Growth and Inequality: Experimental Evidence on How Misperceptions Affect Redistribution

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Abstract

Many policies affect future inequality, from taxation to investments in children's equal access to education. Citizens' support for such policies may therefore depend on their beliefs about how inequality will evolve over time. In three large-scale experiments, I examine how well individuals predict future inequality in the presence of economic growth, and I examine how beliefs about inequality matter for people's preferences for redistribution. I show that although most people underestimate future increases in inequality, this is inconsequential as beliefs about inequality do not influence preferences for redistribution. Rather, what matters is whether individuals know if redistribution is costly for themselves.

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1 Introduction

Many policies affect future inequality. For instance, when voters decide on a level of redistribution, they not only change the current allocation of income but also how wealth accumulates over time. And when voters decide on intergenerational policies such as investments in children's equal access to education, their choices will influence future inequality in human capital and income. Whether voters support such policies is therefore likely to depend on their beliefs about how inequality will evolve over time. Beliefs are central to people's attitudes and behaviour (Thomas and Thomas, 1928; Stantcheva, 2021), and previous studies show that a person's beliefs about his future income can influence his preferences for redistribution (cf. the prospect of upward mobility hypothesis, Hirschman and Rothschild, 1973; Benabou and Ok, 2001; Cojocaru, 2014). Thus, if people have erroneous beliefs about future inequality, it may cause them to support suboptimal policies, which leads to substantial welfare losses as it is often more difficult and expensive to equalise outcomes later in life (Heckman, 2006; Bhalotra et al., 2017; Hjort et al., 2017; Bütikofer et al., 2019; Schiariti et al., 2021).

But it is not obvious how (erroneous) beliefs about inequality influence policy support as the perceived level of inequality affects both the benefits and the costs of redistribution: on the one hand, if an individual believes that inequality will rise drastically, then he perceives a greater need for redistribution to counteract the increase in inequality. But on the other hand, greater inequality also implies that it is more expensive for a net contributor to redistribute as his earnings make up a larger share of the tax base. The total effect of erroneous beliefs will thus depend on whether misperceiving the benefits or the personal costs of redistribution matter the most.

In this paper, I examine people's ability to predict future inequality in the presence of economic growth, and I study how people's (possibly erroneous) forecasts affect their preferences for redistribution in an incentivised voting experiment. To the best of my knowledge, this is the first study to take a forward-looking perspective and examine how beliefs about future inequality influence support for redistributive policies.[1] I find that most people underestimate future increases in inequality, but this is inconsequential as beliefs about inequality do not influence the demand for redistribution. Rather, what matters is whether individuals know if redistribution is costly for themselves.

[1] A related literature demonstrates that individuals often misperceive the current extent of inequality in income, wealth, and education (Kluegel and Smith, 1986; Hauser and Norton, 2017), and studies on social preferences have examined intertemporal preferences for altruism and collaboration (e.g., Breman, 2011; Andreoni and Serra-Garcia, 2019; Chopra et al., 2021). But none of these studies consider forecasts and possible misperceptions about future inequality, which is the focus of this paper.

I use an experimental approach to obtain a causal understanding of how perceived increases in inequality influence preferences for redistribution in incentivised decisions. The experimental approach also makes it possible to control a number of critical features, including uncertainty about growth rates and shocks to individuals' incomes, which would not be possible with observational data. The paper reports the results of three incentivised experiments, and I refer to these as Study 1, Study 2, and Study 3.

In Study 1, subjects predict how inequality develops in a group where all incomes grow exponentially over multiple rounds. Subjects' forecasts provide a measure of their ability to anticipate how inequality evolves over time. Then, subjects decide on how to redistribute earnings in the group in a tax-transfer scheme. To measure the behavioural effects of misperceiving future inequality, I vary in a between-subjects design whether subjects are informed about post-redistribution earnings based on their own forecast (treatment *Forecast*) or the actual earnings (*Realized*). Comparing subjects' preferred tax rate in *Realized* and *Forecast* yields the causal effect of making erroneous forecasts on subjects' desired level of redistribution. This effect may stem from two different errors in how people make forecasts: first, subjects may wrongly estimate how the incomes develop relative to each other, leading to an error in the forecasted income ratios. Second, subjects may wrongly estimate how much the incomes increase, leading to an error in the forecasted income levels. Together, the ratio and level errors comprise the total effect of making erroneous forecasts. To decompose the total effect, I introduce a third treatment, *Ratio*, in Study 1. In this treatment, subjects observe the true income ratios, but the incomes are at the level of the subjective forecasts.

My pre-registered hypotheses are that subjects underestimate the growth in absolute inequality in Study 1 but correctly estimate relative inequality. Incomes grow at uniform rates in Study 1 (changed in Study 2), and this leads to an exponential increase in absolute inequality, whereas relative inequality does not change. The hypotheses are based on the literature within cognitive psychology that demonstrates how people tend to underestimate exponential growth (cf. exponential growth bias, Wagenaar and Sagaria, 1975).

To form hypotheses about how inequality beliefs influence redistributive preferences, I build a stylized model of inequality aversion in the presence of growth, taking into account that people may make biased forecasts. The model predicts that people who underestimate future inequality support less redistribution than they would if they correctly estimated future inequality. For the experiment, this means that subjects are predicted to vote for a lower tax rate in *Forecast* than in *Realized*. As subjects are hypothesised to make correct forecasts of relative inequality in Study 1, the model predicts that subjects in *Ratio* should vote similarly to subjects in *Forecast*.

I find that subjects underestimate the increase in absolute inequality and that they are markedly better at predicting that there is no change in relative inequality. But beliefs about the level of inequality do not influence subjects' preferred tax rate – in contrast to the predictions of the theoretical model. Rather, erroneous forecasts influence the demand for redistribution only if subjects engage in one of two types of misperceptions of personal costs: first, if net contributors wrongly believe that they will gain from redistribution, they on average vote for a tax rate that is 50 percentage points higher compared to net contributors who realise that they will lose from redistribution. Second, if net contributors wrongly believe that redistribution comes at negligible personal costs, they on average vote for a tax rate that is 14 percentage points higher than net contributors who realise that redistribution comes at a cost. Controlling for these two misperceptions, subjects vote for the same tax rates across the three treatments, and this means that they are willing to give up the same share of their income across the different treatments and perceived levels of inequality. This implies that subjects who perceive a greater level of inequality are willing to pay more for redistribution in absolute amounts. Subjects are on average willing to pay (in experimental currency units) $267 ($176) more for redistribution in *Realized* than in *Forecast* (*Ratio*), indicating a greater concern for inequality. But these greater concerns for inequality cancel out with the increased personal costs, leaving the tax rate unchanged.

Study 2 examines whether these results are robust to a setting in which inequality grows in a different way over time. Earnings in Study 2 increase at a larger rate the greater the initial endowment is, and this implies that not only absolute but also relative inequality increases over time. In this case, I find that subjects underestimate the increase in both absolute and relative inequality. But supporting the results from Study 1, the erroneous forecasts only cause subjects to vote for higher tax rates if they wrongly believe that they gain from taxation (increased tax rate of 25 percentage points). Thus, Study 2 alleviates any concern that the irrelevance of inequality beliefs for the demand for redistribution in Study 1 was caused by voters caring about relative and not absolute inequality.

Study 3 examines whether Study 1 and 2 truly capture that beliefs about inequality are irrelevant for subjects' preferred level of redistribution or if subjects are influenced by the act of making a forecast. In this experiment, subjects vote on redistribution in a group without making forecasts. Instead, subjects decide on how to redistribute earnings from the *Realized* treatment or from forecasts made in a pilot study for Study 1. I replicate the findings from Study 1 as beliefs about inequality only influence preferred tax rates if people believe that redistribution comes at negligible personal costs (increased tax rate of 30 percentage points). This demonstrates that the results in Study 1 and 2 are driven by the perceived level of future inequality rather than possible confounding effects of e.g. reference points or uncertainty about one's forecast.

This paper makes three main contributions. First, the paper is (to my knowledge) the first to take a forward-looking perspective and examine how subjective inequality forecasts may influence demand for redistribution. Numerous studies examine distributional preferences in static voting experiments (e.g., Tyran and Sausgruber, 2006; Messer et al., 2010; Agranov and Palfrey, 2015; Sauermann, 2018). In addition, some studies add a temporal perspective to examine the temporal discounting of altruism and collaboration (e.g., Rogers and Bazerman, 2008; Breman, 2011; Andreoni and Serra-Garcia, 2019; Chopra et al., 2021) and the extent to which people habituate to inequality over time (e.g., Lerner, 1980; Roth and Wohlfart, 2018; Mijs, 2019).[2] I abstract from these factors and focus instead on how beliefs about future inequality influence preferences for redistribution.

Second, this paper contributes to the literature on misperceptions of inequality by demonstrating that the exponential nature of economic growth may cause people to underestimate inequality. The temporal aspect is largely absent from the literature, which shows that individuals often hold wrong beliefs about the extent of inequality in wealth, income, and education (e.g., Bartels, 2005; Osberg and Smeeding, 2006; Norton and Ariely, 2011; Niehues, 2014; Gugushvili et al., 2020; Lergetporer et al., 2020). Only few studies examine the causes of these misperceptions. Knell and Stix (2020) explain that people might extrapolate from the individuals they encounter in their everyday lives, leading to self-centered reference groups (see also Cruces et al., 2013; Balcells et al., 2015; Londoño-Vélez, 2022).[3] Moreover, Karadja et al. (2017) find that media consumption correlates with having less biased views about one's relative position in society (see also Diermeier et al., 2017; Phillips et al., 2020). I show that an additional factor may be that people do not fully understand the (exponential) development in inequality over time. Intuitively, this misunderstanding will lead individuals to underestimate inequality unless they continuously update their beliefs.

Third, the current paper extends the literature on how informing individuals about in-

[2]The literature describes that such habituation may take place if (i) people adopt different fairness principles depending on what suits their own interest (Messick and Sentis, 1979; Rodriguez-Lara and Moreno-Garrido, 2012, but see Cappelen et al., 2007), (ii) they adapt their meritocratic beliefs as a result of self-justification (Deffains et al., 2016; Cassar and Klein, 2019) or to maintain just-world beliefs (Lerner and Simmons, 1966; Bénabou and Tirole, 2006a; Trump, 2018), or (iii) because they live in homogeneous areas in which meritocracy is more important (Newman et al., 2015; Wu and Chou, 2017). Yet, García-Castro et al. (2020) find that people who perceive more cues about inequality and poverty on a daily basis are accepting of inequality.

[3]Relatedly, studies suggest that neighbourhood indicators of inequality are better predictors of perceived inequality than national-level indicators (Xu and Garand, 2010; Page and Goldstein, 2016; Franko, 2017; Wang et al., 2018).

equality influences redistributive preferences as I study these effects in an incentivised and controlled setting. Earlier studies have found that providing information increases people's concerns about inequality, but it has limited effects on stated preferences unless people hold wrong beliefs about whether they gain or lose from redistribution (e.g., Kuziemko et al., 2015; Ballard-Rosa et al., 2017; McCall et al., 2017; Engelhardt and Wagener, 2018; Trump and White, 2018; Hvidberg et al., 2020; Fehr et al., 2021). Typical explanations for the general lack of effects on preferences include that citizens (i) think policies are ineffective, (ii) distrust the government, or (iii) think inequalities are justified, e.g. due to differences in effort. I abstract from all these explanations. Instead, I use incentivised choices to examine the role of personal costs. Such a "cost" explanation has not been addressed in the earlier "cheap talk" studies, where individuals express their concerns about inequality and their support for redistributive policies without personal consequences.

This paper proceeds as follows: in Section 2.1, I describe the experimental design for Study 1. Section 2.2 presents the theoretical framework which gives rise to the main hypotheses. I present data from Study 1 in Section 2.3. In Section 3, I examine the robustness of the results from Study 1 in a second study where subjects earn higher interest rates if they have higher initial endowments. Section 4 presents a third study where subjects do not forecast inequality before they vote on redistribution. I discuss further results in Section 5, and Section 6 concludes. The Appendix includes an analysis of attrition and additional tables. The Online Supplement includes the experimental instructions, extensions of the theoretical model, a description of pilot studies, a power analysis as well as further results, tables, and figures.

2 Study 1: Voting After Inequality Forecast

2.1 Experimental Design

The experiment consists of five parts completed in a single online session (see Figure 1 for an overview). First, subjects play a standard and a modified dictator game. From the standard dictator game, I obtain a measure of inequality aversion. In the modified dictator game, subjects distribute income between two recipients with an efficiency loss, and this yields a measure of subjects' efficiency concerns. Second, subjects fill in information about their demographics, which serve as control variables in the analysis. In the third part, subjects are asked to predict the development of different incomes in a group, which provides an individual forecast of inequality in the group. For the fourth task, subjects are randomly allocated to one of the income classes in the group, and they vote for redistribution in a tax-transfer scheme. Finally, subjects complete an attitudinal survey that provides a number of control variables for the

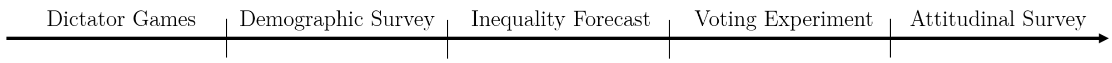


Figure 1: Timeline of the experiment

analysis. Instructions are presented in Online Supplement S.10.

2.1.1 Dictator Games

Standard Dictator Game. First, subjects play a standard dictator game with a continuous action space (Forsythe et al., 1994) under role uncertainty. Each subject is paired at random with one other subject. One person (the dictator) is given USD 1 and decides how much he wants to give to his partner (the recipient).[4] After answering two control questions to ensure that subjects understand the game, subjects decide as a dictator (strategy method, Selten, 1967). Following Arechar et al. (2018), subjects are not allowed to continue before they answer the two control questions correctly. If subjects answer incorrectly, they are informed about this and are asked to try again. A random draw determines who becomes the dictator, and the dictator's decision is implemented. To avoid spillovers to the remaining parts of the experiment, subjects are not informed about the outcome of the dictator game before they continue with the experiment.

Modified Dictator Game. The modified dictator game elicits a proxy for preferences for efficiency relative to equity.[5] In this task, subjects are divided into groups of three. One person (C) is the dictator. He decides how to allocate USD .9 between the two other subjects, Person A and Person B. But 50 percent of the money that is given to Person B is lost, and this creates a conflict between equity and efficiency (without a vested interest, see e.g. Engelmann and Strobel, 2004, Hong et al., 2015, and Chen and Fischbacher, 2020).[6] Subjects must correctly

[4] An endowment of USD 1 is somewhat smaller than the typical pie size in dictator games in lab experiments. Nevertheless, as I explain in Section 2.1.6, the current study uses Amazon's Mechanical Turk (MTurk), and such a pie size is generous compared to other dictator games on MTurk (e.g., Dreber et al., 2016; Capraro and Rand, 2018). Moreover, dictator giving with such pie sizes correlates with prosociality in other games (Capraro and Rand, 2018), and Amir et al. (2012) find that subjects on MTurk give 33.2 percent of their endowment with a pie size of USD 1, and this is very close to the average giving of 28.4 percent that Engel (2011) find in a meta-analysis of dictator games.

[5] With "efficiency", I refer to the total income in each society and not to e.g. Pareto efficiency.

[6] In both Engelmann and Strobel (2004), Hong et al. (2015), and Chen and Fischbacher (2020), subjects decide on how to allocate income between two recipients with different costs of equality. An alternative approach would have been to make subjects allocate money between themselves and others with an efficiency loss (Andreoni and Vesterlund, 2001; Andreoni and Miller, 2002; Schildberg-Hörisch, 2010; Jakiela, 2013; Fisman et al., 2017). But such an allocation decision depends on concerns for both efficiency, equity, and self-interest, and it is thus

answer two control questions to continue. If subjects answer incorrectly, they are informed about this and are asked to try again.

To make the task simple for the dictators, they choose between 7 different allocations, where they observe the earnings for Person A and Person B as well as the total earnings.[7] Using the strategy method, all subjects make decisions as the dictator under role uncertainty, and one decision is randomly drawn and implemented. To avoid spillovers, subjects are not informed about the outcome of the modified dictator game before they continue with the experiment.

2.1.2 Demographic Survey

After the dictator games, subjects provide information about their age, gender, ethnicity, education, and employment status, which serves as control variables in Section 2.3. The demographic survey also serves as a filler task to mitigate possible spillovers from the dictator games to the later experimental tasks that focus on redistribution within a group.

2.1.3 Inequality Forecast

Setting. For the third part of the experiment, subjects are divided into groups of seven. Two individuals are 'poor', three are 'middle class', and two are 'rich'. Subjects are informed about the initial income of individuals in each income class. All subjects are informed that every group member will receive an interest of 25 percent on their endowment for 30 periods, but they are not informed about the final endowments.[8] The initial (final) income for each income class is \$1 (\$808), \$4 (\$3,231), and \$7 (\$5,655), respectively.

Subjective Forecast. After subjects are informed about the setting, they are asked to estimate what the income is for a member of each income class after 30 rounds with compounded interest. The task is incentivised as subjects earn 5 cents for each income class that they estimate correctly (with a 10 percent margin of error). Based on their estimates for each of the income classes, I calculate the subjects' forecast of inequality in the group as well as their ability to forecast exponential developments (see details in Section 2.3). To rule out motivated reasoning, subjects are not aware of what will be their own income when making their

ill-suited for the purposes of this elicitation task.

[7]Specifically, the options are as follows (in cents): $(x_A, x_B) = \{(30, 30), (40, 25), (50, 20), (60, 15), (70, 10), (80, 5), (90, 0)\}$.

[8]I use high interest rates over many periods to increase the difference between the treatments (described below) in the case that subjects make erroneous forecasts. If there is a treatment effect, then larger treatment differences should increase the power of the study (Hansen and Collins, 1994; Meyvis and Van Osselaer, 2018).

forecasts.

2.1.4 Voting on Redistribution

The fourth part of the experiment expands on the subjective inequality forecast. Subjects are randomly assigned to one of the three income classes, and they are asked to redistribute earnings using a tax-transfer scheme.[9] Each group member is paid according to his post-redistribution income in the group (with an exchange rate of 2000:1). When deciding on how to redistribute earnings in their group, subjects are randomly assigned to one of three information treatments (see Table 1 for an overview).

Table 1: Overview of experimental treatments

	Treatment	**Interest Rates**	**Subjects make income forecast**	**Subjects see true income levels**	**Subjects see true income ratios**
Study 1	Forecast	Uniform	Yes	No	No
	Ratio	Uniform	Yes	No	Yes
	Realized	Uniform	Yes	Yes	Yes
Study 2	ForecastR	Unequal	Yes	No	No
	RealizedR	Unequal	Yes	Yes	Yes
Study 3	ForecastNo	Uniform	No	No	No
	RealizedNo	Uniform	No	Yes	Yes

Tax-Transfer Scheme. Subjects decide on redistribution in a proportional tax-transfer scheme, in which all group members pay a fraction of their income and receive a lump-sum transfer.[10] Redistribution is costly. Subjects are informed that 2 percent of the transfers are lost ('leaky bucket', Okun, 1975). This creates an equity-efficiency trade-off. I use a relatively small leakage percentage to ensure some demand for redistribution and to make it unlikely that efficiency concerns dominate the subjects' decisions (Beckman et al., 2004; Krawczyk, 2010; Durante et al., 2014; Tepe et al., 2021).

To ensure that the subjects understand the tax-transfer scheme, a table shows the post-tax earnings of all group members in case a tax rate of $\tau \in \{0, 20, 40, 60, 80, 100\}$ is implemented (see Figure 2). Depending on the treatment (described below), the post-tax earnings shown

[9] Endowments are determined at random since earlier studies show that this makes individuals more altruistic and more likely to redistribute (Hoffman et al., 1994, 1996; Cherry et al., 2002; Cappelen et al., 2010; Balafoutas et al., 2013; Lefgren et al., 2016; Cappelen et al., 2020), cf. the accountability principle (Konow, 1996). This design feature hereby makes it easier to examine the relation between social preferences and subjective forecasts.

[10] As social preferences are highly dependent on context (e.g., Ambec et al., 2019), the experimental instructions frame the decision in the language of "tax" and "earnings".

in the table are based on either the subjects' estimates from the forecast task or the actual earnings in the final round. By using multiple control questions, I make sure that subjects understand and engage with the information provided in this table. Subjects must answer all control questions correctly to continue the study. If they answer incorrectly, they are informed about this and are asked to try again.

On the page before, you guessed that the poor would earn $700, that the middle class (you) would earn $2000, and that the rich would earn $3150 in period 30 before taxes. Based on these amounts, the following table shows **how much a person from each group would earn after taxes** for different tax rates.

Note: To keep the table small, it shows only a few examples of tax rates. You are free to choose any tax rate between 0 and 100, including tax rates not listed in the table.

Tax Rate	**0%**	**20%**	**40%**	**60%**	**80%**	**100%**
Poor	$700	$944	$1187	$1431	$1674	$1918
YOU	$2000	$1984	$1967	$1951	$1934	$1918
Rich	$3150	$2904	$2657	$2411	$2164	$1918
Total	$13700	$13645	$13590	$13536	$13481	$13426

Figure 2: Earnings for different tax rates

Note: this example shows the information table that is provided to a subject in *Forecast* who estimated that the poor, middle income, and rich group members would earn $700, $2,000, and $3,150 in round 30, respectively. The complete instructions are provided in Online Supplement S.10.

After the subjects state their preferred tax rate, one of these tax rates is chosen at random and implemented. All subjects have an equal chance of being pivotal, and it is therefore optimal for all subjects to truthfully report their preferred tax rate. This so-called random dictator procedure is widely used in voting experiments (Feddersen et al., 2009; Krawczyk, 2010; Höchtl et al., 2012; Shayo and Harel, 2012; Durante et al., 2014; Jensen and Markussen, 2021) because it yields the same theoretical prediction as median voting but is simpler to understand. Subjects are explicitly informed that it is optimal for them to state their preferred tax rate.

The tax is applied to the pre-tax income in the final round rather than to the initial endowment. This makes the redistribution decision easier for the subjects. First, subjects see the consequences of the tax directly from the table. If the tax was applied to the endowment before compounded interest, subjects would need to make forecasts for all combinations of tax rates and income groups to understand the post-tax earnings. Second, redistributing earnings in the final round ensures a logical progression from the previous forecast task, and subjects do not need to recall their answers to the previous task to make a decision in the voting experiment.

A key feature of the experimental design is that subjects decide on their preferred level of redistribution at a single point in time. Consequently, time preferences do not matter for the decision, and the task therefore isolates the causal effect of forecasts on social preferences.

Treatments. Subjects are randomised into one of three treatments that vary the information available to the subjects when they choose a tax rate (see Figure 3).

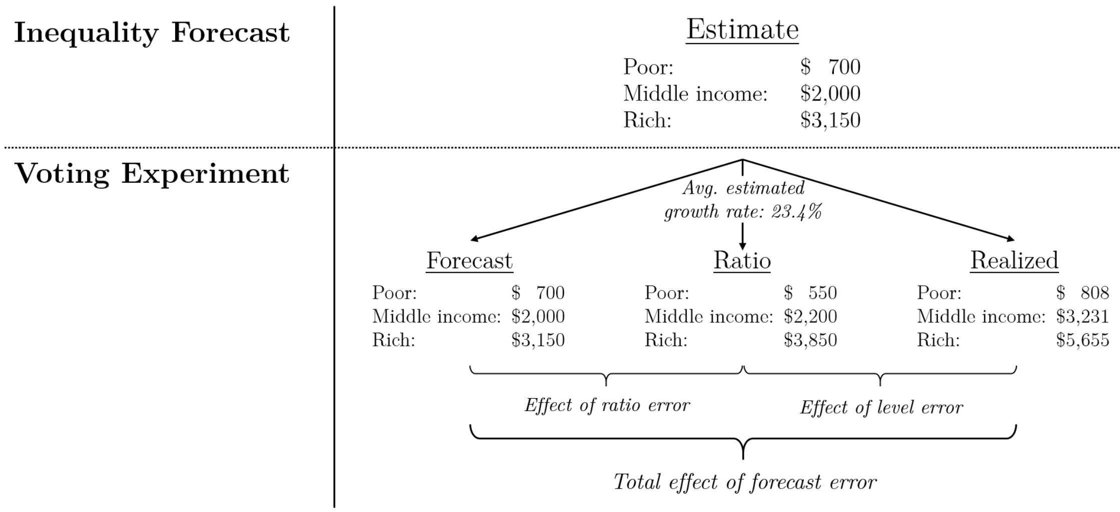


Figure 3: Illustration of received information in each treatment

Note: this figure illustrates the pre-redistribution incomes that a subject will see in each of the information treatments (between-subject), given a forecast of $700, $2,000, and $3,150 for the respective income classes. In *Forecast*, subjects see their own estimates of the pre-redistribution incomes. In *Ratio*, subjects see information where all incomes grow at the average of the subjectively forecasted interest rates. Finally, subjects in the *Realized* treatment are informed about the actual pre-redistribution earnings that arise based on the 25 percent growth rate. Together, the three treatments make it possible to uncover the total effect of erroneous forecasts and decompose it into the effects of ratio and level errors.

In the *Forecast* treatment, subjects see the effect of redistribution based on the level of inequality they estimated in the forecast task. Subjects in the *Realized* treatment instead see the post-redistribution incomes based on the actual level of inequality in round 30. Contrasting these two treatments yields the causal effect of erroneous forecasts on preferences for redistribution. If the preferences differ, then this may be due to two kinds of forecast errors: subjects may wrongly estimate how the incomes develop relative to each other, and they may wrongly estimate the future income levels of the income classes. The *Ratio* treatment disentangles the effects of the two types of errors. In this treatment, subjects see the effect of redistribution when all incomes increase at the same rate, set to the average of the subjectively forecasted interest rates. Because the rate is the same for all incomes, the resulting ratios between the incomes are correct. And because the rate is set to the average of the forecasted rates, the income levels reflect the levels of the subjective forecasts.

In sum, the comparison between *Forecast* and *Realized* provides a test of the influence of forecast errors on preferences for redistribution. The contrast between *Ratio* and *Realized* tests how forecasting wrong income levels affects redistributive preferences. Finally, comparing *Forecast* and *Ratio* sheds light on the influence of inconsistent forecasts that distort the perceived relative inequality.

2.1.5 Attitudinal Survey

After the subjects complete the voting experiment, they answer a survey that elicits a number of control variables. First, subjects answer a question about their general risk preferences (Dohmen et al., 2011) and a generalised trust question (Lundmark et al., 2015) as risk and trust both correlate with social preferences (Bekkers, 2003; Gärtner et al., 2017; Macdonald, 2020). Then, subjects place themselves politically on a left-right scale and report the extent to which they believe merit determines success (Fong, 2001; Fisman et al., 2017; Haerpfer et al., 2020; Kerschbamer and Müller, 2020). Subjects are then asked to estimate the percentage of wealth owned by each wealth quintile in the US (i.e., the wealth distribution) and state their ideal wealth distribution (e.g., Norton and Ariely, 2011, Norton et al., 2014, and Franks and Scherr, 2019).[11] Afterwards, subjects answer whether they think income differences in the US are too large and the extent to which they think the government is responsible for reducing income differences. Subjects also locate their position in society using the image of a ladder (e.g., Bobzien, 2020; Knell and Stix, 2020).[12] Finally, subjects answer the 10-item version of the Martin-Larsen Approval Motivation Scale (MLAMS, Martin, 1984), which measures

[11] This procedure has been widely debated (Easterbrook, 2021), and it has received critique for biasing respondents towards the equal distribution as this may serve as an anchor (Eriksson and Simpson, 2012; Eriksson and Simpsons, 2013). But the approach does not involve any default, and it avoids the salience of a middle response as in e.g. the ISSP inequality diagrams. Also, I expect income shares to be easier for the broader public to understand than measures such as estimating average incomes across different occupations (e.g., Kuhn, 2019, 2020), and it reflects the general discourse on income and wealth inequality, which often focuses on (top) income shares due to its simplicity (Saez, 2017).

[12] Specifically, the social-ladder question shows an image of a ladder and asks the following: "In our society there are groups which tend to be towards the top and groups which tend to be towards the bottom. Below is a scale that runs from top (10) to bottom (1). Where would you put yourself now on this scale?"

desire for social approval.[13] To allow comparison across measures and scale lengths, responses are standardised as proportions of the maximum possible (POMP) score, ranging between zero and one (Cohen et al., 1999; Mellenbergh, 2019).[14]

2.1.6 Procedure

For Study 1, 1,584 subjects were recruited on Amazon's Mechanical Turk (MTurk) between 13 November and 3 December 2021, and the experiment was implemented in Qualtrics.[15] Studies on MTurk receive the most attention from respondents when they are published, and I therefore started the data collection on a Saturday to avoid biasing the sample against people with full employment (Casey et al., 2017).

The current sample was limited to respondents in the US who had completed 100 Human Intelligence Tasks (HITs), requiring an approval rate of at least 99 percent as recommended by Matherly (2019) and Amazon Mechanical Turk (2019). To ensure high-quality data, the experiment employed a pre-registered strategy with several screeners in addition to the comprehension checks mentioned above (Thomas and Clifford, 2017; Zhang et al., 2022). I provide details in Appendix A. In total, the above screeners led to the exclusion of 10.7 percent of the responses, and the main sample thus consists of 1,415 subjects.[16] With this sample size,

[13]The scale asks subjects to rate on a 5-point Likert scale from "Disagree Strongly" to "Agree Strongly" items such as "I would rather be myself than be well thought of" (reverse-coded) and "It is not important for me that I behave 'properly' in social situations" (reverse-coded). Scores on the MLAMS are positively correlated with self-monitoring, public self-consciousness, social anxiety, and fear of negative evaluation (Martin, 1984; Wei et al., 2005; Wu and Wei, 2008).

[14]I use POMP scores rather than computing standardised (z) scores since the subjects' responses are skewed on the questions regarding trust, political attitudes, meritocratic beliefs, income differences, and the government's responsibility. Such skewness can make the z scores misleading (Cohen et al., 1999). Moreover, since POMP scores do not depend on the variance for this particular sample, it has the additional advantage of enabling comparisons between studies in a manner that is robust to sampling differences.

[15]MTurk is increasingly used for experiments within the social sciences as it tends to provide reliable, high-quality data (McCredie and Morey, 2019; Chmielewski and Kucker, 2020) with a much more diverse subject pool than traditional convenience samples (Paolacci and Chandler, 2014; Huff and Tingley, 2015). Many classical behavioural and experimental findings have been replicated using MTurk, both within economics (Horton et al., 2011; Amir et al., 2012), psychology (Klein et al., 2014), and political science (Coppock, 2019). Particularly important for the current study, previous experiments on MTurk have found that subjects trust experimental manipulations and respond to interactive experiments in a similar manner as in the lab (Summerville and Chartier, 2013; Arechar et al., 2018).

[16]This share of excluded responses on MTurk resembles that of earlier studies. For instance, Wood et al. (2017) find that approximately 10 percent make careless responses, and Kennedy et al. (2020) exclude 6.8 percent of their responses. All results are qualitatively robust to including all subjects who completed the study.

I expected based on power simulations to have 80 percent power to detect an effect size of Hedge's $g_p = 0.22$ (Goulet-Pelletier and Cousineau, 2018), corresponding to a difference in tax rates of 6.6 percentage points (details in Online Supplement S.3). In the main sample, 42 percent were males, the mean age was 40 years, 81 percent were White or Caucasian, 41 percent had obtained a Bachelor's degree, 16 percent had obtained a Master's degree, 65 percent were employed (part or full time), and 13 percent were self-employed. The full set of summary statistics are provided in Tables C1 and C2 in the Appendix.

There has been some concern that MTurk participants openly discuss studies with each other and thereby become aware of e.g. a study's purpose and the correct answers to control questions (Chandler et al., 2014). To alleviate any such concerns, I monitored the communities on MTurk Crowd and TurkerView as well as the subreddits r/TurkerNation, r/mturk, and r/HITsWorthTurkingFor while the study ran to ensure that sharing of such information did not occur (Brawley and Pury, 2016; Deng et al., 2016; Aguinis et al., 2020).[17]

For completing the study, all subjects received USD 1 in addition to the payment from the dictator games, the voting experiment, and the incentivised questions. The median earnings were USD 3.2, and the median completion time was approximately 15 minutes (which is an upper bound as it also takes into account time spent off task with the experiment open in the background).

2.2 Theory

2.2.1 Setup

In the previous section, I explained the experiment that I use to examine (i) people's ability to predict future inequality and (ii) how (possibly erroneous) forecasts influence preferences for redistribution. Before turning to the results of this experiment, I provide ex-ante (preregistered) hypotheses by developing a stylized model of social preferences that incorporates subjective forecasts. This model builds on the quadratic version of the Fehr and Schmidt (1999) model. It features increasing marginal disutility from inequality, which will be essential for income growth to influence preferences for redistribution. I extend the model to account for subjective beliefs of growth by incorporating the general framework from Stango and Zinman

[17] A technical error caused issues for subjects in the *Ratio* treatment during the first two hours of the study. For this reason, there were some initial inquiries on MTurk Crowd and Turkerview into whether there were problems with the study. Moreover, some community members shared a link to the study on MTurk Crowd as the expected hourly wage of this study was somewhat larger than most other studies on MTurk. Most importantly, across all fora, no workers mentioned (i) that the study was about redistribution, (ii) any details about their respective treatments, or (iii) how to answer control questions.

(2009). Finally, I introduce a tax-transfer scheme for redistribution (Meltzer and Richard, 1981), and the hypotheses concern what tax rate subjects vote for in this scheme.

Modelling Social Preferences. I use a quadratic version of the Fehr and Schmidt (1999) model (henceforth FS-model) as a framework for social preferences.[18] This model implies that the marginal disutility of inequality is increasing in the level of inequality, and this has several advantages over the linear version: first, this model corroborates the transfer principle (Pigou, 1912; Dalton, 1920) to yield e.g. the intuitive result that a rich individual would approve of a transfer from the middle class to people living close to the subsistence minimum. Second, the model is consistent with the finding that although some individuals prefer to have more than others, many dislike having too much more (Hadad and Malul, 2017). Third, it captures that individuals tend to dislike inequality more when it reflects need or poverty (Scott et al., 2001; Michelbach et al., 2003; Faravelli, 2007; Kittel et al., 2020).[19]

Formally, consider n individuals indexed by $i \in \{1, \dots, n\}$, and let x_i denote the real income for individual i. Denote by $\beta_i \in [0, 1)$ the individual-specific disutility from advantageous inequality, and let $\alpha_i \geq \beta_i$ be the disutility from disadvantageous inequality. Then, the utility of individual i is given as

$$U_i(x_i, ..., x_n) = x_i - \alpha_i \frac{1}{n-1} \sum_{j \neq i} (\max\{x_j - x_i, 0\})^2 - \beta_i \frac{1}{n-1} \sum_{j \neq i} (\max\{x_i - x_j, 0\})^2 \qquad (1)$$

In Online Supplement S.2.5, I demonstrate that the predictions derived from the utility function specified in Equation 1 are qualitatively robust to including explicit preferences for efficiency (total surplus).

Social Preferences and Exponential Growth Bias. The novel aspect of the current theoretical framework is that it combines the quadratic FS-model with misperceptions of growth in a general framework that draws on Stango and Zinman (2009). For simplicity and

[18] Quadratic difference aversion is similar in spirit to the frameworks applied by e.g. De Bruyn and Bolton (2008) and Barr et al. (2009), which build on Bolton and Ockenfels's (2000) ERC model. Specifically, they model inequality aversion based on the quadratic difference between the individual's income and the mean income. This is, however, ill-suited to study distributive preferences since it implies that redistribution only matters for individuals if their own income or the average income is affected. Hence, it cannot explain e.g. why a middle-income voter would prefer to transfer money from the rich to the poor.

[19] On a more technical level, the quadratic FS-model has the advantage that it leads to interior solutions in widely used settings such as the dictator and ultimatum games. This is in contrast to the linear FS-model, which predicts that dictators/proposers either keep the entire endowment or split evenly. In the words of Fehr and Schmidt (1999, p. 848), this is "a prediction that is clearly refuted by the data". In a meta-analysis of dictator games, Engel (2011) reports that 42 percent of subjects give intermediate amounts.

because time preferences play no role in the experiment (cf. Section 2.1), I abstract from temporal discounting.

To capture how individuals estimate the future value (FV) of an endowment specified in present value (PV), I assume that individual i estimates the growth of the endowment by a function $f(r, T, \theta)$, where r is the real interest rate (or real rate of growth), T is the time horizon, and θ is the forecast bias:[20]

$$FV = PV \cdot f(r, T, \theta) \tag{2}$$

To make assumptions about the function f, I draw on an extensive literature that shows how individuals tend to linearise exponential developments. This exponential growth bias (henceforth EGB) is prevalent regardless of the number of data points that people observe (Wagenaar and Timmers, 1978) and how the data is presented (Wagenaar and Sagaria, 1975; Wagenaar and Timmers, 1979). Moreover, individuals tend to be naïve about their own bias (Levy and Tasoff, 2017; Cordes et al., 2019), and this implies that individuals are unlikely to take the necessary steps to alleviate problems caused by EGB. Common theoretical frameworks for EGB suppose that perceived total growths do not depend on the initial amount but only on the interest rate and the time horizon (e.g., Stango and Zinman, 2009; Levy and Tasoff, 2016), and this has received experimental support (e.g., McKenzie and Liersch, 2011).

I assume that the function f is strictly convex in both r and T (i.e., $f_r > 0, f_{rr} > 0, f_T > 0$, and $f_{TT} > 0$). That is, the model also allows for cases where growth is not exponential, and I thus refer to the bias as a forecast bias rather than as EGB. The forecast bias implies that the individual underestimates the convexity of the development when making the forecast (i.e., $f_\theta < 0, f_{r\theta} < 0$, and $f_{T\theta} < 0$), and this leads the individual to underestimate the income growth.[21] For simplicity, I assume that individual i exhibits the same degree of forecast bias towards the growth of his own and others' incomes.

At a given point in time with T remaining time periods, expanding the utility function from Equation 1 implies that individual i forecasts his utility as follows:

$$\begin{aligned} U_i(x_i, ..., x_n) = x_i \cdot f(r, T, \theta) - \alpha_i \frac{1}{n-1} f(r, T, \theta)^2 \sum_{j \neq i} (\max\{(x_j - x_i), 0\})^2 \\ - \beta_i \frac{1}{n-1} f(r, T, \theta)^2 \sum_{j \neq i} (\max\{(x_i - x_j), 0\})^2 \end{aligned} \tag{3}$$

[20] An alternative interpretation in the spirit of Benabou and Ok (2001) is that there is some degree of inertia or commitment power in fiscal policy such that decisions made today to some extent persist into the economy in period T.

[21] I provide examples using the particular functional forms from Stango and Zinman (2009) and Levy and Tasoff (2016) in Online Supplement S.2.1 and S.2.2.

The above utility function models inequality aversion based on absolute differences in income, which is sufficient to provide predictions for the case with uniform growth rates. In Online Supplement S.2.4, I show that the qualitative predictions hold if one extends the model so that the individual receives disutility from both absolute and relative inequality. Intuitively, even though relative inequality is constant under uniform growth rates, absolute inequality increases. Thus, the individual will experience disutility from increasing inequality as long as the utility function puts some weight on the disutility from absolute inequality.

Tax-Transfer Scheme. In the spirit of Meltzer and Richard (1981), I assume that a proportional tax is determined by a random dictator and levied on the entire population. The tax revenue finances lump-sum transfers that are paid out equally to all citizen.[22] I focus on the setting of the experiment where redistribution takes place only in the final period. To introduce an equity-efficiency trade-off, I furthermore assume that the tax entails an efficiency loss. Denoting the tax rate by $\tau \in [0, 1]$, I assume that the amount paid out to each citizen is $\lambda\tau\bar{x}$, where $\lambda \in (0, 1]$ is the efficiency of the tax, and $\bar{x} = \frac{1}{n}\sum_{j=1}^{n} x_j$ is the average income.[23] Thus, the post-redistribution income that individual i receives is $(1-\tau)x_i + \lambda\tau\bar{x}$. In line with the experimental setup, I assume that individuals are only concerned with post-redistribution incomes at time T. That is, individual i expects to receive the following utility at time T:

$$\begin{aligned} U_i(x_1, ..., x_n) = {} & [(1-\tau)x_i + \lambda\tau\bar{x}] \cdot f(r, T, \theta) \\ & - \alpha_i \frac{1}{n-1}(1-\tau)^2 f(r, T, \theta)^2 \sum_{j \neq i}(\max\{x_j - x_i, 0\})^2 \\ & - \beta_i \frac{1}{n-1}(1-\tau)^2 f(r, T, \theta)^2 \sum_{j \neq i}(\max\{x_i - x_j, 0\})^2 \end{aligned} \tag{4}$$

2.2.2 Analysis

I now examine what tax rate the individual prefers, and how this is influenced by the forecast bias. Note that the 'preferred' tax rate may not be 'optimal' since the preferred tax rate is

[22] In Online Supplement S.2.7, I show that the predictions are qualitatively robust to assuming instead a lump sum tax, where the individuals with above-average incomes transfer a fixed amount to the individuals with below-average incomes.

[23] The notion that the tax involves an efficiency loss is a standard simplification used in the literature to describe an equity-efficiency trade-off (Alesina and Giuliano, 2011). It reflects possible distortions to the labour supply caused by income taxation. In the literature, such a distortion is sometimes considered as convex, but I adopt a linear efficiency loss to reflect the more simple experimental design from Section 2.1 (see e.g. Krawczyk, 2010, or Tepe et al., 2021, for examples of linear tax schemes). In Online Supplement S.2.6, I show that the qualitative predictions of the theory remain the same when one assumes convex distortionary costs of taxation.

based on beliefs that may be biased (in the spirit of a perception-perfect strategy, O'Donoghue and Rabin, 2001).

Due to the random dictator procedure, there is a strictly positive probability that any individual's vote is pivotal. That is, the model captures in a simple way the predictions from strategy-proof social choice functions as it is optimal for all individuals to vote truthfully. Maximising the utility specified in Equation 4 with respect to τ yields the following preferred tax rate for individual i:

$$\tau_i^b(x_1, \dots, x_N; \theta) = 1 - \frac{x_i - \lambda \bar{x}}{2\phi_i f(r, T, \theta)}, \tag{5}$$

where

$$\phi_i(x_1, \dots, x_N) = \alpha_i \frac{1}{n-1} \sum_{j \neq i} (\max\{x_j - x_i, 0\})^2 + \beta_i \frac{1}{n-1} \sum_{j \neq i} (\max\{x_i - x_j, 0\})^2. \tag{6}$$

Here, τ_i^b denotes that the individual is influenced by forecast bias, and ϕ_i reflects the individual's concerns for inequality. The preferred tax rate increases in inequality aversion (α, β) and the efficiency of the tax (λ). Moreover, it increases in the subjective estimate of growth ($f(r, T, \theta)$), which implies that more biased individuals prefer less redistribution, ceteris paribus. Also, τ_i^b increases when the incomes of persons who earn more than individual i increase. But the effect of individual i's own income on the preferred tax rate is ambiguous: an increase in x_i results in higher costs of redistribution for individual i; however, if he earns much more than the other individuals, the increased inequality may cause him to prefer a higher tax rate.

For poor individuals ($x_i < \lambda \bar{x}$), a higher tax rate results in both higher earnings and more equality in the group. Thus, they choose the highest tax rate, leading to the corner solution of $\tau_i^b = 1$.

Net contributors ($x_i > \lambda \bar{x}$) face a trade-off between their own earnings and equality. Denote by τ_i^* the optimal tax rate for individual i with a perfect forecast. This tax rate corresponds to $\tau_i^b(x_1, \dots, x_N; \theta = 0)$. It is the relevant benchmark for subjects in the *Realized* treatment as these subjects observe the correct level of inequality in the final round. From Equation 5, one can see that $\tau_i^* \geq \tau_i^b$. That is, all else equal, biased individuals (e.g., in the *Forecast* treatment) vote for less redistribution than unbiased individuals.[24]

[24] In a within-subject perspective, note that as time progresses the true development of incomes aligns expectations with the true final outcomes. So, as the biased individual approaches the final period, his preferred tax rate coincides with the optimal tax rate ($\lim_{T \to 0} \tau_i^b = \tau_i^*$).

2.2.3 Hypotheses

The first hypothesis concerns the subjects' forecasts of inequality. In the above model, I follow the literature on EGB and assume that individuals underestimate exponential growth and thereby the development in absolute inequality. However, because the time horizon and real interest rate are the same for all income classes, I assume that individuals attribute the same overall growth rate to all members of their group. This would lead to an accurate estimate of the relative inequality in the final round. I test these model assumptions in the first hypothesis:

Hypothesis 1

1. *Subjects on average underestimate the extent of absolute inequality in the final round.*

2. *Subjects on average correctly estimate the extent of relative inequality in the final round.*

Next, I turn to the voting part of the experiment, which examines the behavioural implications of making biased forecasts of inequality. As outlined above, the theoretical model predicts that, for the same level of inequality, subjects prefer a higher tax rate in the *Realized* treatment than in the *Forecast* and *Ratio* treatments. If H1.2 is true, there should be no difference between *Forecast* and *Ratio*. This yields the following hypothesis:

Hypothesis 2 *Comparing individuals with the same degree of inequality aversion, middle-income and rich subjects on average*

1. *vote for a higher tax rate in* Realized *than in* Forecast*.*

2. *vote for a higher tax rate in* Realized *than in* Ratio*.*

3. *vote for the same tax rate in* Forecast *and* Ratio*.*

2.3 Results

Having thus provided ex-ante hypotheses, I now analyse subjects' inequality forecasts and how these forecasts affect preferences for redistribution in the voting experiment. Then, I examine two mechanisms suggested by the theory: that forecast bias only matters for redistributive preferences in *Forecast* and that subjects randomised into the poor income class vote for more redistribution than subjects randomised into the middle or rich income classes. Throughout, I follow the pre-analysis plan (see pre-registration). Table 2 provides descriptive statistics for the middle-income and rich subjects; the subjects that H2 is about. Descriptive statistics for the poor subjects are presented in Table S.13. All reported p-values are from two-sided tests.

Table 2: Descriptive statistics for middle-income and rich subjects, Study 1

	N	Tax	DG	Efficiency	Actual SD	SD(F)	Actual CV	CV(F)	EGB
Forecast	349	47.73	37.37	3.48	1831.99	760.59	0.57	0.58	0.47
Ratio	305	42.97	37.32	3.69	1831.99	888.05	0.57	0.57	0.47
Realized	359	40.74	38.25	3.30	1831.99	564.74	0.57	0.57	0.46
Total	1013	43.82	37.67	3.48	1831.99	729.56	0.57	0.58	0.47

Note: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. Efficiency corresponds to subjects' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by subjects' forecasted income levels in the group. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009). As noted in Footnote 17, a technical error caused issues for subjects in the *Ratio* treatment during the first two hours of the data collection, and this explains why there are fewer observations in this treatment.

2.3.1 H1: Do People Underestimate Future Inequality?

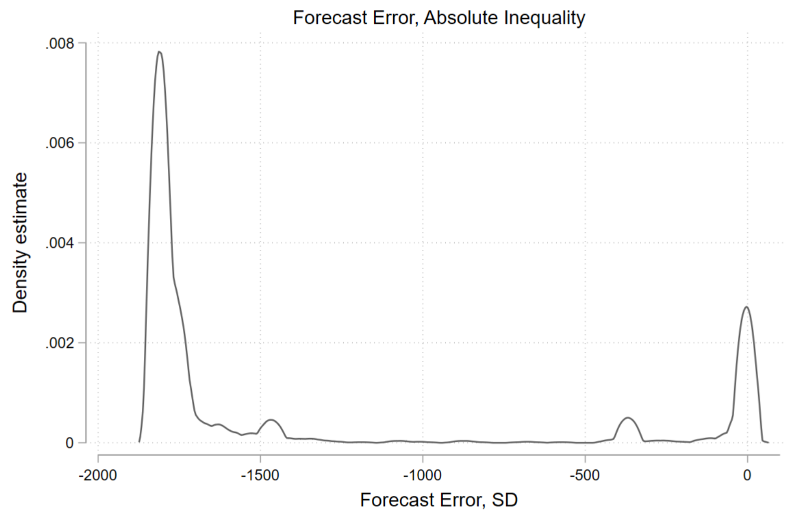
To test H1, I first obtain the level of absolute and relative inequality that is implied by the subjects' forecasts. Specifically, I calculate the standard deviation and Absolute Gini coefficient to examine absolute inequality, and I compute the coefficient of variation and the Gini coefficient as measures of relative inequality.

Supporting H1.1, subjects greatly underestimate the level of absolute inequality in the pre-tax incomes in the final round. Looking at both the standard deviation and the Absolute Gini coefficient (Figures 4 and S.7), it is evident that most subjects make negative forecasting errors with only few subjects making zero or positive forecasting errors. For both measures, underestimation is statistically significant ($p < .001$, bootstrapped t-test).

For relative inequality, Figure 5 suggests that many subjects have nearly accurate forecasts for the coefficient of variation (similar for the Gini coefficient, see Figure S.8). The statistical evidence is mixed, however. On average, subjects significantly underestimate relative inequality in terms of the coefficient of variation ($p = .003$, bootstrapped t-test), but there is no significant difference in terms of the Gini coefficient ($p = .389$, bootstrapped t-test). Hence, while subjects clearly perform better at forecasting relative than absolute inequality, the data only partially support H1.2.

The distribution of subjects forecasts of absolute inequality also suggests that there are a number of subjects who accurately forecast the level of inequality (Figure 4). Closer inspection reveals that 129 subjects (9 percent) are within ±\$1 of the correct answer for all three income

Figure 4: Forecast error of absolute inequality, Study 1



Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}} \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N} \right]^{\frac{1}{2}}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors. See Figure C1 for a corresponding figure with the full sample.

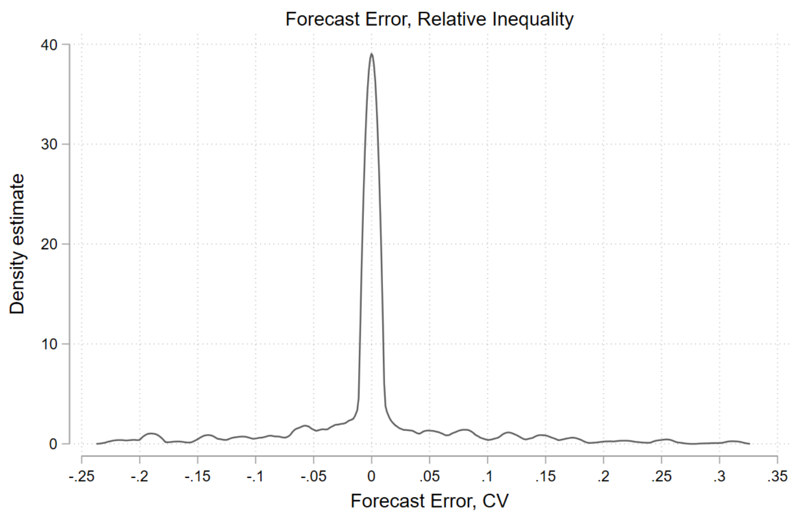
classes, which indicates that these subjects calculated the true incomes. While the subjects were explicitly asked to provide a "guess", it is difficult to hinder subjects in actually computing the correct answers to factual questions in online experiments (e.g., Goodman et al., 2013).[25]

To examine EGB in the data, I first compute a measure of EGB as in Stango and Zinman (2009) and define the bias as $\theta \equiv \frac{1-\log\left(\frac{FV}{PV}\right)}{T\log(1+r)}$ (see also Almenberg and Gerdes, 2012, and Song, 2020).[26] Each subject makes three forecasts (one for each income class), and I use the average value of θ as an estimate for the subjects' degree of EGB. Excluding subjects who calculate the resulting incomes, I find that 55 of 1,286 subjects (4 percent) overestimate growth on average (i.e., $\theta < 0$), whereas 1,231 subjects (96 percent) underestimate growth on average (i.e., $0 < \theta < 1$). In Online Supplement S.4, I comment on heterogeneity in the subjects' forecast errors.

[25] Of the subjects who calculated the correct answers, 95 were later randomised into the middle or rich income class. These subjects are not influenced by what treatment they are randomised into for the voting part as they will see the correct level of inequality regardless. Hence, including them in analysis of H2 should lead to an underestimation of treatment differences. Nevertheless, the results in the next section do not change if these are excluded from the analysis (see Table S.10).

[26] Note that this measure is but a proxy of subjects' EGB. For elaborate discussions on how to best model EGB, see e.g. Levy and Tasoff (2016), Foltice and Langer (2017), and Königsheim et al. (2018).

Figure 5: Forecast error of relative inequality, Study 1



Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 0.005$). The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors. See Figure C2 for a corresponding figure with the full sample.

I sum up the results on H1 below:

Result 1 *In Study 1 (uniform growth), subjects underestimate absolute inequality after compounded interest. They are markedly better at predicting relative inequality, but there is partial evidence for a slight underestimation of relative inequality.*

Having thus shown that subjects make forecast errors, I now turn to the behavioural implications of these errors.

2.3.2 H2: How Do Forecasts Influence Redistribution?

Contrary to H2, the average tax rate is greater in *Forecast* (47.73) than in *Realized* (40.74). Figure 6 suggests that the preferred tax rate in *Forecast* first-order stochastically dominates the preferred tax rates in *Ratio* and *Realized*.[27] To provide formal tests, I apply tobit regressions since the tax is bounded between 0 and 100, and they reveal that the treatment difference is statistically significant when controlling for dictator giving (as a proxy for inequality aversion), demographic controls, and attitudinal controls (all $p's < 0.01$, see Table 3). The statistical inference is robust to using instead a Symmetrically Censored Least Squares (SCLS) estimator ($p = .009$, Powell, 1986) and a Mann-Whitney U-test (MWU, $p = 0.022$, Wilcoxon, 1945; Mann and Whitney, 1947).[28]

Contrary to H2.2, the average preferred tax rate is 2.23 greater in *Ratio* than in *Realized*; yet, this is neither significant for tobit regressions, the SCLS estimator, nor the MWU-test (all $p's > 0.137$).

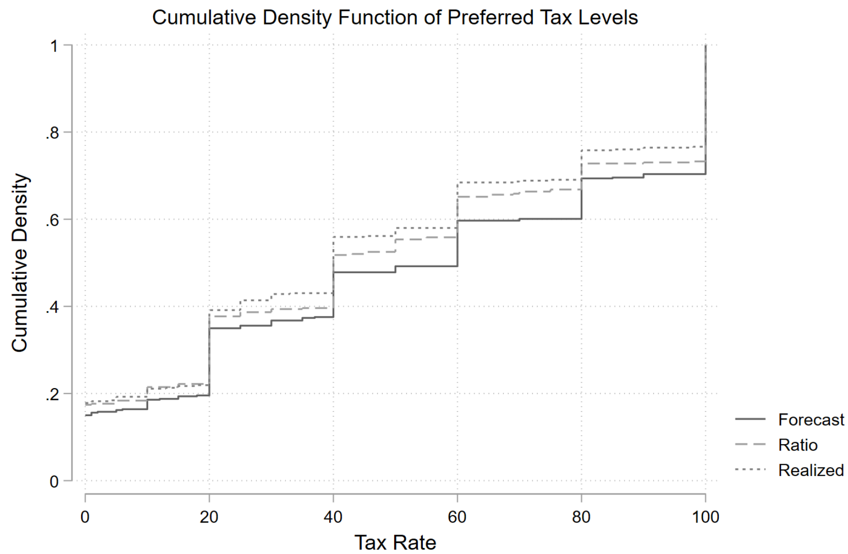
Turning to H2.3, the tax rate in *Forecast* is 4.76 higher than in *Ratio*, and this difference is marginally statistically significant when controlling for dictator giving, demographics, and attitudinal controls (all $p's > 0.083$, see Table 3). While this difference is not significant for the SCLS estimator ($p = .188$), it is also marginally statistically significant when using a MWU-test ($p = .094$).

Perceived Gains and Perceived Low Personal Costs. An exploratory inspection of the data reveals that the treatment differences are caused not by the perceived inequality per se; rather, the channel is that many middle-income subjects grossly underestimate their personal

[27]Figure 6 also reveals that subjects bunch at the tax rates 0, 20, 40, 60, 80, and 100. This is to be expected as subjects observe the post-redistribution incomes for precisely these tax rates (cf. Figure 2), and it confirms that the subjects pay attention to the experimental instructions.

[28]Since the tax rate is restricted between 0 and 100, the distributions for the tax rates in each treatment cannot be assumed to be neither symmetrical nor identical apart from location. Thus, the MWU-test is not valid for comparing medians but rather for examining whether one distribution dominates the other.

Figure 6: Preferred tax rate by treatment, Study 1



Note: the figure presents the cumulative density function (or empirical distribution function) of the subjects' tax decisions by treatment.

costs of redistributing. They do so in one of two ways. First, 58 of the 207 middle-income subjects in *Forecast* (28 percent) make forecasts that are so biased that they wrongly believe they will gain from redistribution in the final round. This is in contrast to the middle-class and rich subjects in *Ratio* and *Realized* who by construction correctly observe that redistribution comes at a personal cost. Believing that one will gain from redistribution leads on average to an increase in preferred tax of 46 percentage points (cf. Column 4 in Table 3). Second, 199 of the 389 middle-income subjects in *Forecast* and *Ratio* (51 percent) underestimate inequality to such an extent that redistribution seems to come at very low personal costs (defined here as \$3, corresponding to a payment of USD 0.0015, but the exact definition is inconsequential).[29] If these subjects only cared about their own payoff, they would be practically indifferent between different tax rates. Thus, it requires only a small extent of inequality aversion for these subjects to vote for higher taxes ('weak inequality aversion', Tyran and Sausgruber, 2006). Indeed, believing that redistribution comes at very low personal costs predicts an

[29] As this analysis is exploratory, the cutoff for "very low personal costs" is determined a posteriori and follows from the discreteness observed in voting behaviour among the middle class (see Figure S.16). However, the results are robust to using instead any value up to \$65, corresponding to a payment of USD 0.0325.

increase in preferred tax rate of 14 percentage points (cf. Column 5 in Table 3).[30]

Including dummies for perceived gains and perceived low personal costs in the above tobit regressions renders all treatment differences between *Forecast*, *Ratio*, and *Realized* insignificant. The differences are also insignificant when one considers SCLS estimators (all $p's > .784$) and if one conducts MWU-tests without subjects who wrongly perceive personal gains or low personal costs (all $p's > .330$).

WTP for Redistribution. The above analysis reveals that the extent of inequality does not generally affect the subjects' tax preferences (discussed in Online Supplement S.5.3). In other words, subjects are on average willing to give up approximately the same share of their income regardless of the level of inequality. But this implies that subjects are willing to pay more in *absolute* terms for redistribution when absolute inequality is greater. Consider the willingness-to-pay (WTP) for a subject to be his net cost of implementing his preferred tax rate after efficiency loss and disbursement of the tax revenues. Looking only at the subjects who correctly perceive that redistribution is costly, subjects are on average willing to pay (in experimental currency) \$266.83 (\$175.50) more for redistribution in *Realized* than in *Forecast* (*Ratio*), and the treatment differences are significant (Kruskal-Wallis test, $p < .001$).[31] This result resonates with survey-evidence that information about inequality can make people more concerned about inequality without changing their taxation preferences (Zilinsky, 2014; Kuziemko et al., 2015). Also, it may explain why observational data reveal no relation between increased concerns about inequality and support for government intervention (Wright, 2018).

2.3.3 Mechanism 1: Does EGB Correlate With Preferred Tax Rate?

The theoretical framework in Section 2.2 predicts a difference in behaviour between *Realized* and *Forecast* caused by the subjects' forecast bias (EGB). To test this mechanism, I look into the relation between EGB and the tax rate that subjects vote for. The theory predicts that there should be a correlation between EGB and subjects' preferred tax level in *Forecast* where the subjects' degree of EGB determines the information available to them about redistribution.

[30] Note that these particular misperceptions only occur for middle-income subjects; it is per construction more obvious for the rich subjects that redistribution comes at great personal costs. Yet, biases among the middle-income subjects are particularly interesting to examine as they are often more likely to determine the outcome of a vote, cf. the median voter theorem (Black, 1948; Meltzer and Richard, 1981).

[31] This result is somewhat related to the literature that demonstrates how stake sizes often have modest to no effect on the share that subjects give in standard dictator and ultimatum games, implying that the dictator/proposer gives more in absolute amounts (Camerer and Hogarth, 1999; Engel, 2011; Larney et al., 2019).

Table 3: EGB and tax inconsistency

	(1)	(2)	(3)	(4)	(5)
Ratio	-8.41*	-7.85*	-7.49*	0.08	-1.23
	(4.56)	(4.53)	(4.32)	(4.44)	(4.44)
Realized	-12.68***	-12.98***	-13.88***	-6.53	-2.58
	(4.36)	(4.34)	(4.14)	(4.25)	(4.44)
Dictator Giving	0.73***	0.70***	0.58***	0.61***	0.62***
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Perceived Gains				46.17***	50.03***
				(8.09)	(8.18)
Low Personal Cost					13.55***
					(4.70)
Demographic Controls	No	Yes	Yes	Yes	Yes
Attitudinal Controls	No	No	Yes	Yes	Yes
Observations	1013	1013	1013	1013	1013

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. Perceived gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). The demographic controls are age, gender, ethnicity, education, employment status, and self-reported relative income. The attitudinal controls are efficiency preferences, risk preferences, image concerns, trust, meritocratic beliefs, and political attitudes (left-right scale, inequality preferences, and government responsibility for reducing inequality). See Table C3 for the full specification. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

In contrast, EGB should be uncorrelated to the subjects' behaviour in *Realized* where subjects are informed about the true level of inequality.

Using the same measure of EGB as above, I find that EGB is a marginally significant predictor of the preferred tax rate in *Forecast* (tobit: $p = .085$, cf. Table 4; SCLS: $p = .044$; Spearman's ρ: $p = 0.108$). Yet, EGB is insignificant once the regression controls for perceived gains and perceived low personal costs (tobit: $p = .866$; SCLS: $p = .789$; Spearman's ρ: $p = .494$). This supports the above finding that these two ways of underestimating costs are the channels through which forecast biases matter in the current setting – not inequality *per se*.

In *Realized*, EGB is not significant regardless of what controls are used (all $p's > 0.627$, cf. Table 5; SCLS: $p = .789$; Spearman's ρ: $p = 0.507$).[32] Note that in this treatment, all middle-income and rich subjects correctly perceive the personal costs of redistribution, for which reason I do not control for any underestimation of costs.

The next result summarises how subjective forecasts influence preferred tax rates:

Result 2 *Subjects vote for higher tax rates when their information is based on subjective forecasts. This effect is driven by middle-income subjects who erroneously believe either (i) that they gain from tax-financed redistribution or (ii) that redistribution comes at low personal costs.*

2.3.4 Mechanism 2: Income Class and Preferences for Redistribution

The theory assumes that individuals are motivated by their own earnings and equality in the group. For subjects in the 'poor' group, the two motivators work in the same direction, leading to a preference for full redistribution. To test whether the theory truly reflects the motivation that subjects have in this experiment, one may therefore look at the poor subjects. The corner prediction of full redistribution is difficult to test, however, as any decision error will lead to a deviation in one direction only. That is, even if the assumptions of the theory are true, it is possible that the tax rate will be below 100 for some poor subjects. In contrast to the difficulties with testing the corner prediction, it is easy to test the directional prediction that poor subjects vote for higher taxes than middle-class and rich subjects, and I therefore proceed with this test in the following.

[32] One might hypothesise that EGB should be positively correlated with preferred tax by means of cognitive ability: earlier studies have found a negative relation between performance on cognitive tests and giving in dictator games (Brandstätter and Güth, 2002; Ben-Ner et al., 2004), and cognitive ability is negatively correlated with EGB (Goda et al., 2019). Yet, as EGB does not correlate with preferred tax in *Realized*, this suggests that EGB only matters in the case of specific misperceptions.

Table 4: EGB and preferred tax, Forecast

	(1)	(2)	(3)	(4)	(5)
EGB	18.56*	18.70*	17.19*	9.18	1.90
	(10.73)	(10.98)	(10.39)	(10.06)	(11.26)
Dictator Giving	0.48***	0.46***	0.37**	0.46***	0.48***
	(0.16)	(0.17)	(0.16)	(0.15)	(0.15)
Perceived Gains				44.92***	49.26***
				(8.34)	(8.90)
Low Personal Cost					11.17
					(7.86)
Demographic Controls	No	Yes	Yes	Yes	Yes
Attitudinal Controls	No	No	Yes	Yes	Yes
N	349	349	349	349	349

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009). Perceived gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). The demographic controls are age, gender, ethnicity, education, employment status, and self-reported relative income. The attitudinal controls are efficiency preferences, risk preferences, image concerns, trust, meritocratic beliefs, and political attitudes (left-right scale, inequality preferences, and government responsibility for reducing inequality). See Table S.11 for the full specification. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table 5: EGB and preferred tax, Realized

	(1)	(2)	(3)
EGB	-3.08	-3.35	-4.39
	(8.98)	(9.27)	(9.01)
Dictator Giving	0.77***	0.82***	0.70***
	(0.14)	(0.14)	(0.14)
Demographic Controls	No	Yes	Yes
Attitudinal Controls	No	No	Yes
N	359	359	359

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009). The demographic controls are age, gender, ethnicity, education, employment status, and self-reported relative income. The attitudinal controls are efficiency preferences, risk preferences, image concerns, trust, meritocratic beliefs, and political attitudes (left-right scale, inequality preferences, and government responsibility for reducing inequality). See Table S.12 for the full specification. Robust standard errors in parentheses.

* $p < .10$, ** $p < .05$, *** $p < .01$.

Across all treatments, the poor subjects vote for more redistribution (median: 80) than subjects in the middle and rich income classes (medians: 40 and 25), and this difference is statistically significant ($p < .001$, cf. Table 6). Still, only 43 percent of the poor vote for full redistribution. This suggests that there may be other concerns that influence how subjects vote. For example, one subject in *Ratio* noted that she "felt guilty taking a large amount of someone else's money".[33] It may also be the case that subjects are influenced by their general (negative) attitudes towards taxation ('tax aversion', Sussman and Olivola, 2011; Kessler and Norton, 2016). Using right-wing political attitudes as a proxy for tax aversion, I find that moving from the extreme left to the extreme right predicts a decrease in preferred tax rate of 44 percentage points among the poor (tobit: $p < .001$). Finally, some poor subjects might be concerned with the total earnings in their group (cf. Klor and Shayo, 2010). Going from being minimally concerned about efficiency to being maximally concerned predicts a 12 percentage points lower tax rate, but this is not statistically significant (tobit: $p = .160$). I summarise the findings as follows:

[33] Another poor subject commented: "I tried not to tax it too much because that will be taking more money." Opposingly, other poor subjects mentioned self-interest or fairness. For instance, "I honestly just picked the tax rate that gave me the most profit" and "I picked 100 (...) which I think is overall the most fair choice for everyone involved since there is no way to pick or influence which group you are a part of."

Result 3 *Under uniform growth rates, subjects randomised into the poor income class vote for higher tax rates than middle-income and rich subjects.*

Table 6: Class and preferred tax

	Forecast	Ratio	Realized	Total
Middle Class	-18.99***	-33.69***	-40.84***	-31.50***
	(6.84)	(7.38)	(6.68)	(4.03)
Rich	-49.27***	-50.46***	-44.67***	-48.45***
	(7.52)	(8.06)	(7.23)	(4.41)
Dictator Giving	0.30**	0.71***	0.36***	0.43***
	(0.14)	(0.15)	(0.13)	(0.08)
N	506	421	488	1415

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. The baseline is a subject randomised into the poor income class. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

3 Study 2: Unequal Growth Rates

In Study 1, I found that individuals underestimate how much (absolute) inequality increases in the presence of uniform growth, but perceived inequality did not affect preferred tax rates. Rather, what mattered was severely underestimating personal costs of redistribution. Yet, another possibility is that perceived inequality was irrelevant because *relative* and not *absolute* inequality is what matters for people's redistributive preferences. This concern could be critical for the robustness of the previous results as the development of inequality differs widely across domains (e.g., wage growth vs. stock market returns), time periods, and countries.[34] To address this concern, Study 2 has subjects earn higher interest rates the larger their endowment is, and this implies that both absolute and relative inequality increase over time. As we will see in the following, the conclusions from Study 1 replicate in this setting.

[34]In the US, for instance, Piketty et al. (2018) show that pre-tax income growth rates were fairly uniform between 1946 and 1980: the bottom 50 percent, the next 40 percent, and the top 10 percent experienced growth rates of 2.09, 2.13, and 1.73, respectively (corresponding to overall growths of 102, 105, and 79 percent). In contrast, between 1980 and 2014 the growth rates diverged to 0.03, 1.04, and 2.36, which corresponds to overall growths of 1, 42, and 121 percent. This development does not only reflect a discrepancy in wage growth. In the US Survey of Consumer Finances, for instance, the top 1 percent richest households had a higher-than-average interest rate by a factor of 1.3 in the 2010, 2013, and 2016 waves (Bricker et al., 2018; Saez and Zucman, 2019; see Fagereng et al., 2020, for evidence of a similar pattern in Norway).

3.1 Experimental Design

3.1.1 Experiment

Study 2 generally follows the design for Study 1 presented in Section 2.1. The main difference is the interest rates that the members of each income class receive. Subjects are informed that the poor, middle-income, and rich subjects receive interest rates of 24, 26, and 27 percent, respectively. Over the 30 rounds of compounded interest, the small differences in growth per round lead to widely different overall growth of 635, 1,026, and 1,301 percent. Hence, the initial (final) income levels are \$1 (\$635), \$4 (\$4,104), and \$7 (\$9,104). The only other difference compared to Study 1 is that the efficiency loss of redistribution is increased from 2 to 10 percent. This change is made to ensure that taxation is still costly for the middle class, so they experience a trade-off between maximising own payoff and equity as in Study 1.

In the voting part, subjects are randomised into either the *RealizedR* or *ForecastR* treatment (the *R* reflects that relative inequality is also affected by growth). As in Study 1, subjects in *RealizedR* receive information about the correct post-redistribution earnings, and subjects in *ForecastR* observe the post-redistribution earnings based on their subjective forecast.

3.1.2 Procedure

For Study 2, 1,105 new subjects were recruited on MTurk between 13 November and 3 December 2021. The procedures were identical to those used in Study 1, and applying the same screeners led to a main sample of 980 subjects as 11.3 percent of the responses were excluded.[35] In this sample, 44 percent were males, the mean age was 40 years, 77 percent were White or Caucasian, 43 percent had obtained a Bachelor's degree, 16 percent had obtained a Master's degree, 66 percent were employed (part or full time), and 14 percent were self-employed. The full set of summary statistics are presented in Tables S.16 and S.17.

Subjects again received USD 1 in addition to the payment from the dictator games, the voting experiment, and the incentivised forecast task. The median earnings were USD 3.5, and the median completion time was 15 minutes, which again includes any time spent off task with the browser running in the background.

3.2 Theory

The model in Section 2.2 can be extended to the case with unequal interest rates, and it yields the same qualitative predictions. For the sake of brevity, I relegate the presentation of the theoretical framework and its predictions to the Online Supplement S.2.3. Note that the

[35] As in Study 1, the results are qualitatively robust to including all responses.

theoretical framework used to derive pre-registered hypotheses was not adapted based on the previous results because all studies were conducted in parallel.

3.2.1 Hypotheses

As in Section 2.2, the model follows the literature on EGB and assumes that individuals underestimate exponential developments for all compounding rates and initial amounts. As both the absolute and relative inequality increase throughout the 30 rounds, I therefore first test the model's assumptions about perceived growth, which implies that subjects underestimate both types of inequality:

Hypothesis 3 *When interest rates correlate positively with initial endowments, subjects on average underestimate both absolute and relative inequality in the final round.*

Regarding voting behaviour, middle-income and rich subjects who exhibit EGB (i.e., in *ForecastR*) are expected to underestimate the extent of inequality in round 30 and thus underestimate the need for redistribution. The model in Online Supplement S.2.3 therefore leads to the following hypothesis:

Hypothesis 4 *Comparing individuals with the same degree of inequality aversion, middle-income and rich subjects on average vote for a higher tax rate in* RealizedR *than in* ForecastR*.*

3.3 Results

As in Study 1, I first analyse subjects' inequality forecasts and how these influence preferences in the voting experiment. I then examine whether EGB only matters for redistributive preferences in *ForecastR* and whether subjects randomised into the poor income class vote for higher taxes than those in the middle-income and rich classes. Descriptive statistics are shown in Table 7 for the middle-income and rich subjects and in Table S.18 for the poor subjects.

3.3.1 H3: Do People Underestimate Growth in Inequality?

H3 receives clear support from the data. Figures S.9-S.14 reveal a clear tendency for the subjects to underestimate both absolute and relative inequality across all measures. The underestimation is statistically significant (all $p's < 0.001$, bootstrapped t-tests).

In Study 2, 88 subjects (9 percent) provide forecasts that are within ± 1 of the correct answer for all three income classes.[36] Excluding these subjects, 45 of 892 subjects (5 percent)

[36] Of the subjects who calculated the correct answers, 64 were randomised into the middle or rich income class. The results in the next section do not change if these are excluded from the analysis (see Table S.20).

Table 7: Descriptive statistics for middle-income and rich subjects, Study 2

	N	Tax	DG	Efficiency	Actual SD	SD(F)	Actual CV	CV(F)	EGB
ForecastR	345	39.08	37.19	3.52	3223.32	1290.88	0.71	0.60	0.49
RealizedR	353	39.83	37.76	3.44	3223.32	938.57	0.71	0.59	0.51
Total	698	39.46	37.48	3.48	3223.32	1112.70	0.71	0.59	0.50

Note: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. Efficiency corresponds to subjects' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by subjects' estimates of income levels in the group. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009).

overestimate growth on average (i.e., $\theta < 0$), whereas 847 subjects (95 percent) underestimate growth on average (i.e., $0 < \theta < 1$). I discuss heterogeneity in subjects' forecasts in Online Supplement S.4.

I summarise these results as follows:

Result 4 *In case of unequal growth rates, the subjects underestimate both the absolute and relative inequality in the final round.*

Having established that subjects fail to anticipate the development in inequality also under unequal growth rates, I now examine how this influences redistributive preferences.

3.3.2 H4: Taxation

The descriptive statistics presented in Table 7 and the distribution of tax decisions shown in Figure S.15 suggest that there are no meaningful differences between *ForecastR* (mean: 39.08) and *RealizedR* (mean: 39.83). This conclusion does not change when adding controls in the tobit regressions (all $p's > 0.623$, cf. Table S.19), and it is robust to using the SCLS estimator ($p = .158$), and the MWU-test ($p = .892$).

Nevertheless, an exploratory inspection of the data reveals that EGB again matters for the subjects who grossly underestimate the personal costs of redistributing. In *ForecastR*, 48 of 203 middle-class subjects (24 percent) wrongly believe that they will gain from redistribution, and they on average vote for a tax rate that is 15.27 percentage points higher than the other middle-class subjects ($p = .004$). Wrongly perceiving the costs as minimal leads to an increase in preferred tax rate of 10.04 percentage points, but this difference fails to reach statistical significance ($p = .150$).[37]

[37]The importance of costs receives qualitative support by statements made by middle-income subject in

Similar to Study 1, the fact that tax decisions do not change with the level of inequality suggests that people exhibit a larger WTP for redistribution when absolute and relative inequality are large. Looking only at the subjects who correctly perceive that redistribution is costly, subjects are on average willing to pay (in experimental currency) \$329.45 more for redistribution in *RealizedR* than in *ForecastR*, and this difference is statistically significant (MWU, $p = .003$).

3.3.3 Mechanism 3: Does EGB Correlate With the Preferred Tax Rate?

To further shed light on the influence of forecast bias, I now examine how EGB correlates with the subjects' preferred tax rate within the two treatments. As in Study 1, I find suggestive evidence that EGB correlates with preferred tax rate in *ForecastR* (tobit: $p = .055$, cf. Table S.21; SCLS: $p = .158$; Spearman's ρ: $p = .091$), but this relation is weaker and non-robust when I control for perceived gains and perceived low personal costs (tobit: $p = .077$, cf. Table S.21; SCLS: $p = .182$; Spearman's ρ: $p = .410$).

Surprisingly, EGB is related to the subjects' preferred tax rate in *RealizedR* (tobit: $p = .042$, cf. Table S.22; SCLS: $p = .021$; Spearman's ρ: $p = .023$). Yet, this relation is insignificant once demographic controls are added ($p = .110$), and it diminishes further when attitudinal controls are included ($p = .259$). This suggests that EGB does not influence people's preferred tax rates in *RealizedR*; rather, EGB correlates with factors (in particular ethnicity) that in turn are correlated with voting preferences. I summarise these findings as follows:

Result 5 *In case of unequal growth rates, erroneous forecasts lead individuals to vote for higher tax rates to the extent that people wrongly believe that they gain from taxation.*

3.3.4 Mechanism 4: Income Class and Preferences for Redistribution

Once more, I examine the behaviour of the poor subjects to test the model's assumption that self-interest and inequality aversion are the main determinants of subjects' behaviour in the current setting. The median tax rate is 80 among the poor subjects; only 44 percent vote for full redistribution. Importantly, the poor subjects on average vote for a tax rate that is 24.57 (31.72) percentage points greater than the middle-class (rich) subjects. These differences are statistically significant ($p < .001$, cf. Table S.23) and robust (MWU: $p < .001$). Also in Study

RealizedR: "I chose a tax rate of 0% because the table indicated that would result in the highest amount for me." Now contrast this to a middle-income subject in *ForecastR* who underestimated the personal costs: "Given that the assignments to the class you are assigned to are random, the only fair option would be to equally distribute the post tax money. So, since the 100% tax rate gives equal money to all, that was my choice, especially since my own loss in revenue when compared to 0% tax was very minimal."

2, I proxy for tax aversion by political attitudes and find that going from the extreme left to the extreme right predicts a strong decrease in preferred tax rate among the poor (67 percentage points, $p < .001$). Efficiency concerns are once more statistically insignificant ($p = .466$). I sum up the behaviour of the poor as follows:

Result 6 *Under unequal growth rates, subjects randomised into the poor income class vote for higher tax rates than middle-income and rich subjects.*

4 Study 3: Voting Without Forecast

Study 1 and 2 demonstrated that subjects often do not anticipate correctly how inequality develops in the presence of growth. Yet, these inequality beliefs were irrelevant for subjects' preferences for redistribution. What mattered was instead the extent to which subjects misperceived their personal costs of redistribution. However, one concern is that Study 1 and 2 might capture other effects than inequality beliefs as subjects may be influenced by the very act of making a forecast. For instance, subjects may be surprised by how much inequality has increased by the compounding of interest and react to the surprise rather than to the level of inequality. Subjects may also understand that their forecast is uncertain and thus try to hedge against e.g. earning too little or having too much inequality in the group. Finally, it may be the case that the initial endowments serve as reference points. Then, the poor may be perceived to be well-off also at very low tax rates since their earnings are much higher in the final round than in the initial round. To address these concerns, I conduct a third study in which subjects do not engage in the subjective forecast task. Instead, subjects are randomised into one of two treatments, where the pre-tax incomes in the voting experiment correspond to either (i) the true income levels in the final round of Study 1 or (ii) forecasted income levels from the pilot for Study 1 (see Online Supplement S.1 for a description of the pilot study). This removes any possible spill-overs from the forecast task and thus provides a clean test for whether the results in Study 1 and 2 were driven by inequality beliefs. As we shall see, Study 3 replicates the results in Study 1 and Study 2.

4.1 Experimental Design

4.1.1 Experiment

Study 3 generally follows the design of Study 1 presented in Section 2.1. The major difference is that subjects do not make any subjective forecast. Instead, they participate in the voting part directly after the demographic survey. Here, subjects are randomised into one of two treatments, which differ only in the income levels of the poor, middle-income, and rich persons. In the *RealizedNo* treatment, subjects are informed that the income for a person from each income class is $808, $3,231, and $5,655, respectively (similar to the information provided in the *Realized* treatment in Study 1). In the *ForecastNo* treatment, subjects observe one of eight income forecasts that subjects provided in a pilot study.[38] As in Study 1, subjects see a table of post-redistribution earnings for various tax rates, and they are required to engage with this information to complete a mandatory comprehension check. In both treatments, the observed post-redistribution incomes correspond to the actual payoffs (again using an exchange rate of 2000:1). Thus, I avoid any reference to "forecasts" or "estimates".

4.1.2 Procedure

For Study 3, 1,186 new subjects were recruited on MTurk between 13 November and 3 December 2021. All procedures and screeners were identical to those in Study 1, except for the two screeners that were based on the forecast task which were not possible to implement in Study 3. The remaining screeners led to the exclusion of 92 responses (7.8 percent), yielding a main sample of 1,094 subjects.[39] In this sample, 41 percent were males, the mean age was 40 years, 79 percent were White or Caucasian, 38 percent had obtained a Bachelor's degree, 17 percent had obtained a Master's degree, 65 percent were employed (part or full time), and 14 percent were self-employed. The full set of summary statistics are provided in Tables S.26 and S.27.

Subjects again received USD 1 in addition to the payment from the dictator games, the voting experiment, and the incentivised questions. The median earnings were USD 2.7, and the median completion time was 11.5 minutes, which again includes any time spent off task.

4.2 Theory

The theoretical framework presented in Section 2.2 assumes that the decision-maker's subjective forecasts only matter via beliefs about inequality in the final round. Thus, the model is also directly applicable to the case where subjects simply vote based on the forecasts from the pilot study. Again, the theoretical framework used to derive pre-registered hypotheses was not adapted to the previous results because all studies were conducted in parallel.

[38] Avoiding the most extreme forecasts from the pilot study, subjects are randomised into one of the following pre-tax income allocations $(x_P; x_M; x_R)$: $\{(8;31;55), (12;46;81), (13;51;89), (30;120;210), (38;150;263), (41;162;284), (156;624;1092), (579;2315;4052)\}$.

[39] Once more, the results are qualitatively robust to including all subjects.

4.2.1 Hypothesis

In the pilot study, subjects on average underestimated the extent of absolute inequality in the final round. Thus, subjects in *ForecastNo* observe a smaller extent of absolute inequality than subjects in *RealizedNo* (see Table S.28 for an overview). This leads to the following hypothesis:

Hypothesis 5 *Comparing individuals with the same degree of inequality aversion, middle-income and rich subjects on average vote for a higher tax rate in* RealizedNo *than in* ForecastNo.

4.3 Results

In the following, I first analyse how inequality influences preferences in the voting experiment. I then examine whether poor subjects vote for more redistribution than middle-income and rich subjects. Table 8 provides descriptive statistics for the middle-income and rich subjects; Table S.30 provides information on the poor subjects.

Table 8: Descriptive statistics for middle-income and rich subjects, Study 3

	N	Tax	DG	Efficiency
ForecastNo	388	51.98	38.65	3.41
RealizedNo	397	46.75	38.70	3.38
Total	785	49.33	38.68	3.40

Note: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. Efficiency corresponds to subjects' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency).

4.3.1 H5: How Does Information About Inequality Influence Redistribution?

Contrary to H5, the average preferred tax rate is slightly higher in *ForecastNo* (52 percent) than in *RealizedNo* (47 percent). A closer look at the data suggests that this difference is driven entirely by middle-income subjects who face low levels of inequality (see Figure S.16). For these subjects, redistribution bears almost no personal costs, suggesting that perceiving low costs might be the critical factor as in the previous studies.

More formally, the difference in average preferred tax rates is marginally significant when controlling for dictator giving ($p = .067$) and becomes significant when including demographic and attitudinal controls ($p = .047$ and $p = .009$). The difference is robust to using a MWU-test ($p = .050$) but not to using an SCLS estimator ($p = .143$). In this study, there is no effect of perceived gains as the forecasted incomes from the pilot were chosen such that all middle-income and rich subjects should realise that equalising incomes comes at a personal cost. However, some of the forecasted incomes imply very low personal costs for the middle income group. As in previous studies, these low personal costs imply that a middle-income subject who only cares about his own payoff will be indifferent between different tax rates. Hence, these middle-income subjects will vote for greater redistribution even if they are only slightly inequality averse. Accordingly, subjects who faced low personal costs on average vote for tax rates that are 29.55 percentage points greater ($p < .001$, cf. Table S.29). Moreover, when including a dummy for low personal costs, there is no difference between *ForecastNo* and *RealizedNo* ($p = .613$, cf. Table S.29; SCLS: $p = .956$; MWU: $p = .824$). Corroborating the results from Study 1 and Study 2, this demonstrates that it is not the extent of inequality per se that matters. Rather, what matters is observing such low personal costs of redistribution that even slightly inequality averse individuals vote for higher tax rates.

As in Study 1 and Study 2, people exhibit a larger WTP for redistribution when inequality is large. For subjects who correctly perceive that redistribution is costly, average WTP for redistribution is $293.44 higher in *RealizedNo* than in *ForecastNo*, and this difference is statistically significant (MWU, $p < .001$).

Result 7 *When subjects do not make forecasts, perceptions of inequality influence preferred tax rates only to the extent that people believe that redistribution comes at very low personal costs.*

4.3.2 Mechanism 5: How Do the Poor Vote?

Similar to Study 1 and Study 2, the median tax rate is 80 among the poor subjects, and 47 percent vote for full redistribution. The poor on average vote for a tax that is 15.30 higher than the middle-class subjects and 25.01 higher than the rich subjects. These differences are statistically significant ($p < .001$, cf. Table S.31) and robust (MWU: $p < .001$). With political attitudes as a proxy for tax aversion (and still controlling for dictator giving as a proxy for inequality aversion), I find that going from the extreme left to the extreme right predicts a decrease in preferred tax rate among the poor of 42 percentage points ($p = .003$). Efficiency concerns are again not statistically significant ($p = .686$).

Result 8 *When subjects do not make forecasts, subjects randomised into the poor income class vote for higher tax rates than middle-income and rich subjects.*

5 General Discussion

In the following, I discuss exploratory findings across Studies 1-3. In doing so, note that the three studies were run in parallel with very similar experimental designs, and I can therefore compare behaviour also across the three studies. First, I demonstrate that forecast bias significantly correlates with misperceiving wealth inequality in the US. Second, I compare results from Studies 1 and 3 to discuss the influence of making a forecast on preferences for redistribution in the current setting. Third, I comment on the correlation between perceived inequality in society and voting preferences.

In Online Supplement S.5, I show that the current studies replicate previous results on giving in dictator games, underestimation of wealth inequality in the US, and the prevalence of exponential growth bias. I furthermore comment on the relative importance of self-interest, inequality aversion, efficiency concerns, and image concerns for redistributive preferences. Finally, I demonstrate that the extent of inequality in a group does not predict subjects' voting preferences regardless of how inequality is operationalised.

5.1 EGB Significantly Predicts Misperceptions of Inequality in Society

As shown in Study 1 and Study 2, subjects on average underestimate the increase in inequality that occurs when growth rates are uniform or unequal, and this is driven by the fact that most subjects (92 percent) exhibit EGB. Such misperceptions should lead individuals to underestimate inequality in the field unless they continuously update their beliefs about inequality (which is highly unlikely, see e.g. Carroll, 2003; Mankiw and Reis, 2006; Reis, 2006). Accordingly, subjects' who exhibit a larger extent of EGB in the abstract setting of the current studies tend to more severely underestimate wealth inequality in the US (Spearman's $\rho = -0.121$, $p < .001$). This relation holds when adding controls for demographics and attitudinal measures ($p < .001$, cf. Table S.37), and the relation remains when restricting the sample to subjects who did not calculate the correct answer (all $p's < 0.001$, cf. Table S.38). Moreover, the rank correlation is robust for both Study 1 and Study 2, it is robust to using subjects' forecasted inequality levels instead of their bias, and it is robust to using Kendall's tau instead of Spearman's ρ (all $p's < 0.030$). Even though a rank correlation of -0.12 is only of modest size, it is economically considerable, especially considering that it is based on a comparison between an abstract forecasting task and the complex issue of wealth inequality in the US. While the evidence is purely correlational, it is striking that it is robust to including a wide range of controls, such as education, employment status, income, and inequality attitudes. This suggests that the stylized setting of the experiment does capture elements that contribute to biased beliefs about inequality in the field.

5.2 Effect of Making a Forecast

The analyses of Studies 1-3 demonstrate that the level of inequality does not generally affect subjects' preferences for redistribution regardless of whether subjects first engage in the forecasting task. A different question concerns whether preferences for redistribution are influenced by witnessing the level of inequality in round 1. For instance, the initial distribution may serve as a reference point, and subjects may believe that as long as all income classes earn more than the initial levels, there is no need for redistributing earnings. This line of reasoning is similar to the idea of maximising income with a floor constraint (cf. Boulding's principle, Boulding, 1962; Frohlich et al., 1987; Traub et al., 2005).

One can test this reasoning by comparing *Realized* and *RealizedNo* as the only difference between these treatments is that only subjects in *Realized* make a forecast. If the initial earnings serve as reference points, subjects in *Realized* should be less inclined to redistribute as the poor are well-off in the sense that their earnings are much higher in round 30 than in round 1. Supporting this idea, subjects on average vote for a higher tax rate in *RealizedNo* than in *Realized* when controlling for dictator givings (9 percentage points, $p = .024$, cf. Figure S.17), and this difference is robust (additional controls in Table S.36; SCLS: $p = .017$; MWU: $p = .033$).

5.3 Perceived Inequality in Society and Voting Preferences

A large literature on perceptions of inequality discusses the role of beliefs for people's preferences, showing that people who perceive more inequality tend to be more supportive of government redistribution (Fong, 2001; Hayes, 2013; Bobzien, 2020, but see García-Sánchez et al., 2018). I find the same pattern in this study: subjects who perceive more wealth inequality in the US are more inclined to state that it is the government's responsibility to reduce income differences (Spearman's $\rho = .110$, $p < .001$), and they are less likely to place themselves to the right on a left-right political spectrum (Spearman's $\rho = -.204$, $p < .001$).

While some studies interpret this relation as if it was causal, it is likely that preferences also influence perceptions: individuals who are more concerned about inequality might be more inclined to find, notice, and recall information about inequality. As people tend to underestimate inequality on average, this means that individuals who are more concerned about inequality will believe that there is greater inequality in society. Indeed, pooling all treatments I find that people who believe wealth inequality in the US to be higher also vote for a greater tax rate in the (unrelated) voting experiment. The estimated effect has the interpretation that moving from a believed Gini of 0 to a believed Gini of 1 correlates with an increase in preferred tax rate of 25 percentage points ($p = .029$, also when including demographic controls, cf. Table S.41).[40] This shows that people who perceive greater inequality in society are also more concerned about inequality in an abstract environment. Hence, the relation between inequality perceptions and preferences may be more complex than hitherto recognised (see also Kteily and Sheehy-Skeffington, 2017). Specifically, perceptions of inequality in society influence preferences, but inequality preferences may also influence perceptions (i.e., two-way causality).

6 Conclusion

Many policies affect future inequality, from taxation to investments in children's equal access to education. In this paper, I have examined how well people predict future inequality and whether people's possibly erroneous forecasts influence their preferences for redistribution. I have demonstrated that people tend to underestimate how much inequality may increase in the presence of economic growth. Following the literature on exponential growth bias, people underestimate absolute inequality when growth rates are uniform, and they underestimate both absolute and relative inequality when growth rates are positively related to initial incomes. In addition, I have shown that biased perceptions of inequality may increase support for redistribution if subjects wrongly believe (i) that they will gain from tax-financed redistribution, or (ii) that redistribution comes at negligible personal costs. When subjects learn that inequality is greater than they thought, they become more concerned about inequality as evidenced by a greater willingness-to-pay for redistribution in absolute amounts. Yet, net contributors also realise that they incur greater personal costs at a given tax rate when income differences are large. The greater costs offset the greater concern for redistribution, leaving the tax rate unchanged. Hence, this paper suggests that most individuals underestimate future inequality, but underestimating inequality does not matter for policy support *per se*. Rather, what matters is whether individuals know if redistribution is costly for themselves.

Yet, there are some issues that limit the external validity of the current studies. First, subjects make redistributive decisions using windfall earnings. This design feature typically makes individuals more inclined to redistribute, which in turn makes it easier to detect in-

[40] Beliefs about wealth Gini in the US correlate significantly with attitudinal variables; thus, I refrain from including these variables to avoid multicollinearity. Specifically, people who think wealth inequality is higher tend to support the political left, believe that meritocracy matters less for success, think that inequality is too large, and think that the government is responsible for reducing inequality (all $p's < .001$, also when considering instead rank correlations).

teractions between information provision and preferences for redistribution. It is likely that people on average prefer less redistribution outside this experiment if they feel that incomes to a greater extent reflect what individuals deserve.

Second, subjects make decisions completely on their own. In real life, people are influenced by peers, experts, media, etc., and many organisations and think tanks make forecasts about future inequality. This paper does not address what type of information people seek and avoid in the field.

Finally, subjects only make forecasts once, and this precludes opportunities for learning. One might believe that people learn from their mistakes in the field, and that they therefore provide better estimates of future inequality. Yet, in the field, many confounding factors can obscure trends in inequality, citizens do not receive any feedback on the accuracy of their beliefs, and there is a long time lag between making a forecast and the realisation of "future" inequality. Another factor that indicates how learning in the field might be limited is the prevalence of exponential growth bias and that people do not realise their bias (Cordes et al., 2019). This stresses the importance of future studies that examine how one can improve forecasts (see e.g. Banerjee and Majumdar, 2020, and Song, 2020, for the contexts of exponential growth relating to the COVID-19 pandemic and pensions).

Future studies should also consider how EGB interacts with other cognitive biases. In the current studies, subjects make forecasts about future income levels without knowing their own position in the group. In the field, however, people have beliefs about their own social position and whether they benefit or lose from redistribution. In such a setting, it is likely that EGB interacts with other biases such as motivated reasoning (Kunda, 1990). Intuitively, net contributors may be motivated to believe that inequality is limited and does not rise much over time as this would enable them to support a low degree of redistribution without creating a self-image as someone who is self-interested. To engage in such motivated reasoning, individuals may avoid information about inequality (strategic ignorance, cf. Dana et al., 2007, Conrads and Irlenbusch, 2013, Grossman, 2014, and Grossman and van der Weele, 2017) or their predictions about future inequality may be distorted (motivated beliefs, cf. Epley and Gilovich, 2016, Gino et al., 2016, Exley and Kessler, 2019, and Zimmermann, 2020).

Another interesting avenue for future studies is to examine how inequality information interacts with non-consequentialist motives for voting. The present study builds on the assumption that people have consequentialist motives; that is, they are only concerned with the vote to the extent that it may influence the post-redistribution earnings in the group. Nevertheless, a substantial literature on distributive preferences suggests that voters also want to e.g. express a preference for a certain outcome (Brennan and Buchanan, 1984), maintain a positive self-image (Bénabou and Tirole, 2006b), or follow deontological principles (Andreoni et al., 2020). Shayo and Harel (2012) and Paetzel et al. (2014) show that consequentialist concerns increase with the likelihood that a voter is pivotal, and the small group size used in the current experiments warrants this paper's focus on consequentialist motives. In natural settings, however, referenda are often characterised by a vast number of voters (e.g., millions in democratic elections). It would thus be an interesting avenue for future research to examine how different non-consequentialist motives are affected by inequality information.

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A Screeners

In the following, I describe the pre-registered screeners that I applied in each of the studies to ensure high-quality data.[41]

Note that I recruited subjects for all three studies on MTurk at the same time. Subjects were randomly allocated into one of the three studies only after passing the VPN/VPS and the bot tests.[42] Across the three studies, subjects were informed that they were only allowed to participate from the US and without using a VPS or VPN. This was confirmed prior to the study using IP Hub (Kennedy et al., 2020, but see Dennis et al., 2020). IP Hub detected 96 individuals who tried to access the study from outside the US, and 186 individuals tried to access the study using a VPS, VPN, or other proxy. To detect bots, the survey included two honeypots (coded in JavaScript). Following Moss and Litman (2018a), these honeypots were survey items hidden from human subjects, which would be read by a computer. Respondents who answered any of these questions were thus confirmed bots, and they were not allowed to continue.[43] There were 6 bots, supporting the evidence by Moss and Litman (2018b) and Zhang et al. (2022) that farmers rather than bots are the biggest threat to data quality on MTurk.[44]

A.1 Study 1

As described in Section 2.1.6, I screened out 10.6 percent of the 1,584 subjects for Study 1. First, I excluded subjects who made forecasts that failed to rank the three income groups as *poor* $<$ *middle* $<$ *rich*. This led to the exclusion of 34 respondents. Second, Wood et al.

[41] The study did not involve any attention checks such as the widely used Instructional Manipulation Checks (Oppenheimer et al., 2009). For one thing, these have become so common that their diagnostic value for MTurk samples is fairly limited (Hauser and Schwarz, 2016; Thomas and Clifford, 2017). In addition, there has been critique that attention checks alter subjects' behaviour (Hauser and Schwarz, 2015; Hauser et al., 2018), and that they may result in participants providing lower quality data. This has led Qualtrics to recommend that researchers do not use them (Vannette, 2017).

[42] There have been recent concerns about poor data quality from so-called "farmers" (Moss and Litman, 2018b). These subjects participate from outside the US, possibly masking their location using a Virtual Private Server (VPS) or Virtual Private Network (VPN).

[43] Designing bot detection in this way has advantages over using (re)CAPTCHAs as some bots are now sophisticated enough to pass CAPTCHAs (Sivakorn et al., 2016; Al-Fannah, 2017). Moreover, it is an unobtrusive approach, saving time and making it easier for people with visual impairments to complete the study (Bursztein et al., 2010).

[44] One honeypot was placed on the page of the consent form, the other on the page with demographic questions. Interestingly, two of the six bots were detected on the demographic questions. This implies that the bots operate alongside humans as noted by Zhang et al. (2022). If researchers wish to protect their online studies against bots, it is therefore not sufficient to only place honeypots at the beginning or end of one's online experiment.

(2017) show that subjects who answer more than one item per second provide responses of poor quality. I therefore excluded 2 additional respondents based on their response times in the attitudinal survey (see also Aguinis et al., 2020).[45] Third, I followed Kennedy et al. (2020) and included a consistency check. In the demographic survey, subjects were asked about their age, and subjects provided their year of birth in the attitudinal survey (see also Zhang et al., 2022). This led to the additional exclusion of 38 subjects who did not provide matching ages and years of birth. Fourth, I placed a screener at the end of the voting experiment to further improve the detection of farmers. Subjects were asked to describe how the tax influenced the equality of earnings in their group in 1-2 sentences. This helped identify respondents (typically farmers) who are not proficient in English (Dennis et al., 2020; Zhang et al., 2022). Following Chmielewski and Kucker (2020), I flagged responses that grossly misused the English language, nonsense phrases, and single words unrelated to the question (e.g., "nice" and "good"). This led to the exclusion of an additional 72 responses. After collecting the data, an additional problem emerged with subjects who made forecasts that implied zero growth in all incomes, implying that they did not exert effort in understanding the subjective forecast task. To make results as accurate as possible, I exclude an additional 23 subjects who provided such answers although this screener was not pre-registered. Removing this screener does not change the results of this study.

A.2 Study 2

I applied the same screeners in Study 2 as in Study 1, and it led to the exclusion of 11.3 percent of the 1,105 subjects. Specifically, I excluded 23 subjects who did not make forecasts that ranked the income groups as $poor < middle < rich$. One additional subject was excluded because he answered more than one item per second in the attitudinal survey. An additional 35 subjects gave inconsistent responses for their age and year of birth. Based on the text screener, I excluded 47 further subjects. Finally, as in Study 1 I applied the additional (not pre-registered) screener, whereupon subjects who made forecasts with only zero growth were removed. There were 19 such subjects. Again, the additional screener does not change the results of the experiment.

[45] Similar to Wood et al. (2017), I measured response times using Qualtrics and calculated items per second as $\frac{K-1}{T_{CS}-T_{C1}}$, where K is the number of items on a page, T_{CS} is the time taken to click submit, and T_{C1} is the time taken to make the first click on the page. I subtract 1 in the numerator as the timing variable reflects the time taken to answer all the items after the first click, and I assume that the first click corresponds to one item on the page.

A.3 Study 3

I applied the same screeners in Study 3 as in Study 1 (except the two based on forecast tasks), and it led to the exclusion of 7.8 percent of the 1,186 subjects. In this study, 2 subjects answered more than one item per second in the attitudinal survey. An additional 35 subjects provided inconsistent responses for their age and year of birth. There were 55 other subjects who failed the text screener.

B Attrition

In the following, I use logit regressions to examine what factors explain whether subjects complete the study (see Table B1). For this analysis, I only consider subjects who pass all screeners until the point where they drop out. Moreover, as mentioned in Footnote 17 a technical error in *Ratio* caused issues during the first two hours of the study. To test differential attrition, I therefore also exclude subjects in *Ratio* who began the study during the first two hours. As the attitudinal survey was the last that subjects completed, I am not able to examine how attitudes affect whether subjects complete the study.

One concern is that subjects may be more willing to complete the study if they are randomised into the rich income group compared to other income groups. I find that subjects are indeed 2.6 percentage points more likely to complete the study if they are in the rich income group, but this difference is only marginally significant ($p = .072$).

A second concern is that there may be differential attrition by treatment. The only significant difference is that subjects in *RealizedNo* are 3.8-7.8 percentage points more likely to complete the study than subjects in any other treatment (although the difference with *Forecast* is only marginally significant). Importantly, subjects in *RealizedNo* did not have to perform the forecast task, and the experiment was therefore a few minutes shorter for these subjects than for subjects in Study 1 or Study 2. Consequently, the significant differences in attrition is likely to be caused by the length of the experiment rather than the content of the experiment.

A final thing to notice is that better educated subjects are more likely to complete the study. Pooling subjects with a bachelor's, master's, doctorate, or professional degree shows that subjects with such educations are on average 5.4 percentage points more likely to complete the study ($p < .001$). This result is intuitive as the experiment is somewhat more cognitively demanding than many other studies on MTurk, in particular the forecast task and the estimation of wealth quintiles. But if anything, differential attrition by education would imply that the estimates for subjects' misperceptions is conservative in the current paper.

Table B1: Attrition

	(1)	(2)	(3)	(4)
Middle Class	0.0060	0.0081		
	(0.01)	(0.01)		
Rich	0.0261*	0.0256*		
	(0.01)	(0.01)		
Ratio			-0.0240	-0.0287
			(0.02)	(0.02)
Realized			-0.0404*	-0.0416*
			(0.02)	(0.02)
ForecastR			-0.0270	-0.0270
			(0.02)	(0.02)
RealizedR			-0.0327	-0.0360*
			(0.02)	(0.02)
ForecastNo			-0.0061	-0.0081
			(0.02)	(0.02)
RealizedNo			0.0376*	0.0375*
			(0.02)	(0.02)
Dictator Giving		-0.0006*		-0.0006*
		(0.00)		(0.00)
Efficiency from MDG		-0.0006		-0.0018
		(0.00)		(0.00)
Age		-0.0027***		-0.0031***
		(0.00)		(0.00)
Male		0.0046		0.0134
		(0.01)		(0.01)
Black or African American		-0.0410*		-0.0454**
		(0.02)		(0.02)
Hispanic or Latino		-0.0531*		-0.0695**
		(0.03)		(0.03)
Asian American		-0.0322		-0.0273
		(0.03)		(0.03)
Other ethnicity		-0.0729		-0.1009**
		(0.04)		(0.05)
High school degree or equivalent (e.g. GED)		0.1122		0.0846
		(0.11)		(0.11)
Some college, no degree		0.1591		0.1345
		(0.11)		(0.11)
Associate degree (e.g. AA, AS)		0.1222		0.0989

	(1)	(2)	(3)	(4)
		(0.11)		(0.11)
Bachelor's degree (e.g. BA, BS)		0.1846*		0.1678
		(0.11)		(0.10)
Master's degree (e.g. MA, MS, MEd)		0.1935*		0.1891*
		(0.11)		(0.11)
Doctorate or pro degree (e.g. MD, DDS, PhD)		0.2400**		0.2228**
		(0.11)		(0.11)
Self-employed		0.0195		0.0207
		(0.02)		(0.02)
Unemployed		0.0241		0.0134
		(0.02)		(0.02)
Student		0.0406		0.0211
		(0.03)		(0.03)
Retired		0.0053		-0.0046
		(0.02)		(0.03)
Other employment		0.0208		0.0125
		(0.03)		(0.03)
Observations	4095	4095	4219	4219

Note: logit regressions with a study completion dummy as the dependent variable, reporting average partial effects. The baseline is a person who is randomised into the poor income group and the *Forecast* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

C Additional Tables and Figures

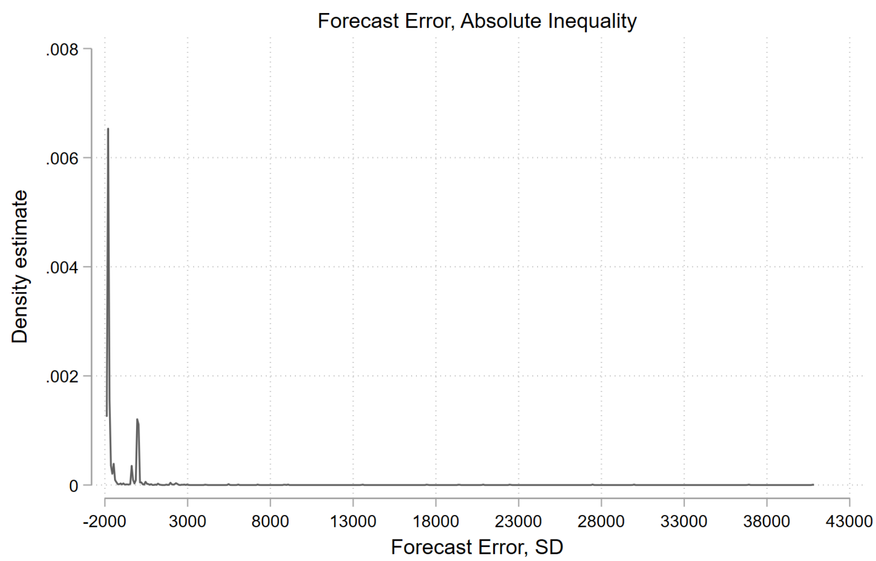
Table C1: Sample characteristics, Study 1

	Freq.	Percent
Female	826	58.4
Male	589	41.6
White or Caucasian American	1152	81.4
Black or African American	100	7.1
Hispanic or Latino	52	3.7
Asian American	88	6.2
Other ethnicity	23	1.6
Less than a high school diploma	7	0.5
High school degree or equivalent (e.g. GED)	116	8.2
Some college, no degree	270	19.1
Associate degree (e.g. AA, AS)	156	11.0
Bachelor's degree (e.g. BA, BS)	585	41.3
Master's degree (e.g. MA, MS, MEd)	219	15.5
Doctorate or professional degree (e.g. MD, DDS, PhD)	62	4.4
Employed (part or full time)	917	64.8
Self-employed	181	12.8
Unemployed	159	11.2
Student	55	3.9
Retired	67	4.7
Other employment	36	2.5
Total	1415	100.0

Table C2: Summary statistics by treatment, Study 1

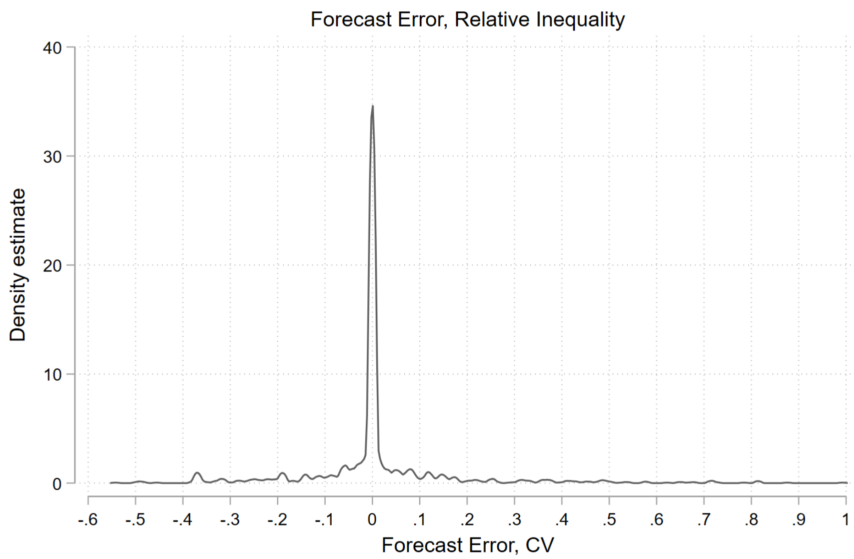
	Forecast	Ratio	Realized	Total
Risk	0.47	0.47	0.46	0.47
Trust	0.53	0.54	0.53	0.53
Political Right	0.44	0.43	0.42	0.43
Belief in Meritocracy	0.63	0.62	0.59	0.62
Inequality Too Large	0.80	0.81	0.80	0.81
Government Responsibility	0.57	0.57	0.59	0.58
MLAMS	0.37	0.38	0.38	0.38
Social Ladder	0.43	0.43	0.42	0.43

Figure C1: Forecast error of absolute inequality, Study 1, full sample



Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$.

Figure C2: Forecast error of relative inequality, Study 1, full sample



Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 0.005$). The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$.

Table C3: EGB and tax inconsistency, Study 1

	(1)	(2)	(3)	(4)	(5)
Ratio	-8.41*	-7.85*	-7.49*	0.08	-1.23
	(4.56)	(4.53)	(4.32)	(4.44)	(4.44)
Realized	-12.68***	-12.98***	-13.88***	-6.53	-2.58
	(4.36)	(4.34)	(4.14)	(4.25)	(4.44)
Dictator Giving	0.73***	0.70***	0.58***	0.61***	0.62***
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Age		-0.36**	-0.19	-0.20	-0.18
		(0.17)	(0.17)	(0.17)	(0.17)
Male		-7.93**	-5.01	-4.96	-4.45
		(3.75)	(3.64)	(3.58)	(3.57)
Black or African American		-1.40	-2.99	-2.59	-2.62
		(7.08)	(6.74)	(6.62)	(6.59)
Hispanic or Latino		5.41	2.56	2.99	4.61
		(9.39)	(8.88)	(8.73)	(8.71)
Asian American		-1.73	-5.31	-5.21	-5.13
		(7.58)	(7.25)	(7.15)	(7.11)
Other ethnicity		-2.36	0.71	0.81	2.15
		(15.89)	(15.15)	(14.92)	(14.87)
High school degree or equivalent (e.g. GED)		-23.79	-16.01	-19.39	-18.70
		(24.56)	(23.45)	(23.04)	(22.83)
Some college, no degree		-16.71	-14.65	-19.06	-17.66
		(24.19)	(23.08)	(22.68)	(22.47)
Associate degree (e.g. AA, AS)		-23.46	-19.14	-26.34	-25.14
		(24.57)	(23.45)	(23.07)	(22.86)
Bachelor's degree (e.g. BA, BS)		-17.67	-16.47	-21.13	-19.67
		(24.21)	(23.12)	(22.72)	(22.51)
Master's degree (e.g. MA, MS, MEd)		-9.23	-10.19	-16.05	-15.28
		(24.58)	(23.47)	(23.08)	(22.86)
Doctorate or pro degree (e.g. MD, DDS, PhD)		-26.05	-26.43	-32.04	-30.52
		(25.73)	(24.59)	(24.19)	(23.98)
Self-employed		7.24	7.09	6.42	5.74
		(5.70)	(5.45)	(5.36)	(5.34)
Unemployed		0.86	0.54	-1.09	-2.24
		(6.04)	(5.81)	(5.73)	(5.72)
Student		-9.64	-13.12	-14.09	-14.19
		(9.94)	(9.50)	(9.38)	(9.34)
Retired		3.57	-0.23	0.85	-0.22

	(1)	(2)	(3)	(4)	(5)
		(9.77)	(9.38)	(9.23)	(9.19)
Other employment		1.29	7.03	5.90	3.93
		(12.58)	(12.11)	(11.86)	(11.80)
Income$_p$		-30.25***	-8.16	-11.45	-10.66
		(10.83)	(10.78)	(10.63)	(10.59)
Efficiency from MDG			-1.51**	-1.60**	-1.62**
			(0.68)	(0.67)	(0.66)
Risk$_p$			-12.85*	-11.84	-11.32
			(7.39)	(7.27)	(7.24)
Trust$_p$			16.34**	16.13**	15.06**
			(7.60)	(7.48)	(7.45)
Political Right$_p$			-25.49***	-22.35***	-22.19***
			(7.88)	(7.76)	(7.72)
Meritocracy$_p$			-14.01*	-12.97*	-13.70*
			(7.46)	(7.34)	(7.30)
Inequality Too Large$_p$			22.50***	21.17**	20.96**
			(8.34)	(8.21)	(8.17)
Government Responsibility$_p$			10.77	12.73*	12.79*
			(7.28)	(7.17)	(7.14)
MLAMS$_p$			-7.09	-9.03	-9.81
			(11.83)	(11.65)	(11.60)
Perceived Gains				46.17***	50.03***
				(8.09)	(8.18)
Low Personal Cost					13.55***
					(4.70)
Observations	1013	1013	1013	1013	1013

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Perceived gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. The baseline is a person in *Forecast* who is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Online Supplement

Growth and Inequality: Experimental Evidence on How Misperceptions Affect Redistribution

Jonas Pilgaard Kaiser

February 2, 2023

S.1 Pilot Studies

S.1.1 Study 1 Pilot

S.1.1.1 Experimental Design

The experimental design for the Study 1 pilot was similar to the Study 1 described in Section 2.1. But the pilot study gave rise to some changes that I describe in the following.

Inequality Forecast. In the spirit of Lergetporer et al. (2020), the pilot included a question about how many additional rounds subjects believed it would take for the poor to reach the level of earnings that the rich had in round 30. While Lergetporer et al. (2020) successfully use a similar question within education, I removed this question from the main study. First, the pilot showed that subjects found this question particularly difficult. Second, the interpretation of this question was not obvious within economic inequality as EGB may lead to both under- and overestimation of the number of additional rounds. That is, underestimation may be due to underestimation of inequality, but overestimation may also follow from subjects underestimating how fast incomes grow.

Voting Experiment. In the pilot study, subjects were compensated with an exchange rate of 1000:1. As the subjects spent less time than anticipated when completing the experiment, this exchange rate was changed to 2000:1 in the main experiment.

The pilot study included an efficiency loss of 10 percent, but this was lowered to 2 percent in Study 1 to focus on inequality (with efficiency loss as a tie breaker for the middle-income group). The main study nevertheless revealed that this change did not influence the importance of efficiency preferences (see Online Supplement S.5.2).

Additional Measures. The pilot did not include the modified dictator game or Martin-Larson Approval Motivation Scale. These measures of efficiency and image concerns were later added to better explain subjects' motives for choosing different levels of redistribution, (see Online Supplement S.5.2).

Screeners. As discussed in Section 2.1.6 of the paper, one concern with online experiments is that some participants may use bots cooperatively such that the bot fills out some things and the human completes other tasks (e.g., reCAPTCHAs). To better detect bots, I added a second bot screener on the page that asked about demographics.

In addition, a coding error in the pilot prevented accurate timing data of the individual parts of the experiment. This error was fixed for the main study.

Items in the Attitudinal Survey. In the pilot study, subjects were asked to indicate (Yes/No) whether they knew what compounding interest was (following McKenzie and Liersch, 2011). If subjects answered "Yes", they were asked to explain the concept in 1-2 sentences. These items were removed from the main study. The pilot demonstrated that a substantial fraction of subjects simply answered this question via a search engine (Google or similar) – even though they were not incentivised to answer the question correctly. Some extent of cheating is to be expected on factual questions in online surveys (e.g., Goodman et al., 2013). But the fraction of subjects who cheated was so large (between 1/3 and 1/4 compared to 1/10 in Goodman et al., 2013) that the item had very little informative value. This could suggest that many subjects found the question very difficult, and even subjects who know what compounding is may find an explanation on Google rather than typing one themselves to save time.

In the pilot study, subjects also answered an item from the International Social Survey Programme (ISSP) survey in which they see five diagrams of income distributions and pick the one they think best describes the US ((Niehues, 2014; Gimpelson and Treisman, 2018; Bobzien, 2020; Knell and Stix, 2020)). This qualitative measure was removed as it provided little additional information relative to the estimation of wealth quintiles (Norton and Ariely, 2011; Norton et al., 2014; Franks and Scherr, 2019).[1]

[1]Already in the pilot study, I noted that the quintile estimation task was difficult for many subjects: across the 73 subjects in the pilots for Study 1 and Study 2, only 41 provided valid responses in the sense that they reported (weakly) larger shares of wealth for wealthier quintiles (with at least one share being strictly larger).

S.1.1.2 Procedure

For the pilot study, 61 subjects were recruited on Amazon's Mechanical Turk (MTurk) on 9-11 July 2021, and the experiment was implemented in Qualtrics. As with the main study, I collected the data over a weekend to avoid biasing the sample against people with full employment (Casey et al., 2017). I applied the same restriction as in the main study that only respondents in the US who had completed 100 Human Intelligence Tasks (HITs) with at least 99 percent approval rate were allowed to participate. I applied the same screeners, except that the timing screener was not possible due to a coding error. Applying the screeners lead to a main sample of 55 subjects as 11 percent of the total responses were excluded. Summary statistics are provided in Table S.1.

S.1.1.3 Analysis

In the following, I depict the results from the pilot study. Due to the very limited sample size of $N = 45$, I focus on descriptive statistics (summarised in Table S.2) and graphical illustrations in this section and refrain from conducting any statistical inference.

Before turning to the main experimental tasks, note that subjects in the pilot provided answers comparable to the literature within social preference, exponential growth bias, and misperceptions of inequality (see Online Supplement S.5.1 for the main study). First, subjects gave on average 35.3 percent of their endowment to the recipient in the dictator game. This is close to the 33.2 percent that Amir et al. (2012) find using an MTurk sample and the same stake size as the current experiment, and it is not far from the average dictator giving of 28.4 percent that Engel (2011) find in a meta-analysis of dictator games. Second, the average exponential growth bias in the pilot was $\hat{\theta} = .45$ (using the functional form from Stango and Zinman, 2009), and this lies between the average bias of $\hat{\theta} = .44$ that Almenberg and Gerdes (2012) find in their restricted sample and the average bias of $\hat{\theta} = .67$ that Song (2020) finds in his control group. Third, subjects provide answers to the wealth estimation task that suggest a wealth Gini of .55, and they on average prefer a much more equal society, corresponding to a wealth Gini of .18. This is approximately the same as Norton and Ariely (2011) and Franks and Scherr (2019) find using the same task with US samples.

This is in contrast to higher rates of valid responses in earlier lab studies that use student samples. It is very likely that the discrepancy results from (i) that the lab studies focus solely on this estimation task, (ii) experimenters in the lab may explain the concept of a 'quintile' to subjects who do not understand it, and (iii) the lab studies use only students as subjects, whereas the MTurk sample is much more diverse. Nevertheless, I kept the measure in the study as I still considered it useful for exploratory purposes, and removing the least sophisticated subjects bias the underestimation of inequality in a conservative direction.

Table S.1: Sample characteristics, Study 1 pilot

	Freq.	Percent
Female	25	45.5
Male	30	54.5
White or Caucasian American	43	78.2
Black or African American	4	7.3
Hispanic or Latino	2	3.6
Asian American	4	7.3
Other Ethnicity	2	3.6
High school degree or equivalent (e.g. GED)	4	7.3
Some college, no degree	5	9.1
Associate degree (e.g. AA, AS)	4	7.3
Bachelor's degree (e.g. BA, BS)	31	56.4
Master's degree (e.g. MA, MS, MEd)	7	12.7
Doctorate or professional degree (e.g. MD, DDS, PhD)	4	7.3
Employed (part or full time)	41	74.5
Self-employed	8	14.5
Unemployed	3	5.5
Retired	2	3.6
Other employment	1	1.8
Total	55	100.0

Subjective Forecasts. As seen in Figures S.1 and S.2, most subjects underestimate the extent of absolute inequality, whereas subjects on average are much better at predicting the extent of relative inequality. These figures also show that the result holds regardless of whether absolute inequality is measured as the standard deviation or the absolute Gini coefficient and regardless of whether relative inequality is measured by the coefficient of variation or the Gini coefficient.

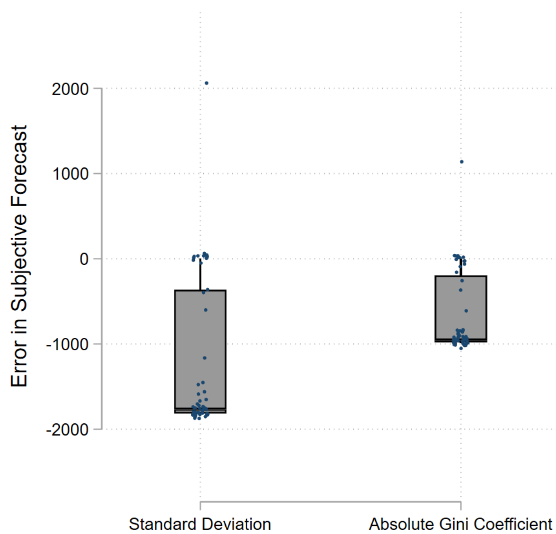
Voting Behaviour. Figure S.3 shows the subjects' tax decision. Here, I only examine responses by middle-income and rich subjects, and I separate responses by treatment. Thus, there are too few data points to draw any conclusions from this sample.

Table S.2: Descriptive statistics for middle-income and rich subjects, Study 1 pilot

	N	Tax	DG	Actual SD	SD (F)	Actual CV	CV (F)	EGB
Forecast	16	37.81	39.06	1831.99	640.34	0.57	0.55	0.37
Ratio	11	21.82	27.73	1831.99	373.58	0.57	0.60	0.47
Realized	11	34.09	32.73	1831.99	845.34	0.57	0.58	0.44
Total	38	32.11	33.95	1831.99	622.46	0.57	0.57	0.42

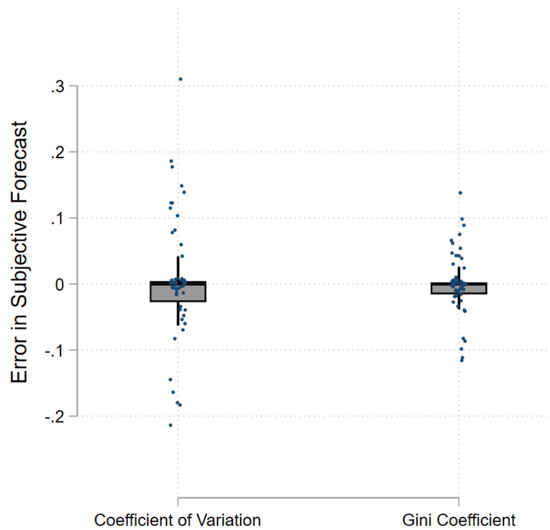
Note: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by subjects' forecasted income levels in the group. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009).

Figure S.1: Forecast error of absolute inequality, Study 1 pilot



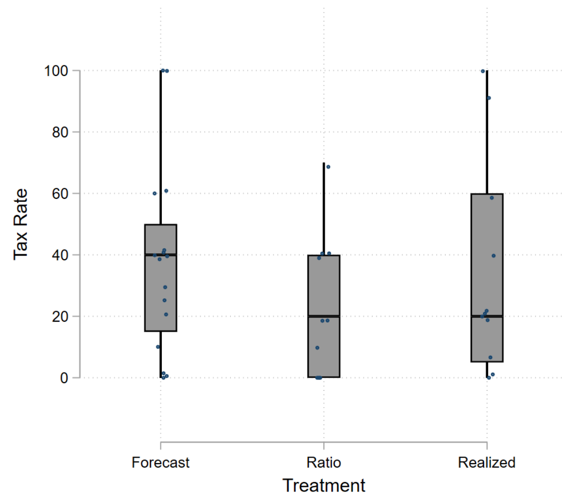
Note: box plots show the median as well as upper and lower quartiles of forecast errors (raw data). Spikes extend to the largest or smallest values within 1.5 times the upper or lower quartiles, respectively. The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$, and the absolute Gini coefficient is calculated as $AG(\mathbf{x}) = \bar{x}\left(\frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i\right)$, where x_i are ranked-ordered incomes such that $x_i \leq x_{i+1}$.

Figure S.2: Forecast error of relative inequality, Study 1 pilot



Note: box plots show the median as well as upper and lower quartiles of forecast errors (raw data). Spikes extend to the largest or smallest values within 1.5 times the upper or lower quartiles, respectively. The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$, and the Gini coefficient is calculated as $G(\mathbf{x}) = \frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i$, where x_i are ranked-ordered incomes such that $x_i \leq x_{i+1}$.

Figure S.3: Preferred tax rate by treatment, Study 1 pilot



Note: box plots show the median as well as upper and lower quartiles of preferred tax rates (raw data). Spikes extend to the largest or smallest values within 1.5 times the upper or lower quartiles, respectively.

S.1.2 Study 2 Pilot

S.1.2.1 Experimental Design

The pilot for Study 2 gave rise to the same design changes as described above (Online Supplement S.1.1.1), except that the efficiency loss was not changed to maintain the trade-off between own payoff and equity for the middle-income subjects.

S.1.2.2 Procedure

For the pilot for Study 2, 38 new subjects were recruited on MTurk on 9-11 July 2021. All procedures were identical to those in Study 1. Applying the same screeners resulted in a final sample of 30 subjects. Table S.3 summarises the sample characteristics.

S.1.2.3 Analysis

In the following, I again depict the results from the pilot study and refrain from any statistical inference due to the small sample size. Descriptive statistics are summarised in Table S.4.

In the pilot for Study 2, average dictator giving was 29.67, average exponential growth bias was $\hat{\theta} = .49$, and the estimated and preferred wealth Gini in the US were .58 and .11,

Table S.3: Sample characteristics, Study 2 pilot

	Freq.	Percent
Female	14	46.7
Male	16	53.3
White or Caucasian American	23	76.7
Black or African American	1	3.3
Hispanic or Latino	1	3.3
Asian American	3	10.0
Other ethnicity	2	6.7
Less than a high school diploma	0	0
High school degree or equivalent (e.g. GED)	5	16.7
Some college, no degree	4	13.3
Associate degree (e.g. AA, AS)	1	3.3
Bachelor's degree (e.g. BA, BS)	14	46.7
Master's degree (e.g. MA, MS, MEd)	6	20.0
Doctorate or professional degree (e.g. MD, DDS, PhD)	0	0
Employed (part or full time)	24	80.0
Self-employed	3	10.0
Unemployed	3	10.0
Student	0	0
Retired	0	0
Other employment	0	0
Total	30	100.0

respectively. Thus, the sample closely resembled the one used in the pilot for Study 1, and the average behaviour of this sample largely corresponded to what is reported in the literature.

Subjective Forecasts. Figures S.4 and S.5 show that subjects tend to underestimate both the absolute and relative inequality in round 30. This finding holds regardless of the specific inequality measure that I apply.

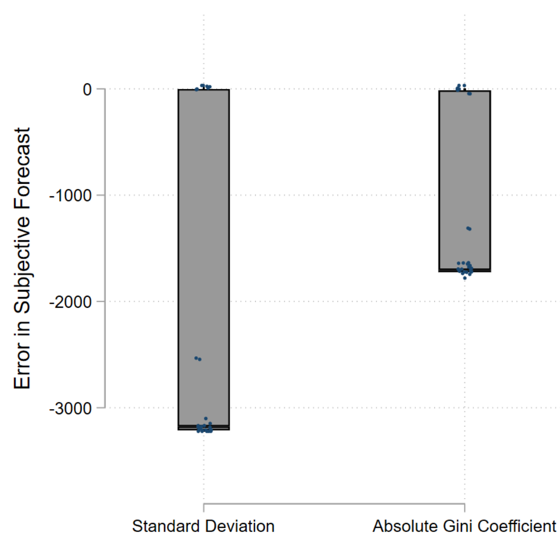
Voting Behaviour. Figure S.6 shows subjects' tax decision by treatment, and I again refrain from drawing any conclusions from this due to the small sample size.

Table S.4: Descriptive statistics for middle-income and rich subjects, Study 2 pilot

	N	Tax	DG	Actual SD	SD (F)	Actual CV	CV (F)	EGB
ForecastR	11	46.91	27.27	3223.32	614.09	0.71	0.59	0.58
RealizedR	12	20.83	29.17	3223.32	1683.51	0.71	0.68	0.34
Total	23	33.30	28.26	3223.32	1172.05	0.71	0.64	0.45

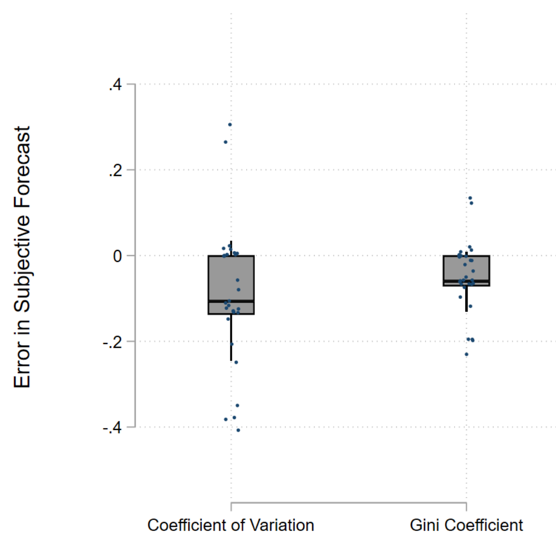
Note: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by subjects' forecasted income levels in the group. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009).

Figure S.4: Forecast error of absolute inequality, Study 1 pilot



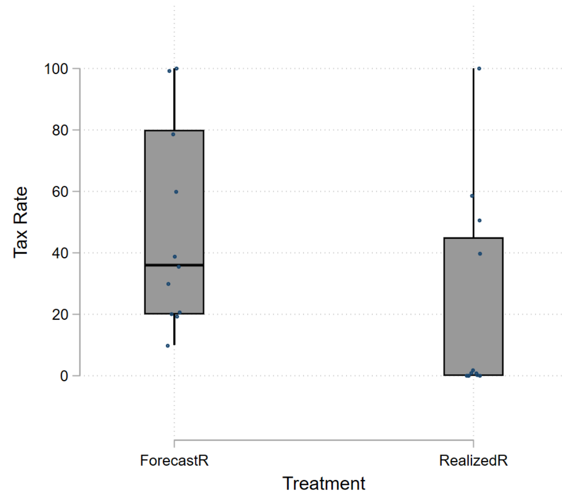
Note: box plots show the median as well as upper and lower quartiles of forecast errors (raw data). Spikes extend to the largest or smallest values within 1.5 times the upper or lower quartiles, respectively. The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$, and the absolute Gini coefficient is calculated as $AG(\mathbf{x}) = \bar{x}\left(\frac{N+1}{N} - \frac{2}{N^2 \bar{x}} \sum_{i=1}^{N}(N+1-i)x_i\right)$, where x_i are ranked-ordered incomes such that $x_i \leq x_{i+1}$.

Figure S.5: Forecast error of relative inequality, Study 2 pilot



Note: box plots show the median as well as upper and lower quartiles of forecast errors (raw data). Spikes extend to the largest or smallest values within 1.5 times the upper or lower quartiles, respectively. The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$, and the Gini coefficient is calculated as $G(\mathbf{x}) = \frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i$, where x_i are ranked-ordered incomes such that $x_i \leq x_{i+1}$.

Figure S.6: Preferred tax rate by treatment, Study 2 pilot



Note: box plots show the median as well as upper and lower quartiles of preferred taxes (raw data). Spikes extend to the largest or smallest values within 1.5 times the upper or lower quartiles, respectively.

S.2 Theoretical Extensions

In the following, I first demonstrate how the theoretical model outlined in Section 2.2 is specified when I assume that $f(r, T, \theta)$ takes the functional forms assumed in Stango and Zinman (2009) and Levy and Tasoff (2016), respectively. Afterwards, I outline the model under the assumption income-specific real interest rates. I then proceed to extend the main specification with aversion towards relative inequality and with efficiency concerns. Finally, I demonstrate that the predictions hold under alternative tax-transfer schemes, specifically assuming either convex efficiency loss or a lump-sum tax.

S.2.1 Special Case: EGB as Modelled by Stango and Zinman (2009)

In the following, I derive the results presented in Section 2.2 for the specific case in which $f(r, T, \theta)$ takes the form assumed in Stango and Zinman (2009) (see also Almenberg and Gerdes, 2012, and Song, 2020). That is, I assume people misperceive exponential growth bias in the following way:

$$FV = PV \cdot (1 + r)^{(1-\theta)T},$$

where FV is the future value, PV is the present value, $r > 0$ is a constant real interest rate, T is the number of periods, and θ reflects the degree of exponential growth bias. Making the same assumptions on individual utility and the tax scheme as in Section 2.2, this implies that individual i receives the following utility:

$$\begin{aligned} U_i(x_1, ..., x_n) = & [(1-\tau)x_i + \lambda\tau\bar{x}] \cdot (1+r)^{(1-\theta)T} \\ & - \alpha_i \frac{1}{n-1}(1-\tau)^2(1+r)^{2(1-\theta)T} \sum_{j \neq i} (\max\{x_j - x_i, 0\})^2 \\ & - \beta_i \frac{1}{n-1}(1-\tau)^2(1+r)^{2(1-\theta)T} \sum_{j \neq i} (\max\{x_i - x_j, 0\})^2 \end{aligned} \tag{7}$$

Individual i then prefers the tax level τ_i^b that he thinks will maximise his utility, where τ_i^b again denotes that the individual may be influenced by EGB:

$$\tau_i^b(x_1, \ldots, x_N; \theta) = 1 - \frac{x_i - \lambda\bar{x}}{2\phi_i(1+r)^{(1-\theta)T}} \tag{8}$$

As in Section 2.2, the tax is trivially set equal to 1 if the individual can both increase his own earnings and equality by increasing the tax. Hence, I assume in the following that a higher tax involves lower earnings for the individual (i.e., $x_i - \lambda\bar{x} > 0$).

From the partial derivatives of τ_i^b, one obtains that the preferred tax level increases in inequality aversion (α, β) and the efficiency of the tax (λ):

$$\begin{aligned} \frac{\partial\tau_i^b}{\partial\alpha_i} &= \frac{x_i - \lambda\bar{x}}{2\phi_i^2(1+r)^{(1-\theta)T}} \cdot \frac{1}{n-1} \sum_{j \neq i} (\max\{x_j - x_i, 0\})^2 > 0 \\ \frac{\partial\tau_i^b}{\partial\beta_i} &= \frac{x_i - \lambda\bar{x}}{2\phi_i^2(1+r)^{(1-\theta)T}} \cdot \frac{1}{n-1} \sum_{j \neq i} (\max\{x_i - x_j, 0\})^2 > 0 \\ \frac{\partial\tau_i^b}{\partial\lambda} &= \frac{\bar{x}}{2\phi_i(1+r)^{(1-\theta)T}} > 0 \end{aligned}$$

One also obtains that τ_i^b increases in the incomes of persons who earn more than individual i ($x_k > x_i$). For persons with incomes below x_i, there is a trade-off between the gain from increased tax revenue and the less need for redistribution. Consequently, $\frac{\partial\tau_i^b}{\partial x_k} < 0$ for $x_k < x_i$ holds only if the difference in the incomes of individuals i and k is sufficiently large:

$$\begin{aligned} x_k > x_i : \ \frac{\partial\tau_i^b}{\partial x_k} &= \frac{\frac{1}{n}\lambda\phi_i + (x_i - \lambda\bar{x})2\alpha_i(x_k - x_i)}{2(1+r)^{(1-\theta)T}\phi_i^2} > 0 \\ x_k < x_i : \ \frac{\partial\tau_i^b}{\partial x_k} &= \frac{\frac{1}{n}\lambda\phi_i - (x_i - \lambda\bar{x})2\beta_i(x_i - x_k)}{2(1+r)^{(1-\theta)T}\phi_i^2} < 0 \\ & \text{if } (1-\tau_i^b)2\beta_i(x_i - x_k) > \frac{\lambda}{(1+r)^{(1-\theta)T}} \frac{n-1}{2n} \end{aligned}$$

The effect of the individual i's own endowment is also ambiguous: an increase in x_i implies a higher cost of redistribution, but if x_i is already large, an increase results in more disutility from a higher level of inequality. Importantly, the notion of a "large" x_i depends on how x_i is relative to the other incomes. If, for instance, many individuals have incomes above x_i, the overall inequality will decrease from individual i's perspective:

$$\frac{\partial \tau_i^b}{\partial x_i} = -\frac{(1-\frac{1}{n}\lambda)\phi_i - (x_i - \lambda\bar{x})2\left[-\alpha_i \sum_{j\neq i}\max\{x_j - x_i, 0\} + \beta_i \sum_{j\neq i}\max\{x_i - x_j, 0\}\right]}{2(1+r)^{(1-\theta)T}\phi_i^2} < 0$$

$$\text{if } (1-\tau_i^b)2\left[-\alpha_i \sum_{j\neq i}\max\{x_j - x_i, 0\} + \beta_i \sum_{j\neq i}\max\{x_i - x_j, 0\}\right] < \frac{n-\lambda}{(1+r)^{(1-\theta)T}}\frac{n-1}{2n}$$

I now turn to the partial derivatives that depend on the subjective forecast of growth. First, note that the preferred tax rate increases in the individual's estimate of real growth, $(1+r)^{(1-\theta)T}$. This implies that more biased individuals prefer less redistribution. Moreover, the preferred tax rate is increasing in both the real interest rate and the time horizon:

$$\frac{\partial \tau^b}{\partial \theta} = -\frac{x_i - \lambda\bar{x}}{2\phi_i(1+r)^{(1-\theta)T}}(1-\theta)T^2 \cdot \log(1+r) < 0$$

$$\frac{\partial \tau^b}{\partial r} = \frac{x_i - \lambda\bar{x}}{2\phi_i(1+r)^{(1-\theta)T+1}}(1-\theta)T > 0$$

$$\frac{\partial \tau^b}{\partial T} = \frac{x_i - \lambda\bar{x}}{2\phi_i(1+r)^{(1-\theta)T}}(1-\theta)^2 T \cdot \log(1+r) > 0$$

Notably, the effects of both the real interest rate and the time horizon are zero in case of complete bias (i.e., $\left.\frac{\partial \tau^b}{\partial r}\right|_{\theta=1} = 0$ and $\left.\frac{\partial \tau^b}{\partial T}\right|_{\theta=1} = 0$).

S.2.2 Special Case: EGB as Modelled by Levy and Tasoff (2016)

In the following, I examine a special case of the framework presented in Section 2.2, where I model $f(r, T, \theta)$ by the functional form used by Levy and Tasoff (2016) (see also Levy and Tasoff, 2020). In this specification, individual i is assumed to make a forecast by combining a linear and an exponential projection:

$$FV = PV\left[(1+(1-\theta)r)^T + \theta T r\right] \tag{9}$$

where I again assume for simplicity that the real interest rate $r > 0$ is fixed. This leads to the following utility for individual i:

$$\begin{aligned} U_i(x_1, ..., x_n) = {} & [(1-\tau)x_i + \lambda\tau\bar{x}] \cdot \left[(1+(1-\theta)r)^T + \theta T r\right] \\ & - \alpha_i \frac{1}{n-1}(1-\tau)^2\left[(1+(1-\theta)r)^T + \theta T r\right]^2 \sum_{j\neq i}(\max\{x_j - x_i, 0\})^2 \\ & - \beta_i \frac{1}{n-1}(1-\tau)^2\left[(1+(1-\theta)r)^T + \theta T r\right]^2 \sum_{j\neq i}(\max\{x_i - x_j, 0\})^2 \end{aligned} \tag{10}$$

Maximising the utility from Equation 10 with respect to the tax rate yields the preferred tax level τ_i^b under the influence of EGB:

$$\tau_i^b(x_1, \ldots, x_N; \theta) = 1 - \frac{x_i - \lambda \bar{x}}{2\phi_i \left[(1 + (1-\theta)r)^T + \theta T r\right]} \tag{11}$$

As in Section 2.2, the preferred tax is 1 if the individual can both increase his own earnings and equality by increasing the tax. Thus, I now assume that $x_i - \lambda \bar{x} > 0$. As the tax rate specified in Equation 11 is analogous to the tax rate based on the framework from Stango and Zinman (2009), the partial derivatives with respect to x_i, x_k, α_i, β_i, and λ_i all resemble the above expressions. Hence, I only examine the comparative statics that work through the EGB in the following.

When EGB is modelled as proposed by Levy and Tasoff (2016), I again find that the preferred tax rate is decreasing in EGB, and it is increasing in the real interest rate as well as the time horizon:

$$\frac{\partial \tau^b}{\partial \theta} = -\frac{x_i - \lambda \bar{x}}{2\phi_i \left[(1 + (1-\theta)r)^T + \theta T r\right]^2} T r \left[\underbrace{(1 + (1-\theta)r)^{T-1}}_{>1} - 1\right] < 0$$

$$\frac{\partial \tau^b}{\partial r} = -\frac{x_i - \lambda \bar{x}}{2\phi_i \left[(1 + (1-\theta)r)^T + \theta T r\right]^2} \left[T(1 + (1-\theta)r)^{T-1}(1-\theta) + \theta T\right] < 0$$

$$\frac{\partial \tau^b}{\partial T} = -\frac{x_i - \lambda \bar{x}}{2\phi_i \left[(1 + (1-\theta)r)^T + \theta T r\right]^2} \left[T \log(1 + (1-\theta)r) \cdot (1 + (1-\theta)r)^T + \theta r\right] < 0$$

In contrast to the analysis that draw on Stango and Zinman (2009), this framework allows for effects of both the real interest rate and the time horizon under complete bias ($\theta = 1$). This is because even under complete bias, this framework takes into account that people linearise developments; and a linear projection is also influenced by the interest rate and time horizon.

S.2.3 Income-Specific Real Interest Rates

In the following, I abandon the assumption from Section 2.2 that all group members obtain the same real interest rate. Instead, I assume that individuals with higher endowments earn a higher interest rate, resembling the empirical case of unequal income growth (cf. the Introduction). Formally, denote individual i's forecast of his own endowment by $f_i(r, T, \theta)$ and his forecast of individual j's endowment by $f_j(r, T, \theta)$. Then, $f_i(r, T, \theta) \geq f_j(r_T, \theta)$ iff $x_i \geq x_j$.

The average endowment grows at a rate equal to $\frac{1}{n}\sum_{j=1}^n f_j x_j$. Define then $f_{min} \equiv \text{argmin}_j f_j(r, T, \theta)$ and $f_{max} \equiv \text{argmax}_j f_j(r, T, \theta)$ to be the smallest and largest growths in endowments, respectively. It follows that $\bar{x} f_{min} \leq \frac{1}{n}\sum_{j=1}^n f_j x_j \leq \bar{x} f_{max}$. By the intermediate value theorem, there exists $\tilde{f} \in [f_{min}, f_{max}]$ such that $\bar{x}\tilde{f} = \frac{1}{n}\sum_{j=1}^n f_j x_j$. The post-redistribution income for individual i is then $(1-\tau)x_i f_i(r, T, \theta) + \lambda \tau \bar{x} \tilde{f}$. Thus, he estimates that he will obtain the following utility, where I suppress the arguments for the function f to simplify notation:

$$\begin{aligned} U_i(x_1, ..., x_n) = (1-\tau)x_i f_i + \lambda\tau\bar{x}\tilde{f} \\ - \alpha_i \frac{1}{n-1}(1-\tau)^2 \sum_{j\neq i}(\max\{x_j f_j - x_i f_i, 0\})^2 \\ - \beta_i \frac{1}{n-1}(1-\tau)^2 \sum_{j\neq i}(\max\{x_i f_i - x_j f_j, 0\})^2 \end{aligned} \tag{12}$$

Maximising Equation 12 with respect to τ yields individual i's preferred tax rate:

$$\tau_i^b(x_1, \ldots, x_N; \theta) = 1 - \frac{x_i f_i - \lambda\bar{x}\tilde{f}}{2\frac{1}{n-1}\left[\alpha_i \sum_{j\neq i}(\max\{x_j f_j - x_i f_i, 0\})^2 + \beta_i \sum_{j\neq i}(\max\{x_i f_i - x_j f_j, 0\})^2\right]} \tag{13}$$

For any individual i with $x_i f_i \leq \lambda\bar{x}\tilde{f}$, increasing the tax rate leads to both higher earnings and more equality in the group, leading to the corner solution of a tax rate of 1. To examine the trade-off between personal earnings and equality, I therefore assume $x_i f_i > \lambda\bar{x}\tilde{f}$ to hold in the following.

As in Section 2.2, denote the optimal tax rate for individual i by τ_i^*. This corresponds to τ_i^b in the absence of bias ($\theta = 0$), and it is thus obvious that the two tax rates coincide when $T = 0$ as the individual is not making any forecast at this point (i.e., $\lim_{T\to 0}\tau_i^b = \tau_i^*$). In addition, one can see from Equation 13 that $\tau_i^* \geq \tau_i^b$ as the forecast bias causes the individual to underestimate future inequality and therefore to vote for less redistribution than would maximise his utility.

S.2.3.1 Preferred Taxes for Different vs. Same Interest Rates

In the following, I compare the preferred tax rates specified in Equations 5 and 13. In other words, I compare how much redistribution individuals prefer in the case where everyone obtains the same real interest on their endowment (f) to the case where interest rates and endowments are positively related (f_i).

Intuitively, there are two effects of making interest rates positively correlated with endowments. On the one hand, it affects the tax base and thereby the transfers that individuals receive. On the other hand, it leads to greater inequality (both in absolute and relative terms) as long as the common interest rate f does not exceed the largest interest rate f_{max} by too much. As the marginal disutility from inequality is assumed to be increasing (cf. Section 2.2.1), individuals generally prefer more redistribution when interest rates vary. Nevertheless, when incomes are approximately the same, the effect of a change in the tax base might dominate. Thus, which of the two tax rates is larger will depend on how incomes and interest rates are distributed.

Comparing the tax rates formally, one obtains that the preferred tax rate specified under unequal interest rates (Equation 13) will be greater than that under a uniform interest rate (Equation 5) if the following condition holds:

$$\frac{x_i f - \lambda \bar{x} f}{\alpha_i \sum_{j \neq i} (\max\{x_j f - x_i f, 0\})^2 + \beta_i \sum_{j \neq i} (\max\{x_i f - x_j f, 0\})^2}$$

$$\geq$$

$$\frac{x_i f_i - \lambda \bar{x} \tilde{f}}{\alpha_i \sum_{j \neq i} (\max\{x_j f_j - x_i f_i, 0\})^2 + \beta_i \sum_{j \neq i} (\max\{x_i f_i - x_j f_j, 0\})^2}$$

This inequality depends on the specific distribution of incomes and interest rates. For the current study, the values are set in such a manner that middle-income and rich individuals unambiguously prefer higher taxes under unequal interest rates.

One obtains additional intuition behind the above result by examining how individual i's preferred tax from Equation 13 depends on the real interest rates. Specifically, consider an individual k for whom $x_k > x_i$. Then, individual i votes for a higher tax for larger f_k:

$$\left.\frac{\partial \tau_i^b}{\partial f_k}\right|_{x_k > x_i} = \frac{\frac{1}{n} x_k \lambda [\dots] + (x_i f_i - \lambda \bar{x} \tilde{f}) \alpha_i 2 (x_k f_k - x_i f_i) x_k}{2 \frac{1}{n-1} [\dots]} > 0$$

In contrast, if individual k has a lower endowment than individual i ($x_k < x_i$), there is a trade-off between the increased transfer that arises due to the greater tax base and the less need for redistribution to reduce inequality. One can show that $\left.\frac{\partial \tau_i^b}{\partial f_k}\right|_{x_k < x_i} < 0$ holds if and only if

$$\left[x_i f_i - \lambda \bar{x} \tilde{f}\right] \beta_i (x_i f_i - x_k f_k) > \frac{1}{n} \lambda \left[\alpha_i \sum_{j \neq i} (\max\{x_j f_j - x_i f_i, 0\})^2 + \beta_i \sum_{j \neq i} (\max\{x_i f_i - x_j f_j, 0\})^2\right]$$

Thus, what determines the sign of the effect is the extent of inequality aversion (α_i, β_i) as well as the difference between $x_i f_i$ and $x_k f_k$, compared to all other pairwise comparisons between $x_i f_i$ and other incomes. When individual k's endowment is close to that of individual i, the effect of the increased tax base dominates the reduction in inequality, and this leads to an increase in τ_i^b. In contrast, if individual k has a much lower endowment than individual i, the effect of the reduction in inequality matters the most, causing individual i to vote for a lower tax rate.

S.2.4 Including Aversion to Relative Inequality

In this section, I extend the theoretical framework developed in Section 2.2 to account for aversion towards relative inequality. I model the concern for relative inequality based on the coefficient of variation, defined as $\frac{1}{\bar{x}}\left[\sum_{i=1}^{n}\frac{(x_i-\bar{x})^2}{n}\right]^{\frac{1}{2}}$ (Niño-Zarazúa et al., 2017). I assume that individual i weighs disutility from relative inequality by $\gamma_i < 1$. For tractability, I here set $\lambda = 1$, which implies that there is no efficiency loss from redistribution. Hence, the individual's utility function is defined as follows:

$$
\begin{aligned}
U_i(x_1,...,x_n) = {} & [(1-\tau)x_i + \tau\bar{x}] \cdot f(r,T,\theta) \\
& - \alpha_i \frac{1}{n-1}(1-\tau)^2 f(r,T,\theta)^2 \sum_{j\neq i}(\max\{x_j - x_i, 0\})^2 \\
& - \beta_i \frac{1}{n-1}(1-\tau)^2 f(r,T,\theta)^2 \sum_{j\neq i}(\max\{x_i - x_j, 0\})^2 \\
& - \gamma_i \frac{1}{\bar{x}}\left[\sum_{i=1}^{n}\frac{(1-\tau)^2(x_i-\bar{x})^2}{n}\right]^{\frac{1}{2}}
\end{aligned}
\tag{14}
$$

As in Section 2.2, individual i maximises this utility function with respect to τ to find his preferred tax level:

$$
\tau_i^b(x_1,\ldots,x_N;\theta) = 1 - \frac{x_i - \bar{x} - \gamma_i \frac{1}{n\bar{x}f(r,T,\theta)}\left(\sum_{i=1}^{n}(x_i-\bar{x})^2\right)^{\frac{1}{2}}}{2\phi_i f(r,T,\theta)} \tag{15}
$$

By comparing Equation 15 with Equation 5, one can see that introducing aversion towards relative inequality leads, ceteris paribus, to preferences for more redistribution. In the following, I focus on the case where $x_i > \bar{x} + \gamma_i \frac{1}{n\bar{x}f(r,T,\theta)}\left(\sum_{i=1}^{n}(x_i-\bar{x})^2\right)^{\frac{1}{2}}$ as individual i would otherwise prefer the corner response of $\tau_i^b = 1$.

Again, I compare the tax rate under the influence of forecast bias with the optimal tax rate, τ_i^*. In this specification, $\tau_i^* \geq \tau_i^b$ holds when the following condition holds:

$$
\frac{x_i - \bar{x}}{2\frac{1}{n\bar{x}}\left(\sum_{i=1}^{n}(x_i-\bar{x})^2\right)^{\frac{1}{2}}} \geq \gamma_i \tag{16}
$$

This implies that the requirement for γ_i varies with the level of income. Specifically, the left-hand side of Equation 16 increases in income, so only absolute inequality aversion will matter for the convergence of the tax levels when incomes are large. One can furthermore show that $\lim_{T\to 0}\tau_i^b = \tau_i^*$.[2] Hence, the results derived in Section 2.2 also hold under aversion to relative inequality, provided that this aversion is not excessive.

To obtain a better intuition, I now extend the model using the (intermediate) Krtscha measure (1994) instead of the coefficient of variation. The Krtscha measure is the product

[2] In this specification, the sign of $\frac{\partial}{\partial T}(\tau_i^* - \tau_i^b)$ not only depends on T but also γ_i. For instance, if $f(r,T,\theta)$ takes the functional form specified in Stango and Zinman (2009), $\frac{\partial}{\partial T}(\tau_i^* - \tau_i^b) > 0$ holds when $(x_i - \bar{x})\frac{(1+r)^{(1-\theta)T}}{1-\theta} - (1+r)^T > \frac{\theta}{1-\theta}$.

of the coefficient of variation (a relative measure) and the standard deviation (an absolute measure). Thus, it accounts also for relative inequality aversion, and because it results in a condition on γ_i that does not depend on income, it is easier to interpret.[3] Extending the model with inequality aversion in the form of the Krtscha measure yields the following utility for individual i:

$$\begin{aligned} U_i(x_1, ..., x_n) = {} & [(1-\tau)x_i + \tau\bar{x}] \cdot f(r,T,\theta) \\ & - \alpha_i \frac{1}{n-1}(1-\tau)^2 f(r,T,\theta)^2 \sum_{j\neq i}(\max\{x_j - x_i, 0\})^2 \\ & - \beta_i \frac{1}{n-1}(1-\tau)^2 f(r,T,\theta)^2 \sum_{j\neq i}(\max\{x_i - x_j, 0\})^2 \\ & - \gamma_i \frac{1}{n\bar{x}}(1-\tau)^2 \sum_{i=1}^{n}(x_i - \bar{x})^2 \end{aligned} \tag{17}$$

Assuming this type of inequality aversion implies that $\tau_i^* \geq \tau_i^b$ holds whenever $\frac{x_i - \bar{x}}{2\frac{1}{n\bar{x}}\sum_{i=1}^{n}(x_i-\bar{x})^2} \geq \gamma_i$. This requirement does not depend on overall income growth. Importantly, one can show that $\gamma_i < 1.17$ satisfies this condition for the current experiment, which means that the qualitative predictions derived in Section 2.2 hold as long as the individual does not care more about relative inequality than he cares about his own income. Moreover, $\lim_{T\to 0} \tau_i^b = \tau_i^*$ still applies.

S.2.5 Including Efficiency Concerns

In the following, I extend the model from Section 2.2 to account for efficiency preferences.

Assume individual i weighs efficiency concerns by $\delta_i \in [0, 1)$, and let $y \equiv \sum_{j=1}^{n} x_j$ denote the aggregate real income in society at $t = 0$. Then, individual i obtains the following utility:

$$\begin{aligned} U_i(x_1, ..., x_n) = {} & [(1-\tau)x_i + \lambda\tau\bar{x}] \cdot f(r,T,\theta) \\ & + \delta_i y(1-\tau(1-\lambda)) \cdot f(r,T,\theta) \\ & - \alpha_i \frac{1}{n-1}(1-\tau)^2 f(r,T,\theta)^2 \sum_{j\neq i}(\max\{x_j - x_i, 0\})^2 \\ & - \beta_i \frac{1}{n-1}(1-\tau)^2 f(r,T,\theta)^2 \sum_{j\neq i}(\max\{x_i - x_j, 0\})^2 \end{aligned} \tag{18}$$

[3]The Krtscha measure has the desirable property of unit consistency (Zheng, 2007) – as opposed to e.g. the intermediate measures proposed by Kolm (1976) and Bossert and Pfingsten (1990). This implies that the ranking of income distributions does not depend on the unit that income is measured in. Because it is the product of two common relative and absolute measures, it is also fairly simple and easy to interpret (Subramanian and Jayaraj, 2015). Finally, Krtscha (1994) refers to it as a "compromise measure", and it is perceived to be close to the center of the spectrum between absolute and relative inequality measures (Bosmans et al., 2014).

In the following, I focus on the case where $x_i > \lambda\bar{x} - \delta_i y(1-\lambda)$ as individual i would otherwise prefer the corner response of $\tau_i^b = 1$. Maximising equation 18 yields the preferred tax level of individual i:

$$\tau_i^b(x_1, \ldots, x_N; \theta) = 1 - \frac{x_i - \lambda\bar{x} + \delta_i y(1-\lambda)}{2\phi_i f(r, T, \theta)} \tag{19}$$

As in Section 2.2, one may compare the optimal tax rate for individual i (τ_i^*) with the tax rate influenced by EGB (τ_i^b). As in the above analyses, one can see from Equation 19 that $\tau_i^* \geq \tau_i^b$, and one obtains again that the two tax rates coincide when $T = 0$ as the individual is not making any forecast at this point (i.e., $\lim_{T\to 0} \tau_i^b = \tau_i^*$).

By comparing Equation 19 with Equation 5, one can see that introducing concerns for efficiency leads, ceteris paribus, to a preferences for less redistribution (as this involves an efficiency loss). Naturally, the more individual i values efficiency, the lower is his preferred tax rate ($\frac{\partial \tau_i^b}{\partial \delta_i} = -\frac{y(1-\lambda)}{2\phi_i f(r,T,\theta)} < 0$). Furthermore, including efficiency concerns imply that the individual becomes more responsive to changes in the efficiency of the tax ($\frac{\partial \tau_i^b}{\partial \lambda} = \frac{\bar{x}+\delta_i y}{2\phi_i f(r,T,\theta)} > 0$. Finally, all effects of increases in income – both for individual i and for other individuals x_k – are influenced in the direction of lower tax rates because the efficiency loss is greater for higher incomes.

Including efficiency concerns does not alter any of the effects of inequality aversion (α, β) or forecasts (r, T, θ); these effects are analogous to the above cases.

S.2.6 Convex Efficiency Loss

In Section 2.2, I assume the efficiency loss is linear in the tax, which reflects the experimental design outlined in Section 2.1.4. This efficiency loss reflects the disincentive effect from taxes (MaCurdy, 1992; Ziliak and Kniesner, 1999; Kumar, 2008; Sausgruber et al., 2021), and it serves as a tie-breaker for incomes in the middle class. But it may be more plausible that the efficiency loss is convex in the income tax – an assumption that is also seen in the literature on the equity-efficiency trade-off (e.g., Alesina and Giuliano, 2011). In the following, I thus adapt the model to a quadratic efficiency loss and show that the predictions remain the same as under the assumption of a linear efficiency loss.

Under this assumption, the utility of individual i is as follows:[4]

$$\begin{aligned} U_i(x_1, ..., x_n) = &\left[(1-\tau)x_i + \bar{x}\left(\tau - (1-\lambda)\tau^2\right)\right] \cdot f(r,T,\theta) \\ &- \alpha_i \frac{1}{n-1}(1-\tau)^2 f(r,T,\theta)^2 \sum_{j \neq i}(\max\{x_j - x_i, 0\})^2 \\ &- \beta_i \frac{1}{n-1}(1-\tau)^2 f(r,T,\theta)^2 \sum_{j \neq i}(\max\{x_i - x_j, 0\})^2 \end{aligned} \tag{20}$$

From this utility function, individual i obtains his preferred tax level:

$$\tau_i^b(x_1, \ldots, x_N; \theta) = \frac{\bar{x} - x_i + 2\phi_i f(r,T,\theta)}{2(1-\lambda)\bar{x} + 2\phi_i f(r,T,\theta)} \tag{21}$$

I now focus on individuals with $x_i > \bar{x}$ to restrict the analysis to those who face a trade-off between equality in the group and their own earnings.

While the assumption of a convex efficiency loss yields a tax rate that looks somewhat different than the tax specified in Equation 5, it yields qualitatively similar results. First, the preferred tax rate is higher for subjects with greater concerns about inequality, and it increases with the efficiency of the tax:

$$\begin{aligned} \frac{\partial \tau_i^b}{\partial \alpha_i} &= 2f(r,T,\theta) \cdot \frac{\bar{x}(1-2\lambda) + x_i}{\left[2(1-\lambda)\bar{x} + 2\phi_i f(r,T,\theta)\right]^2} \cdot \frac{1}{n-1} \sum_{j \neq i}(\max\{x_j - x_i, 0\})^2 > 0 \\ \frac{\partial \tau_i^b}{\partial \beta_i} &= 2f(r,T,\theta) \cdot \frac{\bar{x}(1-2\lambda) + x_i}{\left[2(1-\lambda)\bar{x} + 2\phi_i f(r,T,\theta)\right]^2} \cdot \frac{1}{n-1} \sum_{j \neq i}(\max\{x_i - x_j, 0\})^2 > 0 \\ \frac{\partial \tau_i^b}{\partial \lambda} &= \frac{\bar{x} - x_i - 2\phi_i f(r,T,\theta)}{\left[2(1-\lambda)\bar{x} + 2\phi_i f(r,T,\theta)\right]^2} > 0 \end{aligned}$$

Similar to the case of linear efficiency loss, the preferred tax rate increases with the subjective forecast in growth:

$$\frac{\partial \tau_i^b}{\partial f(r,T,\theta)} = 2\phi_i \cdot \frac{\bar{x}(1-2\lambda) + x_i}{\left[2(1-\lambda)\bar{x} + 2\phi_i f(r,T,\theta)\right]^2} > 0$$

Thus, individual i prefers more redistribution when the real interest rate is larger and when the time horizon is longer. Moreover, individual i votes for a lower tax rate the more biased he is. Thus, while the biased and optimal tax rates coincide for $T = 0$ (i.e., $\lim_{T \to 0} \tau_i^b = \tau_i^*$), longer time horizons yield the general result that individuals who exhibit EGB vote for less redistribution than would be in their own long-run interest (i.e., $\tau_i^* \geq \tau_i^b$).

[4] To understand the transfer derived from the income tax, note that $\tau\bar{x} - (1-\lambda)\tau^2\bar{x} = \bar{x}(\tau - (1-\lambda)\tau^2)$. Analogously, one could derive the transfer under a linear efficiency loss as $\tau\bar{x} - (1-\lambda)\tau\bar{x} = \lambda\tau\bar{x}$.

S.2.7 Lump Sum Tax Scheme

In the following, I build on the setup from Section 2.2.1, but I examine the individual's preferences under a lump sum rather than a proportional tax scheme. Specifically, I assume that a lump sum tax is levied on individuals with an income above the mean ($x_i > \bar{x}$). I denote by $p \in (0,1)$ the fraction of the population with such an income, and they each pay τ in tax. I assume furthermore that individuals with an income below the mean receive $\lambda\tau\frac{p}{1-p}$, where $\lambda \in (0,1]$ denotes the efficiency of the redistribution scheme. Also, I assume that the tax preserves the order of the individuals' income.

The preferred tax for individuals with incomes below the mean is trivially set to 1 as these obtain greater utility both from increased income and decreased inequality. Hence, I here examine the redistributive preferences for individual i with $x_i > \bar{x}$. For any time t with T remaining time periods, individual i's utility is then:[5]

$$\begin{aligned} U_i(x_i,...,x_n) = {} & (x_i - \tau) \cdot f(r,T,\theta) \\ & - \alpha_i \frac{1}{n-1} f(r,T,\theta)^2 \sum_{j \neq i} (\max\{x_j - x_i, 0\})^2 \\ & - \beta_i \frac{1}{n-1} f(r,T,\theta)^2 \left[\sum_{x_j \geq \bar{x}} (\max\{(x_i - x_j), 0\})^2 + \sum_{x_j < \bar{x}} \left(x_i - x_j - \lambda\tau \frac{1}{1-p} \right)^2 \right] \end{aligned} \tag{22}$$

As individual i maximises Equation 22 with respect to τ, his preferred tax under bias becomes:

$$\tau_i^b(x_1,\dots,x_N;\theta) = \frac{1}{\lambda} \sum_{x_j < \bar{x}} (x_i - x_j) - \frac{1-p}{2\beta_i \frac{1}{n-1} f(r,T,\theta)\lambda^2} \tag{23}$$

Again, individuals vote for less redistribution than would be in their long-run interest if they are biased (i.e., $\tau_i^* \geq \tau_i^b$). This also relates to the result that individual i desires more redistribution for greater forecasts of growth ($\frac{\partial \tau_i^b}{\partial f(r,T,\theta)} = \frac{1-p}{\beta_i \frac{1}{n-1} f_i^2 \lambda^2} > 0$). Thus, the individual prefers more redistribution for greater real interest rates and longer time horizons.

Moreover, this setting yields the same results that individual i prefers a higher tax rate when he is more concerned about advantageous inequality (β) and when redistribution is more efficient (λ). Note, however, that in this case, disadvantageous inequality aversion (α) does not matter for the individuals who earn more than the mean. This occurs because the tax does not affect any comparison between individuals who pay the same lump sum tax.

[5] With this tax scheme, note that the tax does not influence inequality aversion with respect to individuals who earn more than individual i as these also pay the tax. Regarding individuals who earn less than individual i, the effect of the tax depends on whether the individuals earn more or less than the mean. For individuals who earn less than the mean, the inequality is affected as follows: $x_i - \tau - \left(x_j + \frac{p}{1-p}\lambda\tau\right) = x_i - x_j - \tau\lambda\frac{1}{1-p}$.

The effect of individual i's income is now unambiguous: an increase in x_i leads individual i to vote for higher taxes ($\frac{\partial \tau_i^b}{\partial x_i} = \frac{1}{\lambda}(1-p) > 0$). The intuition is as follows: for proportional taxes (Section 2.2, an increase in x_i affected both the need for redistribution and the personal cost of redistribution at a given tax rate. For lump sum taxes, however, the latter effect is no longer present as the lump sum tax is unaffected by x_i as long as it is greater than the mean income. In contrast, the effect of an increase in x_k for $x_k < \bar{x}$ is now unambiguously negative ($\left.\frac{\partial \tau_i^b}{\partial x_k}\right|_{x_k<\bar{x}} = -\frac{1}{\lambda}(1-p) < 0$). The intuition behind the ambiguous result in Section 2.2 is that under a proportional tax scheme, an increase in $x_k < \bar{x}$ leads to (i) a lower need for redistribution and (ii) a larger transfer to individual i. Now, however, individual i does not receive any transfer, and so the second effect is excluded.

S.3 Ex-Ante Power Analysis

With the available funding, I aimed to recruit 1,329 subjects for Study 1, 886 subjects for Study 2, and 886 subjects for Study 3. Using the code presented in the supplementary material (Stata, version 16), I determine the minimum detectable effect size with a power of 0.8 (e.g., Aberson, 2019) via simulations. Here, I focus solely on testing H2; this only concerns the middle-income and rich subjects (5/7 of the sample), and it requires subjects to be divided into different treatments. It is therefore the hypothesis that I have the least power to test.[6]

In the power analysis for Study 1, I employ a mean tax rate of 32 percent and a standard deviation of 30, which are the observed values in the pilot study. The simulations show that this yields a power of approximately 80 percent for both the tobit regression and the MWU-test for detecting an effect size of Hedge's $g_p = 0.23$ (Goulet-Pelletier and Cousineau, 2018), corresponding to a difference in tax rate of 6.8 percentage points.

As explained in Section 2.1.6, the final sample was a bit larger than expected (1,415 vs. 1,329). Using this sample size with the same assumptions that I made a priori (to avoid the problems of ex-post power calculations, Hoenig and Heisey, 2001), simulations show that I could expect 80 percent power for both tobit and MWU to detect an effect size of Hedge's $g_p = 0.22$, corresponding to a difference in tax rate of 6.6 percentage points.

[6] Note, however, that I do not expect every subject to provide useful responses that pass all screeners. For instance, Kennedy et al. (2020) find that 6.8 percent of subjects on MTurk provide low-quality data, measured across five different indicators. Similarly, Wood et al. (2017) find that approximately 10 percent of participants provide inconsistent responses. I thus expect 10 percent of subjects to fail one of the screeners that I employ in this study, corresponding roughly to what I find in the pilot study. In addition, it is common that many respondents opt out of the study without payment. I thus follow the recommendation by i.a. Aguinis et al. (2020) and over-recruit by 30 percent on MTurk, such that I invite in total 4,031 subjects to participate. Consequently, the exact number of subjects who will pass the inclusion criteria for each of the studies/treatments is uncertain.

S.4 Heterogeneity in Subjects' Forecasts

S.4.1 Study 1

First, I look at what factors predict whether subjects calculate the answers to all three income classes. A logit model reveals that men are 5 percentage points more likely to calculate the answer, while Black or African Americans are 7 percentage points less likely to do so (see Table S.5). No other factor is significant.

Second, I look at correlates of subjects' EGB. Here, I find that the only significant predictor for the subjects' degree of bias is gender as males tend to be slightly less biased ($\beta = -0.080, p < .001$, see Table S.6). Similar to the findings of i.a. Kemp (1984) and Levy and Tasoff (2016), EGB does not correlate with background characteristics such as education, employment, ethnicity, or age in this sample (contrary to Stango and Zinman, 2009).

Table S.5: Characteristics of subjects who calculate the answer, Study 1

	(1)	(2)	(3)	(4)
Dictator Giving	-0.0003		-0.0000	-0.0000
	(0.0004)		(0.0004)	(0.0004)
Efficiency from MDG	0.0030		0.0018	0.0014
	(0.0029)		(0.0029)	(0.0029)
MLAMS$_p$	-0.0028		-0.0116	0.0140
	(0.0424)		(0.0438)	(0.0444)
Age		-0.0003	-0.0004	-0.0003
		(0.0007)	(0.0007)	(0.0007)
Male		0.0484***	0.0468***	0.0468***
		(0.0155)	(0.0156)	(0.0159)
Black or African American		-0.0728***	-0.0729***	-0.0723***
		(0.0183)	(0.0182)	(0.0183)
Hispanic or Latino		-0.0207	-0.0211	-0.0207
		(0.0378)	(0.0376)	(0.0370)
Asian American		0.0106	0.0114	0.0162
		(0.0314)	(0.0317)	(0.0329)
Other ethnicity		-0.0433	-0.0434	-0.0463
		(0.0521)	(0.0518)	(0.0480)
High school degree or equivalent (e.g. GED)		-0.1085	-0.1068	-0.1147
		(0.1398)	(0.1380)	(0.1328)
Some college, no degree		-0.0855	-0.0840	-0.0901
		(0.1399)	(0.1382)	(0.1332)
Associate degree (e.g. AA, AS)		-0.0965	-0.0952	-0.1020

	(1)	(2)	(3)	(4)
		(0.1409)	(0.1393)	(0.1341)
Bachelor's degree (e.g. BA, BS)		-0.0597	-0.0583	-0.0629
		(0.1405)	(0.1388)	(0.1335)
Master's degree (e.g. MA, MS, MEd)		-0.0235	-0.0214	-0.0234
		(0.1424)	(0.1407)	(0.1356)
Doctorate or pro degree (e.g. MD, DDS, PhD)		-0.0787	-0.0767	-0.0802
		(0.1441)	(0.1425)	(0.1374)
Self-employed		-0.0037	-0.0043	-0.0006
		(0.0258)	(0.0259)	(0.0264)
Unemployed		-0.0092	-0.0079	-0.0092
		(0.0267)	(0.0270)	(0.0265)
Student		-0.0581**	-0.0572**	-0.0550*
		(0.0284)	(0.0288)	(0.0298)
Retired		-0.0464	-0.0462	-0.0454
		(0.0314)	(0.0311)	(0.0315)
Other employment		0.0150	0.0160	0.0214
		(0.0624)	(0.0630)	(0.0634)
Income_p		0.0642	0.0625	0.0537
		(0.0477)	(0.0475)	(0.0482)
Risk_p				0.0142
				(0.0302)
Trust_p				-0.0578*
				(0.0305)
Political Right_p				-0.0134
				(0.0358)
Meritocracy_p				0.0215
				(0.0335)
Inequality Too Large_p				0.0503
				(0.0383)
Government Responsibility_p				-0.0706**
				(0.0340)
Observations	1415	1415	1415	1415

Note: logit regressions with a dummy for making exact forecasts as the dependent variable, reporting average partial effects. The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.

* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.6: Explaining subjects degree of EGB, Study 1

	(1)	(2)
Age	-0.000	-0.000
	(0.001)	(0.001)
Male	-0.081***	-0.080***
	(0.017)	(0.017)
Black or African American	0.019	0.021
	(0.028)	(0.029)
Hispanic or Latino	0.010	0.010
	(0.045)	(0.044)
Asian American	-0.022	-0.026
	(0.039)	(0.040)
Other ethnicity	-0.047	-0.049
	(0.058)	(0.057)
High school degree or equivalent (e.g. GED)	0.162	0.164
	(0.173)	(0.178)
Some college, no degree	0.141	0.151
	(0.173)	(0.178)
Associate degree (e.g. AA, AS)	0.122	0.130
	(0.174)	(0.179)
Bachelor's degree (e.g. BA, BS)	0.089	0.099
	(0.173)	(0.178)
Master's degree (e.g. MA, MS, MEd)	0.084	0.096
	(0.174)	(0.179)
Doctorate or pro degree (e.g. MD, DDS, PhD)	-0.049	-0.037
	(0.180)	(0.184)
Self-employed	-0.034	-0.033
	(0.025)	(0.025)
Unemployed	-0.045*	-0.046*
	(0.025)	(0.026)
Student	-0.035	-0.034
	(0.042)	(0.042)
Retired	-0.080*	-0.078*
	(0.042)	(0.042)
Other employment	0.015	0.007
	(0.047)	(0.047)
Income_p	-0.060	-0.052
	(0.049)	(0.051)
Dictator Giving		-0.001

	(1)	(2)
		(0.000)
Efficiency from MDG		-0.003
		(0.003)
Risk_p		-0.015
		(0.033)
Trust_p		-0.001
		(0.032)
Political Right_p		0.071**
		(0.033)
Meritocracy_p		-0.029
		(0.034)
$\text{Inequality Too Large}_p$		0.023
		(0.038)
$\text{Government Responsibility}_p$		0.023
		(0.033)
MLAMS_p		0.001
		(0.052)
Constant	0.483***	0.459**
	(0.177)	(0.188)
Observations	1286	1286

Note: OLS regressions with EGB as the dependent variable, estimated by the functional form specified in Stango and Zinman (2009). The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

S.4.2 Study 2

For Study 2, I also look at what factors predict whether subjects calculate the answers to all three income classes. Once more, men are 8 percentage points more likely to calculate the correct answer. But in this case, no further demographic variables are statistically significant (cf. Table S.7).

Second, I again look at correlates of subjects' EGB. I find that subjects who are Black or African American are somewhat more biased ($\beta = 0.077, p = 0.009$), and subjects who report being positioned higher in society are less biased ($\beta = -0.212, p < .001$). Nonetheless, EGB is again prevalent across all subgroups (see Table S.8).

Table S.7: Characteristics of subjects who calculate the answer, Study 2

	(1)	(2)	(3)	(4)
Dictator Giving	0.0003		0.0007	0.0008
	(0.0005)		(0.0005)	(0.0005)
Efficiency from MDG	-0.0005		-0.0026	-0.0021
	(0.0037)		(0.0036)	(0.0036)
MLAMS_p	0.0590		0.0255	0.0124
	(0.0550)		(0.0568)	(0.0550)
Age		-0.0011	-0.0013	-0.0011
		(0.0008)	(0.0009)	(0.0009)
Male		0.0729***	0.0783***	0.0829***
		(0.0204)	(0.0206)	(0.0205)
Black or African American		-0.0165	-0.0157	-0.0155
		(0.0296)	(0.0297)	(0.0299)
Hispanic or Latino		-0.0055	-0.0081	-0.0127
		(0.0403)	(0.0383)	(0.0377)
Asian American		0.0127	0.0170	0.0197
		(0.0345)	(0.0350)	(0.0352)
Other ethnicity		-0.0422	-0.0395	-0.0294
		(0.0453)	(0.0462)	(0.0551)
Some college, no degree		0.0287	0.0279	0.0269
		(0.0282)	(0.0277)	(0.0300)
Associate degree (e.g. AA, AS)		0.0194	0.0208	0.0209
		(0.0329)	(0.0326)	(0.0351)
Bachelor's degree (e.g. BA, BS)		0.0510*	0.0527**	0.0509*
		(0.0264)	(0.0259)	(0.0277)
Master's degree (e.g. MA, MS, MEd)		0.1323***	0.1323***	0.1175***
		(0.0408)	(0.0401)	(0.0395)
Doctorate or pro degree (e.g. MD, DDS, PhD)		0.0506	0.0531	0.0421
		(0.0488)	(0.0504)	(0.0477)
Self-employed		0.0169	0.0156	0.0086
		(0.0287)	(0.0285)	(0.0274)
Unemployed		0.0053	0.0045	-0.0071
		(0.0365)	(0.0362)	(0.0337)
Student		0.0429	0.0377	0.0386
		(0.0609)	(0.0593)	(0.0622)
Retired		-0.0208	-0.0213	-0.0247
		(0.0462)	(0.0455)	(0.0462)
Other employment		-0.0288	-0.0310	-0.0236

	(1)	(2)	(3)	(4)
		(0.0556)	(0.0543)	(0.0614)
Income$_p$		0.0432	0.0568	0.0918*
		(0.0526)	(0.0534)	(0.0549)
Risk$_p$				-0.0358
				(0.0377)
Trust$_p$				-0.0745**
				(0.0372)
Political Right$_p$				-0.0548
				(0.0415)
Meritocracy$_p$				-0.0447
				(0.0368)
Inequality Too Large$_p$				-0.0035
				(0.0420)
Government Responsibility$_p$				-0.0261
				(0.0340)
Observations	978	978	978	978

Note: logit regressions with a dummy for making exact forecasts as the dependent variable, reporting average partial effects. The baseline is a person who is White or Caucasian American and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Note that $N = 978$ rather than $N = 980$ as I drop the two subjects with less than high school degree from the regression; they perfectly predict failure and make education inestimable. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.8: Explaining subjects degree of EGB, Study 2

	(1)	(2)
Age	-0.000	-0.001
	(0.001)	(0.001)
Male	-0.038*	-0.034
	(0.021)	(0.021)
Black or African American	0.074**	0.077***
	(0.029)	(0.030)
Hispanic or Latino	-0.004	0.002
	(0.050)	(0.049)
Asian American	-0.003	-0.008
	(0.041)	(0.042)
Other ethnicity	0.080	0.080
	(0.058)	(0.058)

	(1)	(2)
High school degree or equivalent (e.g. GED)	0.312*	0.311*
	(0.170)	(0.163)
Some college, no degree	0.305*	0.308*
	(0.169)	(0.163)
Associate degree (e.g. AA, AS)	0.342**	0.348**
	(0.170)	(0.163)
Bachelor's degree (e.g. BA, BS)	0.270	0.277*
	(0.169)	(0.162)
Master's degree (e.g. MA, MS, MEd)	0.284*	0.297*
	(0.171)	(0.164)
Doctorate or pro degree (e.g. MD, DDS, PhD)	0.166	0.180
	(0.176)	(0.170)
Self-employed	-0.010	-0.002
	(0.027)	(0.028)
Unemployed	-0.035	-0.038
	(0.035)	(0.035)
Student	0.018	0.021
	(0.056)	(0.056)
Retired	0.017	0.018
	(0.051)	(0.052)
Other employment	0.124***	0.121***
	(0.040)	(0.042)
Income_p	-0.202***	-0.212***
	(0.056)	(0.060)
Dictator Giving		0.001
		(0.001)
Efficiency from MDG		0.001
		(0.004)
Risk_p		-0.009
		(0.040)
Trust_p		-0.015
		(0.043)
Political Right_p		0.072
		(0.044)
Meritocracy_p		0.066
		(0.044)
$\text{Inequality Too Large}_p$		0.062
		(0.048)
$\text{Government Responsibility}_p$		-0.001

	(1)	(2)
		(0.039)
MLAMS_p		0.029
		(0.065)
Constant	0.357**	0.220
	(0.171)	(0.178)
Observations	892	892

Note: OLS regressions with EGB as the dependent variable, estimated by the functional form specified in Stango and Zinman (2009). The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

S.5 Further Discussion

S.5.1 Replicating Previous Research

This paper builds on research within (i) social preferences, (ii) underestimation of inequality, and (iii) exponential growth bias. In the following, I briefly comment on how the current experiments replicate earlier work (see overview in Table S.9). The results are generally comparable to those from previous experiments, and this supports the notion that subjects provide meaningful answers to the current experimental tasks.

Dictator Giving. In all studies, subjects are asked to make a decision as the dictator in a standard dictator game (strategy method). Across the three studies, subjects give on average 37.8 percent of their endowment. This is close to the 33.2 percent that Amir et al. (2012) find using an MTurk sample and the same stake size as the current experiment. It is also not far from the average dictator giving of 28.4 percent that Engel (2011) find in a meta-analysis of dictator games.

Underestimating Wealth Inequality in the US. In the attitudinal survey, subjects are asked to estimate the percentage of wealth owned by each wealth quintile (i.e., the wealth distribution) and state their ideal wealth distribution.[7] Across all three studies, subjects

[7]As in the pilot study, some subjects struggled with the idea of quintiles and did not report a monotonic relationship with the top quintiles being more wealthy than the lower quintiles. Here, I restrict the sample to the subjects who provide a monotonic relation. This was the case for 948 subjects (67 percent) in Study 1, 941 subjects (65 percent) in Study 2, and 715 (65 percent) in Study 3. If the subjects who provide valid responses

tended to underestimate wealth inequality, with their answers implying a Gini coefficient of .58 compared to the true value of .72 (2019, World Inequality Database).[8] Such underestimation is comparable to the results of Norton and Ariely (2011) and Franks and Scherr (2019), who find average beliefs of .50 and .51, respectively.

Asked about their ideal wealth distribution, subjects' answers imply a wealth Gini of .18. This is again comparable to the results of Norton and Ariely (2011) and Franks and Scherr (2019), whose subjects exhibit preferences corresponding to a wealth Gini of .21 and .19, respectively.

Exponential Growth Bias. In Study 1, subjects made forecasts for three income groups with uniform growth rates. With the functional specification of exponential growth bias from Stango and Zinman (2009), subjects in this study exhibited an average bias of $\hat{\theta} = .46$. This is close to the average bias of $\hat{\theta} = .49$ in Study 2, where subjects faced unequal growth rates. Both these estimates are comparable to the average bias of $\hat{\theta} = .44$ that Almenberg and Gerdes (2012) find in their restricted sample (nationally representative of Sweden), and it is slightly less biased than what Song (2020) finds in his control group ($\hat{\theta} = .67$) from a rural area in China.

S.5.2 What Concerns Influence Subjects' Preferred Tax Rate?

For the theoretical framework in Section 2.2, I assume that self-interest and inequality aversion influence how people vote. In this section, I discuss the importance of these and other concerns, which have been found to be influential in previous studies.

Self-Interest. The above analysis shows that subjects randomised into the 'poor' income class vote for greater taxes across all studies, and believing that one gains from the tax leads to a preferences for more taxation. Together, these findings demonstrate that self-interest indeed matters in the current voting experiment.

to the task of estimating wealth distributions are more knowledgeable or sophisticated than other subjects, then this additional sample restriction implies that my estimate of subjects' misperceptions is conservative.

[8] To ensure that subjects' estimates are comparable to the correct wealth shares, I calculate the Gini in the US from quintiles rather than using more accurate, individualised data. Naturally, this approach disregards any within-quintile inequality, and it thus underestimates the true US wealth Gini. Calculations are from 2019, using data from World Inequality Database (n.d.). It shows that Americans in the top quintile of the wealth distribution held 85 percent of the wealth, and the remaining quintiles owned 11.5, 3.2, 0.4, and 0.0 percent, respectively.

Table S.9: Replicating previous studies

	Study 1	Study 2	Study 3	All	Literature
DG	37.45	37.50	38.82	37.81	Engel (2011): 28.4 Amir et al. (2012): 33.2
EGB	0.46	0.49	.	0.47	Almenberg and Gerdes (2012): .44 Song (2020): .67
Gini-Beliefs	0.58	0.57	0.57	0.58	Norton and Ariely (2011): .50 Franks and Scherr (2019): .51
Gini-Preferences	0.18	0.19	0.18	0.18	Norton and Ariely (2011): .21 Franks and Scherr (2019): .19

Note: averages are taken over all subjects. DG is the share that subjects give as dictators in the standard dictator game. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009). Gini-Beliefs are calculated based on the subjects' estimates of the wealth distribution in the US, and Gini-Preferences are calculated from subjects' ideal wealth distribution in the US.

Inequality Aversion. As evident from the analysis above, inequality aversion matters for the preferred level of redistribution as subjects who give more in the standard dictator game also vote for a higher tax rate. Yet, giving in the dictator game does not change the subjects' responsiveness to increases in inequality: there are no interaction effects between dictator givings and the treatment effects in any of the studies (all $p's > .214$). Moreover, there are no differences in treatment effects across subsamples that give more or less than the median in the dictator game (Wald chi-square test for coefficients across tobit regressions, all $p's > .183$).

Efficiency. People with greater preferences for efficiency vote for significantly lower taxes in all three studies (all $p's < .001$), and this result also holds when one controls for dictator givings, demographics, risk preferences, trust, and political attitudes (see Tables S.14, S.24, and S.32). And the effect is economically significant as well: across the three studies, going from being minimally concerned about efficiency to being maximally concerned leads to a decrease in preferred tax rate of 20-31 percentage points. Interestingly, the importance of efficiency concerns is not different in treatments with a 2 percent efficiency loss (Study 1 and 3, $APE = -4.22$) compared to the case of a 10 percent efficiency loss (Study 2, $APE = -4.11$), which is insignificant according to a Wald chi-square test for coefficients across tobit regressions ($p = .918$; see Table S.39 for all pairwise comparisons between treatments). This corroborates the results from Tepe et al. (2021), who find a large effect of introducing an efficiency loss but that it does not make a difference whether the efficiency loss is 5 or 20 percent.

(Self-)Image Concerns. Using the 10-item Martin-Larsen Approval Motivation Scale, I find that image concerns do not correlate with subjects' preferred tax rates in any of the current studies (all $p's > .118$, cf. Tables S.15, S.25, and S.33). Earlier studies demonstrate that (self-)image concerns can make people behave prosocially (Murnighan et al., 2001; Andreoni and Petrie, 2004; Ariely et al., 2009; Lacetera and Macis, 2010). And in fact, pooling all treatments I find that image concerns are a marginally significant predictor of greater dictator givings: moving from the least to the most concerned about image increases dictator givings by 5.15 percentage points ($p = .060$). In the modified dictator game, image concerns also predict a greater preference for equity compared to efficiency ($p = .008$). The fact that image concerns do not predict subjects' behaviour in the voting experiment suggests that subjects are able to make payoff-maximising decisions without compromising their (self-)image. This could for instance be the case if subjects justify their selfish behaviour by appealing to efficiency preferences, following the literature on how individuals often choose fairness principles in a self-serving manner (Messick and Sentis, 1979; Rodriguez-Lara and Moreno-Garrido, 2012).

S.5.3 Inequality Concepts and Preferences for Redistribution.

There are many ways to conceive and operationalise inequality (Kolm, 1976; Cowell, 2016), making inequality an essentially contested concepts (Gallie, 1955). Much debate concerns the importance of absolute and relative inequality (Atkinson and Brandolini, 2010; Wade, 2013; Niño-Zarazúa et al., 2017; Greenstein, 2020), with experimental evidence suggesting that people consider both when evaluating how equal earnings are in a group (Amiel and Cowell, 1992, 1999; Harrison and Seidl, 1994; Celse, 2017).

The current experiment provides evidence suggesting that concerns for inequality and personal costs cancel each other out when making choices about redistribution, regardless of whether the choice affects absolute or relative inequality. The first line of evidence comes from a comparison between *Realized* and *RealizedR*. Because these differ in whether the growth rates are uniform or unequal, a contrast between the two sheds light on the importance of an increase in relative inequality. As seen in Table S.40, there are no differences between the two treatments when controlling for dictator givings, demographics, or (political) attitudes. One possible concern about this comparison is that the treatments also differ in efficiency loss (2 percent in *Realized* versus 10 percent in *RealizedR*). Yet, adding interaction effects to control for this difference does not change the conclusion. There are no significant interaction effects of efficiency concerns and treatment effects (all $p's > .144$). Moreover, the treatment effect is non-significant for subsamples with all possible splits on efficiency concerns (see Figure S.18). Moreover, as explained above, efficiency concerns do not matter more for 10 percent efficiency loss compared to 2 percent efficiency loss (similar to Tepe et al., 2021).

Another way to examine the possible role of perceived inequality on preferences for redistribution is to exploit within-treatment variation in inequality in *Forecast*, *Ratio*, *ForecastR*, and *ForecastNo*. For completeness, I examine the predictive power of a series of possible operationalisation of inequality measures, and for each inequality measure (z) I use the following transformations: $f(z) = z$, $f(z) = z^2$, $f(z) = \frac{1}{z}$, and $f(z) = \log(z)$. To test effects of absolute inequality, I report the effect of the standard deviation, the absolute Gini coefficient, and the income difference between the rich and poor. As seen in Table S.34, none of these conceptualisations are significant predictors of the tax rate that a subject votes for.[9] For the relative measures, I examine the coefficient of variation, the Gini coefficient, and the ratio between the incomes of the rich and the poor. Again, none of these measures are significant predictors of the tax rate that a subject votes for (see Table S.35).

As explained in the above analysis, the fact that no inequality measure correlates with subjects' preferences for the tax rate does not imply that people do not care about inequality. Rather, it could be explained by an increase in inequality leading to both an increase in the WTP for redistribution and the personal costs of redistribution. According to this explanation, the two effects cancel out such that the share of their income that subjects are willing to give up remains constant.

S.6 Additional Tables and Figures, Study 1

Table S.10: EGB and tax inconsistency, restricted sample in Study 1

	(1)	(2)	(3)	(4)	(5)
Ratio	-9.31*	-8.67*	-7.79*	0.67	-0.89
	(4.79)	(4.77)	(4.54)	(4.68)	(4.68)
Realized	-12.59***	-12.98***	-14.01***	-5.77	-0.62
	(4.60)	(4.59)	(4.37)	(4.50)	(4.74)
Dictator Giving	0.63***	0.60***	0.48***	0.51***	0.51***
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Age		-0.28	-0.12	-0.14	-0.11
		(0.18)	(0.18)	(0.17)	(0.17)

[9] One exception is the inverse of the absolute Gini in ForecastR ($p = .028$). But this is likely a result of random variation as it is not a consistently significant predictor, and it is the only significant predictor from 60 regressions; 12 inequality measures $\times$ (4 treatments + pooling of treatments).

	(1)	(2)	(3)	(4)	(5)
Male		-7.79*	-5.72	-5.85	-5.25
		(3.97)	(3.85)	(3.78)	(3.76)
Black or African American		-0.24	-2.03	-1.43	-1.18
		(7.15)	(6.80)	(6.67)	(6.62)
Hispanic or Latino		4.87	1.75	2.15	4.20
		(9.94)	(9.37)	(9.19)	(9.16)
Asian American		-3.91	-7.79	-8.02	-8.20
		(8.23)	(7.88)	(7.75)	(7.70)
Other ethnicity		-2.45	0.64	0.95	2.87
		(16.00)	(15.23)	(14.97)	(14.91)
High school degree or equivalent (e.g. GED)		-12.06	-2.93	-6.36	-4.71
		(26.44)	(25.34)	(24.87)	(24.61)
Some college, no degree		-5.25	-2.07	-6.67	-4.33
		(26.10)	(24.99)	(24.53)	(24.28)
Associate degree (e.g. AA, AS)		-15.32	-9.37	-16.82	-14.59
		(26.47)	(25.34)	(24.90)	(24.65)
Bachelor's degree (e.g. BA, BS)		-5.33	-2.50	-7.20	-4.73
		(26.14)	(25.03)	(24.58)	(24.33)
Master's degree (e.g. MA, MS, MEd)		2.98	4.44	-2.25	-1.07
		(26.54)	(25.43)	(24.97)	(24.71)
Doctorate or $_p$pro degree (e.g. MD, DDS, PhD)		-14.98	-15.77	-21.56	-19.18
		(27.74)	(26.56)	(26.10)	(25.85)
Self-employed		3.87	3.66	3.06	2.31
		(5.94)	(5.68)	(5.58)	(5.55)
Unemployed		-1.09	-0.72	-2.33	-3.47
		(6.23)	(5.98)	(5.89)	(5.86)
Student		-9.04	-12.35	-13.10	-12.80
		(10.05)	(9.60)	(9.46)	(9.42)
Retired		2.68	-1.61	-0.24	-1.21
		(10.15)	(9.73)	(9.56)	(9.50)
Other employment		0.35	8.03	6.47	3.90
		(13.27)	(12.78)	(12.49)	(12.41)
Income$_p$		-30.66***	-9.83	-13.75	-13.14
		(11.51)	(11.42)	(11.24)	(11.18)
Efficiency from MDG			-1.77**	-1.88***	-1.90***
			(0.71)	(0.70)	(0.70)
Risk$_p$			-10.55	-9.54	-9.00
			(7.74)	(7.61)	(7.56)
Trust$_p$			19.22**	19.17**	18.02**

	(1)	(2)	(3)	(4)	(5)
			(7.99)	(7.85)	(7.81)
Political Right$_p$			-23.23***	-19.53**	-19.14**
			(8.30)	(8.16)	(8.11)
Meritocracy$_p$			-12.83*	-11.94	-12.88*
			(7.78)	(7.65)	(7.60)
Inequality Too Large$_p$			25.01***	23.51***	23.11***
			(8.80)	(8.64)	(8.59)
Government Responsibility$_p$			12.20	14.57*	14.86**
			(7.70)	(7.57)	(7.52)
MLAMS$_p$			-11.09	-13.41	-14.47
			(12.29)	(12.08)	(12.01)
Perceived Gains				47.17***	52.20***
				(8.20)	(8.31)
Low Personal Cost					15.60***
					(4.85)
Observations	918	918	918	918	918

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The sample is restricted to those subjects who do not calculate the correct inequality forecasts. Perceived gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). The baseline is a person who is randomised into the *Forecast* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.11: EGB and preferred tax, Forecast

	(1)	(2)	(3)	(4)	(5)
EGB	18.56*	18.70*	17.19*	9.18	1.90
	(10.73)	(10.98)	(10.39)	(10.06)	(11.26)
Dictator Giving	0.48***	0.46***	0.37**	0.46***	0.48***
	(0.16)	(0.17)	(0.16)	(0.15)	(0.15)
Age		-0.56*	-0.34	-0.35	-0.34
		(0.29)	(0.29)	(0.28)	(0.27)
Male		-3.37	0.28	-0.81	-0.81
		(6.98)	(6.82)	(6.55)	(6.53)
Black or African American		-13.20	-9.77	-8.25	-8.79
		(13.01)	(12.34)	(11.81)	(11.78)

	(1)	(2)	(3)	(4)	(5)
Hispanic or Latino		-13.71	-14.52	-13.30	-10.67
		(18.08)	(17.04)	(16.30)	(16.35)
Asian American		-0.21	1.18	0.44	0.30
		(15.57)	(14.90)	(14.41)	(14.36)
Other ethnicity		17.82	14.33	14.03	14.99
		(25.52)	(24.60)	(23.61)	(23.51)
High school degree or equivalent (e.g. GED)		1.16	-5.47	-3.93	1.09
		(32.22)	(30.53)	(29.19)	(29.25)
Some college, no degree		0.27	-9.35	-10.99	-6.26
		(31.29)	(29.67)	(28.37)	(28.41)
Associate degree (e.g. AA, AS)		-0.39	-7.70	-16.30	-12.20
		(31.90)	(30.28)	(29.00)	(28.98)
Bachelor's degree (e.g. BA, BS)		-0.69	-11.85	-14.12	-9.77
		(31.30)	(29.74)	(28.45)	(28.46)
Master's degree (e.g. MA, MS, MEd)		13.33	-3.41	-9.56	-6.08
		(32.15)	(30.54)	(29.23)	(29.17)
Doctorate or $_p$pro degree (e.g. MD, DDS, PhD)		-0.98	-11.64	-17.15	-13.24
		(34.29)	(32.68)	(31.33)	(31.30)
Self-employed		4.65	3.38	1.24	1.57
		(10.75)	(10.28)	(9.87)	(9.85)
Unemployed		-7.83	-12.74	-16.95*	-18.34*
		(10.41)	(10.00)	(9.67)	(9.69)
Student		-13.53	-19.17	-21.10	-20.98
		(19.25)	(18.45)	(17.87)	(17.83)
Retired		-10.45	-8.99	-5.12	-5.18
		(17.37)	(16.73)	(16.08)	(16.05)
Other employment		4.45	16.20	12.98	11.89
		(22.79)	(21.70)	(20.51)	(20.45)
Income_p		-41.40**	-12.15	-22.69	-23.23
		(19.43)	(19.20)	(18.56)	(18.50)
Efficiency from MDG			-1.26	-1.47	-1.47
			(1.17)	(1.13)	(1.12)
Risk_p			-23.25*	-19.75	-18.82
			(12.48)	(11.97)	(11.96)
Trust_p			11.12	11.66	11.44
			(12.71)	(12.24)	(12.20)
Political Right_p			-14.60	-7.97	-7.59
			(13.73)	(13.17)	(13.14)
Meritocracy_p			-26.62*	-22.08*	-23.97*

	(1)	(2)	(3)	(4)	(5)
			(13.64)	(13.14)	(13.17)
Inequality Too Large$_p$			36.53**	33.25**	32.36**
			(15.18)	(14.58)	(14.54)
Government Responsibility$_p$			8.61	13.54	14.05
			(13.18)	(12.68)	(12.65)
MLAMS$_p$			25.05	18.47	14.89
			(21.33)	(20.52)	(20.60)
Perceived Gains				44.92***	49.26***
				(8.34)	(8.90)
Low Personal Cost					11.17
					(7.86)
N	349	349	349	349	349

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009). Perceived gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.12: EGB and preferred tax, Realized

	(1)	(2)	(3)
EGB	-3.08	-3.35	-4.39
	(8.98)	(9.27)	(9.01)
Dictator Giving	0.77***	0.82***	0.70***
	(0.14)	(0.14)	(0.14)
Age		-0.38	-0.08
		(0.28)	(0.28)
Male		-1.77	0.08
		(5.75)	(5.61)
Black or African American		-4.78	-6.63
		(10.63)	(10.32)
Hispanic or Latino		3.95	-2.55
		(15.15)	(14.63)
Asian American		-9.67	-13.10
		(10.27)	(9.90)

	(1)	(2)	(3)
Other ethnicity		59.86	81.62**
		(39.47)	(39.75)
High school degree or equivalent (e.g. GED)		-85.13**	-76.89*
		(40.96)	(39.89)
Some college, no degree		-81.70**	-79.43**
		(40.67)	(39.46)
Associate degree (e.g. AA, AS)		-73.79*	-71.52*
		(41.45)	(40.30)
Bachelor's degree (e.g. BA, BS)		-75.38*	-74.04*
		(40.82)	(39.65)
Master's degree (e.g. MA, MS, MEd)		-76.05*	-74.06*
		(41.41)	(40.18)
Doctorate or $_p$pro degree (e.g. MD, DDS, PhD)		-69.77	-81.93*
		(43.29)	(42.13)
Self-employed		12.69	12.52
		(8.02)	(7.77)
Unemployed		3.72	5.42
		(9.57)	(9.31)
Student		11.86	5.85
		(15.60)	(15.15)
Retired		-3.18	-10.93
		(14.82)	(14.56)
Other employment		8.42	8.28
		(18.76)	(18.38)
Income$_p$		-7.36	10.02
		(17.01)	(17.02)
Efficiency from MDG			-1.56
			(1.07)
Risk$_p$			14.35
			(11.73)
Trust$_p$			4.64
			(12.43)
Political Right$_p$			-22.27*
			(12.75)
Meritocracy$_p$			-10.26
			(12.03)
Inequality Too Large$_p$			20.22
			(13.63)
Government Responsibility$_p$			10.81

	(1)	(2)	(3)
			(11.33)
MLAMS_p			-16.26
			(18.61)
N	359	359	359

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009). The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.13: Descriptive statistics for poor subjects

	N	Tax	DG	Efficiency	Actual SD	SD (F)	Actual CV	CV (F)	EGB
Forecast	157	66.62	36.46	3.69	1831.99	588.49	0.57	0.59	0.45
Ratio	116	66.72	38.29	3.40	1831.99	1058.10	0.57	0.59	0.44
Realized	129	65.43	36.16	3.46	1831.99	643.42	0.57	0.58	0.44
Total	402	66.27	36.90	3.53	1831.99	741.63	0.57	0.59	0.44

Note: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. Efficiency corresponds to subjects' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by subjects' estimates of income levels in the group. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009).

Table S.14: Efficiency and tax preferences, Study 1

	(1)	(2)	(3)	(4)
Ratio	-7.56	-7.84*	-7.35	-7.49*
	(4.66)	(4.54)	(4.52)	(4.32)
Realized	-12.45***	-13.01***	-13.24***	-13.88***
	(4.45)	(4.35)	(4.33)	(4.14)
Efficiency from MDG	-3.26***	-2.34***	-2.05***	-1.51**
	(0.71)	(0.71)	(0.70)	(0.68)
Dictator Giving		0.68***	0.66***	0.58***
		(0.09)	(0.09)	(0.09)
Age			-0.33*	-0.19
			(0.17)	(0.17)

	(1)	(2)	(3)	(4)
Male			-7.26*	-5.01
			(3.74)	(3.64)
Black or African American			-1.30	-2.99
			(7.06)	(6.74)
Hispanic or Latino			5.23	2.56
			(9.35)	(8.88)
Asian American			-1.68	-5.31
			(7.56)	(7.25)
Other ethnicity			-2.79	0.71
			(15.87)	(15.15)
High school degree or equivalent (e.g. GED)			-25.58	-16.01
			(24.39)	(23.45)
Some college, no degree			-17.25	-14.65
			(24.01)	(23.08)
Associate degree (e.g. AA, AS)			-24.21	-19.14
			(24.40)	(23.45)
Bachelor's degree (e.g. BA, BS)			-18.54	-16.47
			(24.04)	(23.12)
Master's degree (e.g. MA, MS, MEd)			-10.19	-10.19
			(24.41)	(23.47)
Doctorate or pro degree (e.g. MD, DDS, PhD)			-27.05	-26.43
			(25.56)	(24.59)
Self-employed			7.12	7.09
			(5.68)	(5.45)
Unemployed			0.35	0.54
			(6.02)	(5.81)
Student			-10.74	-13.12
			(9.92)	(9.50)
Retired			3.02	-0.23
			(9.74)	(9.38)
Other employment			0.59	7.03
			(12.54)	(12.11)
Income_p			-28.56***	-8.16
			(10.81)	(10.78)
Risk_p				-12.85*
				(7.39)
Trust_p				16.34**
				(7.60)
Political Right_p				-25.49***

	(1)	(2)	(3)	(4)
				(7.88)
Meritocracy$_p$				-14.01*
				(7.46)
Inequality Too Large$_p$				22.50***
				(8.34)
Government Responsibility$_p$				10.77
				(7.28)
MLAMS$_p$				-7.09
				(11.83)
Observations	1013	1013	1013	1013

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The baseline is a person who is randomised into the *Forecast* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.15: Image concerns and tax preferences, Study 1

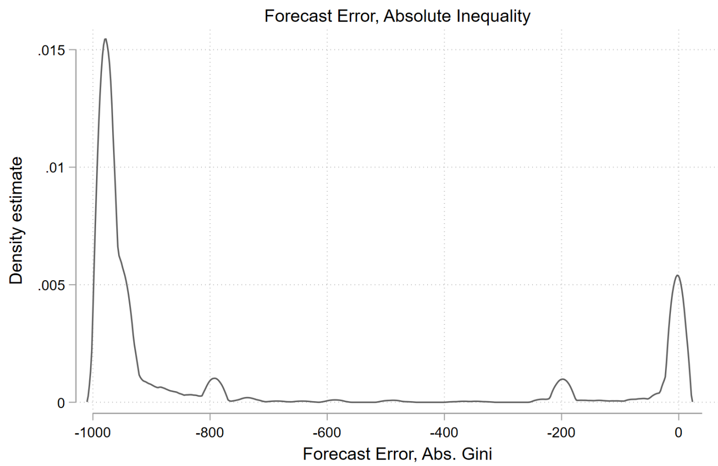
	(1)	(2)	(3)	(4)
Ratio	-8.65*	-8.69*	-8.06*	-7.49*
	(4.69)	(4.56)	(4.53)	(4.32)
Realized	-12.12***	-12.87***	-13.12***	-13.88***
	(4.49)	(4.36)	(4.34)	(4.14)
MLAMS$_p$	19.16	15.24	11.80	-7.09
	(12.25)	(11.93)	(12.10)	(11.83)
Dictator Giving		0.73***	0.70***	0.58***
		(0.09)	(0.09)	(0.09)
Age			-0.34**	-0.19
			(0.17)	(0.17)
Male			-7.41*	-5.01
			(3.79)	(3.64)
Black or African American			-1.29	-2.99
			(7.08)	(6.74)
Hispanic or Latino			5.48	2.56
			(9.39)	(8.88)
Asian American			-2.11	-5.31
			(7.59)	(7.25)
Other ethnicity			-2.34	0.71
			(15.89)	(15.15)

	(1)	(2)	(3)	(4)
High school degree or equivalent (e.g. GED)			-23.59	-16.01
			(24.57)	(23.45)
Some college, no degree			-16.32	-14.65
			(24.20)	(23.08)
Associate degree (e.g. AA, AS)			-23.15	-19.14
			(24.58)	(23.45)
Bachelor's degree (e.g. BA, BS)			-17.55	-16.47
			(24.22)	(23.12)
Master's degree (e.g. MA, MS, MEd)			-9.25	-10.19
			(24.59)	(23.47)
Doctorate or pro degree (e.g. MD, DDS, PhD)			-26.29	-26.43
			(25.74)	(24.59)
Self-employed			7.47	7.09
			(5.70)	(5.45)
Unemployed			0.64	0.54
			(6.04)	(5.81)
Student			-9.98	-13.12
			(9.94)	(9.50)
Retired			3.43	-0.23
			(9.76)	(9.38)
Other employment			0.75	7.03
			(12.58)	(12.11)
Income$_p$			-30.59***	-8.16
			(10.84)	(10.78)
Efficiency from MDG				-1.51**
				(0.68)
Risk$_p$				-12.85*
				(7.39)
Trust$_p$				16.34**
				(7.60)
Political Right$_p$				-25.49***
				(7.88)
Meritocracy$_p$				-14.01*
				(7.46)
Inequality Too Large$_p$				22.50***
				(8.34)
Government Responsibility$_p$				10.77
				(7.28)

	(1)	(2)	(3)	(4)
Observations	1013	1013	1013	1013

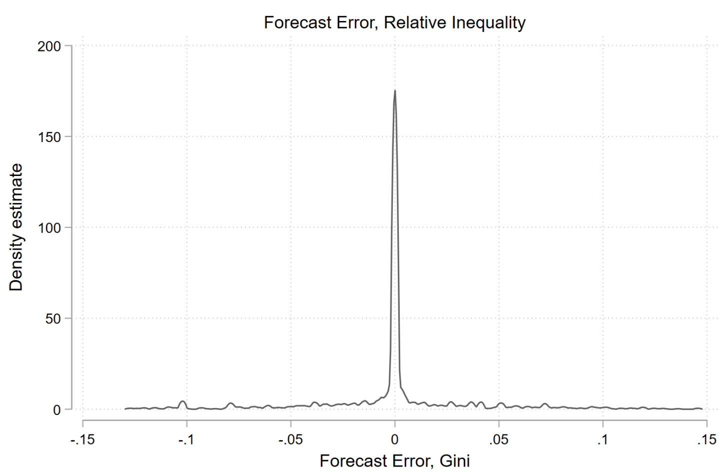
Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The baseline is a person who is randomised into the *Forecast* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Figure S.7: Forecast error of absolute inequality, Abs. Gini, Study 1



Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 10$). The Absolute Gini coefficient is calculated as $AG(\mathbf{x}) = \bar{x}\left(\frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i\right)$, where x_i are ranked-ordered incomes such that $x_i \leq x_{i+1}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors.

Figure S.8: Forecast error of relative inequality, Gini, Study 1



Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 0.001$). The Gini coefficient is calculated as $G(\mathbf{x}) = \frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i$, where x_i are ranked-ordered incomes such that $x_i \leq x_{i+1}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors.

S.7 Additional Tables and Figures, Study 2

Table S.16: Sample characteristics, Study 2

	Freq.	Percent
Female	548	55.9
Male	432	44.1
White or Caucasian American	752	76.7
Black or African American	94	9.6
Hispanic or Latino	41	4.2
Asian American	71	7.2
Other ethnicity	22	2.2
Less than a high school diploma	2	0.2
High school degree or equivalent (e.g. GED)	83	8.5
Some college, no degree	181	18.5
Associate degree (e.g. AA, AS)	99	10.1
Bachelor's degree (e.g. BA, BS)	419	42.8
Master's degree (e.g. MA, MS, MEd)	156	15.9
Doctorate or professional degree (e.g. MD, DDS, PhD)	40	4.1
Employed (part or full time)	647	66.0
Self-employed	139	14.2
Unemployed	89	9.1
Student	36	3.7
Retired	45	4.6
Other employment	24	2.4
Total	980	100.0

Table S.17: Summary statistics by treatment, Study 2

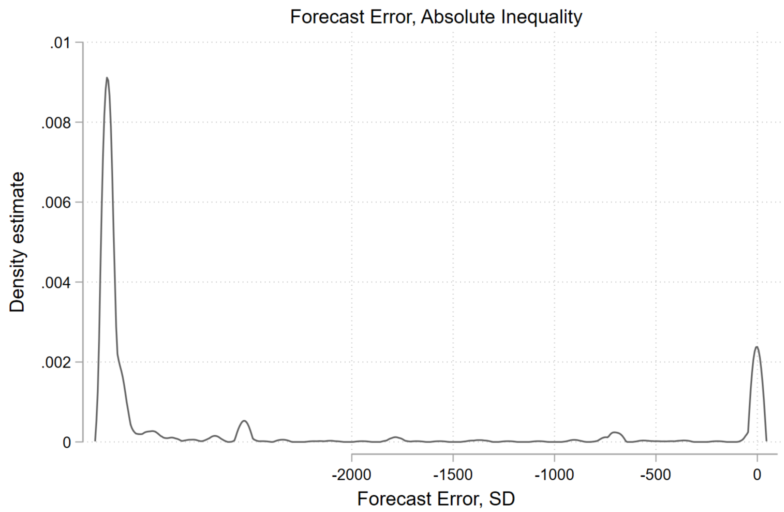
	Forecast	Ratio	Realized	Total
Risk	0.47	0.47	0.46	0.47
Trust	0.53	0.54	0.53	0.53
Political Right	0.44	0.43	0.42	0.43
Belief in Meritocracy	0.63	0.62	0.59	0.62
Inequality Too Large	0.80	0.81	0.80	0.81
Government Responsibility	0.57	0.57	0.59	0.58
MLAMS	0.37	0.38	0.38	0.38
Social Ladder	0.43	0.43	0.42	0.43

Table S.18: Descriptive statistics for poor subjects, Study 2

	N	Tax	DG	Efficiency	Actual SD	SD (F)	Actual CV	CV (F)	EGB
Forecast	157	66.62	36.46	3.69	1831.99	588.49	0.57	0.59	0.45
Ratio	116	66.72	38.29	3.40	1831.99	1058.10	0.57	0.59	0.44
Realized	129	65.43	36.16	3.46	1831.99	643.42	0.57	0.58	0.44
Total	402	66.27	36.90	3.53	1831.99	741.63	0.57	0.59	0.44

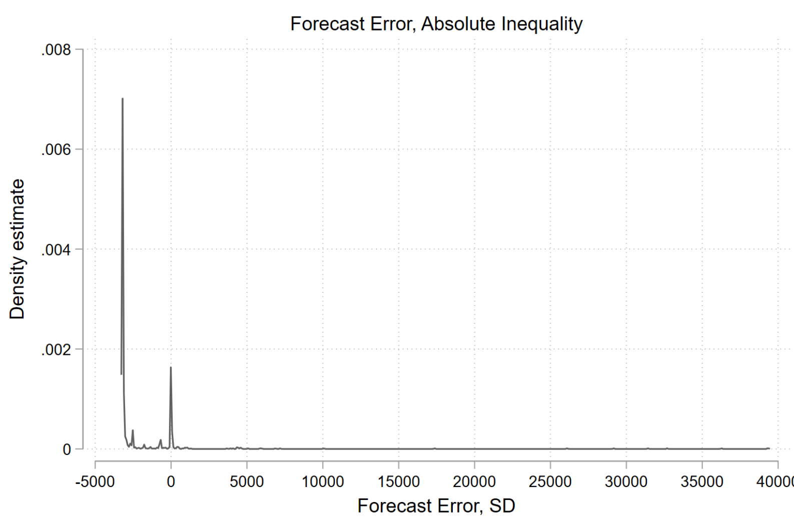
Note: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. Efficiency corresponds to subjects' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by subjects' estimates of income levels in the group. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009).

Figure S.9: Forecast error of absolute inequality, Study 2



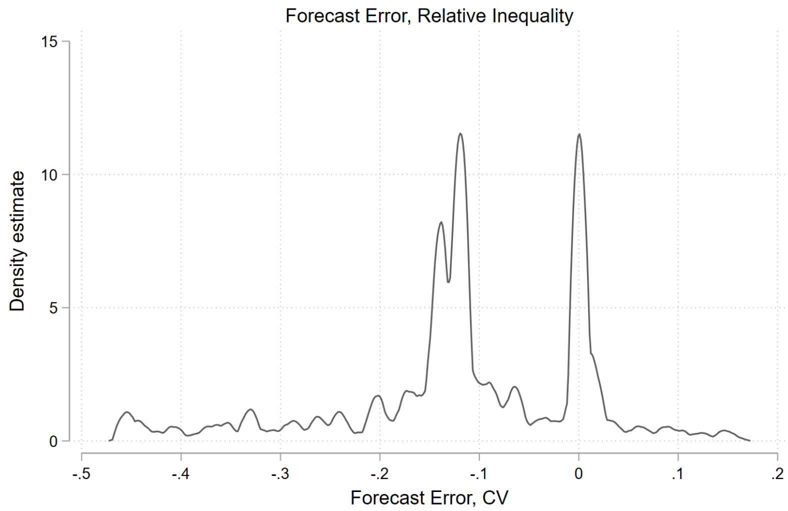
Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors. For the full sample, see Figure S.10.

Figure S.10: Forecast error of absolute inequality, Study 2, full sample



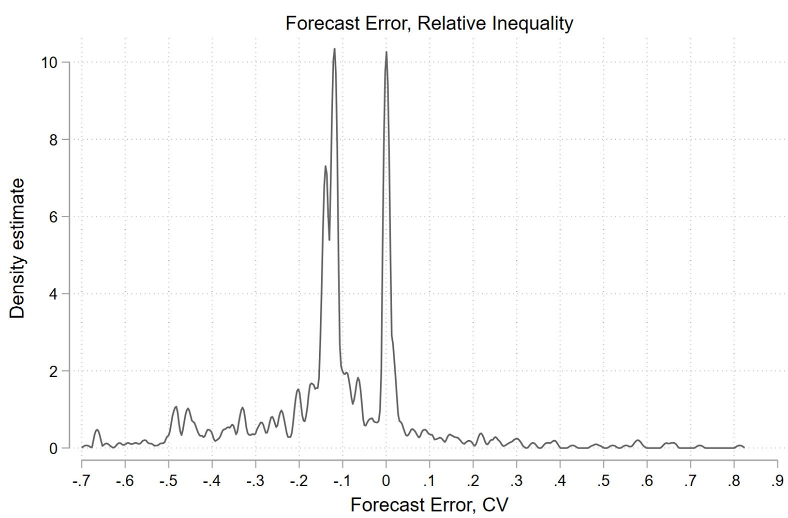
Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$.

Figure S.11: Forecast error of relative inequality, Study 2



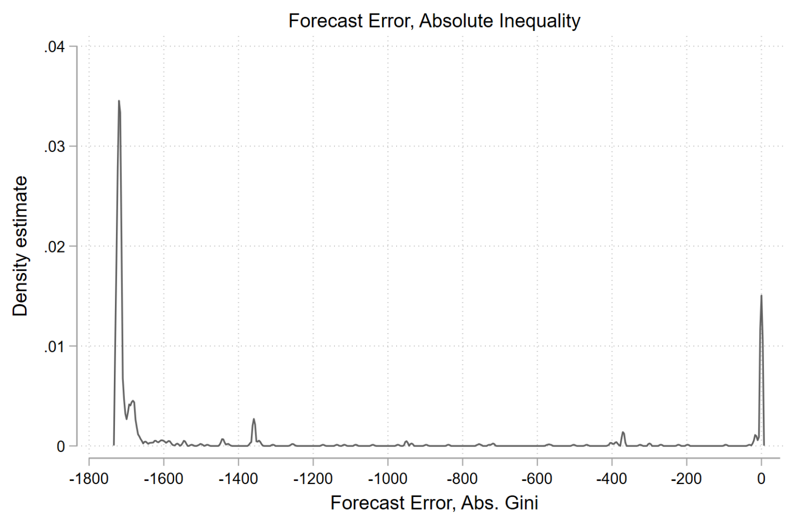
Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 0.005$). The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors. For the full sample, see Figure S.12.

Figure S.12: Forecast error of relative inequality, Study 2, full sample



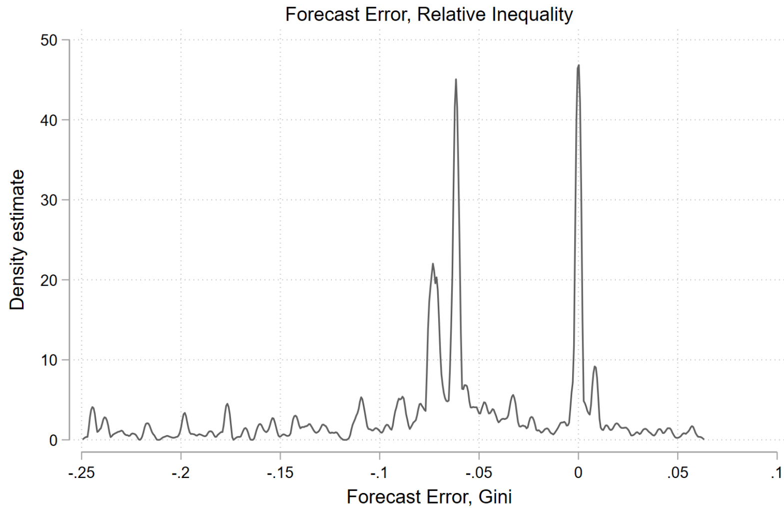
Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 0.005$). The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$.

Figure S.13: Forecast error of absolute inequality, Abs. Gini, Study 2



Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 10$). The Absolute Gini coefficient is calculated as $AG(\mathbf{x}) = \bar{x}\left(\frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i\right)$, where x_i are ranked-ordered incomes such that $x_i \leq x_{i+1}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors.

Figure S.14: Forecast error of relative inequality, Gini, Study 2



Note: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 0.001$). The Gini coefficient is calculated as $G(\mathbf{x}) = \frac{N+1}{N} - \frac{2}{N^2\bar{x}} \sum_{i=1}^{N}(N + 1 - i)x_i$, where x_i are ranked-ordered incomes such that $x_i \leq x_{i+1}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors.

Table S.19: EGB and tax inconsistency, Study 2

	(1)	(2)	(3)	(4)	(5)
RealizedR	1.57	1.16	1.91	4.94	7.30*
	(4.03)	(4.02)	(3.88)	(4.00)	(4.32)
Dictator Giving	0.77***	0.79***	0.68***	0.68***	0.68***
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Age		-0.15	-0.05	-0.05	-0.07
		(0.19)	(0.18)	(0.18)	(0.18)
Male		-1.62	-1.66	-1.83	-1.50
		(4.16)	(4.06)	(4.04)	(4.04)
Black or African American		-5.83	-6.45	-5.83	-5.62
		(6.73)	(6.62)	(6.58)	(6.58)
Hispanic or Latino		17.87*	14.99	13.36	13.19
		(10.01)	(9.69)	(9.63)	(9.62)
Asian American		-5.47	-3.95	-3.04	-3.46
		(7.76)	(7.59)	(7.54)	(7.54)
Other ethnicity		23.72	26.88*	27.17*	27.45*
		(14.79)	(14.59)	(14.48)	(14.48)

	(1)	(2)	(3)	(4)	(5)
High school degree or equivalent (e.g. GED)		25.23	40.66	38.66	36.52
		(50.41)	(48.59)	(48.23)	(48.20)
Some college, no degree		33.67	48.01	45.51	42.85
		(50.25)	(48.45)	(48.09)	(48.07)
Associate degree (e.g. AA, AS)		26.90	39.44	36.31	33.56
		(50.34)	(48.52)	(48.16)	(48.14)
Bachelor's degree (e.g. BA, BS)		43.13	56.16	53.24	50.74
		(50.16)	(48.35)	(47.99)	(47.97)
Master's degree (e.g. MA, MS, MEd)		50.64	60.07	57.94	55.04
		(50.40)	(48.58)	(48.22)	(48.20)
Doctorate or pro degree (e.g. MD, DDS, PhD)		65.53	71.03	68.45	66.02
		(51.23)	(49.38)	(49.02)	(48.99)
Self-employed		3.43	3.04	3.03	2.99
		(6.07)	(5.95)	(5.91)	(5.90)
Unemployed		13.13*	13.62*	14.78**	14.02*
		(7.54)	(7.33)	(7.28)	(7.29)
Student		4.79	-1.77	0.33	0.65
		(11.28)	(10.98)	(10.92)	(10.91)
Retired		-6.49	-9.60	-9.61	-9.68
		(10.50)	(10.28)	(10.21)	(10.20)
Other employment		-3.50	-4.30	-2.70	-3.93
		(12.70)	(12.38)	(12.30)	(12.32)
Income_p		1.27	10.61	10.43	11.24
		(11.91)	(11.91)	(11.83)	(11.83)
Efficiency from MDG			-2.36***	-2.38***	-2.40***
			(0.78)	(0.78)	(0.78)
Risk_p			-4.85	-4.02	-4.69
			(8.12)	(8.06)	(8.07)
Trust_p			22.98***	21.49**	21.50**
			(8.77)	(8.71)	(8.70)
Political Right_p			-2.34	-3.39	-3.35
			(8.78)	(8.72)	(8.71)
Meritocracy_p			-19.90**	-19.77**	-20.49**
			(8.55)	(8.49)	(8.50)
$\text{Inequality Too Large}_p$			5.15	4.77	5.50
			(9.80)	(9.73)	(9.73)
$\text{Government Responsibility}_p$			20.14**	18.97**	18.82**
			(8.07)	(8.02)	(8.01)
MLAMS_p			-1.29	0.76	1.18

	(1)	(2)	(3)	(4)	(5)
			(13.48)	(13.41)	(13.40)
Perceived Gains				22.39***	24.64***
				(7.82)	(7.97)
Low Personal Cost					10.04
					(6.98)
Observations	698	698	698	698	698

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Perceived gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. The baseline is a person in *ForecastR* who is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.20: EGB and tax inconsistency, restricted sample in Study 2

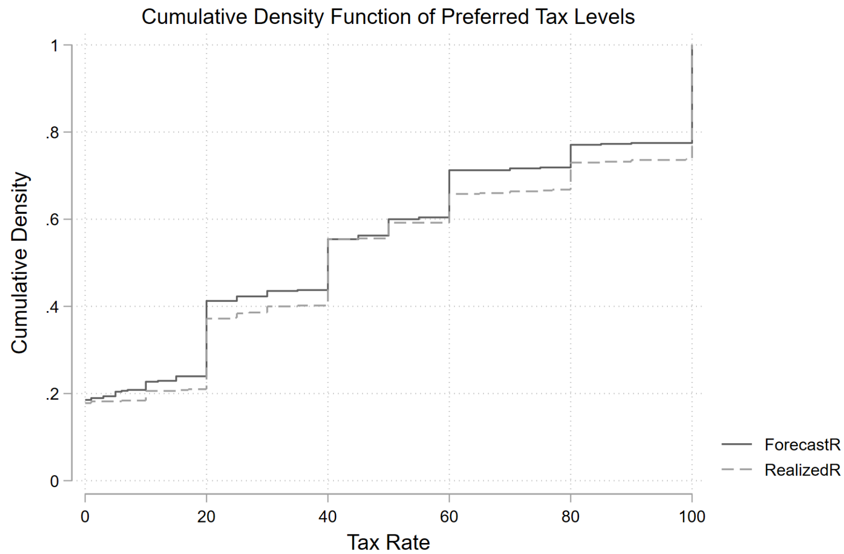
	(1)	(2)	(3)
RealizedR	1.80	1.04	1.28
	(4.20)	(4.18)	(4.04)
Dictator Giving	0.76***	0.78***	0.68***
	(0.11)	(0.11)	(0.11)
Age		-0.14	-0.03
		(0.20)	(0.19)
Male		-2.59	-2.88
		(4.38)	(4.28)
Black or African American		-6.82	-8.11
		(6.89)	(6.79)
Hispanic or Latino		14.08	11.46
		(10.59)	(10.26)
Asian American		-6.68	-5.92
		(8.03)	(7.84)
Other ethnicity		25.47*	29.46*
		(15.38)	(15.20)
High school degree or equivalent (e.g. GED)		24.65	41.19
		(50.06)	(48.18)
Some college, no degree		33.00	49.03
		(49.91)	(48.05)
Associate degree (e.g. AA, AS)		26.86	40.00

	(1)	(2)	(3)
		(50.02)	(48.12)
Bachelor's degree (e.g. BA, BS)		42.36	57.59
		(49.82)	(47.95)
Master's degree (e.g. MA, MS, MEd)		52.38	63.65
		(50.10)	(48.22)
Doctorate or pro degree (e.g. MD, DDS, PhD)		65.34	72.49
		(50.99)	(49.06)
Self-employed		2.91	3.19
		(6.27)	(6.13)
Unemployed		11.03	12.07
		(7.70)	(7.48)
Student		5.06	-1.48
		(11.48)	(11.17)
Retired		-7.33	-10.33
		(10.76)	(10.57)
Other employment		-9.58	-10.19
		(13.00)	(12.68)
Income_p		-3.46	8.07
		(12.46)	(12.51)
Efficiency from MDG			-2.35***
			(0.82)
Risk_p			-6.44
			(8.39)
Trust_p			20.24**
			(9.07)
Political Right_p			1.11
			(9.01)
Meritocracy_p			-19.45**
			(8.96)
$\text{Inequality Too Large}_p$			6.22
			(10.05)
$\text{Government Responsibility}_p$			24.61***
			(8.29)
MLAMS_p			-0.57
			(13.88)
Observations	634	634	634

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The sample is restricted to those subjects who do not calculate the correct inequality forecasts. Perceived

gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). The baseline is a person who is randomised into the *ForecastR* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Figure S.15: Preferred tax rate by treatment



Note: the figure presents the cumulative density function (or empirical distribution function) of the subjects' tax decisions by treatment.

Table S.21: EGB and preferred tax, ForecastR

	(1)	(2)	(3)	(4)	(5)
EGB	-16.10*	-12.87	-11.21	-8.34	-15.79*
	(8.38)	(8.47)	(8.25)	(8.20)	(8.89)
Dictator Giving	0.78***	0.79***	0.67***	0.67***	0.66***
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Age		-0.20	-0.16	-0.16	-0.20
		(0.25)	(0.24)	(0.24)	(0.24)
Male		-7.13	-4.73	-5.04	-4.24
		(5.57)	(5.46)	(5.39)	(5.38)
Black or African American		4.64	5.20	6.27	7.28
		(9.18)	(8.99)	(8.89)	(8.86)
Hispanic or Latino		22.07*	18.12*	16.30	15.61
		(11.23)	(10.89)	(10.73)	(10.68)
Asian American		-1.02	-0.86	0.31	-0.57
		(9.95)	(9.85)	(9.72)	(9.66)
Other ethnicity		-53.36	-45.00	-43.32	-38.16
		(41.42)	(41.54)	(41.07)	(40.84)
High school degree or equivalent (e.g. GED)		24.63	45.47	44.18	42.18

	(1)	(2)	(3)	(4)	(5)
		(48.39)	(46.65)	(46.00)	(45.72)
Some college, no degree		28.16	47.41	45.15	41.42
		(47.86)	(46.18)	(45.53)	(45.28)
Associate degree (e.g. AA, AS)		18.86	34.25	31.47	27.03
		(48.13)	(46.34)	(45.70)	(45.47)
Bachelor's degree (e.g. BA, BS)		39.77	58.19	55.67	51.94
		(47.75)	(46.06)	(45.42)	(45.17)
Master's degree (e.g. MA, MS, MEd)		53.79	69.24	68.28	63.05
		(48.26)	(46.57)	(45.92)	(45.70)
Doctorate or $_p$pro degree (e.g. MD, DDS, PhD)		58.01	69.10	67.45	64.13
		(50.34)	(48.63)	(47.97)	(47.71)
Self-employed		5.07	5.93	5.87	6.02
		(8.07)	(7.93)	(7.82)	(7.76)
Unemployed		1.95	4.78	6.67	4.88
		(10.23)	(9.99)	(9.87)	(9.84)
Student		-3.74	-9.61	-6.23	-5.14
		(14.26)	(13.83)	(13.67)	(13.59)
Retired		-7.85	-13.62	-14.68	-15.07
		(17.18)	(17.09)	(16.93)	(16.88)
Other employment		-10.16	-11.78	-9.42	-11.52
		(18.19)	(17.63)	(17.40)	(17.31)
Income$_p$		-19.97	-10.45	-10.27	-9.81
		(16.36)	(16.38)	(16.17)	(16.07)
Efficiency from MDG			-2.49**	-2.47**	-2.59**
			(1.10)	(1.08)	(1.08)
Risk$_p$			-11.91	-11.22	-11.74
			(11.23)	(11.08)	(11.02)
Trust$_p$			30.18**	27.94**	27.80**
			(11.70)	(11.56)	(11.49)
Political Right$_p$			4.00	1.98	2.18
			(11.78)	(11.64)	(11.57)
Meritocracy$_p$			-20.27*	-20.81*	-21.87*
			(11.69)	(11.54)	(11.49)
Inequality Too Large$_p$			6.86	6.21	8.52
			(13.51)	(13.34)	(13.32)
Government Responsibility$_p$			18.66*	16.24	16.27
			(10.69)	(10.58)	(10.52)
MLAMS$_p$			-6.64	-3.07	-2.30
			(18.84)	(18.66)	(18.55)

	(1)	(2)	(3)	(4)	(5)
Perceived Gains				20.80***	23.27***
				(7.44)	(7.50)
Low Personal Cost					15.33**
					(7.23)
N	345	345	345	345	345

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009). Perceived gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.22: EGB and preferred tax, RealizedR

	(1)	(2)	(3)
EGB	-19.17**	-15.27	-10.48
	(9.41)	(9.52)	(9.28)
Dictator Giving	0.75***	0.77***	0.68***
	(0.15)	(0.15)	(0.15)
Age		-0.05	0.11
		(0.28)	(0.28)
Male		1.16	1.22
		(6.21)	(6.14)
Black or African American		-16.73*	-19.94**
		(9.84)	(9.80)
Hispanic or Latino		7.56	4.23
		(19.70)	(19.03)
Asian American		-7.94	-3.95
		(11.98)	(11.67)
Other ethnicity		35.71**	37.45**
		(16.81)	(16.76)
Some college, no degree		9.89	8.63
		(12.18)	(11.83)
Associate degree (e.g. AA, AS)		10.22	9.91
		(14.14)	(13.70)
Bachelor's degree (e.g. BA, BS)		17.03	14.33

	(1)	(2)	(3)
		(11.46)	(11.18)
Master's degree (e.g. MA, MS, MEd)		19.98	13.43
		(13.04)	(12.82)
Doctorate or pro degree (e.g. MD, DDS, PhD)		39.10**	29.19*
		(17.26)	(16.86)
Self-employed		2.95	1.76
		(9.09)	(8.91)
Unemployed		26.28**	25.94**
		(11.03)	(10.76)
Student		25.17	17.90
		(18.16)	(17.65)
Retired		-7.44	-9.48
		(13.58)	(13.34)
Other employment		5.06	6.75
		(17.76)	(17.41)
Income_p		19.16	30.02*
		(17.54)	(17.61)
Efficiency from MDG			-2.26**
			(1.12)
Risk_p			-0.32
			(11.99)
Trust_p			10.05
			(13.41)
Political Right_p			-13.81
			(13.43)
Meritocracy_p			-16.58
			(12.50)
Inequality Too Large_p			4.78
			(14.20)
Government Responsibility_p			17.81
			(12.15)
MLAMS_p			4.82
			(19.51)
N	353	353	353

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. EGB is the extent of exponential growth bias, estimated by the functional form specified in Stango and Zinman (2009). The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum

possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.23: Class and preferred tax, Study 2

	(ForecastNo)	(RealizedNo)	(Total)
Middle Class	-35.70***	-43.83***	-39.93***
	(6.51)	(6.58)	(4.63)
Rich	-45.72***	-58.79***	-52.52***
	(7.04)	(7.27)	(5.06)
Dictator Giving	0.52***	0.48***	0.50***
	(0.13)	(0.14)	(0.09)
N	480	500	980

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. The baseline is a subject randomised into the poor income class. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.24: Efficiency and tax preferences, Study 2

	(1)	(2)	(3)	(4)
RealizedR	1.67	1.44	1.05	1.91
	(4.09)	(4.00)	(3.98)	(3.88)
Efficiency from MDG	-4.09***	-2.63***	-2.77***	-2.36***
	(0.79)	(0.81)	(0.80)	(0.78)
Dictator Giving		0.67***	0.69***	0.68***
		(0.11)	(0.11)	(0.10)
Age			-0.16	-0.05
			(0.19)	(0.18)
Male			-1.29	-1.66
			(4.12)	(4.06)
Black or African American			-4.82	-6.45
			(6.67)	(6.62)
Hispanic or Latino			17.23*	14.99
			(9.93)	(9.69)
Asian American			-4.91	-3.95
			(7.70)	(7.59)
Other ethnicity			24.36*	26.88*
			(14.72)	(14.59)
High school degree or equivalent (e.g. GED)			29.04	40.66

	(1)	(2)	(3)	(4)
			(49.93)	(48.59)
Some college, no degree			37.55	48.01
			(49.77)	(48.45)
Associate degree (e.g. AA, AS)			29.63	39.44
			(49.86)	(48.52)
Bachelor's degree (e.g. BA, BS)			47.79	56.16
			(49.69)	(48.35)
Master's degree (e.g. MA, MS, MEd)			53.68	60.07
			(49.92)	(48.58)
Doctorate or pro degree (e.g. MD, DDS, PhD)			69.80	71.03
			(50.75)	(49.38)
Self-employed			4.05	3.04
			(6.03)	(5.95)
Unemployed			11.82	13.62*
			(7.47)	(7.33)
Student			2.63	-1.77
			(11.21)	(10.98)
Retired			-7.37	-9.60
			(10.41)	(10.28)
Other employment			-2.98	-4.30
			(12.59)	(12.38)
Income_p			3.28	10.61
			(11.81)	(11.91)
Risk_p				-4.85
				(8.12)
Trust_p				22.98***
				(8.77)
Political Right_p				-2.34
				(8.78)
Meritocracy_p				-19.90**
				(8.55)
$\text{Inequality Too Large}_p$				5.15
				(9.80)
$\text{Government Responsibility}_p$				20.14**
				(8.07)
MLAMS_p				-1.29
				(13.48)
Observations	698	698	698	698

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The baseline is a person who is randomised into the *ForecastR* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.25: Image concerns and tax preferences, Study 2

	(1)	(2)	(3)	(4)
RealizedR	1.85	1.52	1.13	1.91
	(4.17)	(4.03)	(4.02)	(3.88)
MLAMS$_p$	14.51	14.76	6.14	-1.29
	(14.15)	(13.73)	(13.82)	(13.48)
Dictator Giving		0.77***	0.79***	0.68***
		(0.10)	(0.10)	(0.10)
Age			-0.14	-0.05
			(0.19)	(0.18)
Male			-1.65	-1.66
			(4.16)	(4.06)
Black or African American			-5.57	-6.45
			(6.75)	(6.62)
Hispanic or Latino			17.97*	14.99
			(10.01)	(9.69)
Asian American			-5.75	-3.95
			(7.78)	(7.59)
Other ethnicity			23.98	26.88*
			(14.80)	(14.59)
High school degree or equivalent (e.g. GED)			25.59	40.66
			(50.41)	(48.59)
Some college, no degree			34.07	48.01
			(50.25)	(48.45)
Associate degree (e.g. AA, AS)			27.40	39.44
			(50.35)	(48.52)
Bachelor's degree (e.g. BA, BS)			43.31	56.16
			(50.15)	(48.35)
Master's degree (e.g. MA, MS, MEd)			50.72	60.07
			(50.39)	(48.58)
Doctorate or pro degree (e.g. MD, DDS, PhD)			65.68	71.03
			(51.22)	(49.38)
Self-employed			3.47	3.04

	(1)	(2)	(3)	(4)
			(6.07)	(5.95)
Unemployed			12.96*	13.62*
			(7.55)	(7.33)
Student			4.49	-1.77
			(11.30)	(10.98)
Retired			-6.48	-9.60
			(10.50)	(10.28)
Other employment			-3.55	-4.30
			(12.70)	(12.38)
Income$_p$			1.42	10.61
			(11.91)	(11.91)
Efficiency from MDG				-2.36***
				(0.78)
Risk$_p$				-4.85
				(8.12)
Trust$_p$				22.98***
				(8.77)
Political Right$_p$				-2.34
				(8.78)
Meritocracy$_p$				-19.90**
				(8.55)
Inequality Too Large$_p$				5.15
				(9.80)
Government Responsibility$_p$				20.14**
				(8.07)
Observations	698	698	698	698

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The baseline is a person who is randomised into the *Forecast* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

S.8 Additional Tables and Figures, Study 3

Table S.26: Sample characteristics, Study 3

	Freq.	Percent
Female	644	58.9
Male	450	41.1
White or Caucasian American	861	78.7
Black or African American	89	8.1
Hispanic or Latino	56	5.1
Asian American	65	5.9
Other ethnicity	23	2.1
Less than a high school diploma	7	0.6
High school degree or equivalent (e.g. GED)	93	8.5
Some college, no degree	238	21.8
Associate degree (e.g. AA, AS)	104	9.5
Bachelor's degree (e.g. BA, BS)	411	37.6
Master's degree (e.g. MA, MS, MEd)	190	17.4
Doctorate or professional degree (e.g. MD, DDS, PhD)	51	4.7
Employed (part or full time)	708	64.7
Self-employed	152	13.9
Unemployed	99	9.0
Student	46	4.2
Retired	54	4.9
Other employment	35	3.2
Total	1094	100.0

Table S.27: Summary statistics by treatment, Study 3

	ForecastNo	RealizedNo	Total
Risk	0.47	0.49	0.48
Trust	0.54	0.55	0.54
Political Right	0.43	0.41	0.42
Belief in Meritocracy	0.59	0.59	0.59
Inequality Too Large	0.82	0.82	0.82
Government Responsibility	0.59	0.62	0.60
Social Ladder	0.43	0.44	0.44

Table S.28: Observed absolute inequality, ForecastNo

SD	Freq.	Percent
18	67	12.4
26	71	13.2
29	70	13.0
68	66	12.2
85	69	12.8
92	68	12.6
354	60	11.1
1313	68	12.6
Total	1415	100.0

Note: for comparison, subjects in *RealizedNo* faced an absolute inequality of $SD = 1832$ (as in Study 1). See Figure S.16 for an illustration of the average preferred tax rate across all levels of absolute inequality.

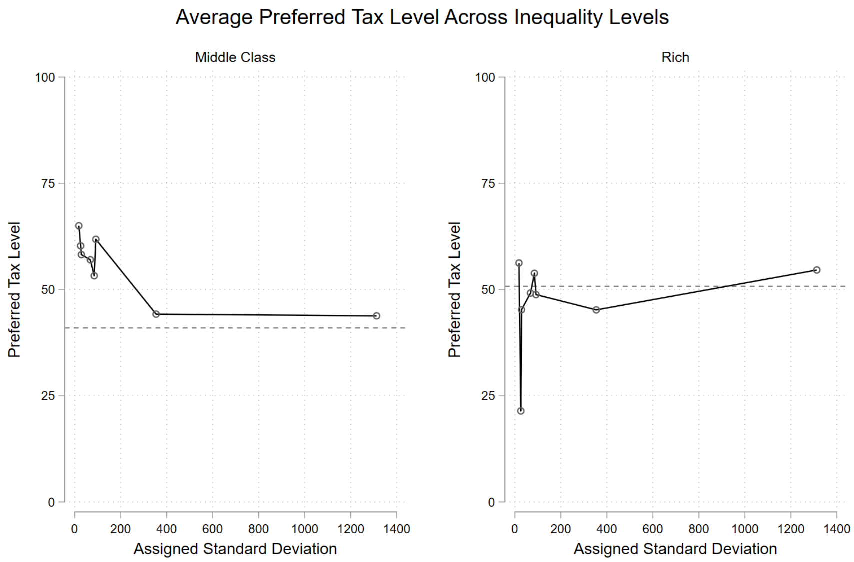
Table S.29: EGB and tax inconsistency, Study 3

	(1)	(2)	(3)	(4)
RealizedNo	-8.32*	-8.89**	-10.95***	2.47
	(4.53)	(4.46)	(4.19)	(4.93)
Dictator Giving	0.77***	0.74***	0.64***	0.65***
	(0.11)	(0.11)	(0.11)	(0.11)
Age		-0.10	0.22	0.23
		(0.21)	(0.20)	(0.20)
Male		-9.41**	-5.70	-4.96
		(4.63)	(4.45)	(4.38)
Black or African American		-11.45	-12.12	-15.21*
		(8.39)	(7.90)	(7.79)
Hispanic or Latino		-15.20	-13.87	-13.84
		(10.92)	(10.30)	(10.10)
Asian American		8.11	3.94	2.56
		(9.47)	(8.91)	(8.77)
Other ethnicity		28.95*	24.60*	23.34
		(15.29)	(14.40)	(14.23)
High school degree or equivalent (e.g. GED)		-47.70	-46.19	-43.88
		(31.42)	(29.17)	(28.95)
Some college, no degree		-41.37	-41.08	-36.79
		(31.09)	(28.86)	(28.66)
Associate degree (e.g. AA, AS)		-50.36	-45.86	-43.24
		(31.55)	(29.29)	(29.07)
Bachelor's degree (e.g. BA, BS)		-47.04	-50.74*	-47.46*
		(31.09)	(28.90)	(28.70)
Master's degree (e.g. MA, MS, MEd)		-51.50	-54.36*	-49.55*
		(31.46)	(29.28)	(29.07)
Doctorate or pro degree (e.g. MD, DDS, PhD)		-37.26	-39.58	-35.10
		(32.73)	(30.48)	(30.24)
Self-employed		-1.23	-2.83	-1.01
		(6.57)	(6.22)	(6.13)
Unemployed		19.15**	13.63	12.45
		(8.81)	(8.31)	(8.19)
Student		19.95	12.68	12.31
		(12.28)	(11.58)	(11.43)
Retired		18.17	13.00	13.18
		(11.21)	(10.60)	(10.42)
Other employment		2.74	10.86	14.16

	(1)	(2)	(3)	(4)
		(12.04)	(11.38)	(11.19)
Income_p		-17.37	-0.76	3.04
		(12.90)	(12.57)	(12.41)
Efficiency from MDG			-3.03***	-3.28***
			(0.85)	(0.84)
Risk_p			12.87	13.33
			(8.91)	(8.78)
Trust_p			11.74	11.41
			(9.45)	(9.30)
Political Right_p			-17.31*	-16.53*
			(9.23)	(9.09)
Meritocracy_p			-17.99**	-21.12**
			(8.52)	(8.41)
$\text{Inequality Too Large}_p$			29.21***	27.06***
			(10.26)	(10.11)
$\text{Government Responsibility}_p$			22.14**	26.00***
			(8.95)	(8.86)
MLAMS_p			-13.51	-11.54
			(14.10)	(13.90)
Low Personal Cost				29.55***
				(6.04)
Observations	785	785	785	785

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. The baseline is a person in *ForecastNo* who is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Figure S.16: Tax across inequality levels



Note: the figure shows average preferred tax levels in *ForecastNo* for each of the inequality information treatments in Study 3, separated by middle-income and rich subjects. The dash line is the average preferred tax rate in *RealizedNo*.

Table S.30: Descriptive statistics for poor subjects

	N	Tax	DG	Efficiency
ForecastNo	151	69.47	39.23	3.83
RealizedNo	158	67.90	37.37	3.59
Total	309	68.67	38.28	3.71

Note: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. Efficiency corresponds to subjects' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency).

Table S.31: Class and preferred tax, Study 3

	ForecastNo	RealizedNo	Total
Middle Class	-26.86***	-30.67***	-28.76***
	(7.43)	(6.48)	(4.91)
Rich	-45.32***	-47.95***	-46.62***
	(7.95)	(7.05)	(5.30)
Dictator Giving	0.31**	0.45***	0.38***
	(0.14)	(0.13)	(0.10)
N	539	555	1094

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. The baseline is a subject randomised into the poor income class. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.32: Efficiency and tax preferences, Study 3

	(1)	(2)	(3)	(4)
RealizedNo	-8.69*	-8.38*	-9.27**	-10.95***
	(4.55)	(4.46)	(4.49)	(4.19)
Efficiency from MDG	-5.20***	-4.09***	-4.88***	-3.03***
	(0.88)	(0.88)	(0.88)	(0.85)
Dictator Giving		0.66***		0.64***
		(0.11)		(0.11)
Age			0.04	0.22
			(0.21)	(0.20)
Male			-10.74**	-5.70
			(4.65)	(4.45)
Black or African American			-8.77	-12.12
			(8.47)	(7.90)
Hispanic or Latino			-15.73	-13.87
			(10.98)	(10.30)
Asian American			7.32	3.94
			(9.50)	(8.91)
Other ethnicity			23.39	24.60*
			(15.29)	(14.40)
High school degree or equivalent (e.g. GED)			-52.81*	-46.19
			(31.74)	(29.17)
Some college, no degree			-46.80	-41.08
			(31.40)	(28.86)

	(1)	(2)	(3)	(4)
Associate degree (e.g. AA, AS)			-58.57*	-45.86
			(31.85)	(29.29)
Bachelor's degree (e.g. BA, BS)			-56.56*	-50.74*
			(31.39)	(28.90)
Master's degree (e.g. MA, MS, MEd)			-57.31*	-54.36*
			(31.77)	(29.28)
Doctorate or pro degree (e.g. MD, DDS, PhD)			-49.67	-39.58
			(32.98)	(30.48)
Self-employed			-3.26	-2.83
			(6.64)	(6.22)
Unemployed			21.52**	13.63
			(8.87)	(8.31)
Student			16.47	12.68
			(12.29)	(11.58)
Retired			17.23	13.00
			(11.33)	(10.60)
Other employment			10.45	10.86
			(12.18)	(11.38)
Income_p			-6.01	-0.76
			(12.96)	(12.57)
Risk_p				12.87
				(8.91)
Trust_p				11.74
				(9.45)
Political Right_p				-17.31*
				(9.23)
Meritocracy_p				-17.99**
				(8.52)
$\text{Inequality Too Large}_p$				29.21***
				(10.26)
$\text{Government Responsibility}_p$				22.14**
				(8.95)
MLAMS_p				-13.51
				(14.10)
Observations	785	785	785	785

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The baseline is a person who is randomised into the *ForecastNo* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they

are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.33: Image concerns and tax preferences, Study 3

	(1)	(2)	(3)	(4)
RealizedNo	-8.67*	-8.32*	-8.89**	-10.95***
	(4.66)	(4.53)	(4.46)	(4.19)
MLAMS_p	-2.70	-2.40	-8.02	-13.51
	(15.09)	(14.69)	(14.86)	(14.10)
Dictator Giving		0.77***	0.74***	0.64***
		(0.11)	(0.11)	(0.11)
Age			-0.11	0.22
			(0.21)	(0.20)
Male			-9.70**	-5.70
			(4.66)	(4.45)
Black or African American			-11.63	-12.12
			(8.40)	(7.90)
Hispanic or Latino			-14.98	-13.87
			(10.93)	(10.30)
Asian American			8.47	3.94
			(9.50)	(8.91)
Other ethnicity			28.94*	24.60*
			(15.29)	(14.40)
High school degree or equivalent (e.g. GED)			-47.06	-46.19
			(31.46)	(29.17)
Some college, no degree			-40.63	-41.08
			(31.13)	(28.86)
Associate degree (e.g. AA, AS)			-49.71	-45.86
			(31.59)	(29.29)
Bachelor's degree (e.g. BA, BS)			-46.10	-50.74*
			(31.15)	(28.90)
Master's degree (e.g. MA, MS, MEd)			-50.46	-54.36*
			(31.54)	(29.28)
Doctorate or pro degree (e.g. MD, DDS, PhD)			-36.09	-39.58
			(32.82)	(30.48)
Self-employed			-1.46	-2.83
			(6.58)	(6.22)
Unemployed			19.35**	13.63
			(8.82)	(8.31)

	(1)	(2)	(3)	(4)
Student			20.34*	12.68
			(12.31)	(11.58)
Retired			18.06	13.00
			(11.21)	(10.60)
Other employment			2.48	10.86
			(12.04)	(11.38)
Income$_p$			-17.62	-0.76
			(12.91)	(12.57)
Efficiency from MDG				-3.03***
				(0.85)
Risk$_p$				12.87
				(8.91)
Trust$_p$				11.74
				(9.45)
Political Right$_p$				-17.31*
				(9.23)
Meritocracy$_p$				-17.99**
				(8.52)
Inequality Too Large$_p$				29.21***
				(10.26)
Government Responsibility$_p$				22.14**
				(8.95)
Observations	785	785	785	785

Note: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The baseline is a person who is randomised into the *ForecastNo* treatment, is White or Caucasian American, has less than high school diploma, and is employed. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

S.9 Additional Tables and Figures, Discussion

Table S.36: Effect of making a forecast

	(1)	(2)	(3)
Realized	-9.26**	-8.34**	-6.45*
	(4.09)	(4.07)	(3.86)
Dictator Giving	0.77***	0.77***	0.68***

	(1)	(2)	(3)
	(0.10)	(0.10)	(0.10)
Age		-0.30	0.02
		(0.20)	(0.19)
Male		-5.72	-2.09
		(4.14)	(4.00)
Black or African American		-8.34	-8.68
		(7.75)	(7.32)
Hispanic or Latino		-5.23	-6.29
		(10.35)	(9.88)
Asian American		4.56	0.16
		(7.98)	(7.55)
Other ethnicity		39.34**	38.85**
		(16.25)	(15.51)
High school degree or equivalent (e.g. GED)		-48.38*	-43.92*
		(26.34)	(24.84)
Some college, no degree		-36.06	-35.26
		(25.88)	(24.37)
Associate degree (e.g. AA, AS)		-39.42	-36.92
		(26.44)	(24.94)
Bachelor's degree (e.g. BA, BS)		-40.27	-41.27*
		(25.90)	(24.38)
Master's degree (e.g. MA, MS, MEd)		-39.19	-41.31*
		(26.28)	(24.75)
Doctorate or pro degree (e.g. MD, DDS, PhD)		-28.81	-38.62
		(27.73)	(26.14)
Self-employed		4.87	2.73
		(5.98)	(5.70)
Unemployed		6.08	2.36
		(7.51)	(7.16)
Student		15.87	7.51
		(11.15)	(10.66)
Retired		9.97	3.76
		(10.08)	(9.63)
Other employment		11.80	18.04
		(11.98)	(11.41)
Income$_p$		-22.02*	-1.58
		(11.75)	(11.56)
Efficiency from MDG			-2.16***
			(0.77)

	(1)	(2)	(3)
Risk_p			14.17*
			(8.14)
Trust_p			4.19
			(8.51)
Political Right_p			-21.29**
			(8.52)
Meritocracy_p			-17.52**
			(7.83)
$\text{Inequality Too Large}_p$			13.93
			(9.41)
$\text{Government Responsibility}_p$			19.25**
			(8.14)
MLAMS_p			-1.97
			(13.07)
Observations	756	756	756

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. The baseline is a person in *RealizedNo* who is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.34: Effect of perceived absolute inequality on tax preferences

	Forecast	Ratio	ForecastR	ForecastNo	All
SD	0.423	0.444	0.608	0.338	0.100
SD^2	0.269	0.554	0.280	0.475	0.512
1/SD	0.176	0.653	0.217	0.186	0.456
log(SD)	0.258	0.152	0.121	0.186	0.101
AbsGini	0.855	0.444	0.802	0.337	0.105
$AbsGini^2$	0.829	0.554	0.891	0.475	0.654
1/AbsGini	0.122	0.653	0.028	0.183	0.579
log(AbsGini)	0.271	0.152	0.135	0.184	0.091
Rich-Poor	0.469	0.444	0.692	0.338	0.094
$(Rich\text{-}Poor)^2$	0.301	0.554	0.323	0.475	0.512
1/(Rich-Poor)	0.182	0.653	0.227	0.186	0.455
log(Rich-Poor)	0.260	0.152	0.117	0.186	0.104

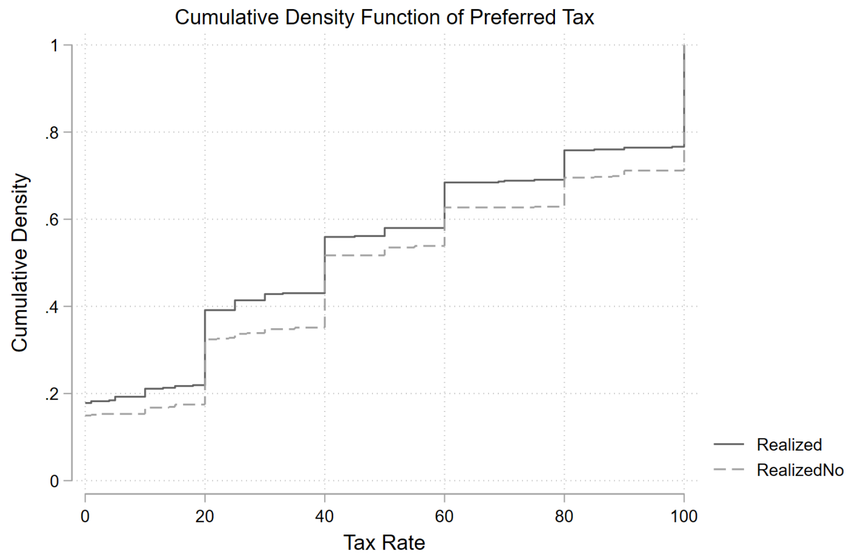
Note: p-values from tobit regressions with preferred tax rate as the dependent variable. All regressions control for dictator givings and a perceived gains dummy equal to one if the subject mistakenly believes he will gain from taxation.

Table S.35: Effect of perceived relative inequality on tax preferences

	Forecast	ForecastR	ForecastNo	All
CV	0.780	0.525	0.271	0.375
CV^2	0.941	0.894	0.271	0.188
1/CV	0.451	0.497	0.272	0.641
log(CV)	0.581	0.270	0.272	0.909
Gini	0.770	0.350	0.277	0.486
$Gini^2$	0.928	0.721	0.277	0.247
1/Gini	0.468	0.502	0.278	0.620
log(Gini)	0.592	0.237	0.278	0.981
Rich/Poor	0.907	0.173	0.950	0.352
$(Rich/Poor)^2$	0.926	0.327	0.958	0.298
1/(Rich/Poor)	0.539	0.077	0.934	0.614
log(Rich/Poor)	0.882	0.173	0.942	0.579

Note: p-values from tobit regressions with preferred tax rate as the dependent variable. All regressions control for dictator givings and a perceived gains dummy equal to one if the subject mistakenly believes he will gain from taxation.

Figure S.17: Preferred tax rate by treatment, Study 3



Note: the figure presents the cumulative density function (or empirical distribution function) of the subjects' tax decisions by treatment.

Table S.37: EGB and misperceptions of inequality

	(1)	(2)	(3)
EGB	-0.0620***	-0.0556***	-0.0524***
	(0.0111)	(0.0113)	(0.0110)
Age		-0.0009**	-0.0004
		(0.0003)	(0.0004)
Male		0.0279***	0.0287***
		(0.0074)	(0.0075)
Black or African American		-0.0461***	-0.0495***
		(0.0143)	(0.0138)
Hispanic or Latino		-0.0441**	-0.0464**
		(0.0209)	(0.0200)
Asian American		-0.0271*	-0.0228
		(0.0143)	(0.0140)
Other ethnicity		0.0177	0.0181
		(0.0257)	(0.0254)
High school degree or equivalent (e.g. GED)		0.0202	0.0336
		(0.0760)	(0.0730)
Some college, no degree		0.0377	0.0428

	(1)	(2)	(3)
		(0.0755)	(0.0725)
Associate degree (e.g. AA, AS)		0.0397	0.0439
		(0.0760)	(0.0730)
Bachelor's degree (e.g. BA, BS)		0.0532	0.0578
		(0.0753)	(0.0724)
Master's degree (e.g. MA, MS, MEd)		0.0685	0.0650
		(0.0759)	(0.0730)
Doctorate or pro degree (e.g. MD, DDS, PhD)		0.0465	0.0469
		(0.0769)	(0.0742)
Self-employed		0.0188*	0.0146
		(0.0110)	(0.0108)
Unemployed		-0.0119	-0.0115
		(0.0124)	(0.0123)
Student		0.0310*	0.0296*
		(0.0184)	(0.0179)
Retired		-0.0331	-0.0306
		(0.0214)	(0.0211)
Other employment		-0.0407	-0.0328
		(0.0268)	(0.0255)
Income_p		-0.0723***	-0.0394*
		(0.0213)	(0.0219)
Dictator Giving			-0.0002
			(0.0002)
Efficiency from MDG			-0.0003
			(0.0014)
Risk_p			0.0292*
			(0.0150)
Trust_p			-0.0377**
			(0.0152)
Political Right_p			-0.0879***
			(0.0174)
Meritocracy_p			-0.0399**
			(0.0158)
Inequality Too Large_p			0.0937***
			(0.0179)
Government Responsibility_p			-0.0615***
			(0.0161)
MLAMS_p			-0.0341
			(0.0233)

	(1)	(2)	(3)
Constant	-0.1191***	-0.1105	-0.1001
	(0.0059)	(0.0767)	(0.0778)
Observations	1589	1589	1589

Note: OLS regressions with Gini error (Gini-Belief - Actual Gini) as dependent variable. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.38: EGB and misperceptions of inequality, restricted sample

	(1)	(2)	(3)
EGB	-0.0493***	-0.0435***	-0.0418***
	(0.0129)	(0.0132)	(0.0127)
Age		-0.0009**	-0.0005
		(0.0004)	(0.0004)
Male		0.0310***	0.0330***
		(0.0080)	(0.0080)
Black or African American		-0.0545***	-0.0566***
		(0.0151)	(0.0145)
Hispanic or Latino		-0.0404*	-0.0416**
		(0.0218)	(0.0205)
Asian American		-0.0243	-0.0196
		(0.0161)	(0.0157)
Other ethnicity		0.0177	0.0163
		(0.0267)	(0.0268)
High school degree or equivalent (e.g. GED)		-0.0221	-0.0061
		(0.0791)	(0.0758)
Some college, no degree		0.0008	0.0094
		(0.0786)	(0.0754)
Associate degree (e.g. AA, AS)		0.0023	0.0098
		(0.0792)	(0.0759)
Bachelor's degree (e.g. BA, BS)		0.0147	0.0219
		(0.0785)	(0.0753)
Master's degree (e.g. MA, MS, MEd)		0.0388	0.0382
		(0.0791)	(0.0760)
Doctorate or pro degree (e.g. MD, DDS, PhD)		0.0164	0.0173
		(0.0803)	(0.0771)
Self-employed		0.0196*	0.0166

	(1)	(2)	(3)
		(0.0118)	(0.0116)
Unemployed		-0.0136	-0.0131
		(0.0130)	(0.0129)
Student		0.0318	0.0280
		(0.0196)	(0.0189)
Retired		-0.0256	-0.0219
		(0.0224)	(0.0219)
Other employment		-0.0522*	-0.0434
		(0.0285)	(0.0268)
Income$_p$		-0.0854***	-0.0452*
		(0.0231)	(0.0236)
Dictator Giving			-0.0002
			(0.0002)
Efficiency from MDG			-0.0007
			(0.0015)
Risk$_p$			0.0262
			(0.0161)
Trust$_p$			-0.0365**
			(0.0163)
Political Right$_p$			-0.0860***
			(0.0185)
Meritocracy$_p$			-0.0460***
			(0.0170)
Inequality Too Large$_p$			0.1034***
			(0.0189)
Government Responsibility$_p$			-0.0604***
			(0.0172)
MLAMS$_p$			-0.0394
			(0.0244)
Constant	-0.1277***	-0.0763	-0.0730
	(0.0074)	(0.0802)	(0.0812)
Observations	1418	1418	1418

Note: OLS regressions with Gini error (Gini-Belief - Actual Gini) as dependent variable. The sample is restricted to those subjects who do not calculate the correct inequality forecasts. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. The baseline is a person who is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

	Forecast	Ratio	Realized	ForecastR	RealizedR	ForecastNo
Ratio	0.554	.	.	.	.	.
Realized	0.889	0.634	.	.	.	.
ForecastR	0.495	1.000	0.581	.	.	.
RealizedR	0.497	0.987	0.581	0.985	.	.
ForecastNo	0.144	0.445	0.176	0.386	0.409	.
RealizedNo	0.201	0.580	0.245	0.523	0.549	0.790

Table S.39: Differences in effect of efficiency concerns across treatments, Wald chi-square tests

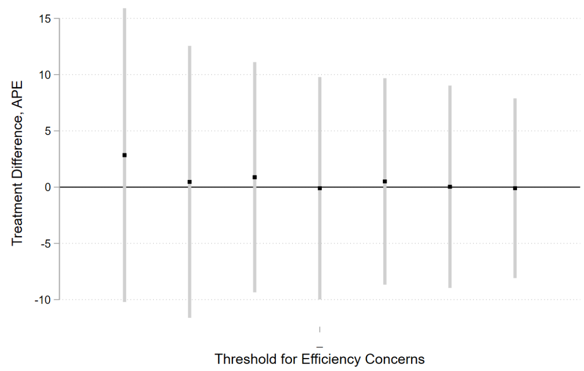
Table S.40: Tax preferences across growth paradigms

	(1)	(2)	(3)
RealizedR	-0.10	-0.01	0.91
	(4.07)	(4.04)	(3.90)
Dictator Giving	0.76***	0.80***	0.70***
	(0.10)	(0.10)	(0.10)
Age		-0.23	-0.04
		(0.20)	(0.19)
Male		-0.58	0.54
		(4.16)	(4.06)
Black or African American		-10.88	-13.79**
		(7.14)	(6.98)
Hispanic or Latino		2.65	-1.88
		(12.03)	(11.60)
Asian American		-9.38	-9.81
		(7.81)	(7.55)
Other ethnicity		39.49***	39.96***
		(15.25)	(14.90)
High school degree or equivalent (e.g. GED)		-91.10**	-81.67**
		(40.80)	(39.69)
Some college, no degree		-85.65**	-79.72**
		(40.62)	(39.50)
Associate degree (e.g. AA, AS)		-82.05**	-74.98*
		(40.94)	(39.82)
Bachelor's degree (e.g. BA, BS)		-78.68*	-73.52*
		(40.59)	(39.46)
Master's degree (e.g. MA, MS, MEd)		-78.26*	-74.62*
		(40.91)	(39.73)

	(1)	(2)	(3)
Doctorate or pro degree (e.g. MD, DDS, PhD)		-62.62	-67.50*
		(41.70)	(40.51)
Self-employed		7.64	7.14
		(6.02)	(5.85)
Unemployed		11.98*	13.24*
		(7.15)	(6.96)
Student		15.05	8.45
		(11.84)	(11.48)
Retired		-5.08	-7.50
		(10.02)	(9.72)
Other employment		7.41	8.21
		(12.86)	(12.60)
Income$_p$		7.69	18.68
		(12.01)	(12.04)
Efficiency from MDG			-2.06***
			(0.77)
Risk$_p$			7.67
			(8.28)
Trust$_p$			6.21
			(8.91)
Political Right$_p$			-17.68*
			(9.17)
Meritocracy$_p$			-11.93
			(8.54)
Inequality Too Large$_p$			12.17
			(9.71)
Government Responsibility$_p$			12.86
			(8.21)
MLAMS$_p$			-6.15
			(13.31)
Observations	712	712	712

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. The baseline is a person in *Realized* who is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Figure S.18: Comparing Realized and RealizedR for different efficiency concerns



Note: the figure shows the average partial effect from a tobit regression that compares *Realized* and *RealizedR*, controlling for dictator giving. Each estimate is taken from a different sample split: Efficiency $\leq i$ for $i \in \{1, ..., 7\}$.

Table S.41: Inequality perceptions and tax inconsistency

	(1)	(2)	(3)	
Estimated US Wealth Gini	24.68**	23.14**	24.48**	24.56**
	(10.70)	(10.60)	(10.50)	(10.44)
Ratio		-4.61	-2.10	-0.18
		(6.06)	(6.20)	(6.06)
Realized		-10.41*	-1.02	-0.17
		(5.87)	(6.04)	(5.89)
ForecastR		-16.48***	-15.64***	-12.65**
		(5.89)	(5.85)	(5.73)
RealizedR		-7.17	2.23	3.63
		(5.95)	(6.12)	(5.98)
ForecastNo		8.74	9.80	10.96*
		(5.83)	(6.05)	(5.91)
RealizedNo		3.85	13.17**	12.68**
		(5.71)	(5.90)	(5.74)
Perceived Gains			37.06***	37.84***
			(8.19)	(7.99)
Low Personal Cost			18.48***	18.00***
			(4.78)	(4.67)
Age				-0.01
				(0.14)
Male				-10.17***

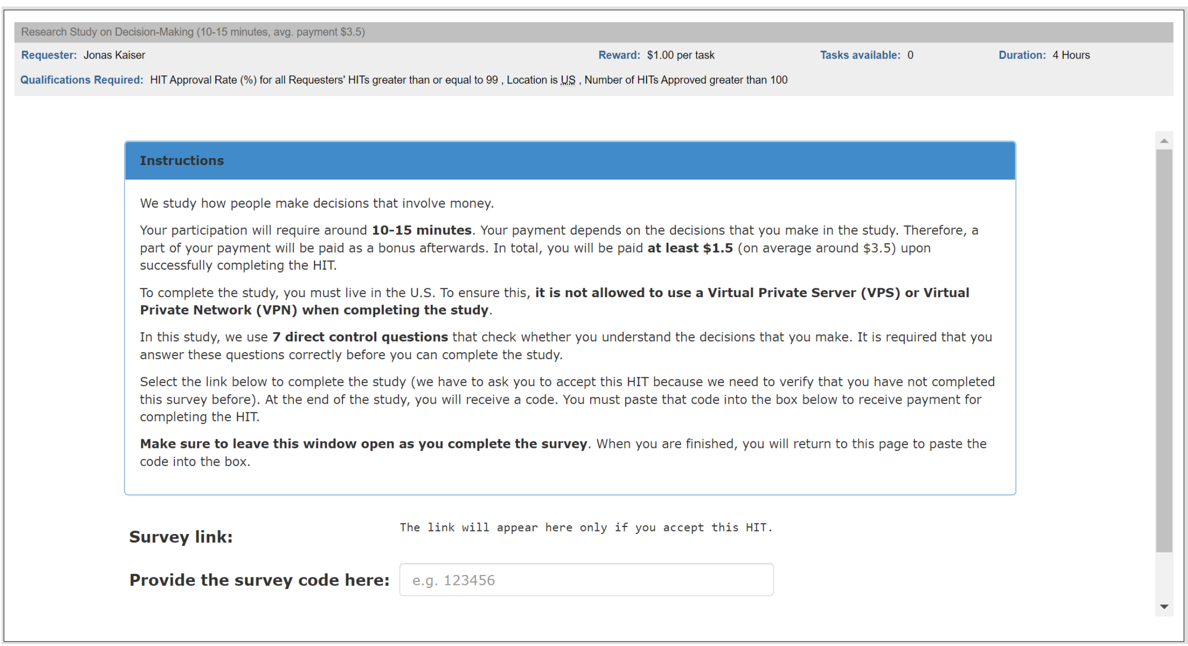
	(1)	(2)	(3)	
				(3.15)
Black or African American				-7.59
				(5.80)
Hispanic or Latino				2.70
				(7.70)
Asian American				-7.78
				(6.67)
Other ethnicity				17.07
				(11.01)
High school degree or equivalent (e.g. GED)				-47.27**
				(22.23)
Some college, no degree				-41.12*
				(21.98)
Associate degree (e.g. AA, AS)				-55.22**
				(22.36)
Bachelor's degree (e.g. BA, BS)				-41.06*
				(21.97)
Master's degree (e.g. MA, MS, MEd)				-38.30*
				(22.27)
Doctorate or pro degree (e.g. MD, DDS, PhD)				-43.92*
				(22.93)
Self-employed				0.56
				(4.73)
Unemployed				5.97
				(5.41)
Student				0.74
				(8.12)
Retired				-2.79
				(7.89)
Other employment				9.04
				(9.36)
Income_p				-22.24**
				(8.84)
Efficiency from MDG				-4.28***
				(0.59)
MLAMS_p				6.34
				(10.18)
Observations	1630	1630	1630	1630

Note: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Perceived gains is a dummy equal to one if the subject mistakenly believes he will gain from taxation. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). Variables with subscript p signal that they are proportions of the maximum possible score, ranging between zero and one. The baseline is a person in *Forecast* who is White or Caucasian American, has less than high school diploma, and is employed. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

S.10 Instructions

On the next page, I include the instructions for a subject who is randomly assigned to the *Forecast* treatment and the middle income class. Afterwards, I include the instructions that vary across the different treatments, using again the example of a subject randomised into the middle class for easy comparison. Instructions for the poor and rich are analogous. There are a few things to note:

- On the first page (MTurk HIT post), I show the information that subjects see before deciding whether to accept the HIT or not. That is, this page is not strictly speaking a part of the experiment, but it is added here for completeness.
- On the second page (consent form), there is a hidden question. This simply asks "Do you see this question" with responses "Yes", "No", and "Don't know". The question is hidden from the subjects using JavaScript, and only bots will be able to answer the question. In prior tests, all bots end up in this honeypot.
- On the third page (dictator game), there is a dropdown menu for the control questions. These present amounts from 0 to 100 cents in 10 cents increments. Both answers must be correct for the subject to continue to the next page. If the answers are incorrect, subjects receive a prompt that this is the case, and they are asked to submit new answers to the control questions.
- On the fifth page (demographics survey), there is a hidden question similar to the one on the second page.
- For the voting decision, the numbers in the table change according to whatever the subject estimated in the forecast task. Subjects must answer the three control questions correctly before they can continue to the next page. If the answers are incorrect, subjects again receive a prompt that this is the case, and they are asked to submit new answers to the control questions.
- When filling in both their beliefs and preferences for the distribution of wealth in the US, the total updates continually. Subjects are not allowed to continue before the total equals 100.
- The 10 items of the Martin-Larsen Approval Motivation Scale appear on the same page. Here, it is split into two as there are too many items for it to be contained on one A4-page.



Research Study on Decision-Making (10-15 minutes, avg. payment $3.5)

Requester: Jonas Kaiser **Reward:** $1.00 per task **Tasks available:** 0 **Duration:** 4 Hours

Qualifications Required: HIT Approval Rate (%) for all Requesters' HITs greater than or equal to 99 , Location is US , Number of HITs Approved greater than 100

Instructions

We study how people make decisions that involve money.

Your participation will require around **10-15 minutes**. Your payment depends on the decisions that you make in the study. Therefore, a part of your payment will be paid as a bonus afterwards. In total, you will be paid **at least $1.5** (on average around $3.5) upon successfully completing the HIT.

To complete the study, you must live in the U.S. To ensure this, **it is not allowed to use a Virtual Private Server (VPS) or Virtual Private Network (VPN) when completing the study**.

In this study, we use **7 direct control questions** that check whether you understand the decisions that you make. It is required that you answer these questions correctly before you can complete the study.

Select the link below to complete the study (we have to ask you to accept this HIT because we need to verify that you have not completed this survey before). At the end of the study, you will receive a code. You must paste that code into the box below to receive payment for completing the HIT.

Make sure to leave this window open as you complete the survey. When you are finished, you will return to this page to paste the code into the box.

Survey link: The link will appear here only if you accept this HIT.

Provide the survey code here: e.g. 123456

Consent Form

You are being invited to take part in the research study 'Study on Decision-Making'. I would like to ask you for your consent to participate in the study and for me to treat your data in agreement with data protection legislation. Before you decide to participate in this study, it is important that you understand why the research is being done and what it will involve. Please take the time to read the following information carefully. Please ask the researcher if there is anything that is not clear or if you need more information. You may print this consent form for your records.

The **purpose** of this study is to learn about how people make decisions that involve money. **Your task** will be to make decisions that involve money (paid as a bonus afterwards) and to answer two surveys, for example, related to your background (e.g., gender, age, and ethnicity) and political attitudes.

Your participation should take about **10-15 minutes**, and you must complete the HIT in one sitting. If you complete the study, you will receive the following **compensation**:

1. A fixed payment of $1.
2. A payment of up to $.15 depending on the accuracy of your responses to some questions.
3. A payment of up to $4.7 (average around $2.4) depending on the decisions that you and other respondents make.

This study is funded by Aarhus University, and you will be paid via Amazon's payment system. Compensation from (2) and (3) will be paid as bonuses. Please note that this study contains several questions that directly ask about your understanding of the decisions that you make. In accordance with the policies set by Amazon Mechanical Turk, you must answer these control questions correctly to complete the study and have your work accepted.

By participating, you will contribute to research and be paid as stated above. There are no risks for participating in this study beyond those encountered in normal everyday life. Please understand that your **participation is voluntary**, and you have the right to withdraw your consent or discontinue participation at any time without penalty. To stop, simply close your browser window.

Your responses will be **confidential**. Your Amazon Worker ID number and your IP address will be kept confidential and will be deleted 6 months after the payment process is completed. Normal personal information such as your gender, age, and ethnicity is collected for scientific analysis. The anonymized data may be published together with the results from this study to comply with open science standards.

If you have questions about this research study or your participation, please contact the principal investigator Jonas Pilgaard Kaiser from Aarhus University, Denmark, by email at **jkaiser@econ.au.dk**.

Thank you very much for your participation!

By clicking the button below, you acknowledge:

- Your participation in the study is voluntary, and you may withdraw your consent and discontinue participation at any time without penalty.
- You do not waive any legal rights or release Aarhus University or its agents from liability for negligence.
- You give consent to treating your personal data and to participate as a subject in the study as described above.

○ I consent, begin the study
○ I do not consent, I do not wish to participate

For your first task, you will be matched with another participant at random. One of you will be the **divider**, and the other will be the **receiver**. The divider gets a **starting amount of $1**. The divider decides how to divide the dollar between him-/herself and the receiver. Note that the amounts will be paid to you and the other participant, respectively, as a bonus after you have completed the HIT.

In the following, you must imagine that **you are the divider**.* Before you continue to your decision, I would like to test your understanding of the task.

Imagine that you give **20 cents to the receiver**. What are **your** earnings?

Imagine that you give **70 cents to the receiver**. What are **your** earnings?

Now for **your decision**. State any amount between 0 and 100 cents that you wish to give to the receiver. (Write only a number between 0 and 100)

*The actual roles will be drawn at random after your response. Thus, there is a 50% chance that you will indeed be the divider and that your decision will determine your payment from this task.

For your second task, you will be matched with two other participants at random. One of you will be the **divider**, and the other two participants will be **receivers.** Let's call the two receivers **Person A** and **Person B**. The divider decides how to divide **90 cents** between Person A and Person B. But **50 percent of the money that is given to Person B is lost**. The divider does not earn money from this task.

In the following, you must imagine that **you are the divider**.* Before you continue to your decision, I would like to test your understanding of the task. **Please indicate whether the following statements are true or false**.

"The more you allocate to Person B, the more money is lost."

"Your decision as a divider affects your own earnings from this task."

Now for **your decision**. Below, you see different allocations to Person A and Person B (after some money is lost). What allocation do you prefer?

A: 30 cents	A: 40 cents	A: 50 cents	A: 60 cents	A: 70 cents	A: 80 cents	A: 90 cents
B: 30 cents	B: 25 cents	B: 20 cents	B: 15 cents	B: 10 cents	B: 5 cents	B: 0 cents
Total: 60 cents	Total: 65 cents	Total: 70 cents	Total: 75 cents	Total: 80 cents	Total: 85 cents	Total: 90 cents

*The actual roles will be drawn at random after your response. Thus, there is a 1/3 chance that you will indeed be the divider and that your decision will determine the payment of the two receivers from this task. The amounts will be paid to the receivers as a bonus after you have completed the HIT.

What is your **age** (in years)?

What is your **gender**?

- Male
- Female

What best describes your **ethnicity**?

- White or Caucasian
- Black or African American
- Hispanic or Latino
- Asian American
- Other

What is the highest **degree** or level of school you have completed?

- Less than a high school diploma
- High school degree or equivalent (e.g. GED)
- Some college, no degree
- Associate degree (e.g. AA, AS)
- Bachelor's degree (e.g. BA, BS)
- Master's degree (e.g. MA, MS, MEd)
- Doctorate or professional degree (e.g. MD, DDS, PhD)

What best describes your current **employment status**?

- Employed (part or full time)
- Self-employed
- Unemployed
- Student
- Retired
- Other

For the next task, you are randomly matched with other respondents in a **group of seven**. Everyone in your group will receive some earnings. These earnings will grow over **30 periods** at an **interest rate of 25 percent per period**. In your group, earnings are as follows:

- **Two** persons are **poor**. They receive **$1** in the first period.
- **Three** persons are **middle class**. They receive **$4** in the first period.
- **Two** persons are **rich**. They receive **$7** in the first period.

First, I want to know how much you think each income will grow over the 30 periods. For each income that you guess correctly (with a 10 percent margin of error), you receive a bonus of 5 cents.

How much do you think a person from each income class will earn in period 30? (Write the dollar amount as a number)

A **poor** person

A **middle class** person

A **rich** person

In your group of seven participants, the computer has randomly assigned you to the **middle** income class. In period 30, there will be a **tax scheme** that redistributes earnings in your group. Your task is to decide what you think is the best tax rate. **Your earnings after the tax are added to your bonus for completing this HIT** (with an exchange rate of 2000:1).

In this tax scheme, all group members pay a fraction of their earnings into a common pot. Two percent of that pot is lost. That is, the total tax revenue is 98 percent of all the money paid into the pot. The total tax revenue is then paid out equally to everyone in your group. So, **the tax makes the earnings in your group more equal, but it lowers the total earnings of your group**.

On the page before, you guessed that the poor would earn $700, that the middle class (you) would earn $2000, and that the rich would earn $3150 in period 30 before taxes. Based on these amounts, the following table shows **how much a person from each group would earn after taxes** for different tax rates.

Note: To keep the table small, it shows only a few examples of tax rates. You are free to choose any tax rate between 0 and 100, including tax rates not listed in the table.

Tax Rate	**0%**	**20%**	**40%**	**60%**	**80%**	**100%**
Poor	$700	$944	$1187	$1431	$1674	$1918
YOU	$2000	$1984	$1967	$1951	$1934	$1918
Rich	$3150	$2904	$2657	$2411	$2164	$1918
Total	$13700	$13645	$13590	$13536	$13481	$13426

First, I want to test that you understand the information in the table above. Please answer the following questions. (Write the dollar amounts as numbers)

How much will **you** earn if the **tax rate is 60 percent**?

How much will **a poor person** earn if the **tax rate is 20 percent**?

What are the **total earnings in your group** if the **tax rate is 100 percent**?

Now, you must decide how you wish to redistribute earnings in your group.* **What tax rate do you wish to implement in your group?** (State a number between 0 and 100)

*After your response, it will be decided at random who gets to determine the tax rate in your group. Thus, there is a 1/7 chance that you will indeed decide the tax rate and that your decision will determine all payments in the group.

You are almost done! You only need to answer a few survey questions more.

On the last page, you had to decide on a tax rate. To ensure that you understood this task, please answer the following question in 1-2 sentences: How did the tax influence the equality of earnings in your group?

What is your **year of birth**?

How do you see yourself: Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?

Please select a value between 0 and 10, where the value 0 means: 'not at all willing to take risks' and the value 10 means 'very willing to take risks'.

I am **not at all** willing to take risks										I am **very** willing to take risks
0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

In your opinion, to what extent is it generally possible to trust people?

People **cannot** generally be trusted										People **can** generally be trusted
0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

In political matters, people talk of "the left" and "the right". How would you place your views on this scale, generally speaking?

Left										**Right**
0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

How would you place your views on this scale?

In the long run, hard work usually brings a better life										Hard work doesn't generally bring success - it's more a matter of luck and connections
0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

The following two questions deal with the **distribution of wealth** in the United States. Wealth is defined as the total value of everything someone owns (savings, car, house, etc.) minus any debt that he or she owes (loans, mortgages, etc.).

Imagine that you divide all US citizens into **five groups** of equal size, ranging from the wealthiest 20 percent to the poorest 20 percent. Below, please indicate **what percent of wealth you think is owned by each of the five groups**.

For example, if you think each group has the same level of wealth, then you should assign 20 percent to each group. If you think one group holds all of the wealth, then you should assign 100 percent to that group.

Top 20% (Richest)	0	%
2nd 20%	0	%
Middle 20%	0	%
4th 20%	0	%
Bottom 20% (Poorest)	0	%
Total	0	%

The following question asks you how you think wealth should **ideally** be distributed in the United States. Please indicate **what percent of wealth you think should be owned by each of the five groups.**

For example, if you think each group should have the same level of wealth, then you should assign 20 percent to each group. If you think one group should hold all of the wealth, then you should assign 100 percent to that group.

Top 20% (Richest)	0	%
2nd 20%	0	%
Middle 20%	0	%
4th 20%	0	%
Bottom 20% (Poorest)	0	%
Total	0	%

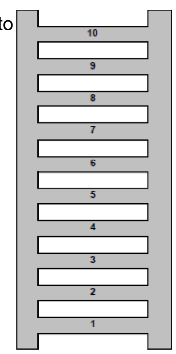
To what extent do you agree or disagree with the following statement: **Differences in income in the United States are too large.**

Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
○	○	○	○	○

To what extent do you agree or disagree with the following statement: **It is the responsibility of the government to reduce the differences in income between people with high incomes and those with low incomes.**

Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
○	○	○	○	○

In our society, there are groups which tend to be towards the top and groups which tend to be towards the bottom. On the right, you see a scale which runs from top to bottom.
Where would you put yourself now on this scale?



- ○ 10 (Top)
- ○ 9
- ○ 8
- ○ 7
- ○ 6
- ○ 5
- ○ 4
- ○ 3
- ○ 2
- ○ 1 (Bottom)

To what extent do you agree or disagree with the following statements?

"I would rather be myself than be well thought of."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly

"I change my opinion (or the way that I do things) in order to please someone else."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly

"In order to get along and be liked, I tend to be what people expect me to be."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly

"I find it difficult to talk about my ideas if they are contrary to group opinion."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly

"I am willing to argue only if I know that my friends will back me up."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly

"I seldom feel the need to make excuses or apologize for my behavior."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly
○	○	○	○	○

"It is not important to me that I behave 'properly' in social situations."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly
○	○	○	○	○

"If there is any criticism or anyone says anything about me, I can take it."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly
○	○	○	○	○

"I am careful at parties and social gatherings for fear that I will do or say things that others won't like."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly
○	○	○	○	○

"I usually do not change my position when people disagree with me."

Disagree Strongly	Disagree	No Opinion	Agree	Agree Strongly
○	○	○	○	○

Here is your MTurk code: 63905

Copy this value to paste into MTurk.

When you have copied it, please click the arrow below to submit your response.

Your response has been recorded.

I greatly thank you for completing this study and thereby improving research!

If you have completed the study satisfactorily, I will soon match you with other workers at random to determine the bonus you receive from completing this HIT. Then, you will receive your payment via Amazon's payment system.

In your group of seven participants, the computer has randomly assigned you to the **middle** income class. In period 30, there will be a **tax scheme** that redistributes earnings in your group. Your task is to decide what you think is the best tax rate. **Your earnings after the tax are added to your bonus for completing this HIT** (with an exchange rate of 2000:1).

In this tax scheme, all group members pay a fraction of their earnings into a common pot. Two percent of that pot is lost. That is, the total tax revenue is 98 percent of all the money paid into the pot. The total tax revenue is then paid out equally to everyone in your group. So, **the tax makes the earnings in your group more equal, but it lowers the total earnings of your group**.

On the page before, you guessed how much the earnings of the poor, middle class (you), and rich would increase over 30 periods before taxes. The following table is based on your estimate of how much the earnings before taxes will grow from period 1 to period 30 on average. The table shows **how much a person from each group would earn after taxes** for different tax rates.

Note: To keep the table small, it shows only a few examples of tax rates. You are free to choose any tax rate between 0 and 100, including tax rates not listed in the table.

Tax Rate	**0%**	**20%**	**40%**	**60%**	**80%**	**100%**
Poor	$550	$871	$1192	$1514	$1835	$2156
YOU	$2200	$2191	$2182	$2174	$2165	$2156
Rich	$3850	$3511	$3172	$2834	$2495	$2156
Total	$15400	$15338	$15277	$15215	$15154	$15092

First, I want to test that you understand the information in the table above. Please answer the following questions. (Write the dollar amounts as numbers)

How much will **you** earn if the **tax rate is 60 percent**?

How much will **a poor person** earn if the **tax rate is 20 percent**?

What are the **total earnings in your group** if the **tax rate is 100 percent**?

Now, you must decide how you wish to redistribute earnings in your group.* **What tax rate do you wish to implement in your group?** (State a number between 0 and 100)

*After your response, it will be decided at random who gets to determine the tax rate in your group. Thus, there is a 1/7 chance that you will indeed decide the tax rate and that your decision will determine all payments in the group.

In your group of seven participants, the computer has randomly assigned you to the **middle** income class. In period 30, there will be a **tax scheme** that redistributes earnings in your group. Your task is to decide what you think is the best tax rate. **Your earnings after the tax are added to your bonus for completing this HIT** (with an exchange rate of 2000:1).

In this tax scheme, all group members pay a fraction of their earnings into a common pot. Two percent of that pot is lost. That is, the total tax revenue is 98 percent of all the money paid into the pot. The total tax revenue is then paid out equally to everyone in your group. So, **the tax makes the earnings in your group more equal, but it lowers the total earnings of your group**.

On the page before, you guessed how much the earnings of the poor, middle class, and rich would increase over 30 periods before taxes. In fact, the poor will earn $808, the middle class (you) will earn $3231, and the rich will earn $5655 in period 30 before taxes. Based on these amounts, the following table shows **how much a person from each group would earn after taxes** for different tax rates.

Note: To keep the table small, it shows only a few examples of tax rates. You are free to choose any tax rate between 0 and 100, including tax rates not listed in the table.

Tax Rate	0%	20%	40%	60%	80%	100%
Poor	$808	$1280	$1751	$2223	$2695	$3167
YOU	$3231	$3218	$3205	$3192	$3180	$3167
Rich	$5655	$5157	$4660	$4162	$3664	$3167
Total	$22619	$22528	$22437	$22346	$22258	$22169

First, I want to test that you understand the information in the table above. Please answer the following questions. (Write the dollar amounts as numbers)

How much will **you** earn if the **tax rate is 60 percent**?

How much will **a poor person** earn if the **tax rate is 20 percent**?

What are the **total earnings in your group** if the **tax rate is 100 percent**?

Now, you must decide how you wish to redistribute earnings in your group.* **What tax rate do you wish to implement in your group?** (State a number between 0 and 100)

*After your response, it will be decided at random who gets to determine the tax rate in your group. Thus, there is a 1/7 chance that you will indeed decide the tax rate and that your decision will determine all payments in the group.

For the next task, you are randomly matched with other respondents in a **group of seven**. Everyone in your group will receive some earnings. These earnings will grow over **30 periods** at different interest rates. In your group, earnings are as follows:

- **Two** persons are **poor**. They receive **$1** in the first period and get an **interest rate of 24% per period**.
- **Three** persons are **middle class**. They receive **$4** in the first period and get an **interest rate of 26% per period**.
- **Two** persons are **rich**. They receive **$7** in the first period and get an **interest rate of 27% per period**.

First, I want to know how much you think each income will grow over the 30 periods. For each income that you guess correctly (with a 10 percent margin of error), you receive a bonus of 5 cents.

How much do you think a person from each income class will earn in period 30? (Write the dollar amount as a number)

A **poor** person [ ]

A **middle class** person [ ]

A **rich** person [ ]

In your group of seven participants, the computer has randomly assigned you to the **middle** income class. In period 30, there will be a **tax scheme** that redistributes earnings in your group. Your task is to decide what you think is the best tax rate. **Your earnings after the tax are added to your bonus for completing this HIT** (with an exchange rate of 2000:1).

In this tax scheme, all group members pay a fraction of their earnings into a common pot. Ten percent of that pot is lost. That is, the total tax revenue is 90 percent of all the money paid into the pot. The total tax revenue is then paid out equally to everyone in your group. So, **the tax makes the earnings in your group more equal, but it lowers the total earnings of your group**.

On the page before, you guessed that the poor would earn $500, that the middle class (you) would earn $2500, and that the rich would earn $5000 in period 30 before taxes. Based on these amounts, the following table shows **how much a person from each group would earn after taxes** for different tax rates.

Note: To keep the table small, it shows only a few examples of tax rates. You are free to choose any tax rate between 0 and 100, including tax rates not listed in the table.

Tax Rate	**0%**	**20%**	**40%**	**60%**	**80%**	**100%**
Poor	$500	$876	$1251	$1627	$2003	$2379
YOU	$2500	$2476	$2451	$2427	$2403	$2379
Rich	$5000	$4476	$3951	$3427	$2903	$2379
Total	$18500	$18130	$17760	$17390	$17020	$16650

First, I want to test that you understand the information in the table above. Please answer the following questions. (Write the dollar amounts as numbers)

How much will **you** earn if the **tax rate is 60 percent**?

How much will **a poor person** earn if the **tax rate is 20 percent**?

What are the **total earnings in your group** if the **tax rate is 100 percent**?

Now, you must decide how you wish to redistribute earnings in your group.* **What tax rate do you wish to implement in your group?** (State a number between 0 and 100)

*After your response, it will be decided at random who gets to determine the tax rate in your group. Thus, there is a 1/7 chance that you will indeed decide the tax rate and that your decision will determine all payments in the group.

In your group of seven participants, the computer has randomly assigned you to the **middle** income class. In period 30, there will be a **tax scheme** that redistributes earnings in your group. Your task is to decide what you think is the best tax rate. **Your earnings after the tax are added to your bonus for completing this HIT** (with an exchange rate of 2000:1).

In this tax scheme, all group members pay a fraction of their earnings into a common pot. Ten percent of that pot is lost. That is, the total tax revenue is 90 percent of all the money paid into the pot. The total tax revenue is then paid out equally to everyone in your group. So, **the tax makes the earnings in your group more equal, but it lowers the total earnings of your group**.

On the page before, you guessed how much the earnings of the poor, middle class, and rich would increase over 30 periods before taxes. In fact, the poor will earn $635, the middle class (you) will earn $4104, and the rich will earn $9104 in period 30 before taxes. Based on these amounts, the following table shows **how much a person from each group would earn after taxes** for different tax rates.

Note: To keep the table small, it shows only a few examples of tax rates. You are free to choose any tax rate between 0 and 100, including tax rates not listed in the table.

Tax Rate	**0%**	**20%**	**40%**	**60%**	**80%**	**100%**
Poor	$635	$1325	$2016	$2706	$3397	$4087
YOU	$4104	$4101	$4097	$4094	$4091	$4087
Rich	$9104	$8101	$7097	$6094	$5091	$4087
Total	$31790	$31154	$30518	$29883	$29247	$28611

First, I want to test that you understand the information in the table above. Please answer the following questions. (Write the dollar amounts as numbers)

How much will **you** earn if the **tax rate is 60 percent**?

How much will **a poor person** earn if the **tax rate is 20 percent**?

What are the **total earnings in your group** if the **tax rate is 100 percent**?

Now, you must decide how you wish to redistribute earnings in your group.* **What tax rate do you wish to implement in your group?** (State a number between 0 and 100)

*After your response, it will be decided at random who gets to determine the tax rate in your group. Thus, there is a 1/7 chance that you will indeed decide the tax rate and that your decision will determine all payments in the group.

For the next task, you are randomly matched with other respondents in a **group of seven**. Everyone in your group will receive some earnings. These earnings will be **added to the bonus you receive for completing this HIT** (with an exchange rate of 2000:1).

In your group, earnings are as follows:

- **Two** persons are **poor**. They receive **$156**.
- **Three** persons are **middle class**. They receive **$624**.
- **Two** persons are **rich**. They receive **$1092**.

The computer has randomly assigned **you** to the **middle** income class.

You may redistribute the earnings in your group via a **tax scheme**. In this tax scheme, all group members pay a fraction of their earnings into a common pot. Two percent of that pot is lost. That is, the total tax revenue is 98 percent of all the money paid into the pot. The total tax revenue is then paid out equally to everyone in your group. So, **the tax makes the earnings in your group more equal, but it lowers the total earnings of your group**. Your task is to decide what you think is the best tax rate.

The following table shows **how much a person from each income class would earn after taxes** for different tax rates.

Note: To keep the table small, it shows only a few examples of tax rates. You are free to choose any tax rate between 0 and 100, including tax rates not listed in the table.

Tax Rate	0%	20%	40%	60%	80%	100%
Poor	$156	$247	$338	$429	$520	$612
YOU	$624	$622	$619	$617	$614	$612
Rich	$1092	$996	$900	$804	$708	$612
Total	$4368	$3617	$2865	$2114	$1363	$4281

First, I want to test that you understand the information in the table above. Please answer the following questions. (Write the dollar amounts as numbers)

How much will **you** earn if the **tax rate is 60 percent**?

How much will **a poor person** earn if the **tax rate is 20 percent**?

What are the **total earnings in your group** if the **tax rate is 100 percent**?

Now, you must decide how you wish to redistribute earnings in your group.* **What tax rate do you wish to implement in your group?** (State a number between 0 and 100)

*After your response, it will be decided at random who gets to determine the tax rate in your group. Thus, there is a 1/7 chance that you will indeed decide the tax rate and that your decision will determine all payments in the group.

For the next task, you are randomly matched with other respondents in a **group of seven**. Everyone in your group will receive some earnings. These earnings will be **added to the bonus you receive for completing this HIT** (with an exchange rate of 2000:1).

In your group, earnings are as follows:

- **Two** persons are **poor**. They receive **$808**.
- **Three** persons are **middle class**. They receive **$3231**.
- **Two** persons are **rich**. They receive **$5655**.

The computer has randomly assigned **you** to the **middle** income class.

You may redistribute the earnings in your group via a **tax scheme**. In this tax scheme, all group members pay a fraction of their earnings into a common pot. Two percent of that pot is lost. That is, the total tax revenue is 98 percent of all the money paid into the pot. The total tax revenue is then paid out equally to everyone in your group. So, **the tax makes the earnings in your group more equal, but it lowers the total earnings of your group**. Your task is to decide what you think is the best tax rate.

The following table shows **how much a person from each income class would earn after taxes** for different tax rates.

Note: To keep the table small, it shows only a few examples of tax rates. You are free to choose any tax rate between 0 and 100, including tax rates not listed in the table.

Tax Rate	0%	20%	40%	60%	80%	100%
Poor	$808	$1280	$1751	$2223	$2695	$3167
YOU	$3231	$3218	$3205	$3192	$3180	$3167
Rich	$5655	$5157	$4660	$4162	$3664	$3167
Total	$22619	$22528	$22437	$22346	$22258	$22169

First, I want to test that you understand the information in the table above. Please answer the following questions. (Write the dollar amounts as numbers)

How much will **you** earn if the **tax rate is 60 percent**?

How much will **a poor person** earn if the **tax rate is 20 percent**?

What are the **total earnings in your group** if the **tax rate is 100 percent**?

Now, you must decide how you wish to redistribute earnings in your group.* **What tax rate do you wish to implement in your group?** (State a number between 0 and 100)

*After your response, it will be decided at random who gets to determine the tax rate in your group. Thus, there is a 1/7 chance that you will indeed decide the tax rate and that your decision will determine all payments in the group.