.064)	(0.064)	(0.069)	(0.069)
Religious (= 1)	0.367	0.134	0.313	0.130	0.098	0.047	0.084	0.041
	(0.299)	(0.314)	(0.302)	(0.315)	(0.062)	(0.065)	(0.062)	(0.064)
Student (= 1)	1.286	1.103	0.912	0.805	0.324^{*}	0.263	0.245	0.199
	(0.789)	(0.794)	(0.800)	(0.799)	(0.169)	(0.167)	(0.170)	(0.167)
Origin:								
Africa		0.556		0.528		0.073		0.074
		(0.496)		(0.510)		(0.098)		(0.098)
Asia		0.657^{*}		0.547		0.153^{*}		0.135
		(0.380)		(0.403)		(0.080)		(0.083)
N. America		0.676		0.524		0.280^{*}		0.260
		(0.667)		(0.714)		(0.167)		(0.166)
S. America		-1.641^{***}		-1.482^{***}		−0.341		−0.346
		(0.591)		(0.528)		(0.229)		(0.218)
Field of Study/Job:								
Biology			0.746	0.643			0.096	0.074
			(0.562)	(0.571)			(0.112)	(0.110)
Computer Sc.			1.011^{*}	0.854			0.167	0.129
			(0.539)	(0.545)			(0.110)	(0.109)
Economics & Business			0.833^{*}	0.656			0.183^{*}	0.143
			(0.486)	(0.503)			(0.099)	(0.100)
Government			0.774	0.783			0.165	0.171^{*}
			(0.491)	(0.487)			(0.102)	(0.101)
Linguistics			0.551	0.557			0.098	0.080
			(0.697)	(0.715)			(0.137)	(0.137)
Psychology			−0.321	−0.256			−0.027	−0.009
			(0.506)	(0.494)			(0.113)	(0.110)
Sociology			0.732	0.795			0.198	0.203
			(0.680)	(0.656)			(0.146)	(0.143)
Constant	1.325	1.457	1.032	1.177				
	(1.191)	(1.190)	(1.211)	(1.205)				
Observations	249	249	249	249	249	249	249	249

Table A.3: Cheating and individial demographics.
Note: Specifications 1-4 represent least square estimations on choices made in the *list game*. Specifications 5-8 represent marginal effects on a dummy variable indicating whether a subject lied in the *list game* or not. The baseline for *Origin* is Europe, while for *Field of Study/Job* is represented by Other. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	OLS		Probit (dy/dx)	
	(1)	(2)	(3)	(4)
Big-5				
Agreebleness	0.104	0.112	0.013	0.015
	(0.326)	(0.325)	(0.072)	(0.072)
Consciousness	−0.172	−0.167	−0.028	−0.027
	(0.306)	(0.305)	(0.066)	(0.066)
Extraversion	−0.186	−0.204	−0.016	−0.018
	(0.384)	(0.380)	(0.079)	(0.079)
Neuroticism	0.306	0.257	0.055	0.048
	(0.206)	(0.210)	(0.045)	(0.046)
Openness	−0.053	−0.097	0.011	0.005
	(0.210)	(0.214)	(0.045)	(0.046)
Female (= 1)		−0.352		−0.046
		(0.315)		(0.067)
Constant	2.903	3.381*		
	(2.005)	(2.037)		
Observations	249	249	249	249

Table A.4: Cheating and personality traits.
Note: Specifications 1-2 represent least square estimations on choices made in the *list game*. Specifications 3-4 represent marginal effects on a dummy variable indicating whether a subject lied in the *list game* or not. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	OLS			Probit (dy/dx)		
	(1)	(2)	(3)	(4)	(5)	(6)
list game:						
1£	−0.579*	−0.630*	−0.682*			
	(0.324)	(0.341)	(0.365)			
3£	−0.400	−0.268	−0.262			
	(0.401)	(0.397)	(0.398)			
5£	0.295	0.455	0.437			
	(0.489)	(0.469)	(0.466)			
Cheater (= 1)				0.018	0.028	0.023
				(0.047)	(0.050)	(0.050)
Risk	0.146	0.163	0.135	0.011	0.012	0.009
	(0.144)	(0.148)	(0.146)	(0.016)	(0.016)	(0.017)
Transfer dictator	0.299	0.292	0.299	0.028	0.028	0.029
	(0.197)	(0.194)	(0.194)	(0.021)	(0.020)	(0.020)
Transfer trust	−0.535**	−0.547**	−0.579**	−0.060**	−0.060**	−0.064**
	(0.221)	(0.223)	(0.230)	(0.029)	(0.029)	(0.028)
Constant	0.006	0.739	1.045			
	(0.776)	(0.950)	(1.012)			
Controls	YES	YES^{+}	YES^{++}	YES	YES^{+}	YES^{++}
Observations	209	209	209	209	209	209

Table A.5: Laboratory behaviour and cheating in the field with over-reported money (in pounds) as a dependent variable.
Note: Specifications 1-3 represent least square estimations on the amount of money over-reported in the field. Dummies 1£, 3£, and 5£ represent choices made in the *list game* (honests are the excluded category). Specifications 4-6 represent marginal effects of cheating in the *list game* on a dummy variable indicating whether a subject lied in the field or not. Specifications (1) and (4) include a dummy for the *NoFtF* treatment, regressions (2) and (5) further control for actual laboratory earnings and, specifications (3) and (6) additionally control for gender effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	OLS			Probit (dy/dx)		
	(1)	(2)	(3)	(4)	(5)	(6)
Yes (=1)	0.045*	0.053*	0.050*	0.094*	0.106*	0.101*
	(0.025)	(0.030)	(0.029)	(0.050)	(0.056)	(0.054)
Risk	0.009	0.010	0.008	0.013	0.015	0.013
	(0.011)	(0.011)	(0.011)	(0.016)	(0.017)	(0.017)
Transfer dictator	0.018	0.018	0.018	0.030	0.029	0.030
	(0.013)	(0.013)	(0.013)	(0.019)	(0.020)	(0.019)
Transfer trust	−0.032**	−0.032*	−0.034**	−0.049*	−0.049*	−0.052*
	(0.016)	(0.016)	(0.016)	(0.029)	(0.028)	(0.028)
Constant	−0.027	0.001	0.020			
	(0.056)	(0.056)	(0.057)			
Controls	YES	YES^{+}	YES^{++}	YES	YES^{+}	YES^{++}
Observations	209	209	209	209	209	209

Table A.6: Laboratory behaviour and cheating in the field with *Cheat field* as a dependent variable.

Note: Variable *Yes* is a dummy which is equal to one if the subject reported a positive payoff in the mind game with the dice. Specifications 1-3 represent least square estimations on the variable *Cheat field*. Specifications 4-6 represent marginal effects on a dummy variable indicating whether a subject lied in the field or not. Specifications (1) and (4) include a dummy for the *NoFtF* treatment, regressions (2) and (5) further control for actual laboratory earnings and, specifications (3) and (6) additionally control for gender effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	FtF	*NoFtF*
Age	23.69	23.61
	(9.13)	(7.24)
female	0.69	0.58
	(0.47)	(0.50)
religious	0.64	0.57
	(0.48)	(0.50)
Current Degree		
Undergraduate	70.34	67.94
Master	14.41	19.85
PhD	10.17	6.87
Other (Worker)	5.08	5.34
Big-5		
Agreeableness	2.82	2.79
	(0.43)	(0.45)
Conscientiousness	3.35	3.39
	(0.47)	(0.51)
Extraversion	3.21	3.19
	(0.38)	(0.40)
Neuroticism	1.96	2.13
	(0.65)	(0.81)
Openness	3.13	3.22
	(0.69)	(0.69)
Observations	118	131

Table A.7: Sample statistics
Note: Sample summary statistics from questionnaire variables. Standard deviations in parenthesis.

A.3 Additional figures

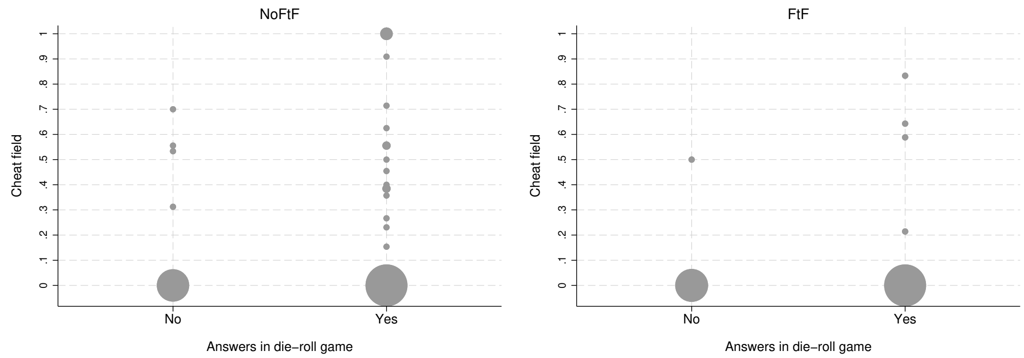


Figure A.3: Comparison of choices in die-roll mind game and cheating in the field for the *NoFtF* (left panel, n=123) and *FtF* (right panel, n=103) treatments with weighted markers. The y-axis indicates the extent of cheating in the field relative to the maximum payoff a subject could claim. The x-axis represents the choices made in the mind game involving the die roll.

A.4 Screenshots

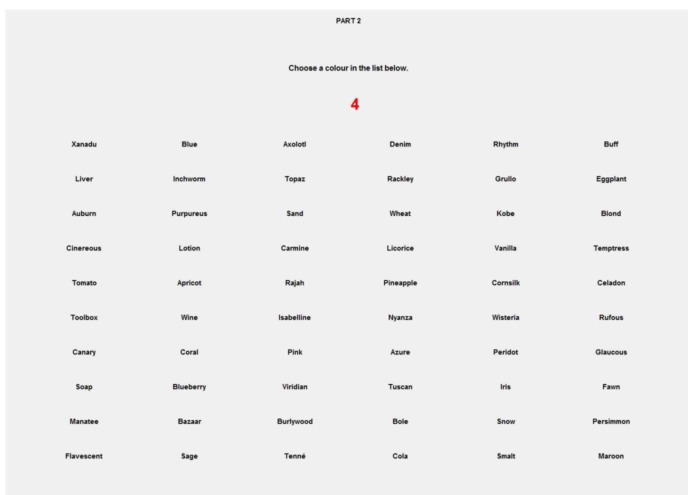


Figure A.4: Colour choice in the *list game*.

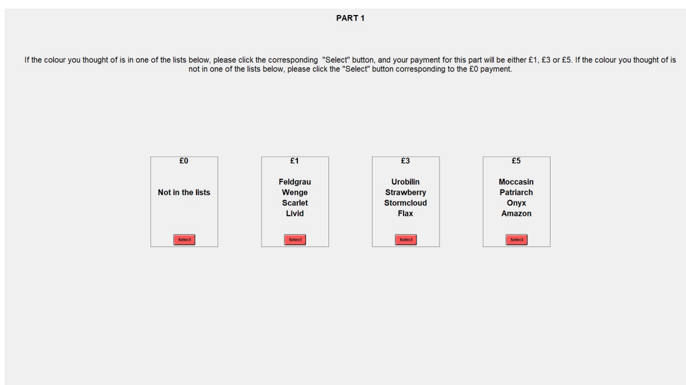


Figure A.5: List choice in the *list game*, where subjects have the opportunity to cheat.

Appendix B

Appendix for “Benchmarking Information Aggregation in Experimental Markets”

B.1 Experimental Instructions

In the following we provide the experimental instructions for the *Market* treatment under *Public information* without bid-ask feedback and for the *BDM* treatment under *Private information* with bid-ask feedback. All other treatments are a combination of these instructions.

B.1.1 *Market* under *Public information* without bid-ask feedback

Instructions

Welcome and thanks for participating in this experiment. Please, read these instructions carefully. They are identical for all the participants with whom you will interact during this experiment. If you have a question, please, raise your hand. One of the experimenters 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 are sorry to have to exclude you from the experiment. Please do also switch off your mobile phone at this moment. At the end of the experiment you will receive a payment. How much you get depends on your decisions and those of other participants. During the experiment the earnings are expressed in ECU (Experimental Currency Units). At the end of the experiment the ECUs collected are converted into Euros according to the exchange rate 1 ECU = 5 Eurocents. In addition there is the 5 Euro show up fee. All your decisions will be treated confidentially.

The experiment

There will be two different assets in this experiment, which will be labeled with different colors. In these instructions we will talk about the BLACK asset and the WHITE asset. In the experiment, however, different colors will be used.

Both assets have one of the following three possible **returns**: 50 ECU, 100 ECU or 150 ECU. The difference between the two assets is the **probabilities** with which these possible returns realize. One of the assets returns the values 50, 100 and 150 with probabilities 3/5, 1/5 and 1/5; while the other asset returns these values with probabilities 1/5, 1/5 and 3/5, respectively.

One way to think about this is that both the BLACK and the WHITE asset represent an envelope with money containing bills of 50 ECU, 100 ECU and 150 ECU. The difference is that the BLACK and WHITE envelope might contain different numbers of each of these bills. One envelope contains 3 bills of 50, 1 bill of 100 and 1 of 150, the other has 1 bill of 50, 1 bill of 100 and 3 of 150. The value of an asset is determined by randomly picking one bill from the envelope. In total the experiment consists of three **repetitions**. In each repetition there will be different assets. You can buy or sell assets in each repetition for ten **trading periods**.

The trading

You will be matched with four other participants in a **group**. In each trading period, you have one share of each asset (BLACK and WHITE) in stock. You will tell us two numbers:

(i) your **buying price**: this is the maximum price at which you are willing to buy one more share of this asset, and

(ii) your **selling price**: this is the minimum price at which you are willing to sell your share of this asset.

Hence, in total you will tell us four numbers, two for each asset.

All group members will tell us their four numbers simultaneously. Afterwards, for each asset BLACK and WHITE, the buying prices of all group members are ranked highest to lowest and the selling prices of all group members are ranked lowest to highest. The **market price** of each asset is determined as follows:

1. First we compare the lowest selling price with the highest buying price.
   - If this selling price is higher than this buying price, then there is no market price (which we will mark with xxx).
   - Otherwise, we proceed to 2.
2. Compare the second-lowest selling price with the second-highest buying price.
   - If this selling price is higher than this buying price, then the market price is the average of the lowest selling price and the highest buying price.
   - Otherwise, we proceed to 3.
3. Compare the third-lowest selling price with the third-highest buying price.
   - If this selling price is higher than this buying price, then the market price is the average of the second-lowest selling price and the second-highest buying price.
   - Otherwise, we proceed to 4.
4. Compare the fourth-lowest selling price with the fourth-highest buying price.
   - If this selling price is higher than this buying price, then the market price is the average of the third-lowest selling price and the third-highest buying price.
   - Otherwise, we proceed to 5.
5. Compare the fifth-lowest (or highest) selling price with the fifth-highest (or lowest) buying price.
   - If this selling price is higher than this buying price, then the market price is the average of the fourth-lowest selling price and the fourth-highest buying price.
   - Otherwise, the market price is the average of this fifth-lowest selling price and this fifth-highest buying price.

For example, assume that, for some asset, the five buying prices are (5400, 100, 21, 7, 1) and the selling prices are (8, 24, 65, 201, 300). The lowest selling price of 8 is lower than the highest buying price of 5400. Hence, we proceed to step 2. The second-lowest selling price of 24 is lower than the second-highest buying price of 100. Hence, we proceed to step 3. The third-lowest selling price of 65 is higher than the third-highest buying price of 21. Hence, two shares are traded in your group and the market price is the average between the second-lowest selling price of 24 and the second-highest buying price of 100, which is 62. Once the market price is determined, all group members with buying prices above the market price and with selling prices below the market price will trade one share of the asset (at the market price). In case there is excess demand or excess supply, group members with higher buying prices and lower selling prices will trade first. In case of ties (equal buying prices or equal selling prices) between group members, a random selection of these will be trading.

Information

At the end of each period you will observe for each asset (BLACK and WHITE):

- the market price;
- whether you sold the asset, you bought the asset, or did not make any trade at all.

Your earnings in the experiment

At the end of the experiment one period is randomly drawn. Your earnings in the experiment are based on your payoff from that randomly drawn period.

First the return of the BLACK and the WHITE asset (either 50, 100 or 150) are determined according to the respective probabilities. In terms of our envelope example you can think of one bill being randomly drawn from the BLACK and one from the WHITE envelope. Recall here that one of the assets returns the values 50, 100 and 150 with probabilities 3/5, 1/5 and 1/5; while the other asset returns these values with probabilities 1/5, 1/5 and 3/5, respectively. All group members know at any point during the experiment which asset has which return probabilities.

Your payoff in this period is then computed as follows:

$$\begin{aligned}
\text{Payoff} = &\text{ Number of shares of BLACK asset} \times \text{Return of BLACK asset} \\
&+ \text{Number of shares of WHITE asset} \times \text{Return of WHITE asset} \\
&- \text{Market price BLACK asset } \textit{if} \text{ a share of this asset is bought} \\
&+ \text{Market price BLACK asset } \textit{if} \text{ a share of this asset is sold} \\
&- \text{Market price WHITE asset } \textit{if} \text{ a share of this asset is bought} \\
&+ \text{Market price WHITE asset } \textit{if} \text{ a share of this asset is sold}
\end{aligned}$$

Questionnaire

At the end of the experiment there will be a short questionnaire for you to fill in.

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 some control questions will appear on your screen that will allow you to test your understanding of the instructions.

B.1.2 *BDM* under *Private information* with bid-ask feedback

Instructions

Welcome and thanks for participating in this experiment. Please, read these instructions carefully. They are identical for all the participants with whom you will interact during this experiment. If you have a question, please, raise your hand. One of the experimenters 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 are sorry to have to exclude you from the experiment. Please do also switch off your mobile phone at this moment. At the end of the experiment you will receive a payment. How much you get depends on your decisions and those of other participants. During the experiment the earnings are expressed in ECU (Experimental Currency Units). At the end of the experiment the ECUs collected are converted into Euros according to the exchange rate 1 ECU = 5 Eurocents. In addition there is the 5 Euro show up fee. All your decisions will be treated confidentially.

The experiment

There will be two different **assets** in this experiment, which will be labeled with different colors. In these instructions we will talk about the BLACK asset and the WHITE asset. In the experiment, however, different colors will be used.

Both assets have one of the following three possible **returns**: 50 ECU, 100 ECU or 150 ECU. The difference between the two assets is the probabilities with which these possible returns realize. In other words the chance to get 50 or 100 or 150 is different for the BLACK compared to the WHITE asset. The only thing you know is that each of these returns is possible with positive probability for both assets.

One way to think about this is that both the BLACK and the WHITE asset represent an envelope with money containing bills of 50 ECU, 100 ECU and 150 ECU. The difference is that the BLACK and WHITE envelope might contain different numbers of each of these bills. The only thing you know is that in each envelope there is at least one bill of each kind.

In total the experiment consists of three **repetitions**. In each repetition there will be different assets. You can buy or sell assets in each repetition for ten **trading periods**.

Signal

At the beginning of each repetition you receive a **signal**. A signal is a piece of information for you about each of the assets. You will receive the following signal. For each asset we will tell you one number 50, 100 or 150. The probability with which we tell you each of these numbers corresponds to the probability with which the asset has this return. Hence the higher the probability that the asset has a certain return, the higher the chance that we show you this number.

In terms of our envelope example you can think about your signal as follows. We randomly draw one bill out of each envelope and show it to you. Hence the more bills of a certain type an envelope contains, the more likely it is that we draw one of these.

In the experiment you will be matched with four other participants in a **group**. Not only you, but also all of the other group members will receive a signal in the same manner as you. Note, however, that different participants might receive different signals.

The trading

In each trading period, you have one share of each asset (BLACK and WHITE) in stock. You will tell us two numbers:

(i) your **buying price**: this is the maximum price at which you are willing to buy one more share of this asset, and

(ii) your **selling price**: this is the minimum price at which you are willing to sell your share of this asset.

Hence, in total you will tell us four numbers, two for each asset.

For each asset (BLACK and WHITE), the central computer will draw a random number between 50 and 150–all numbers in this interval are equally likely to be drawn. The numbers drawn are the random price of the assets.

For each asset (BLACK and WHITE):

- you buy one more share at the random price if your buying price is above this random price;
- you sell your share at the random price if your selling price is below this random price;
- you will neither buy nor sell if the random price is above your buying price and below your selling price;

All group members will tell us their four numbers simultaneously. Afterwards, for each asset BLACK and WHITE, the buying prices of all group members are ranked highest to lowest and the selling prices of all group members are ranked lowest to highest.
The **group value** of each asset is determined as follows:

1. First we compare the lowest selling price with the highest buying price.
   - If this selling price is higher than this buying price, then there is no market price (which we will mark with xxx).
   - Otherwise, we proceed to 2.
2. Compare the second-lowest selling price with the second-highest buying price.
   - If this selling price is higher than this buying price, then the market price is the average of the lowest selling price and the highest buying price.
   - Otherwise, we proceed to 3.
3. Compare the third-lowest selling price with the third-highest buying price.
   - If this selling price is higher than this buying price, then the market price is the average of the second-lowest selling price and the second-highest buying price.
   - Otherwise, we proceed to 4.
4. Compare the fourth-lowest selling price with the fourth-highest buying price.
   - If this selling price is higher than this buying price, then the market price is the average of the third-lowest selling price and the third-highest buying price.
   - Otherwise, we proceed to 5.
5. Compare the fifth-lowest (or highest) selling price with the fifth-highest (or lowest) buying price.

- If this selling price is higher than this buying price, then the market price is the average of the fourth-lowest selling price and the fourth-highest buying price.
- Otherwise, the market price is the average of this fifth-lowest selling price and this fifth-highest buying price.

For example, assume that, for some asset, the five buying prices are (5400, 100, 21, 7, 1) and the selling prices are (8, 24, 65, 201, 300). The lowest selling price of 8 is lower than the highest buying price of 5400. Hence, we proceed to step 2. The second-lowest selling price of 24 is lower than the second-highest buying price of 100. Hence, we proceed to step 3. The third-lowest selling price of 65 is higher than the third-highest buying price of 21. The group value is hence determined as the average between the second-lowest selling price of 24 and the second-highest buying price of 100, which is 62. At a price equal to this group value of 62, two individuals buy a share and two individuals sell a share.

Information

At the end of each period you will observe for each asset (BLACK and WHITE):

- the group value;
- the random price;
- whether you sold the asset, you bought the asset, or did not make any trade at all.

Your earnings in the experiment

At the end of the experiment one period is randomly drawn. Your earnings in the experiment are based on your payoff from that randomly drawn period.
First the return of the BLACK and the WHITE asset (either 50, 100 or 150) are determined according to the respective probabilities. In terms of our envelope example you can think of one bill being randomly drawn from the BLACK and one from the WHITE envelope.

Your payoff in this period is then computed as follows:

$$
\begin{aligned}
\text{Payoff} = {} & \text{Number of shares of BLACK asset} \times \text{Return of BLACK asset} \\
& + \text{Number of shares of WHITE asset} \times \text{Return of WHITE asset} \\
& - \text{Market price BLACK asset } \textit{if} \text{ a share of this asset is bought} \\
& + \text{Market price BLACK asset } \textit{if} \text{ a share of this asset is sold} \\
& - \text{Market price WHITE asset } \textit{if} \text{ a share of this asset is bought} \\
& + \text{Market price WHITE asset } \textit{if} \text{ a share of this asset is sold}
\end{aligned}
$$

Questionnaire

At the end of the experiment there will be a short questionnaire for you to fill in.

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 some control questions will appear on your screen that will allow you to test your understanding of the instructions.

B.2 Sample Information and Questionnaire

Table B.1 provides some descriptive statistics about our experimental sample that we elicited in the post-experimental questionnaire. Apart from demographics (Gender, Age, Origin, Field of studies and years of graduate education), we elicited risk attitudes using a self-assessed measure as in Dohmen et al. (2011). Participants answered the question *"Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?"* on a ten-point scale ranging between "Not at all willing to take risks" and "Very willing to take risks".

We assessed the Machiavellianism score using the Likert-Type Mach Scale (IV) developed in Christie and Geis (1970). We measure the individual propensity to compete with others using the Revised Competitiveness Index (RCI) developed in Houston et al. (2002). This 14-items aggregate index can be subdivided in order to capture the individual "Enjoyment of Competition" and "Contentiousness". We elicited participants' degree of optimism using a revised Life Orientation Test as in Scheier et al. (1994). For each of these three dimensions participants indicated how much they personally agreed or disagreed with some statements using a five-point Likert scale ranging from "strongly disagree" to "strongly agree".

Finally, the Big-5 test assesses five personality traits: Openness, Consciousness, Extraversion, Agreeableness and Neuroticism. These personality dimensions were elicited using a 15-item questionnaire evaluated on a five-point Likert scale running from "strongly disagree" to "strongly agree" (Costa and McCrae, 1992).

	Public information		Private information	
	BDM	*Market*	*BDM*	*Market*
Female	0.50	0.54	0.60	0.56
	(0.50)	(0.50)	(0.49)	(0.49)
Age	20.63	20.69	21.94	21.30
	(2.46)	(3.08)	(2.38)	(2.15)
Years of study	2.02	2.50	2.57	2.25
	(1.22)	(1.74)	(1.54)	(1.18)
Risk	6.05	6.49	6.15	6.20
	(1.79)	(1.87)	(1.97)	(1.87)
Machiavellianism	60.76	61.30	59.82	59.50
	(7.02)	(8.09)	(6.14)	(7.76)
RCI	50.00	47.40	46.91	46.30
	(7.27)	(10.06)	(8.38)	(7.55)
Optimism	20.52	20.32	19.90	20.39
	(4.13)	(4.23)	(3.95)	(3.73)
Big-5:				
Openness	10.94	11.10	10.95	10.52
	(2.26)	(2.60)	(1.97)	(2.25)
Consciousness	10.66	10.64	10.19	10.69
	(2.24)	(2.07)	(2.16)	(1.98)
Extraversion	10.85	10.81	10.52	10.68
	(2.32)	(2.46)	(2.41)	(2.41)
Agreeableness	11.11	11.54	11.20	11.34
	(2.03)	(2.21)	(1.83)	(1.95)
Neuroticism	8.72	9.32	9.40	9.02
	(2.99)	(2.81)	(2.52)	(2.39)
Origin:				
Africa	0.00	0.01	0.02	0.03
Asia	0.07	0.07	0.04	0.05
Dutch	0.20	0.20	0.10	0.21
German	0.39	0.35	0.41	0.35
Middle or South America	0.05	0.03	0.04	0.01
North America	0.01	0.04	0.04	0.00
Oceania	0.00	0.01	0.01	0.01
Other in Europe	0.28	0.29	0.34	0.34
Study:				
Econometrics and Op. Research	0.01	0.00	0.05	0.03
Economics and Business Economics	0.17	0.09	0.08	0.09
Exchange Student	0.05	0.00	0.04	0.02
Fiscal Economics	0.01	0.04	0.00	0.03
Infonomics	0.00	0.00	0.01	0.02
International Business	0.59	0.49	0.34	0.44
Int. Business Economics	0.04	0.06	0.01	0.06
Int. Economic Studies	0.05	0.00	0.01	0.02
Other	0.08	0.27	0.46	0.29
Observations	80	80	80	80

Table B.1: Sample statistics.

Note: Means and standard deviations (in parenthesis) of questionnaire variables.

B.3 Additional Theoretical Background and Proofs

B.3.1 Equilibrium with a More General State Space

In this appendix we reconsider the theoretical predictions derived in Section 2.2.2 for the case where agents perceive a more general state space. As before we derive the theoretical predictions for the *Market* treatment with *Private information* and without bid-ask feedback for signal ρ_1.[1] Let A and B be two assets/urns and assume that all agents have the same beliefs about the number of balls, k, contained into each urn. Given that every outcome has positive probability, each urn must contain at least four balls in order for ambiguity to be possible: $k \geq 4$. Then, agents know that the state space is given by $\Omega = \Omega_A \times \Omega_B$, where

$$\Omega_i = \left\{ \left(\tfrac{1}{k}, \tfrac{1}{k}, \tfrac{k-2}{k}\right), \left(\tfrac{1}{k}, \tfrac{2}{k}, \tfrac{k-3}{k}\right), \ldots, \left(\tfrac{1}{k}, \tfrac{k-2}{k}, \tfrac{1}{k}\right), \ldots, \left(\tfrac{k-3}{k}, \tfrac{2}{k}, \tfrac{1}{k}\right), \left(\tfrac{k-2}{k}, \tfrac{1}{k}, \tfrac{1}{k}\right) \right\}, \; i = A, B.$$

Assume agents have prior beliefs uniformly distributed on all $N_A \times N_B$ possible states contained in Ω where $N_i = \frac{(k-1)(k-2)}{2}$.

Let $r_i \in \{50, 100, 150\}$ be the signal received by one agent for asset i and the function $p(\cdot|r_i) : r_i \to \Omega_i$ be her posterior beliefs. Then, posterior beliefs for an agent with signal $(150, 50)$ can be calculated as follows, where we directly report marginal distributions:

$$p\left(\omega_A = \left(\cdot, \cdot, \tfrac{k-\ell}{k}\right) \middle| 150\right) = (\ell - 1) \frac{\frac{1}{N}\frac{(k-\ell)}{k}}{\frac{1}{N}\frac{\sum_{j=1}^{k-1}(j-1)(k-j)}{k}} = (\ell - 1)\frac{(k-\ell)}{\alpha}$$

for $\ell = 2, \ldots, k-1$ and $\alpha = \frac{k(k-1)(k-2)}{6}$. Analogously the posterior beliefs on states ω_B can be computed. This implies that an agent with signal $(150, 50)$ will have an expected value for assets A and B respectively of:

$$\begin{aligned} E[A|150] &= 150 \left[\frac{1}{\alpha k} \sum_{\ell=1}^{k-1} (\ell - 1)(k-\ell)^2 \right] + 100 \left[\frac{1}{\alpha k} \sum_{\ell=2}^{k-1} \left[(k-\ell) \sum_{j=1}^{\ell-1} j \right] \right] + 50 \left[\frac{1}{\alpha k} \sum_{\ell=2}^{k-1} \left[(k-\ell) \sum_{j=1}^{\ell-1} j \right] \right] \\ &= 150 \frac{(3k-1)}{4k} \end{aligned}$$

[1] Note that as long as both signals ρ_1 and ρ_2 reflect exactly assets distribution over outcomes, the theoretical predictions are independent of the signals distribution.

and

$$
\begin{aligned}
E[B|50] &= 150\left[\frac{1}{\alpha k}\sum_{\ell=2}^{k-1}\left[(k-\ell)\sum_{j=1}^{\ell-1} j\right]\right] + 100\left[\frac{1}{\alpha k}\sum_{\ell=2}^{k-1}\left[(k-\ell)\sum_{j=1}^{\ell-1} j\right]\right] + 50\left[\frac{1}{\alpha k}\sum_{\ell=1}^{k-1}(\ell-1)(k-\ell)^2\right] \\
&= 50\frac{(7k+3)}{4k}.
\end{aligned}
$$

In the same fashion, agents with signal $(50,150)$ will have posterior beliefs that imply an expected value of $50\frac{(7k+3)}{4k}$ for asset A and $150\frac{(3k-1)}{4k}$ for asset B. Agents with signal $(100,50)$ will have an expected value for asset A of 100 and an expected value for asset B of $50\frac{(7k+3)}{4k}$, and agents with signal $(150,100)$ will have an expected value of $150\frac{(3k-1)}{4k}$ and 100 for asset A and B respectively. Note that $150\frac{(3k-1)}{4k} > 100 > 50\frac{(7k+3)}{4k}$ for every $k \geq 4$.

Under our setting it is straightforward to show that a fully revealing rational expectations equilibrium exists and it is unique. Indeed, in the first period the ordered bids for asset A will be

$$
\left(\frac{150(3k-1)}{4k},\ \frac{150(3k-1)}{4k},\ \frac{150(3k-1)}{4k},\ 100,\ \frac{50(7k+3)}{4k}\right)
$$

and the ordered asks will be

$$
\left(\frac{50(7k+3)}{4k},\ 100,\ \frac{150(3k-1)}{4k},\ \frac{150(3k-1)}{4k},\ \frac{150(3k-1)}{4k}\right)
$$

which means that asset A will trade at a price of $\frac{150(3k-1)}{4k} \in [103.125, 112.5)$. Analogously, asset B will trade at a price of $\frac{50(7k+3)}{4k} \in (87.5, 96.875]$.

Given these prices agents recognize that at least three agents have received a signal of 150 for asset A (and 50 for asset B), respectively. If further asks and bids are observed (as in the treatments with bid-ask feedback) then all private information is revealed in the first round. In this case while the price reveals all private information of each individual, it is possible that some residual uncertainty about the state remains as in Radner

(1979). To eliminate all residual uncertainty we would need an infinite number of traders. Note, though, that with a uniform prior the posterior will be concentrated on the true state. Without bid-ask feedback more than one period of trading is needed to reveal all private information (depending on k), but only one period is needed to rank the assets correctly.

B.3.2 Strategic Behaviour: Shading Bids and Asks

Let relative to the previous section, traders shading their bid and asks by ε. less and ask ε more. For asset A this results in bids

$$\left(\frac{150(3k-1)}{4k}-\varepsilon,\ \frac{150(3k-1)}{4k}-\varepsilon,\ \frac{150(3k-1)}{4k}-\varepsilon,\ 100-\varepsilon,\ \frac{50(7k+3)}{4k}-\varepsilon\right)$$

and asks

$$\left(\frac{50(7k+3)}{4k}+\varepsilon,\ 100+\varepsilon,\ \frac{150(3k-1)}{4k}+\varepsilon,\ \frac{150(3k-1)}{4k}+\varepsilon,\ \frac{150(3k-1)}{4k}+\varepsilon\right),$$

leading to a price of

$$p=\frac{1}{2}\left(\frac{150(3k-1)}{4k}-\varepsilon+100+\varepsilon\right)=\frac{950k-150}{8k}$$

which is in $[114.06, 118.75)$. Similarly, for asset B we obtain a price of

$$p=\frac{1}{2}\left(100-\varepsilon+\frac{50(7k+3)}{4k}+\varepsilon\right)=\frac{750k+150}{8k}$$

which is in $(93.75, 98.4375]$. For both assets we find two units being traded.

B.3.3 Ambiguity Aversion

Let $m_A(\omega)$ be the return obtained with asset A in state ω and denote by $\mathbb{E}_p(m_A)=\sum_{\omega\in\Omega} p(\omega)m_A(\omega)$ be the expected return given posterior p. Following Gilboa and Schmeidler (1989) and Cerreia Vioglio (2009) we model agents' utility from holding asset A by

$$u(A) = \inf_{p \in \mathbf{P}} \mathbb{E}_p(m_A),$$

where $\mathbf{P}$ is the set of beliefs the ambiguity averse agent entertains. Further, denote by $\widehat{p}$ the posterior of an agent who does *not* perceive ambiguity.

Proposition 1. *If* $\mathbf{P}$ *includes beliefs* p *such that* $\mathbb{E}_p(m_A) < \mathbb{E}_{\widehat{p}}(m_A)$*, then the ambiguity averse agents will bid less for asset A than the agent who does not perceive ambiguity.*

Proof: Since there is a belief $p \in \mathbf{P}$ such that $\mathbb{E}_p(m_A) < \mathbb{E}_{\widehat{p}}(m_A)$, it follows that $\inf_{p \in \mathbf{P}} \mathbb{E}_p(m_A) < \mathbb{E}_{\widehat{p}}(m_A)$. Hence, the agent perceiving ambiguity will perceive asset A as less valuable. □

B.3.4 Social Comparison Model from Section 4.2.

We expand here on the model discussed in Section 4.2 and show how a swap affects agent i's utility. Before the swap, when agent i holds asset H and agent j holds asset L, agent i's utility is given by

$$V(H,L) = \eta\, EU(H) + \psi\,[\tfrac{3}{25}v(u_{21}) + \tfrac{1}{25}v(-u_{21}) + \tfrac{3}{25}v(u_{32}) + \tfrac{1}{25}v(-u_{32}) + \tfrac{9}{25}v(u_{31}) + \tfrac{1}{25}v(-u_{31})],$$

where $u_{21} \equiv u(100) - u(50)$, $u_{32} \equiv u(150) - u(100)$ and $u_{31} \equiv u(150) - u(50)$ are all positive. Agent i's utility after the swap is given by

$$V(L,H) = \eta\, EU(L) + \psi\,[\tfrac{1}{25}v(u_{21}) + \tfrac{3}{25}v(-u_{21}) + \tfrac{1}{25}v(u_{32}) + \tfrac{3}{25}v(-u_{32}) + \tfrac{1}{25}v(u_{31}) + \tfrac{9}{25}v(-u_{31})].$$

Hence, the swap between assets leads to a decrease in utility of

$$\begin{aligned} V(H,L) - V(L,H) = \eta\,[EU(H) - EU(L)] \\ + \tfrac{2}{25}\,\psi\,[(v(u_{21}) - v(-u_{21})) + (v(u_{32}) - v(-u_{32})) + 4\,(v(u_{31}) - v(-u_{31}))] \end{aligned}$$

for agent i. Since $v(y) - v(-y) > 0$ for all $y > 0$, this decrease is increasing in ψ.

B.4 Additional Tables

	Repetition 1		Repetition 2		Repetition 3	
Matching group	Ranking	Signal	Ranking	Signal	Ranking	Signal
MG 1–2	Red > Green	ρ_1	Yellow < Purple	ρ_2	Blue < Orange	ρ_1
MG 3–4	Red < Green	ρ_1	Yellow > Purple	ρ_1	Blue > Orange	ρ_2
MG 5–6	Red > Green	ρ_2	Yellow < Purple	ρ_1	Blue > Orange	ρ_2
MG 7–8	Red < Green	ρ_2	Yellow > Purple	ρ_2	Blue < Orange	ρ_1

Table B.2: Composition of repetitions over matching groups.
Ranking of the assets (labelled by colors) and signal distributions for the different matching groups.

	Female	*Age*	*Risk*	*Machiav.*	*Optimism*
Constant	0.500***	23.137***	6.050***	60.763***	20.525***
	(0.056)	(1.309)	(0.210)	(0.816)	(0.449)
Private info	0.100	−1.200	0.100	−0.938	−0.625
	(0.079)	(1.851)	(0.297)	(1.154)	(0.635)
Market	0.037	−2.450	0.437	0.537	−0.200
	(0.079)	(1.851)	(0.297)	(1.154)	(0.635)
Private info × Market	−0.075	1.813	−0.387	−0.862	0.687
	(0.112)	(2.617)	(0.420)	(1.631)	(0.898)
Observations	320	320	320	320	320

Table B.3: Balancing check.
Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Prob(Price $H >$ Price L)	LPM (1)	LPM (2)	LPM (3)	Probit (dy/dx) (4)	Probit (dy/dx) (5)	Probit (dy/dx) (6)
Constant	0.966***	0.966***	0.949***			
	(0.017)	(0.017)	(0.030)			
Private info (β)	−0.213***	−0.297***	−0.302***	−0.285***	−0.336***	−0.338***
	(0.056)	(0.100)	(0.099)	(0.081)	(0.094)	(0.093)
Market (γ)	−0.047	−0.047	−0.048	−0.007	−0.007	−0.001
	(0.073)	(0.074)	(0.075)	(0.135)	(0.133)	(0.134)
Private info $\times$ Market (δ)	−0.038	0.018	0.020	−0.042	−0.005	−0.007
	(0.112)	(0.170)	(0.164)	(0.149)	(0.166)	(0.163)
BAF	−0.072	−0.072	−0.088*	−0.127	−0.125	−0.133
	(0.050)	(0.050)	(0.048)	(0.099)	(0.098)	(0.098)
BAF $\times$ Private info	0.022	0.130	0.144	0.100	0.167	0.168
	(0.095)	(0.131)	(0.131)	(0.119)	(0.131)	(0.129)
BAF $\times$ Market	0.139	0.139	0.153*	0.189	0.187	0.194
	(0.088)	(0.088)	(0.089)	(0.166)	(0.165)	(0.165)
BAF $\times$ Market $\times$ Private info	−0.131	−0.248	−0.256	−0.190	−0.260	−0.261
	(0.162)	(0.232)	(0.231)	(0.193)	(0.219)	(0.217)
ρ_1		0.168	0.168		0.118	0.123
		(0.132)	(0.127)		(0.097)	(0.088)
$\rho_1 \times$ Market		−0.115	−0.117		−0.081	−0.088
		(0.224)	(0.211)		(0.154)	(0.137)
$\rho_1 \times$ BAF		−0.219	−0.211		−0.149	−0.131
		(0.210)	(0.201)		(0.145)	(0.134)
$\rho_1 \times$ BAF $\times$ Market		0.238	0.225		0.154	0.142
		(0.324)	(0.316)		(0.219)	(0.205)
Repetition 1			0.101**			0.104**
			(0.045)			(0.047)
Repetition 2			−0.034			−0.029
			(0.049)			(0.044)
$\beta+\delta$	−0.251	−0.279	−0.282	−0.327	−0.341	−0.345
p-value test $\beta+\delta=0$	0.009	0.042	0.031	0.006	0.009	0.007
p-value test $\lvert\beta+\delta\rvert \leq \lvert\beta\rvert$	0.365	0.543	0.549	0.390	0.487	0.483
$\gamma+\delta$	−0.086	−0.029	−0.028	−0.048	−0.012	−0.008
p-value test $\gamma+\delta=0$	0.310	0.851	0.847	0.459	0.903	0.927
Mkt−BDM [Public info, BAF] (τ)	0.092	0.092	0.105	0.182	0.181	0.193
p-value test $\tau=0$	0.054	0.055	0.018	0.048	0.048	0.032
Mkt−BDM [Private info, BAF] (ϕ)	−0.078	−0.138	−0.131	−0.050	−0.085	−0.076
p-value test $\phi=0$	0.463	0.360	0.400	0.516	0.412	0.468
Observations	1572	1572	1572	1572	1572	1572

Table B.4: Ranking with bid-ask feedback.

Note: Robust standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

	Price asset H			Price asset L		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	97.729***	97.730***	96.020***	76.183***	76.183***	74.874***
	(2.253)	(2.255)	(2.489)	(0.937)	(0.938)	(1.042)
Private info (β)	−5.717	−6.920	−7.131	6.304	6.710	6.570
	(4.123)	(4.712)	(4.468)	(4.253)	(4.632)	(4.451)
Market (γ)	3.371	3.371	3.269	−5.073*	−5.073*	−5.158*
	(4.989)	(4.994)	(5.030)	(2.865)	(2.868)	(2.890)
Private info × Market (δ)	−14.947**	−14.648*	−14.588*	−8.438	−7.669	−7.583
	(7.233)	(7.756)	(7.471)	(5.777)	(6.486)	(6.354)
BAF	1.202	1.204	0.918	−0.716	−0.718	−0.925
	(5.371)	(5.376)	(5.313)	(3.077)	(3.083)	(3.076)
BAF × Private info	−4.844	−5.336	−4.934	−1.352	−2.194	−1.915
	(6.643)	(7.319)	(7.057)	(5.593)	(6.184)	(5.955)
BAF × Market	6.023	6.023	6.304	5.470	5.471	5.577
	(8.411)	(8.420)	(8.383)	(4.787)	(4.794)	(4.761)
BAF × Market × Private info	−12.844	−12.044	−12.226	−12.605	−13.818	−13.979
	(10.662)	(11.304)	(11.017)	(8.029)	(9.019)	(8.924)
ρ_1		2.434	2.431		−0.801	−0.754
		(2.884)	(2.341)		(2.893)	(2.681)
ρ_1 × Market		−0.625	−0.624		−1.506	−1.532
		(6.367)	(5.908)		(3.826)	(3.456)
ρ_1 × BAF		0.751	1.252		1.708	1.519
		(5.411)	(5.066)		(4.083)	(3.697)
ρ_1 × BAF × Market		−1.392	−2.005		2.400	2.643
		(8.607)	(7.981)		(5.799)	(5.422)
Repetition 1			5.997***			1.545
			(1.773)			(1.152)
Repetition 2			−0.256			2.715**
			(1.226)			(1.062)
$\beta+\delta$	−20.664	−21.568	−21.719	−2.134	−0.959	−1.013
p-value test $\beta+\delta=0$	0.001	0.000	0.000	0.585	0.833	0.823
p-value test $\lvert\beta+\delta\rvert \leq \lvert\beta\rvert$	0.019	0.029	0.025	0.765	0.812	0.809
$\gamma+\delta$	−11.577	−11.278	−11.319	−13.511	−12.743	−12.741
p-value test $\gamma+\delta=0$	0.027	0.057	0.041	0.007	0.028	0.024
Mkt−BDM [Public info, BAF] (τ)	9.393	9.393	9.573	0.397	0.398	0.420
p-value test $\tau=0$	0.165	0.166	0.153	0.918	0.918	0.912
Mkt−BDM [Private info, BAF] (ϕ)	−18.398	−17.299	−17.241	−20.646	−21.090	−21.142
p-value test $\phi=0$	0.000	0.000	0.000	0.000	0.000	0.000
Observations	1761	1761	1761	1653	1653	1653

Table B.5: Perfect aggregation with bid-ask feedback.

Note: Robust standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

	Asset H		Asset L	
	Public information	*Private information*	*Public information*	*Private information*
BDM	1.214	1.435	0.852	1.287
Market	1.389	1.683	1.345	1.658
Difference	−0.175***	−0.247***	−0.493***	−0.370***
Observations	960	960	960	960

Table B.6: Average number of assets traded.

Note: Statistical significance is determined using a t-test between institutions under the same information condition. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

B.5 Additional Figures

B.5.1 Additional Figures Section 3

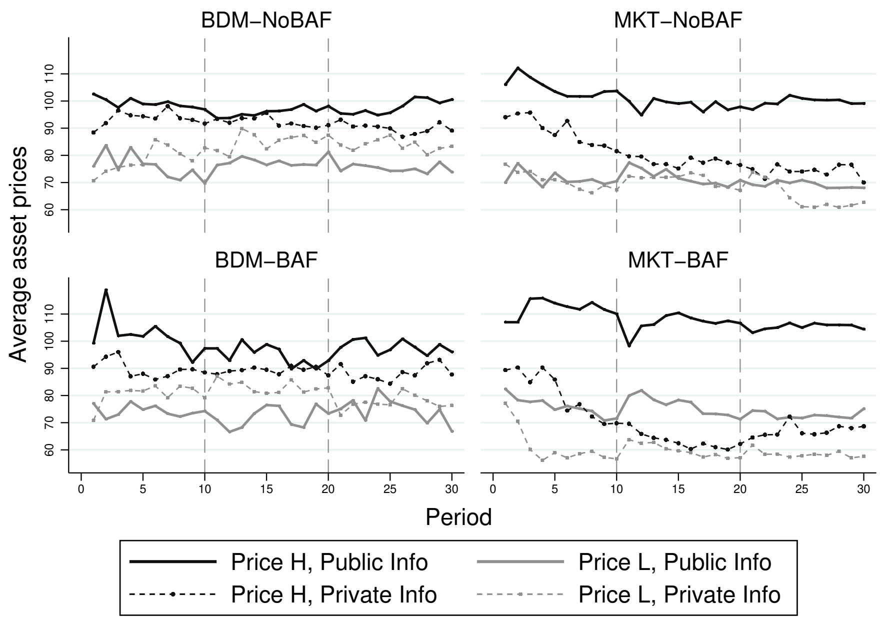


Figure B.1: Average prices of both assets by institution and bid-ask feedback.

B.5.2 Simulation of Prices if All Traders are Price-sensitive

This subsection contains simulations illustrating the price dynamics in artificial samples of only price sensitive traders. We focus on the sample of price-sensitive traders and ask how prices would have looked like if only the price-sensitive traders were trading. To do so we create a market price $p^{t=1}$ from the bids and asks submitted in the first period $t = 1$, of each repetition, by these traders only. We then simulate a price p^t for all $t > 1$ by using the average bids and asks of price-sensitive traders in periods following a price in the window $[p^{t-1} - 2.5, p^{t-1} + 2.5]$.[2] We do this exercise separately for assets H and L and for both the treatments with public and private information to avoid sample selection biases in this comparison. Figure B.2 reproduces Figure 2.1 by focusing on market prices in the full

[2]The reason that we use this window and not just the price p^{t-1} is that not all simulated prices also occur in the experimental data. The window chosen ensures that at least one observation for each of the simulated prices is generated. Also note that we cannot do this exercise for price-insensitive traders as, by definition, they do not react to past prices.

sample on the left (this part is identical to the right panel in Figure 2.1) and in the sample of price-sensitive traders on the right.

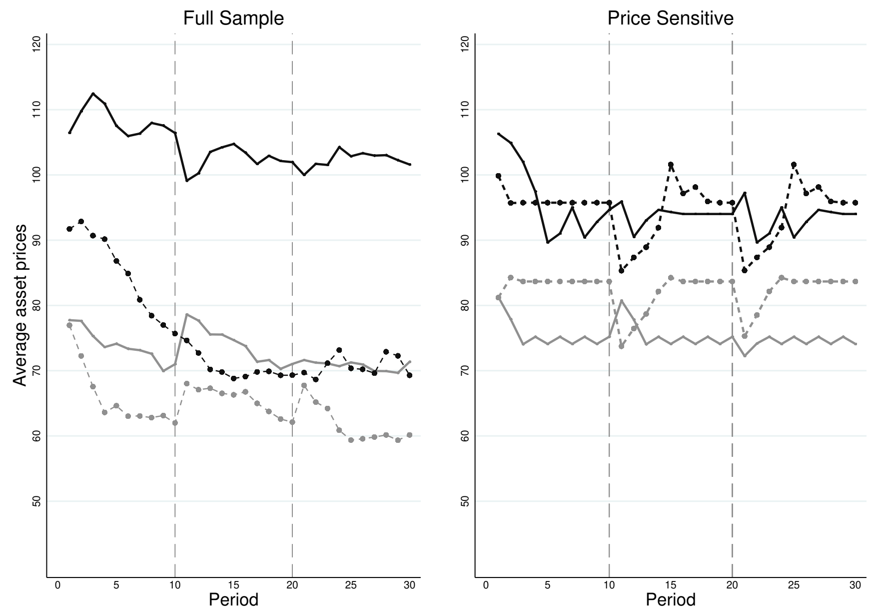


Figure B.2: Actual and simulated average market prices.
Note: Market prices for the whole sample (left panel) and simulated market prices for price sensitive traders only (right panel). Prices of asset *H* (black) and asset *L* (grey) are represented under both *Public information* (solid line) and *Private information* (dashed line).

The figure shows that the gap between market prices under public and private information almost disappears for price-sensitive traders for asset H. For asset L the gap persists but in the opposite direction, resembling the *BDM* treatment shown in Figure 2.1. Hence despite the fact that the presence of so many price-insensitive traders should make it difficult to learn for those who are price-sensitive, they are able to learn quite well. The predominant effect explaining the failure of information aggregation in the market seems to be the direct effect of price-insensitive traders on the market price. However, price-insensitive traders could also have an indirect effect on others' ability to make correct inference from prices, especially when their presence shifts prices "too much" (outside the range studied in Figure 2.1). Comparing prices in the public information condition between the full sample and that of price-sensitive traders shows only small differences. This suggests that the two samples differ indeed mostly in their ability to learn from prices and not e.g. in preferences which should also lead to differences with public information.

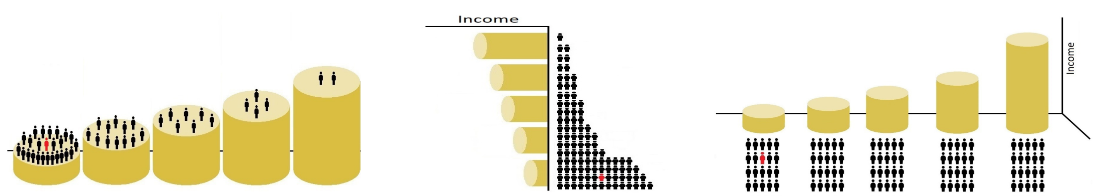
Appendix C

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

C.1 Additional Details Online Studies

C.1.1 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 C.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) (b) (c)

Figure C.1: (a)Version 1, (b) Version 2, (c) Version 3. 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.

C.1.2 Online Experiment: Belief in Meritocracy

This online experiment measured the effect of the prime on typical survey-based measures of belief in meritocracy. Specifically, we conduct an online survey where we use the exact same questionnaire and prime 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 C.1 shows some properties of this sample and compares them to our lab samples and the Next Steps 8 samples.

C.1.3 Online Experiment: Inequality Prime

This online experiment investigated in more detail the effectiveness of the inequality prime. In this survey we use ten different income distributions with levels of inequality ranging

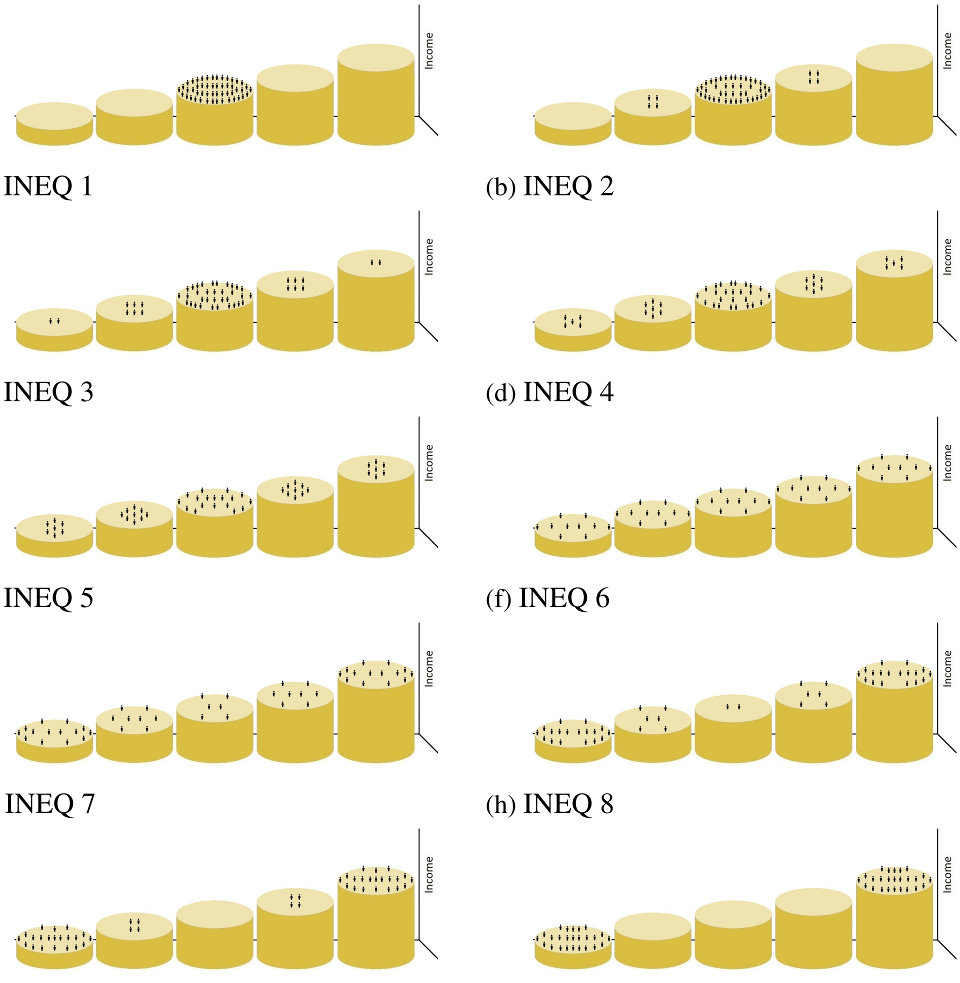
	NS 8	Lab	Lab	Online	Online
sample					
age	25.3	27.1	26.6	33.4	35.9
female	0.55	0.48	0.40	0.69	0.74
student	0.07	0.80	0.82	0.19	0.17
low income	0.57	0.39	0.30	0.56	0.61
high income	0.08	0.29	0.32	0.14	0.08
measurement					
causal	NO	YES	YES	YES	YES
incentivized	NO	YES	YES	NO	NO
type of prime	-	REL	INEQ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

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

from complete equality to very high inequality (see Figure C.2). We again prime participants using 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.

C.1.4 Online Experiment: Blame

This online experiment measured the effect of the primes on a non-incentivized measure of blame. We used the exact same questionnaire and prime 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.



(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 C.2: The distributions used in Survey III.

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 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). We have 107 respondents for the **REL** condition and 109 respondents for the **INEQ** condition. No participants were dropped from the sample. Table C.2 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	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 prime	-	REL	INEQ	REL	INEQ
N	6906	219	221	107	109

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

C.1.5 Online Experiment: Social Trust

This online experiment measured the effect of the prime on typical survey-based measures of social trust. Specifically, we conduct an online survey where we use the exact same questionnaire and prime as in the lab and the exact same outcomes (measures of 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 C.3 shows some properties of this sample and compares them to our lab samples and the Next Steps 8 samples.

C.2 Additional Details Lab Experiment

C.2.1 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.

	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 prime	-	REL	INEQ	REL	INEQ
N	6906	335	333	322	318

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

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.

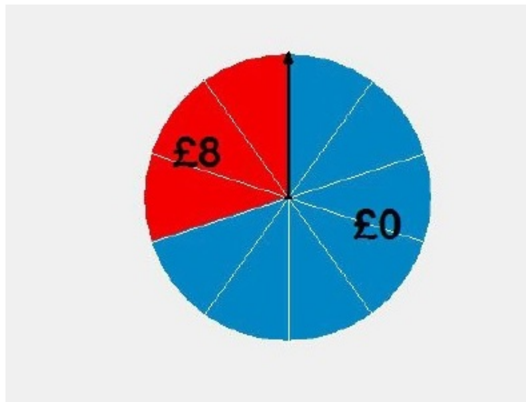


Figure C.3: Example of a wheel of fortune

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

C.2.2 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

C.2.3 Outcomes

C.2.3.0.1 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?

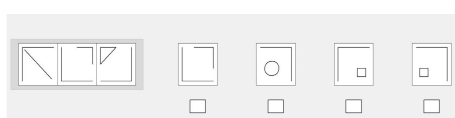


Figure C.4

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

C.2.3.0.2 Task 1b and Task 3

Figure C.5 shows an example of a matrix used in Task 1b and Figure C.6 shows how the coin toss was illustrated on the screen.

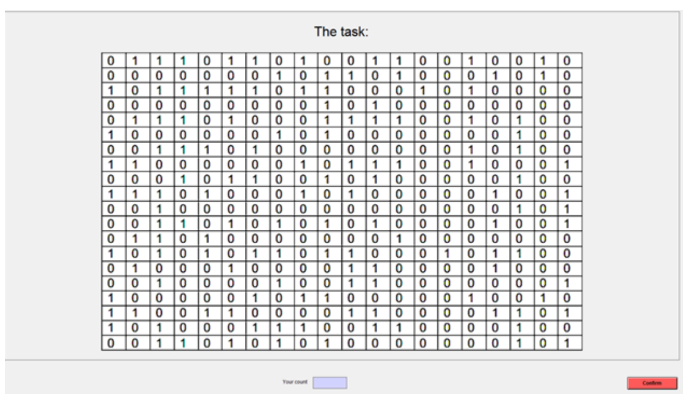


Figure C.5: Example of a Matrix for Task 1b and 3

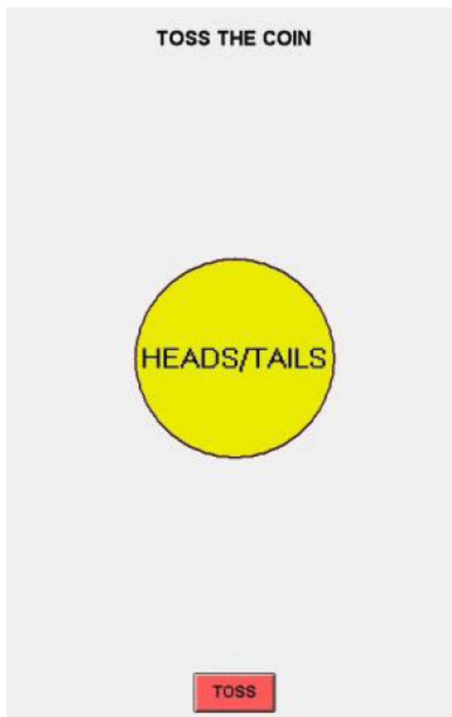


Figure C.6: Screenshot: Coin Toss.

C.3 Sample Characteristics

	Age	Gender	Student	A	B	C	S	Risk	Compet
high prime	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 prime	-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.4: Balancing tests **REL-MTB**.
Note: 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 prime	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 prime	-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.5: Balancing tests **REL-BMT**.
Note: 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 prime	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 prime	-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.6: Balancing tests **INEQ-MTB**.
Note: 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 prime	-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 prime	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.7: Balancing tests **INEQ-BMT**.
Note: 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.

C.4 Additional Results and Discussion

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

C.4.1 Pro-social behaviour

Is the increased level of social trust for those primed 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.

Appendix Table C.8 shows regression results where we regress the share of the pie allocated to others on the prime, income category and controls in the same format as above. We find that those primed 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 primed to a low relative position, a 15 percent increase. This difference is highly statistically significant

	Pro-Social Behavior					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime	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 C.8: Pro-social behaviour REL treatments.
Note: 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.

	Pro-Social Behavior					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime	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 C.9: Pro-social behaviour INEQ treatments.
Note: 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.

($p < 0.0001$) 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) 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 C.9 shows the effects of the inequality prime on pro-social behaviour. Being primed to higher levels of inequality seems to make participants less pro-social. The effect is, however, small statistically significant only at the 10% level.

C.4.2 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 prime and these questions was relatively long we wouldn't expect big effects. Indeed we find no effect of the prime 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 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

[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

Appendix C.10.

Appendix Table C.10 shows that being primed 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 prime for non-students, but students primed 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 primed to higher inequality does not per se have an effect on aspirations (Appendix Table C.11). 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 prime	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 C.10: Aspirations depending on income and whether participants are primed to high relative position.
Note: 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 prime	-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 C.11: Aspirations depending on income and whether participants are primed to high inequality.
Note: Controls are age, gender, risk attitude and self-reported degree of competitiveness.

C.5 Additional Tables

C.5.1 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 C.12: Social Trust in the European Value Survey.
Note: 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.

C.5.2 Additional Tables for Section 3.4

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

Table C.13: Approximate time between prime and elicitation of different outcomes.
Note: 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)	(0.0290)	(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 C.14: Demographic and Experiment-based covariates of main outcomes in lab experiment.

C.5.3 Additional Tables for Section 3.5

	Belief in Meritocracy					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime	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 C.15: Persistence of Effect on Belief in Meritocracy at Step 2.
Note: 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 prime	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 C.16: Persistence of Effect on Belief in Meritocracy at Step 3.
Note: 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 prime (β)	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

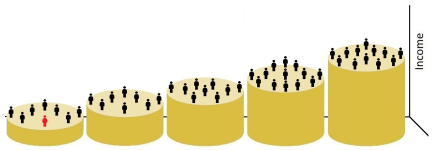
Table C.17: Survey measure of blame **REL** treatments.
Note: Extra Income Controls are fixed effects from initial income questionnaire. The smaller set includes questions 1-4, the larger set all eight questions. Other Controls are age, gender and student status. The larger set also includes risk aversion and a self reported competitiveness measure.

	Blame					
	(1)	(2)	(3)	(4)	(5)	(6)
high prime (β)	-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

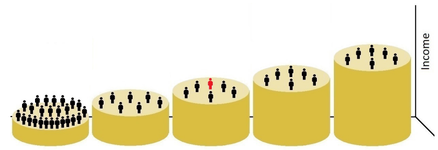
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.18: Survey measure of blame **INEQ** treatments.
Note: 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.

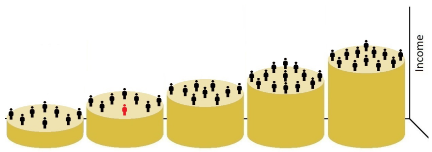
C.6 Additional Figures



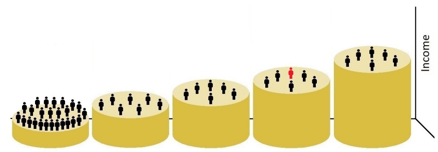
(a) REL Down Lo



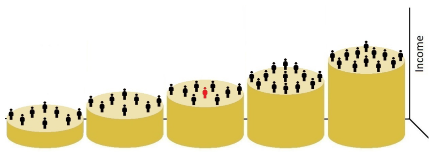
(b) REL Down Lo



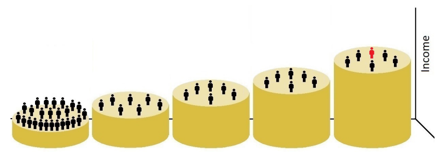
(c) REL Down Lo



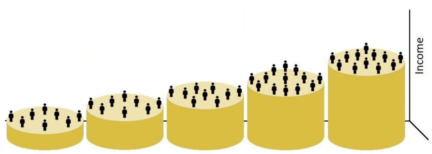
(d) REL Down Lo



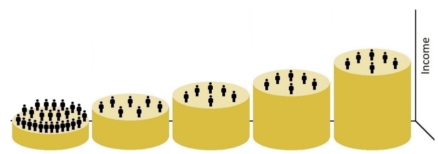
(e) REL Down Lo



(f) REL Down Lo

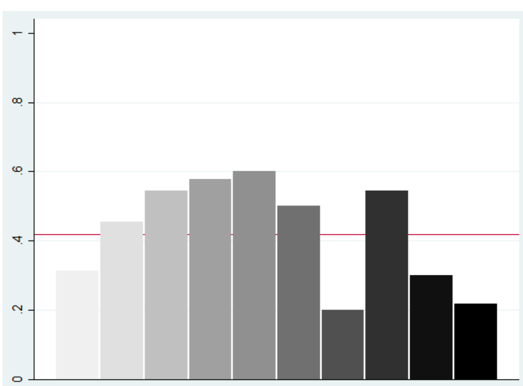


(g) REL Down Lo

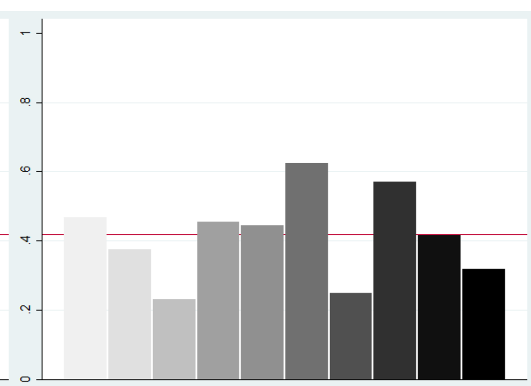


(h) REL Down Lo

Figure C.7: The pictures show the upwards and downwards primes for the different income categories as well as the primes used when relative position is *not* communicated.

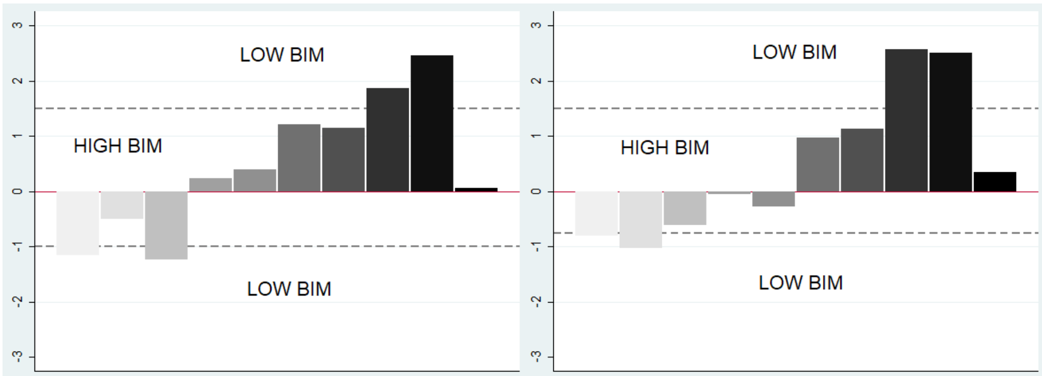


(a) REL-MTB



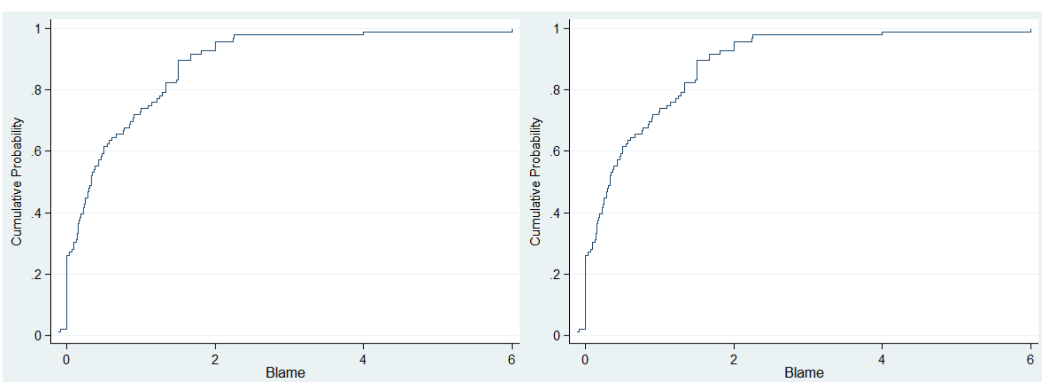
(b) INEQ-MTB

Figure C.8: 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 C.9: 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.



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

Figure C.10: CDF of blame.

References

Abeler, J., Becker, A., and Falk, A. (2014). Representative evidence on lying costs. *Journal of Public Economics*, 113:96–104.

Abeler, J., Nosenzo, D., and Raymond, C. (2019). Preferences for truth-telling. *Econometrica*, 87(4):1115–1153.

Al-Ubaydli, O. and List, J. A. (2013). On the generalizability of experimental results in economics: With a response to camerer. Working Paper 19666, National Bureau of Economic Research.

Al-Ubaydli, O., List, J. A., and Suskind, D. L. (2017). What can we learn from experiments? understanding the threats to the scalability of experimental results. *American Economic Review*, 107(5):282–86.

Alesina, A. and Ferrara, E. L. (2002). Who trusts others? *Journal of Public Economics*, 85:207–34.

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

Alesina, A., Stantcheva, S., and Teso, E. (2018). Intergenerational mobility and preferences for redistribution. *American Economic Review*, 108(2):521–554.

Almas, I., Cappelen, A., and Tungodden, B. (2019). Cutthroat capitalism versus cuddly socialism: Are americans more meritocratic and efficiency-seeking than scandinavians? *Journal of Political Economy*, in press.

Alvaredo, F., Atkinson, A., Piketty, T., and Saez, E. (2013). The top 1 percent in international and historical perspective. *Journal of Economic Perspectives*, 27(3):3–20.

Andersen, S., Gneezy, U., Kajackaite, A., and Marx, J. (2018). Allowing for reflection time does not change behavior in dictator and cheating games. *Journal of Economic Behavior & Organization*, 145:24–33.

Andreoni, J. (1988). Why free ride? strategies and learning in public goods experiments. *Journal of Public Economics*, 37(3):291–304.

Andreoni, J., Nikiforakis, N., and Stoop, J. (2017). Are the rich more selfish than trhe poor or do they just have more money? a natural field experiment. *NBER working paper*.

Ariely, D. (2012). *The (Honest) Truth About Dishonesty: How We Lie to Everyone - Especially Ourselves*. Harper Collins, UK.

Asparouhova, E., Bossaerts, P., Eguia, J., and Zame, W. (2015). Asset pricing and asymmetric reasoning. *Journal of Political Economy*, 123(1):66–120.

Atkinson, A. B. (2015). *Inequality - what can be done?* Harvard University Press, Cambridge, Massachusetts.

Bartels, L. (2016). *Unequal Democracy: The Political Economy of the New Gilded Age*. Princeton University Press.

Becker, G. M., DeGroot, M. H., and Marschak, J. (1964). Measuring utility by a single-response sequential method. *Behavioral Science*, 9(3):226–232.

Benabou, R. and Tirole, J. (2006). Belief in a just world and redistributive politics. *Quarterly Journal of Economics*, pages 699–746.

Berg, J., Dickhaut, J., and McCabe, K. (1995). Trust, reciprocity and social history. *Games and Economic Behavior*, 10(123):122–.

Berg, J. E., Nelson, F. D., and Rietz, T. A. (2008). Prediction market accuracy in the long run. *International Journal of Forecasting*, 24(2):285–300.

Bertrand, M. and Mullainathan, S. (2004). Are emily and greg more employable than lakisha and jamal? a field experiment on labor market discrimination. *American Economic Review*, 94(4):991–1013.

Bgue, L. (2002). Beliefs in justice and faith in people: just world, religiosity and interpersonal trust. *Pers. Individ. Dif.*, 32:375382.

Bjornskov, C. (2008). Social trust and fractionalization: A possible reinterpretation. *European Sociological Review*, 24(3):271283.

Bock, O., Baetge, I., and Nicklisch, A. (2014). hroot: Hamburg registration and organization online tool. *European Economic Review*, 71:117–120.

Bohm, P., Linden, J., and Sonnegard, J. (1997). Eliciting reservation prices: Becker-degroot-marschak mechanisms vs. markets. *The Economic Journal*, 107(443):1079–1089.

Bossaerts, P., Ghirardato, P., Guarnaschelli, S., and Zame, W. R. (2010). Ambiguity in asset markets: Theory and experiment. *The Review of Financial Studies*, 23(4):1325–1359.

Boyce, C., Brown, G., and Moore, S. (2010). Money and happiness: Rank of income, not income, affects life satisfaction. *Psychological Science*, pages 1–5.

Brocas, I., Carrillo, J., and Castro, M. (2015). The nature of information and its effect on bidding behavior: Laboratory evidence in a first price common value auction. *Journal of Economic Behavior & Organization*, 109:26–40.

Brooks, B., Hoff, K., and Pandey, P. (2018). Cultural impediments to learning to cooperate: An experimental study of high- and low-caste men in rural india. *PNAS*, 115(45):11385–11392.

Bucciol, A. and Piovesan, M. (2011). Luck or cheating? a field experiment on honesty with children. *Journal of Economic Psychology*, 32(1):73–78.

Bullock, H. E. (2008). Justifying inequality: A social psychological analysis of beliefs about poverty and the poor. In *The colors of poverty: Why racial and ethnic disparities persist*. Russell Sage Foundation.

Burks, S. V., Carpenter, J. P., and Verhoogen, E. (2003). Playing both roles in the trust game. *Journal of Economic Behavior & Organization*, 51(2):195–216.

Buttrick, N. and Oishi, S. (2017). The psychological consequences of income inequality. *Social and Personality Psychology Compass*, 11(3):12304.

Camerer, C. and Weigelt, K. (1991). Information mirages in experimental asset markets. *The Journal of Business*, 64(4):463–493.

Camerer, C. F. (2015). The promise and success of labfield generalizability in experimental economics: A critical reply to levitt and list. *Handbook of Experimental Economic Methodology*.

Cappelen, A., Drange, A., Soerensen, E. O., and Tungodden, B. (2007). The pluralism of fairness ideals: An experimental approach. *American Economic Review*, 97(3):818–827.

Caskey, J. A. (2009). Information in equity markets with ambiguity-averse investors. *The Review of Financial Studies*, 22(9):3595–3627.

Cerreia Vioglio, S. (2009). Maxmin expected utility on a subjective state space: Convex preferences under risk. *working paper*.

Chan, K., Mestelman, S., Moir, R., and Muller, R. (1996). The voluntary provision of public goods under varying income distributions. *The Canadian Journal of Economics*, 29(1):54–69.

Charron, N. e. a. (2017). Careers, connections, and corruption risks: Investigating the impact of bureaucratic meritocracy. *The Journal of Politics*, 79(1):89–104.

Christie, R. and Geis, F. L. (1970). *Studies in Machiavellianism*. New York: Academic Press.

Cohn, A., Fehr, E., and Maréchal, M. A. (2014). Business culture and dishonesty in the banking industry. *Nature*, 516:86–89.

Cohn, A., Gesche, T., and Maréchal, M. A. (2019a). Honesty in the digital age. *Working Paper*.

Cohn, A. and Maréchal, M. A. (2018). Laboratory measure of cheating predicts school misconduct. *The Economic Journal*, 128(615):2743–2754.

Cohn, A., Maréchal, M. A., and Noll, T. (2015). Bad boys: How criminal identity salience affects rule violation. *The Review of Economic Studies*, 82(4):1289–1308.

Cohn, A., Maréchal, M. A., Tannenbaum, D., and Zünd, C. L. (2019b). Civic honesty around the globe. *Science*, 365(6448):70–73.

Condie, S. and Ganguli, J. V. (2017). The pricing effects of ambiguous private information.

Conrads, J. and Lotz, S. (2015). The effect of communication channels on dishonest behavior. *Journal of Behavioral and Experimental Economics*, 58:88–93.

Copeland, T. E. and Friedman, D. (1991). Partial revelation of information in experimental asset markets. *Journal of Finance*, 46(1):265–295.

Corgnet, B., Deck, C., DeSantis, M., Hampton, K., and Kimbrough, E. O. (2019). Reconsidering rational expectations and the aggregation of diverse information in laboratory security markets. *mimeo*.

Corgnet, B., Deck, C., DeSantis, M., and Porter, D. (2018). Information (non)aggregation in markets with costly signal acquisition. *Journal of Economic Behavior and Organization*, 154:286–320.

Corgnet, B., Kujal, P., and Porter, D. (2013). Reaction to public information in markets: How much does ambiguity matter? *The Economic Journal*, 123(569):699–737.

Costa, P. T. and McCrae, R. R. (1992). *Revised NEO Personality Inventory (NEO PI-R) and NEO Five-Factor Inventory (NEO-FFI)*. Psychological Assessment Resources.

Cote, S., House, J., and Willer, R. (2015). High economic inequality leads higher-income individuals to be less generous. *PNAS - Proceedings of the National Academy of Sciences*, 112(52):15838–15843.

Crockett, S., Friedman, D., and Oprea, R. (2020). Naturally occurring preferences and general equilibrium: A laboratory study. *mimeo*.

Currie, J. (2011). Inequality at birth: Some causes and consequences. *American Economic Review*, 101(3):1–22.

Dai, Z., Galeotti, F., and Villeval, M. C. (2018). Cheating in the lab predicts fraud in the field: An experiment in public transportation. *Management Science*, 64(3):1081–1100.

Das, J., Holla, A., Mohpal, A., and Muralidharan, K. (2016). Quality and accountability in health care delivery: Audit-study evidence from primary care in india. *American Economic Review*, 106(12):3765–99.

Davidai, S. and Gilovich, T. (2016). The headwinds/tailwinds asymmetry: An availability bias in assessments of barriers and blessings. *Journal of Personality and Social Psychology*, 111(6):835–851.

De Bondt, W. and Thaler, R. (1985). Does the stock market overreact? *Journal of Finance*, 40:793–805.

Delhey, J. and Newton, K. (2005). Predicting cross-national levels of social trust: Global pattern or nordic exceptionalism? *European Sociological Review*, 21(4):311327.

DePaulo, B. M., Kashy, D. A., Kirkendol, S. E., Wyer, M. M., and Epstein, J. A. (1996). Lying in everyday life. *Journal of Personality and Social Psychology*, 70(5):979–995.

Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J., and Wagner, G. (2011). Individual risk attitudes: Measurement, determinants and bebehavior consequences. *Journal of the European Economic Association*, 9(3):522–550.

Donnellan, M. B., Oswald, F. L., Baird, B. M., and Lucas, R. E. (2006). The mini-ipip scales: Tiny-yet-effective measures of the big five factors of personality. *Psychological Assessment*, 18(2):192–203.

Drupp, M. A., Khadjavi, M., and Quaas, M. F. (2019). Truth-telling and the regulator. experimental evidence from commercial fishermen. *European Economic Review*, 120:103310.

Eckel, C. C. and Grossman, P. J. (2008). Forecasting risk attitudes: An experimental study using actual and forecast gamble choices. *Journal of Economic Behavior & Organization*, 68(1):1–17.

Epstein, L. G. and Schneider, M. (2010). Ambiguity and asset markets. *Annual Review of Financial Economics*, 2:315–346.

Erat, S. and Gneezy, U. (2012). White lies. *Management Science*, 58(4):723–733.

Falk, A. and Heckman, J. J. (2009). Lab experiments are a major source of knowledge in the social sciences. *Science*, 326(5952):535–538.

Fama, E. (1965). The behavior of stock market prices. *Journal of Business*, 38:34–106.

Fama, E. (1970). Efficient capital markets: A review of theory and empirical work. *Journal of Finance*, 25:383–417.

Fehr, D., Mollerstrom, J., and Perez-Truglia, R. (2019). Your place in the world: The demand for national and global redistribution. *NBER working paper*, 26555.

Fischbacher, U. (2007a). z-tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, 10(2):171–178.

Fischbacher, U. (2007b). z-tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, 10(2):171–178.

Fischbacher, U. and Föllmi-Heusi, F. (2013). Lies in disguise–an experimental study on cheating. *Journal of the European Economic Association*, 11(3):525–547.

Fong, C. (2001). Social preferences, self-interest, and the demand for redistribution. *Journal of Public Economics*, 82(2):225–246.

Forsythe, R. and Lundholm, R. (1990). Information aggregation in an experimental market. *Econometrica*, 58(2):309–347.

Forsythe, R., Nelson, F., Neumann, G., and Wright, J. (1992). Anatomy of an experimental political stock marke. *American Economic Review*, 82:1142–1161.

Friedman, D., Harrison, G., and Salmon, J. (1984). The informational efficiency of experimental asset markets. *Journal of Political Economy*, 92(3):349–408.

Furnham, A. (2003). Belief in a just world: research progress over the past decade. *Personality and Individual Differences*, pages 795–817.

Gächter, S. and Schulz, J. F. (2016). Intrinsic honesty and the prevalence of rule violations across societies. *Nature*, 531:496–499.

Gächter, S. and Thöni, C. (2005). Social learning and voluntary cooperation among like-minded people. *Journal of the European Economic Association*, 3(2/3):303–314.

Gaechter, S., Mengel, F., Tsakas, E., and Vostroknutov, A. (2017). Growth and inequality in public good provision. *Journal of Public Economics*, 50:1–13.

Galeotti, F., Kline, R., and Orsini, R. (2017). When foul play seems fair: Exploring the link between just deserts and honesty. *Journal of Economic Behavior and Organization*, 142:451–467.

Gerlach, P., Teodorescu, K., and Hertwig, R. (2019). The truth about lies: A meta-analysis on dishonest behavior. *Psychological Bulletin*, 145(1):1–44.

Gibson, R., Tanner, C., and Wagner, A. F. (2013). Preferences for truthfulness: Heterogeneity among and within individuals. *American Economic Review*, 103(1):532–48.

Gilboa, I. and Schmeidler, D. (1989). Maxmin expected utility with non-unique prior. *Journal of Mathematical Economics*, 18:141–153.

Gilens, M. (1999). *Why Americans Hate Welfare*. University of Chicago Press, Chicago, IL.

Gneezy, U. (2005). Deception: The role of consequences. *American Economic Review*, 95(1):384–394.

Gneezy, U., Kajackaite, A., and Sobel, J. (2018). Lying aversion and the size of the lie. *American Economic Review*, 108(2):419–53.

Gould, E. (2017). The state of american wages 2016. *Economic Policy Institute*.

Greiner, B. (2015). Subject pool recruitment procedures: organizing experiments with orsee. *Journal of the Economic Science Association*, 1(1):114–125.

Greiner, B., Ockenfels, A., and Werner, P. (2011). The dynamic interplay of inequality and trust - an experimental analysis. *Journal of Economic Behavior and Organization*, 81(2):355–365.

Grosskopf, B., Rentschler, L., and Sarin, R. (2018). An experiment on first-price common-value auctions with asymmetric information structures: The blessed winner. *Games and Economic Behavior*, 109:40–64.

Guidolin, M. and Rinaldi, F. (2013). Ambiguity in asset pricing and portfolio choice: a review of the literature. *Theory and Decision*, 74(2):183–217.

Gurdal, M., Miller, J., and Rustichini, A. (2013). Why blame? *Journal of Political Economy*, 121(6).

Gustavsson, M. and Jordahl, H. (2008). Inequality and trust in sweden: Some inequalities are more harmful than others. *Journal of Public Economics*, 92(1-2):348–365.

Hanna, R. and Wang, S.-Y. (2017). Dishonesty and selection into public service: Evidence from india. *American Economic Journal: Economic Policy*, 9(3):262–90.

Hanson, R., Oprea, R., and Porter, D. (2006). Information aggregation and manipulation in an experimental market. *Journal of Economic Behavior and Organization*, 60(4):449–459.

Hasenfeld, Y. and Rafferty, J. (1989). The determinants of public attitudes toward the welfare state. *Social Forces*.

Haushofer, J. and Fehr, E. (2014). On the psychology of poverty. *Science*, 344(6186):862–867.

Healy, P., Linardi, S., and Lowery, J.R., L. J. (2010). Prediction markets: Alternative mechanisms for complex environments with few traders. *Management Science*, 56(11):1977–1996.

Hermann, D. and Mußhoff, O. (2019). I might be a liar, but i am not a thief: An experimental distinction between the moral costs of lying and stealing. *Journal of Economic Behavior & Organization*, 163:135–139.

Holm, H. and Danielson, A. (2005). Tropic trust versus nordic trust: Experimental evidence from tanzania and sweden. *Economic Journal*.

Horvath, G., Kovarik, J., and Mengel, F. (2012). Limited memory can be beneficial for the evolution of cooperation. *Journal of Theoretical Biology*, 300:193–205.

Houston, J., Harris, P., McIntire, S., and Francis, D. (2002). Revising the competitiveness index using factor analysis. *Psychological Reports*, 90(1):31–34.

Huber, J., Kirchler, M., and Stefan, M. (2014). Experimental evidence on varying uncertainty and skewness in laboratory double-auction markets. *Journal of Economic Behavior & Organization*, 107:798 – 809.

Hübler, O., Menkhoff, L., and Schmidt, U. (2018). Who is cheating? the role of attendants, risk aversion, and affluence. *DIW Berlin Discussion Paper No. 1736*.

Ito, T., Lyons, R., and Melvin, M. (1998). Is there private information in the fx market? the tokyo experiment. *Journal of Finance*, 53(3):1111–1130.

Jacobsen, C., Fosgaard, T. R., and Pascual-Ezama, D. (2018). Why do we lie? a practical guide to the dishonesty literature. *Journal of Economic Surveys*, 32(2):357–387.

Jegadeesh, N. and Titman, S. (1993). Returns to buying winners and selling losers: implications for stock market efficiency. *Journal of Finance*, 48:65–91.

Jiang, T. (2013). Cheating in mind games: The subtlety of rules matters. *Journal of Economic Behavior & Organization*, 93:328–336.

Jimenez-Jimenez, N., Molis, E., and Solano-Garcia, A. (2020). The effect of initial inequality on meritocracy: A voting experiment on tax redistribution. *Journal of Economic Behavior and Organization*, 175:380–394.

Jost, J., Banaji, M., and Nosek, B. (2004). A decade of system justification theory: Accumulated evidence of conscious and unconscious bolstering of the status quo. *Political Psychology*, 25:881919.

Jost, J. and Hunyady, O. (2003). The psychology of system justification and the palliative function of ideology. *European Review of Social Psychology*, 13(1):111–153.

Jost, J. and Hunyady, O. (2005). Antecedents and consequences of system-justifying ideologies. *Current Directions in Psychological Science*.

Kajackaite, A. and Gneezy, U. (2017). Incentives and cheating. *Games and Economic Behavior*, 102:433–444.

Karadja, M., Mollerstrom, J., and Seim, D. (2017). Richer (and holier) than thou? the effect of relative income improvements on demand for redistribution. *Review of Economics and Statistics*, 99(2):201–212.

Kaur, S., Mullainathan, S., Schilbach, F., and Oh, S. (2019). Does financial strain lower worker productivity? *working paper*.

Kelly, N. and Enns, P. (2010). Inequality and the dynamics of public opinion: The self-reinforcing link between economic inequality and mass preferences. *American Journal of Political Science*, 54(4):855–870.

Kessler, J. B. and Vesterlund, L. (2015). The external validity of laboratory experiments: Qualitative rather than quantitative effects. *Handbook of Experimental Economic Methodology*.

Kőszegi, B. and Rabin, M. (2006). A model of reference-dependent preferences. *The Quarterly Journal of Economics*, 121(4):1133–1165.

Korndoerfer, M., Egloff, B., and Schmukle, S. (2015). A large scale test of the effect of social class on prosocial behavior. *PLoS ONE*, 10(7):e0133193.

Kuziemko, I., Norton, M., Saez, E., and Stantcheva, S. (2015). How elastic are preferences for redistribution? evidence from randomized survey experiments. *The American Economic Review*, 105(4):1478–1508.

Langer, E. J. (1975). The illusion of control. *Journal of Personality and Social Psychology*, 32(2):311–328.

Ledyard, J., Hanson, R., and Ishikida, T. (2009). An experimental test of combinatorial information markets. *Journal of Economic Behavior and Organization*, 69(2):182 – 189.

Lerner, M. (1980). The belief in a just world. In Lerner, M. J., editor, *The Belief in a Just World: A Fundamental Delusion*, pages 9–30. NY: Springer, New York.

Levitt, S. D. and List, J. A. (2007). What do laboratory experiments measuring social preferences reveal about the real world? *The Journal of Economic Perspectives*, 21(2):153–174.

Magni, G. (2020). Economic inequality, immigrants and selective solidarity: From perceived lack of opportunity to in-group favoritism. *British Journal of Political Science*, pages 1–24.

Mani, A., Mullainathan, S., Shafir, E., and Zhao, J. (2013). Poverty impedes cognitive function. *Science*, 341(6149):976–980.

Mazar, N., Amir, O., and Ariely, D. (2008). The dishonesty of honest people: A theory of self-concept maintenance. *Journal of Marketing Research*, 45(6):633–644.

Mazar, N. and Ariely, D. (2006). Dishonesty in everyday life and its policy implications. *Journal of Public Policy & Marketing*, 25(1):117–126.

McCoy, S. and Major, B. (2007). Priming meritocracy and the psychological justification of inequality. *Journal of Experimental Social Psychology*, 43:341–351.

Mitchell, R. and Popham, F. (2008). Effect of exposure to natural environment on health inequalities: an observational population study. *The Lancet*, 372(9650):1655–1660.

Mo, C. and Conn, K. (2018). When do the advantaged see the disadvantages of others? a quasi-experimental study of national service. *American Political Science Review*, 112(4):721–741.

Mollerstrom, J., Reme, B.-A., and Sorensen, E. (2015). Luck, choice and responsibility - an experimental study of fairness views. *Journal of Public Economics*, 131:33–40.

Morris, M. and Western, B. (1999). Inequality in earnings at the close of the twentieth century. *Annual Review of Sociology*, 25:623–657.

Muramatsu, N. (2003). Countylevel income inequality and depression among older americans. *Health services research*, 38:1863–1884.

Nannestad, P. (2008). What have we learned about generalized trust, if anything? *Annual Review of Political Science*, 11:413–466.

Newman, B. (2016). Breaking the glass ceiling: Local gender-based earnings inequality and women's belief in the american economic dream. *American Journal of Political Science*, 60(4):1006–1025.

Newman, B., Johnston, C., and Lown, P. (2015). False consciousness or class awareness? local income inequality, personal economic position, and belief in american meritocracy. *American Journal of Political Science*, 59(2):326–340.

Ngangoué, K. (2017). Trading under ambiguity and the effect of learning.

Nishi, A., Shirado, H., Rand, D. G., and Christakis, N. (2015). Inequality and visibility of wealth in experimental social networks. *Nature*, 526.

O'Brien, J. and Srivastava, S. (1991). Dynamic stock markets with multiple assets: An experimental analysis. *The Journal of Finance*, 46(5):1811–1838.

Ostrom, E., Gardner, R., and Walker, J. (1994). *Rules, Games and Common Pool Ressources*. University of Michigan Press, Ann Arbor.

Page, L. and Siemroth, C. (2017). An experimental analysis of information aquisition in prediction markets. *Games and Economic Behavior*, 101:354–378.

Page, L. and Siemroth, C. (2018). How much information is incorporated in financial asset prices? experimental evidence. *SSRN working paper*, 3130307.

Pascual-Ezama, D., Fosgaard, T. R., Cardenas, J. C., Kujal, P., Veszteg, R., Gil-Gmez de Liaño, B., Brian, G., Weichselbaumer, D., Hilken, K., Antinyan, A., Delnoij, J., Proestakis, A., D. Tira, M., Pratomo, Y., Jaber-Lpez, T., and Braas-Garza, P. (2015). Context-dependent cheating: Experimental evidence from 16 countries. *Journal of Economic Behavior & Organization*, 116:379–386.

Pickett, K. and Wilkinson, R. (2010). *The spirit level*. Penguin, London.

Piff, P., Stancato, D., Cote, S., Mendoza-Denton, R., and Keltner, D. (2012). Higher social class predicts unethical behavior. *PNAS- Proceedings of the National Academy of Sciences*, 109(11):4086–4091.

Pittarello, A., Rubaltelli, E., and Motro, D. (2016). Legitimate lies: The relationship between omission, commission, and cheating. *European Journal of Social Psychology*, 46(4):481–491.

Plott, C. and Chen, K. Y. (2002). Information aggregation mechanisms: Concept, design and implementation for a sales forecasting problem. *working paper CalTech*.

Plott, C. and Smith, V. (2008a). *Handbook of Experimental Economic Results*. Elsevier.

Plott, C. and Smith, V. L. (2008b). *Handbook of experimental economics results*, volume 1. Elsevier.

Plott, C. and Sunder, S. (1982). Efficiency of experimental security markets with insider information: An application of rational expectations models. *Journal of Political Economy*, 90(4):663–698.

Plott, C. and Sunder, S. (1988). Rational expectations and the aggregation of diverse information in laboratory security markets. *Econometrica*, 56(5):1085–1118.

Plott, C., Wit, J., and Yang, W. C. (2003). Parimutuel betting markets as information aggregation devices: experimental results. *Economic Theory*, 22(2):311–351.

Potters, J. and Stoop, J. (2016). Do cheaters in the lab also cheat in the field? *European Economic Review*, 87:26–33.

Putnam, R. D. (2000). *Bowling alone: The collapse and revival of American community.* Simon & Schuster., New York.

Quispe-Torreblanca, E., Brown, G. D. A., Boyce, C. J., M.Wood, A., and Neve, J.-E. D. (2020). Inequality and social rank: Income increases buy more life satisfaction in more equal countries. *Personality and Social Psychology Bulletin*, pages 1–21.

Radner, R. (1979). Rational expectations equilibrium: Generic existence and the information revealed by prices. *Econometrica*, 47(3):655–673.

Reny, P. and Perry, M. (2006). Toward a strategic foundation for rational expectations equilibrium. *Econometrica*, 74:1231–1269.

Reuben, E. and Riedl, A. (2013). Enforcement of contribution norms in public good games with heterogeneous populations. *Games and Economic Behavior*, 77:122–137.

Rosenbaum, S. M., Billinger, S., and Stieglitz, N. (2014). Let's be honest: A review of experimental evidence of honesty and truth-telling. *Journal of Economic Psychology*, 45:181–196.

Ross, L. and Nisbett, R. (1991). *The person and the situation: Perspectives of social psychology.* McGraw-Hill.

Roth, C. and Wohlfarth, J. (2018). Experienced inequality and preferences for redistribution. *Journal of Public Economics*, 167:251–262.

Sadrieh, A. and Verbon, H. (2006). Inequality, cooperation and growth: an experimental study. *European Economic Review*, 50(5):1197–1222.

Sarin, R. K. and Weber, M. (1993). Effects of ambiguity in market experiments. *Management Science*, 39(5):602–615.

Scheier, M. F., Carver, C. S., and Bridges, M. W. (1994). Distinguishing optimism from neuroticism (and trait anxiety, self-mastery, and self-esteem): a reevaluation of the life orientation test. *Journal of personality and social psychology*, 67(6):1063.

Schlag, K. H. and van der Weele, J. (2015). A method to elicit beliefs as most likely intervals. *Judgement and Decision Making*, 10(5):456–468.

Schmidt, U., Friedl, A., and de Miranda, K. L. (2015). Social comparison and gender differences in risk taking. (2011).

Schmukle, S., Korndoerfer, M., and Egloff, B. (2019). No evidence that economic inequality moderates the effect of income on generosity. *PNAS - Proceedings of the National Academy of Sciences*, 1167(20):9790–9795.

Scholes, M. (1972). The market for securities: Substitution versus price pressure and effects of information on share prices. *Journal of Business*, 45:179–211.

Seligman, M. (1972). Learned helplessness. *Annual Review Med.*

Slemrod, J. (2007). Cheating ourselves: The economics of tax evasion. *Journal of Economic Perspectives*, 21(1):25–48.

Smeets, P., Bauer, R., and Gneezy, U. (2015). Giving behaviour of millionaires. *PNAS-Proceedings of the National Academy of Sciences*, pages 1–4.

Smith, H. and Pettigrew, T. (2015). Advances in relative deprivation theory and research. *Social Justice Research*, 28:1–6.

Spann, M. and Skiera, B. (2009). Sports forecasting: a comparison of the forecast accuracy of prediction markets, betting odds and tipsters. *Journal of Forecasting*, 28:55–72.

Stiglitz, J. (2002). *The price of inequality*. Allen Lane, London.

Trautmann, S., van de Kuilen, G., and Zeckhauser, R. (2013). Social class and (un)ethical behavior: A framework, with evidence from a large population sample. *Perspectives on Psychological Science*, 8(5).

Trump, K.-S. (2017). Income inequality influences perceptions of legitimate income differences. *British Journal of Political Science*, pages 1–24.

UCL (2018). Centre for longitudinal studies. next steps: Sweeps 1-8, 2004-2016. [data collection]. 14th edition. *UK Data Service*, SN:5545.

Uslaner, E. (2002). *The Moral Foundations of Trust*. Cambridge University Press.

Uslaner, E. (2018). *The Oxgord Handbook of Social and Political Trust*. Oxford University Press.

van den Bos, K. (2020). Unfairness and radicalization. *Annual Review of Psychology*, 71:563–588.

van Dijk, F., Sonnemans, J., and van Winden, F. (2002). Social ties in a public good experiment. *Journal of Public Economics*, 85:275–299.

Wolak, J. and Peterson, D. (2020). The dynamic american dream. *American Journal of Political Science*, 64(4):968–981.

Xiao, E. and Bicchieri, C. (2010). When equality trumps reciprocity. *Journal of Economic Psychology*.

Xu, P. and Garand, J. (2010). Economic context and americans' perceptions of income inequality. *Social Sciences Quarterly*, 91(5):1220–1241.

Zuckerman, M. and Gerbasi, K. (1977). Belief in a just world and trust. *J. Res. Pers.*, 11:306317.