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 education. Therefore, citizens' support for these policies may depend on their beliefs about how inequality will evolve over time. In three large-scale experiments, I examine whether individuals are able to predict how exponential economic growth influences inequality, and I examine how beliefs about inequality influence people's preferences for redistribution. I find that most people underestimate how much inequality increases in the presence of growth, but this does not matter for policy support, as beliefs about inequality do not affect preferences for redistribution. Rather, what matters is whether people know if redistribution will come at a personal cost to themselves.

JEL Classification: C91, D31, D63, D64, D72, D91

Keywords: social preferences, inequality, voting, taxation, forecasts, exponential growth bias

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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. 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 have shown that beliefs about one's future income can affect preferences for redistribution (cf. the prospect of upward mobility hypothesis, Hirschman and Rothschild, 1973; Benabou and Ok, 2001; Cojocaru, 2014). Thus, if people have inaccurate 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).

However, it is not clear how (incorrect) beliefs about inequality influence policy support, as the perceived level of inequality affects both the benefits and costs of redistribution. On the one hand, if an individual believes that inequality will increase significantly, they may perceive a greater need for redistribution to counteract this rise in inequality. On the other hand, greater inequality also implies that it is more costly for a net contributor to redistribute, as their income makes up a larger share of the tax base. The total effect of incorrect beliefs will thus depend on whether the misperceived benefits or personal costs of redistribution matter most.

In this paper, I examine people's ability to predict how exponential economic growth influences inequality, 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 does not impact their demand for redistribution. Rather, what matters is whether individuals know if redistribution is costly for themselves.

I use an experimental approach to obtain a causal understanding of how perceived increases

[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). Studies on social preferences have also 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.

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, referred to 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 incomes 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 incomes based on their own forecast (treatment *Forecast*) or the actual incomes (*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 see 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. In Study 1, all incomes grow at the same rate (changed in Study 2), and this leads to an exponential growth in absolute inequality but no change in relative inequality. 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 stylised model of inequality aversion in the presence of growth, taking into account that people may make biased forecasts. The model predicts that individuals 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 it is hypothesised that subjects will 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 but are markedly better at predicting that there is no change in relative inequality. However, beliefs about the level of inequality do not influence subjects' preferred tax rate, in contrast to the predictions of the theoretical model. Instead, incorrect forecasts only affect preferred tax rates if subjects misperceive their personal costs of redistribution in one of two ways: first, net contributors may wrongly believe that they will gain from redistribution. In this case, they vote for a tax rate that is 50 percentage points higher compared to those who realise that they will lose from redistribution. Second, net contributors may wrongly believe that redistribution comes at negligible personal costs. In this case, they vote for a tax rate that is 14 percentage points higher than those who realise that redistribution comes at a considerable cost. Controlling for these two misperceptions, subjects vote for the same tax rates across the three treatments. This indicates that subjects are willing to give up the same *share* of their income regardless of the extent of absolute inequality. Yet, redistribution is more costly in *absolute amounts* when absolute inequality is large. Therefore, the fact that tax rates are unchanged imply that greater absolute inequality is associated with a greater willingness-to-pay for redistribution. This indicates that subjects are more concerned about inequality when absolute inequality is large, but that the greater concerns cancel out with the increased personal costs.

Study 2 examines whether these results are robust to a setting in which inequality grows differently over time. Incomes in Study 2 increase at a larger rate the greater the initial incomes are, and this leads to an increase in both absolute and relative inequality. 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 subjects 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.

One potential concern with Studies 1 and 2 is that subjects may be influenced by both their perceived level of inequality and factors related to the act of making a forecast. To address this concern, Study 3 isolates the causal impact of the level of absolute inequality on individuals' preferences for redistribution. In this experiment, subjects vote on redistribution in a group without making forecasts. Instead, subjects decide on how to redistribute incomes from the *Realized* treatment or from forecasts made in a pilot study for Study 1. Consistent with the results from Study 1, 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 suggests that the results in Studies 1 and 2 are driven by the perceived level of 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, 2021).[2] I abstract from these factors and focus instead on how beliefs about how economic growth influences inequality influence preferences for redistribution.

Second, this paper contributes to the literature on misperceptions of inequality by demonstrating that people may underestimate inequality due to the exponential nature of economic growth. Previous studies show 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). But few studies examine the causes of these misperceptions, and the temporal aspect is largely absent from the literature. Thus, Knell and Stix (2020) explain that people might extrapolate from the individuals they encounter in their daily lives, leading to self-centered reference groups (see also Cruces et al., 2013; Balcells et al., 2015; Londoño-Vélez, 2022). 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 individuals do not fully understand the (exponential) development in inequality over time.

Third, the current paper extends the literature on how informing individuals about inequality 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 policy preferences unless people hold wrong beliefs about whether they gain or lose from redistribution (e.g. Kuziemko et al.,

[2] A somewhat related literature examines cooperativeness in dynamic public goods games where endowment grow across multiple periods. In Gächter et al. (2017), for example, subjects' endowment at the start of any given period equals their endowment from the end of the previous period. In this setting, average inequality increases over time, and contributions increase in absolute amounts but decrease in relative amounts (see also Noussair and Soo, 2008; Cadigan et al., 2011; Rockenbach and Wolff, 2019). In contrast to this literature, the current study examines demand for redistribution rather than cooperativeness, it examines the effect of misperceptions, and it involves no strategic component.

2015; Ballard-Rosa et al., 2017; McCall et al., 2017; Engelhardt and Wagener, 2018; Trump and White, 2018; Ciani et al., 2021; Fehr et al., 2021; Hvidberg et al., 2023). Typical explanations for the null effects on preferences include that citizens (i) believe that policies are ineffective, (ii) distrust the government, or (iii) believe that inequalities are justified, e.g. due to differences in effort. A novel aspect of this paper is to show that even when these explanations are ruled out, the level of inequality does not influence the demand for redistribution. Instead, greater concerns about inequality are offset by greater personal costs of redistribution among the net contributors. 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: Section 2.1 describes 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 income. 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 Supplementary Materials include the experimental instructions, extensions of the theoretical model, a description of pilot studies, a power analysis, an analysis of attrition, 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 that subjects complete in a single session online (see Figure 1 for an overview). First, subjects play a standard and a modified dictator game to measure their inequality aversion and efficiency concerns, respectively. Second, subjects fill in information about their demographics, which serve as control variables in the analysis. In the third part, subjects 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 an income class in the group and vote for redistribution in a tax-transfer scheme. Finally, subjects complete an attitudinal survey to provide additional control variables for the analysis. Instructions are presented in Supplementary Materials S.12.

Throughout the experiment, subjects answer a total of seven control questions to ensure their understanding of the tasks. If subjects answer incorrectly, they are informed about this and are asked to try again. Following Arechar et al. (2018), subjects are not allowed to continue

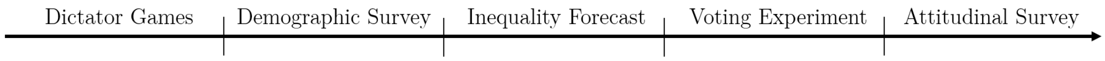


Figure 1: Timeline of the experiment

before they answer the control questions correctly. Additional screeners ensure that subjects provide high-quality data, and I explain these in Appendix A.1.

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 to give to the other person (the recipient).[3] Using the strategy method (Selten, 1967), both subjects decide as a dictator, and a random draw determines whose 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 preferences for efficiency relative to equity.[4] Subjects are divided into groups of three. One person (C) is the dictator and decides how to allocate USD .9 between the two other subjects, Person A and Person B. However, 50 percent of the money given to Person B is lost, creating 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).

To make the task simple for the dictators, they choose between seven different allocations and observe the endowments for Person A and Person B as well as the total endowment. Thus, the dictators decides between the following options (in cents): $(x_A, x_B) = \{(30, 30),$ $(40, 25)$, $(50, 20)$, $(60, 15)$, $(70, 10)$, $(80, 5)$, $(90, 0)\}$. Using the strategy method, all subjects make decisions as the dictator under role uncertainty, and one decision is randomly drawn

[3]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 sample is recruited from 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. See also Snowberg and Yariv (2021) for evidence that greater stake sizes do not influence behaviour in MTurk samples.

[4]With 'efficiency', I refer to the total income and not to e.g. Pareto efficiency.

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 report their age, gender, ethnicity, education, and employment status. This information is used as control variables in Section 2.3. The demographic survey also serves as a filler task to mitigate potential spillover effects from the dictator games to the later tasks that focus on redistribution within a group.

2.1.3 Inequality Forecast

Setting. In 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. Every group member will receive an interest of 25 percent on their income for 30 rounds, but subjects are not informed about the final incomes. The initial (final) income for each income class is \$1 (\$808), \$4 (\$3,231), and \$7 (\$5,655), respectively.[5] Note that the 30 rounds only serve as a frame; there is no waiting or delay involved, and subjects make no decisions between the first and the final round. The high interest rates and many rounds serve to increase the difference between the treatments (described below) if subjects make incorrect forecasts. In this way, possible treatment effects are enhanced, thereby increasing the power of the study (Hansen and Collins, 1994; Meyvis and Van Osselaer, 2018).

Subjective Forecast. After being informed about the setting, subjects are asked to guess the income of a member of each income class after 30 rounds with compounded interest. The task is incentivised, with subjects earning 5 cents for each income class 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 told what their own income will be when making their forecasts. In Section 5.1, I show that forecast bias correlates with misperceptions of inequality in society, also when controlling for demographics and attitudes towards inequality.

[5] For comparison, Gächter et al. (2017) study exponential growth and inequality in a dynamic public goods game. In their experiment, the endowments of the group increase by 50 percent per round in 10 or 15 rounds if all group members contribute all their endowment to the public good.

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 incomes using a tax-transfer scheme. Each group member is paid according to their post-redistribution income in the group (with an exchange rate of 2000:1). When deciding how to redistribute incomes within 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. Redistribution is costly, as 2 percent of the transfers are lost ('leaky bucket', Okun, 1975), thereby creating a trade-off between equity and efficiency. The efficiency loss ensures that it is costly for the middle-income subjects to redistribute even though they do not earn more than the mean income in the group. Yet, the small efficiency loss of 2 percent makes it unlikely that efficiency concerns dominate the subjects' decisions (Beckman et al., 2004; Krawczyk, 2010; Durante et al., 2014; Tepe et al., 2021; see also Supplementary Materials S.7.2).

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 ('random dictator procedure', see e.g. Feddersen et al., 2009; Krawczyk, 2010; Höchtl et al., 2012; Shayo and Harel, 2012; Durante et al., 2014; Jensen and Markussen, 2021).

To ensure that the subjects understand the tax-transfer scheme, a table shows the post-tax incomes of all group members if a tax rate of $\tau \in \{0, 20, 40, 60, 80, 100\}$ were to be implemented (see Figure 2). Depending on the treatment (described below), the post-tax incomes shown in the table are based on either the subjects' estimates from the forecast task or the actual incomes in the final round. The tax is applied to the pre-tax incomes in the final round rather than the initial incomes because it simplifies the redistribution decision for the subjects in two ways: first, subjects see the consequences of the tax directly from the table. If the tax was applied to the incomes before compounded interest, subjects would need to make forecasts for all combinations of tax rates and income groups to understand the post-tax incomes. Second, redistributing incomes 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.

Figure 2: Incomes for different tax rates

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

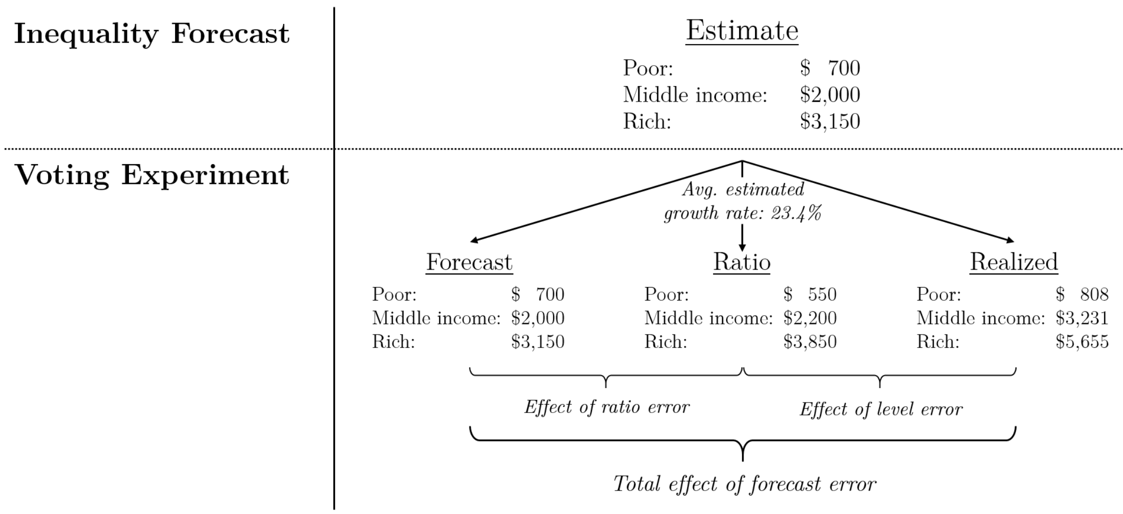
Notes: this example shows the information table that is provided to a subject in *Forecast* who estimated that the poor, middle income, and rich group members would earn $700, $2,000, and $3,150 in round 30, respectively. The complete instructions are provided in Supplementary Materials S.12.

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 how misperceptions from forecasts of inequality causally impacts preferences for redistribution.

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).

In the *Forecast* treatment, subjects see the effect of redistribution based on the level of inequality they estimated in the forecast task. In contrast, subjects in the *Realized* treatment see the post-redistribution incomes based on the actual level of inequality in round 30. Comparing these two treatments yields the causal effect of inaccurate forecasts on preferences for

Figure 3: Illustration of received information in each treatment



Notes: 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 incomes 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.

redistribution. Any differences in preferences between the two treatments may be due to two types of forecast errors: subjects may wrongly estimate how the incomes develop relative to each other, and they may wrongly estimate the 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, which is 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 completing the voting experiment, subjects answer a survey that elicits various 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). Next, subjects place themselves on a left-right political scale and report their beliefs about the importance of merit for obtaining success in life (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). 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 on a 10-point scale from 'top' to 'bottom', using the image of a ladder (e.g. Bobzien, 2020; Knell and Stix, 2020). Finally, subjects answer the 10-item version of the Martin-Larsen Approval Motivation Scale (MLAMS, Martin, 1984), which measures desire for social approval. To allow comparison across measures and scale lengths, responses are standardised as proportions of the maximum possible (POMP) scores, ranging between zero and one (Cohen et al., 1999; Mellenbergh, 2019).[6]

2.1.6 Procedure

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

To ensure high-quality data, 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). The experiment employed a pre-registered strategy with several screeners in addition to comprehension checks (Thomas and Clifford, 2017; Zhang et al., 2022), with details in Appendix A.1. In

[6] 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.

total, the screeners led to the exclusion of 10.7 percent of the responses, and the main sample thus consists of 1,415 subjects.[7] With this sample size, 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 Supplementary Materials S.3). In the main sample, 42 percent were male, 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 S.11 and S.12 in the Supplementary Materials.

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 how inequality is influenced by economic growth and (ii) how (possibly erroneous) forecasts influence preferences for redistribution. I now develop a stylised model to form the hypotheses I test in Section 2.3. The 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 (Stango and Zinman, 2009) and introduce a tax-transfer scheme to examine preferences for redistribution (Meltzer and Richard, 1981).

Modelling Social Preferences. I use a quadratic version of the Fehr and Schmidt (1999) model (henceforth FS-model) as a framework for social preferences.[8] This model implies that

[7] 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 Study 1.

[8] Quadratic difference aversion is similar in spirit to the models used in 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 because 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

the marginal disutility of inequality increases as inequality levels rise, 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 while some individuals prefer to have more than others, many dislike having too much more (Hadad and Malul, 2017). Third, the model captures the idea 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).

Formally, consider n individuals indexed by $i \in \{1, \ldots, n\}$, 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

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

In Supplementary Materials 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 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 incomes specified in present value (PV), I assume that individual i estimates the growth of incomes 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:

$$FV = PV \cdot f(r, T, \theta) \quad (2)$$

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 (EGB) is prevalent regardless of the number of data points people observe (Wagenaar and Timmers, 1978) and how the data are 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). This implies that individuals are unlikely to take the necessary steps

middle-income voter would prefer to transfer money from the rich to the poor.

to alleviate problems caused by EGB. Common theoretical frameworks for EGB assume 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). Thus, I assume that individuals exhibit the same degree of EGB towards all incomes and thus apply the same overall growth, f, to all incomes.

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 EGB. The forecast bias implies that the individual underestimates how much r and T influence the income growth (i.e. $f_\theta < 0, f_{r\theta} < 0$, and $f_{T\theta} < 0$). Supplementary Materials S.2.1 and S.2.2 provide examples using the particular functional forms for f from Stango and Zinman (2009) and Levy and Tasoff (2016).

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

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

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 Supplementary Materials S.2.4, I show that the qualitative predictions hold if one extends the model to include disutility from both absolute and relative inequality. Intuitively, even though relative inequality is constant under uniform growth rates, absolute inequality still increases. Therefore, the individual experiences disutility from increasing inequality as long as the utility function assigns some weight to 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 citizens. I focus on the setting of the experiment where redistribution takes place only in the final period. To reflect the trade-off between equity and efficiency, I assume that the tax entails an efficiency loss. Denoting the tax rate by $\tau \in [0, 1]$, 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.[9] Thus, the post-redistribution income that individual i receives is $(1-\tau)x_i + \lambda\tau\bar{x}$. In line with the experimental

[9]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

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:

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

Supplementary Materials S.2.7 shows 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.

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', as it depends on the individual's 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, and it is therefore optimal for all individuals to vote truthfully. That is, the model captures in a simple way the predictions from strategy-proof social choice functions. Maximising the utility specified in Equation 4 with respect to τ yields the preferred tax rate for individual i:

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

where

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

Here, τ_i^b denotes that the individual is influenced by forecast bias, and ϕ_i reflects the individual's concerns for inequality. For poor individuals ($x_i < \lambda\bar{x}$), a higher tax rate leads to both higher income and greater equality in the group. So, they prefer the highest tax rate of

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). Supplementary Materials S.2.6 shows that the qualitative predictions of the model remain the same with convex distortionary costs of taxation.

$\tau_i^b = 1$ as long as the efficiency of the tax is high (as is the case in the current experiment). Importantly, the assumption that individuals apply the same multiplicative function f to all incomes implies that individuals always know whether they are net contributors or net recipients from taxation. For this reason, middle-income individuals will only be in the corner solution of $\tau_i^b = 1$ if they are extremely averse to disadvantageous inequality ($\alpha \to \infty$) or if they overestimate developments to an extreme extent ($\theta \to \infty$, which may also occur for rich individuals). In contrast, middle-income and rich individuals who are severely biased and/or care very little about inequality ($\phi_i f(r,T,\theta) \leq (x_i - \lambda\bar{x})/2$) will be in the opposite corner solution and prefer the lowest tax rate of $\tau_i^b = 0$.

All other middle-income and rich subjects prefer an intermediate tax rate when trading off their own income and equality. Their 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.

Let τ_i^* denote the optimal tax rate for an individual with a perfect forecast. This tax rate corresponds to $\tau_i^b(x_1, \ldots, x_N; \theta = 0)$. 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.

2.2.3 Hypotheses

The first hypothesis tests whether the subjects' forecasts of inequality align with the assumptions of the model. The model assumes that individuals underestimate exponential growth and therefore underestimate the incomes and absolute inequality in the final round. However, the model assumes that the bias (θ) is the same for each forecast and that future values are obtained by multiplying the initial value with f. Consequently, because the time horizon and real interest rate are the same for all income classes, the model assumes 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. This leads to the first hypothesis:

Hypothesis 1

1. *When interest rates are the same for all individuals, subjects on average underestimate how much absolute inequality increases.*

2. *When interest rates are the same for all individuals, subjects on average correctly estimate that relative inequality does not change.*

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 subjects who are not in a corner solution 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, for which there is interdependence as H2.2 follows from H2.1 and H2.3:

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

I now analyse subjects' inequality forecasts and how the forecasts affect preferences for redistribution in the voting experiment. Throughout, I follow the pre-analysis plan. Table 2 provides descriptive statistics. All reported p-values are from two-sided tests.

Table 2: Descriptive statistics, Study 1

	N	Tax	DG	Efficiency	Actual SD	SD(F)	Actual CV	CV(F)	EGB
Forecast	506	53.59	37.09	3.54	1831.99	707.19	0.57	0.59	0.47
Ratio	421	49.52	37.59	3.61	1831.99	934.91	0.57	0.58	0.46
Realized	488	47.27	37.70	3.34	1831.99	585.54	0.57	0.57	0.45
Total	1415	50.20	37.45	3.49	1831.99	732.99	0.57	0.58	0.46

Notes: averages are taken over all 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). 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. Separate descriptive statistics for the poor and the middle-income/rich subjects are presented in Tables S.18 and S.19.

2.3.1 H1: Do People Underestimate Future Inequality?

To test H1, I first obtain the level of absolute and relative inequality 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.[10]

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 4a and S.8), most subjects make negative forecasting errors with only few subjects making zero or positive forecasting errors. Underestimation is statistically significant for both measures ($p < .001$, bootstrapped t-test).

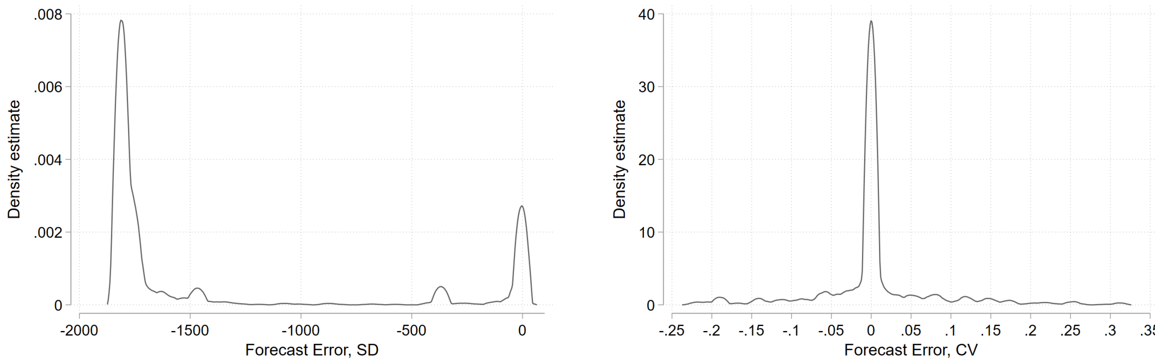
Regarding relative inequality, Figure 4b suggests that many subjects have nearly accurate forecasts for the coefficient of variation (similar for the Gini coefficient, see Figure S.10). However, the statistical evidence is mixed. On average, subjects significantly underestimate relative inequality in 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 perform better at forecasting relative than absolute inequality, the data only partially support H1.2. As I show in Section 2.3.2, the fact H1.2 is not confirmed has implications for the subjects' tax preferences, as some subjects misperceive whether they gain or lose from taxation.

The data also suggest that a number of subjects accurately forecast the level of inequality (Figure 4a). Specifically, 129 subjects (9 percent) make estimates within ±\$1 of the correct answer for all three income classes. Of the subjects who answered correctly, 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. Nevertheless, the results in the next section do not change if these subjects are excluded from the analysis (see Table S.15). Moreover, there is no difference in the preferred tax rates among the subjects who answered correctly and those who did not (treatments combined or analysed separately, all $p's > .656$).

To examine EGB in the data, I compute a measure of EGB as in Stango and Zinman (2009) and define the bias as $\theta \equiv (1 - \log(FV/PV)) \,/\, (T \log(1 + r))$ (see also Almenberg and Gerdes, 2012, and Song, 2020; see Levy and Tasoff, 2016, Foltice and Langer, 2017, and Königsheim et al., 2018 for elaborate discussions on how to best model EGB). Each subject makes three forecasts (one for each income class), and I use the average value of θ as an estimate for the

[10] The model presented in Section 2.2 assumes self-centered inequality aversion (Fehr and Schmidt, 1999). Self-centered inequality is, however, difficult to apply directly to the experiment, as this would e.g. imply that a middle-income and a rich subject in *Realized* experience different levels of inequality because their reference points (own income) differ. Instead, the standard deviation or Absolute Gini are the two measures that are most closely related to the model, since they are measures of absolute inequality and thereby share the same key axiomatic property of translation invariance as the inequality in the FS model.

Figure 4: Forecast error, Study 1



(a) Absolute inequality, SD

(b) Relative inequality, CV

Notes: the figures show the kernel density of subjects' forecast error (epanechnikov). (a) shows the forecast error of absolute inequality ($bw = 20$) with the standard deviation calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$. (b) shows the forecast error of relative inequality ($bw = 0.005$) with the coefficient of variation calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}} \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$. Both figures exclude the 5 percent smallest and largest errors for illustrative purposes. See Figures S.7 and S.9 for the full sample.

subjects' degree of EGB. Of the subjects who did not obtain the correct 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 Supplementary Materials S.5, I comment on the heterogeneity in the subjects' forecast errors. In Section 5.1, I show that forecast errors relate to underestimation of inequality in society.

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 that subjects underestimate relative inequality on average.*

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?

The theoretical model outlined in Section 2.2 assumes that individuals are motivated by their own income and equality in the group. For poor subjects, the two motivators work in the same direction, leading to a preference for full redistribution in all treatments. It is difficult to test the corner solution of full redistribution, however, as any decision error will lead to a deviation in one direction only. Instead, I compare income classes and find that poor subjects do vote for more redistribution than subjects in the middle and rich income classes across all treatments, and this difference is statistically significant ($p < .001$, cf. Table S.13). I discuss the behaviour of poor subjects in Supplementary Materials S.7.2, where I provide suggestive evidence that tax aversion may also influence the behaviour of some poor subjects. In the following, I restrict my attention to the behaviour of middle-income and rich subjects, cf. Hypothesis 2.

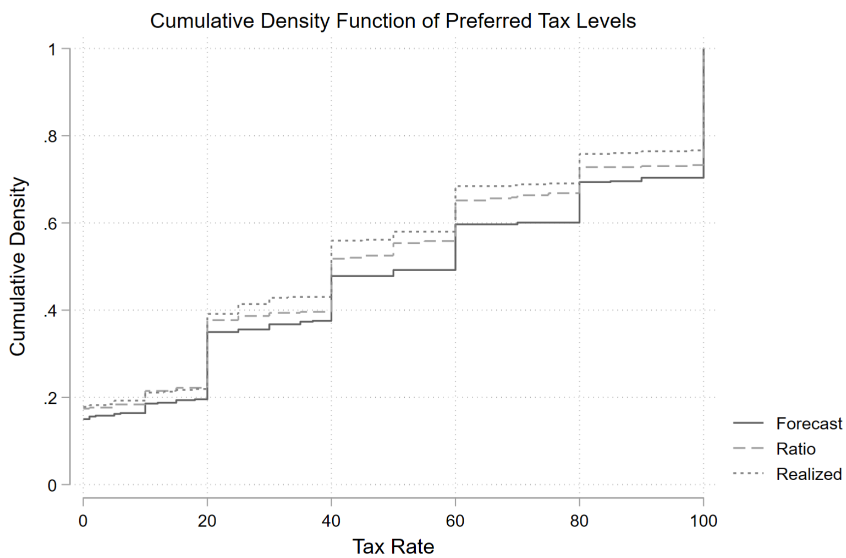
Contrary to H2.1, the average tax rate is greater in *Forecast* (47.73) than in *Realized* (40.74). Figure 5 suggests that the preferred tax rate in *Forecast* first-order stochastically dominates the preferred tax rates in *Ratio* and *Realized*. To test this formally, I use tobit regressions, since the tax is bounded between 0 and 100. They reveal that the treatment difference is statistically significant, also when controlling for dictator giving (a proxy for inequality aversion), demographics, and attitudes (all $p's < 0.01$, see Table 3). The statistical inference is robust to using 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).

Contrary to H2.2, the average preferred tax rate is 2.23 greater in *Ratio* than in *Realized*; yet, this difference 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, also when controlling for dictator giving, demographics, and attitudes (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 an 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, but rather by the fact that many middle-income subjects grossly underestimate their personal costs of redistributing. This happens in two ways. First, 58 of the 207 middle-income subjects in *Forecast* (28 percent) make forecasts that distort relative inequality in such a way that they wrongly believe they will gain from redistribution in the final round. This scenario was ruled out in the model presented in Section 2.2, as the model assumes that individuals apply the same overall growth to all incomes. And while this type of misperception is possible for middle-income subjects in *Forecast*, subjects in *Ratio* and *Realized* observe by construction that redistribution comes at a personal cost. Believing that one will gain from redistribution

Figure 5: Preferred tax rate by treatment, Study 1



Notes: the figure presents the cumulative density function (or empirical distribution function) of the subjects' tax decisions by treatment. Subjects bunch at the tax rates 0, 20, 40, 60, 80, and 100, which is to be expected because 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.

leads on average to an increase in the preferred tax of 46 percentage points (cf. Column 4 in Table 3). A second way that subjects may underestimate their personal costs of redistribution occurs if subjects 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).[11] This misperception occurs for 199 of the 389 middle-income subjects in *Forecast* and *Ratio* (51 percent). 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). Believing that redistribution comes at very low personal costs predicts an increase in the preferred tax rate of 14 percentage points (cf. Column 5 in Table 3).[12]

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 (cf. Table 3). The differences are also insignificant when one considers SCLS estimators (all $p's > .784$) and when one conducts MWU-tests excluding subjects who wrongly perceive personal gains or low personal costs (all $p's > .330$). When including these endogenous controls, the coefficients on the treatment dummies lose their causal interpretation. However, the results suggest that treatment effects are driven by the two specific types of misperceptions. This implies that subjects are on average willing to give up approximately the same share of their income regardless of the level of absolute inequality (discussed further in Supplementary Materials S.7.4).

While preferred tax rates refer to the *share* of income that a net contributor is willing to give up, redistribution is more costly in *absolute amounts* when absolute inequality is large. Hence, the finding that tax rates remain unchanged implies that greater absolute inequality is associated with a greater willingness-to-pay for redistribution. This result is consistent with survey evidence indicating that information about inequality can heighten concerns about inequality without changing taxation preferences (Zilinsky, 2014; Kuziemko et al., 2015). Additionally, this finding may shed light on why observational data show no relation between increased concerns about inequality and support for government intervention (Wright, 2018).

[11] 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.18). However, the results are robust to using instead any value up to \$65, corresponding to a payment of USD 0.0325.

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

Comparing the above finding to the theoretical model outlined in Section 2.2, it appears that the assumption of individuals exhibiting increasing marginal disutility from inequality is refuted by the data: this assumption was key for the prediction that subjects would respond to greater absolute inequality by preferring a higher tax rate.

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 (i) that they gain from tax-financed redistribution or (ii) that redistribution comes at low personal costs.*

Table 3: EGB and tax inconsistency

	(1)	(2)	(3)	(4)	(5)	(6)
Ratio	-8.31*	-8.41*	-7.85*	-7.49*	0.08	-1.23
	(4.69)	(4.56)	(4.53)	(4.32)	(4.44)	(4.44)
Realized	-11.87***	-12.68***	-12.98***	-13.88***	-6.53	-2.58
	(4.49)	(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	No	Yes	Yes	Yes	Yes
Attitudinal Controls	No	No	No	Yes	Yes	Yes
Observations	1013	1013	1013	1013	1013	1013

Notes: 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 they 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.14 for the full specification. Robust standard errors in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Mechanism 1: Forecasts Matter Through Inequality Information. 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 examine the relation between EGB and the tax rate for which subjects vote. 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. In contrast, EGB should be uncorrelated with 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 S.16; 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: $p = .494$).

In *Realized*, EGB is not significant, regardless of what controls are used (all $p's > 0.627$, cf. Table S.17; SCLS: $p = .789$; Spearman's ρ: $p = 0.507$).[13] 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.

These results support the interpretation that forecasts matter through the information that is available to subjects, and it also indicates that it is the two ways of underestimating costs that are the channels through which forecast biases matter – not inequality *per se*.

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 that perceived inequality does not affect preferred tax rates. Rather, what matters is 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 external validity of the previous results, as inequality develops differently across domains (e.g. wage growth vs. stock market returns), time periods, and countries.[14]

[13] 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, EGB does not correlate with preferred tax in *Realized*, suggesting that EGB only matters in the case of specific misperceptions.

[14] In the US, for instance, Piketty et al. (2018) show that pre-tax income growth rates were fairly uniform from 1946 to 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, from 1980 to 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).

To address this concern, Study 2 has subjects earn higher interest rates the larger their initial incomes are, and this implies that both absolute and relative inequality increase over time. As we will see below, the conclusions from Study 1 replicate in this setting.

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 that interest rates are different for each income class, with the poor, middle-income, and rich subjects receiving interest rates of 24, 26, and 27 percent, respectively. The compounded interest over 30 rounds lead to vastly different overall growths 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 to ensure that taxation remains costly for the middle class, as in Study 1.

For 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 actual post-redistribution incomes, whereas subjects in *ForecastR* observe the post-redistribution incomes based on their subjective forecasts.

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. As in Study 1, the results are qualitatively robust to including all responses. In the main sample, 44 percent were male, 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. Tables S.22 and S.23 provide a full set of summary statistics.

As in Study 1, subjects 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 experiment open 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 Supplementary Materials S.2.3. Note that the theoretical framework was not adapted based on the previous results because all studies were conducted in parallel. Thus, the model in Supplementary Materials S.2.3 assumes that individuals experience increasing marginal disutility from absolute inequality, which is the key assumption for the prediction that subjects change their tax preferences when inequality increases. Moreover, the model assumes that individuals exhibit the same bias (θ) when forecasting each of the incomes and that the forecast bias matters in such a way that the degree of underestimation is proportional to the interest rate (as in the standard frameworks by Stango and Zinman, 2009, and Levy and Tasoff, 2016). This assumption is key for the prediction that individuals know whether redistribution is costly for themselves even if they underestimate the extent of relative inequality.

3.2.1 Hypotheses

As in Section 2.2, the model assumes that individuals underestimate exponential developments for all compounding rates and initial amounts, following the literature on EGB. As both absolute and relative inequality increase over the 30 rounds, I 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 incomes, subjects on average underestimate how much absolute and relative inequality increase.*

Looking at voting behaviour, middle-income and rich subjects who exhibit EGB (i.e. in *ForecastR*) are expected to underestimate the extent of absolute inequality in round 30, but they realise that redistribution is costly for themselves. They therefore underestimate the need for redistribution, and the model in Supplementary Materials S.2.3 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 then examine how forecasts influence preferences in the voting experiment. Descriptive statistics are presented in Table 4.

Table 4: Descriptive statistics, Study 2

	N	Tax	DG	Efficiency	Actual SD	SD(F)	Actual CV	CV(F)	EGB
ForecastR	480	45.76	37.31	3.42	3223.32	1251.01	0.71	0.60	0.49
RealizedR	500	48.91	37.69	3.51	3223.32	992.08	0.71	0.60	0.50
Total	980	47.37	37.50	3.47	3223.32	1118.90	0.71	0.60	0.49

Notes: 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). Separate descriptive statistics for the poor and the middle-income/rich subjects are presented in Tables S.24 and S.25.

3.3.1 H3: Do People Underestimate Growth in Inequality?

The data provide clear support for H3. Figures S.11-S.16 reveal that subjects 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. Of these, 64 were randomised into the middle or rich income classes. The results in the next section do not change if these are excluded from the analysis (see Table S.27), and there is no difference in the preferred tax rates among the subjects who answered correctly and those who did not (treatments combined or analysed separately, all $p's > .627$). Of the remaining subjects, 45 subjects (5 percent) 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 Supplementary Materials S.5.

I summarise these results as follows:

Result 3 *When interest rates correlate positively with initial incomes, 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: How Do Forecasts Influence Redistribution?

As in Study 1, the poor subjects vote for higher tax rates than the middle-income and rich subjects, and I discuss this in Supplementary Materials S.7.2.

Looking at the middle-income and rich subjects, the distribution of tax decisions suggest that there are no meaningful differences between *ForecastR* (mean: 39.08) and *RealizedR* (mean: 39.83), cf. Figure S.17. This is confirmed in tobit regressions that show no significant differences regardless of the level of controls (all $p's > 0.623$, cf. Table S.26), and this result is robust to using the SCLS estimator ($p = .158$) and the MWU-test ($p = .892$).

Nevertheless, an exploratory inspection of the data reveals that forecast bias again matters for the subjects who grossly underestimate the personal costs of redistributing. In *ForecastR*, 48 of the 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 ($p = .004$). This type of misperception is not accounted for in the theoretical model presented in Supplementary Materials S.2.3, as it assumes that people exhibit the same degree of bias (θ) when making each income forecast and that the forecast bias matters in such a way that the degree of underestimation is proportional to the interest rate. If this was the case, then individuals would always know whether redistribution benefits themselves or not. Furthermore, wrongly perceiving the costs as minimal leads to an increase in the preferred tax rate of 10.04 percentage points, but this difference fails to reach statistical significance ($p = .150$).

Similar to Study 1, the fact that tax decisions do not change with the level of inequality implies that subjects are willing to pay more for redistribution when absolute and relative inequality are large. Yet, this greater willingness-to-pay is offset by the increased cost of redistribution, suggesting again that the assumption of increasing marginal disutility from inequality does not hold.

I summarise these findings as follows:

Result 4 *When interest rates correlate positively with initial incomes, erroneous forecasts lead individuals to vote for higher tax rates to the extent that people wrongly believe that they gain from taxation.*

In Supplementary Materials S.6, I show that EGB correlates with preferred tax rates in *ForecastR*, but not when controlling for perceived gains and perceived low personal costs (similar to Mechanism 1). I also show that there is no relation between EGB and preferred tax rate in *RealizedR*.

4 Study 3: Voting Without Forecast

Studies 1 and 2 demonstrated that subjects often misunderstand how inequality changes in the presence of economic growth, but correcting subjects' beliefs about inequality did not influence their preferences for redistribution. Rather, what mattered was the extent to which subjects misperceived their personal costs of redistribution. However, a concern is that Studies 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 element of surprise rather than 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, the initial incomes may serve as reference points, leading subjects to perceive the poor as well-off even at low tax rates, since the income of the poor is 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 incomes in the final round of Study 1 or (ii) forecasted income levels from the pilot for Study 1 (see Supplementary Materials S.1 for a description of the pilot study). This removes any possible spillovers from the forecast task and thus provides a clean test for the causal effect of the level of absolute inequality on redistributive preferences. 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 is similar to Study 1, except that subjects do not make any subjective forecast and instead participate in the voting part directly following the demographic survey. Subjects are randomised into one of two treatments, which differ only in the income levels of the poor, middle-income, and rich classes. In the *RealizedNo* treatment, subjects are informed that the income for an individual 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 from a pilot study for Study 1. Specifically, I avoid the most extreme forecasts from the pilot study and randomise subjects into one of the following pre-tax income allocations $(x_P; x_M; x_R)$: $\{(8;31;55), (12;46;81), (13;51;89), (30;120;210), (38;150;263), (41;162;284), (156;624;1092), (579;2315;4052)\}$. In both *RealizedNo* and *ForecastNo*, 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 two screeners that were based on the forecast task, as they 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. As in Studies 1 and 2, the results are qualitatively robust to including all subjects. In the main sample, 41 percent were male, 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.33 and S.34.

Subjects received USD 1 in addition to the payment from the dictator games and the voting experiment. 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 subjective forecasts only matter via beliefs about inequality in the final round. Thus, the model is directly applicable to the case where subjects simply vote based on the forecasts from the pilot study. Again, the theoretical framework was not adapted to the previous results because all studies were conducted in parallel. Regarding the underestimation of personal costs, note that all incomes are chosen such that the relative inequality is the same for all individuals, so the middle-income subjects realise that redistribution comes at a personal cost. Moreover, while personal costs of redistribution are low for some incomes in *ForecastNo*, there is a direct relation between personal costs and the level of absolute inequality such that low costs are always matched by low inequality, whereby the individual cares little about inequality (ϕ_i is low).

4.2.1 Hypothesis

In the pilot study, subjects on average underestimated the extent of absolute inequality in the final round. Therefore, subjects in *ForecastNo* observe a smaller extent of absolute inequality than subjects in *RealizedNo* (see Table S.35 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 analyse how inequality influences preferences in the voting experiment. Table 5 provides descriptive statistics.

Table 5: Descriptive statistics, Study 3

	N	Tax	DG	Efficiency
ForecastNo	539	56.88	38.82	3.53
RealizedNo	555	52.77	38.32	3.44
Total	1094	54.79	38.56	3.49

Notes: 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). Separate descriptive statistics for the poor and the middle-income/rich subjects are presented in Tables S.37 and S.38.

4.3.1 H5: How Does Information About Inequality Influence Redistribution?

Once more, I find that poor subjects vote for higher tax rates than middle-income and rich subjects (discussed in Supplementary Materials S.7.2). Turning to the middle-income and rich subjects, I find – contrary to H5 – that 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.18). For these subjects, redistribution bears almost no personal costs, indicating that perceiving low costs might be the critical factor, as in the previous studies.

Formally, the difference in average preferred tax rates is marginally significant without controls and when controlling for dictator giving ($p = .063$ and $p = .067$), and the difference becomes significant when including demographic and attitudinal controls ($p = .047$ and $p = .009$). The difference is robust to using the MWU-test ($p = .050$) but not to using the SCLS estimator ($p = .143$). In this study, there is no effect of perceived gains because the forecasted incomes from the pilot were chosen such that all middle-income and rich subjects realise that redistribution comes at a personal cost. However, some of the forecasted incomes imply very low personal costs for the middle-income group. As before, these low personal costs imply that a middle-income subject who only cares about their 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 higher ($p < .001$, cf. Table S.36). Moreover, when including a dummy for low personal costs, there is no difference between *ForecastNo* and *RealizedNo* ($p = .613$, cf. Table S.36; SCLS: $p = .956$; MWU: $p = .824$). This corroborates the results from Studies 1 and 2, as it 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. Similar to Studies 1 and 2, the fact that the extent of inequality does not influence tax preferences imply that subjects are willing to pay more for redistribution in absolute amounts when absolute inequality is large. But the result that tax rates do not change with the level of absolute inequality refutes the assumption of increasing marginal disutility from inequality.

Result 5 *When subjects do not make forecasts, the level of absolute inequality only influences preferred tax rates to the extent that redistribution comes at very low personal costs.*

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. I first show that forecast bias significantly correlates with misperceiving wealth inequality in the US. Then, I comment on the correlation between perceived inequality in society and voting preferences.

In Supplementary Materials S.7, I demonstrate that the current studies replicate previous findings on giving in dictator games, underestimation of wealth inequality in the US, and the prevalence of exponential growth bias. I also comment on the relative importance of self-interest, inequality aversion, efficiency concerns, and image concerns for redistributive preferences. Then, I show that seeing the extent of inequality in round 1 and providing a forecast matter for tax preferences in round 30. Finally, I show that the extent of inequality in a group does not predict subjects' tax preferences regardless of how inequality is operationalised.

5.1 EGB Predicts Misperceptions of Inequality in Society

Studies 1 and 2 reveal that 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.45), and the relation remains when restricting the sample to subjects who did not provide exact forecasts for all income classes (all $p's < 0.001$, cf. Table S.46). 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 of modest size, it is economically considerable, especially considering that it pertains to 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 stylised setting of the experiment does capture elements that contribute to biased beliefs about inequality in the field.

5.2 Perceived Inequality in Society and Voting Preferences

The literature on perceptions of inequality has examined the link between inequality beliefs and redistributive 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 likely 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 the preferred tax rate of 25 percentage points ($p = .029$, also when including demographic controls, cf. Table S.49).[15] This shows that people who perceive greater inequality in society are also more con-

[15]Beliefs about wealth Gini in the US correlate significantly with attitudinal variables; thus, I refrain from

cerned about inequality in an abstract environment. Hence, the relation between inequality perceptions and preferences may be more complex than hitherto recognised, as there may be two-way causality between perceptions of inequality and preferences.

6 Conclusion

Many researchers have offered explanations for why the recent increase in inequality has not led to greater demand for redistribution in most industrialised countries: voters may fail to connect inequality with policy action (Bartels, 2005), they might not trust the government (Kuziemko et al., 2015), they may believe that inequalities are justified (Lerner and Simmons, 1966; Trump, 2018), or they may habituate to greater inequality over time (Mijs, 2021). However, this paper demonstrates that even in a setting where these factors do not apply, the level of inequality does not influence the demand for redistribution. Across three large-scale experiments, I have shown that greater inequality is associated with a greater willingness-to-pay for redistribution. However, this greater willingness-to-pay is offset by the fact that greater inequality makes it more expensive for a net contributor to redistribute, as their income makes up a larger share of the tax base. Therefore, the level of inequality does not influence the demand for redistribution in this setting. Consequently, it does not matter that most individuals misperceive the level of inequality and how inequality evolves in the presence of economic growth. Instead, only misperceptions about personal costs of redistribution matter.

Yet, some issues limit the external validity of the current experiments. First, all three experiments use samples from MTurk and recruit only US residents. 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 subject pool that more accurately reflects the behaviours of representative samples than other convenience samples (Snowberg and Yariv, 2021). Moreover, many classical behavioural and experimental findings have been replicated using MTurk, both within economics (Horton et al., 2011; Amir et al., 2012), psychology (Crump et al., 2013), and political science (Coppock, 2019). Yet, it is possible that the current US samples react differently to changes in inequality than other populations. Previous studies have shown that people's beliefs, values, and social norms influence their preferences for redistribution (Blekesaune and Quadagno, 2003; Alesina and Angeletos, 2005; Almås et al., 2020), and it would therefore be interesting to replicate the

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).

current findings among other populations.

Second, subjects make decisions and inequality forecasts completely on their own. In the field, people are often influenced by peers, experts, media, and organisations that make forecasts about future inequality. The current paper does not address what type of information people seek and avoid in the field. Yet, the results of this paper suggest that even with access to such information, the anticipation of future increases in inequality is unlikely to influence redistributive preferences.

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 are better at anticipating how economic growth influences inequality. In the field, however, 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 might be limited in the field is the prevalence of exponential growth bias and the fact that people do not realise their bias (Cordes et al., 2019). This highlights the importance of future studies that explore ways to improve forecasting accuracy (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 experiments, subjects make forecasts about incomes 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 assumes that people have consequentialist motives, that is, they are only concerned with the vote to the extent that it may influence post-redistribution incomes in the group. Nevertheless, research on distributive preferences suggests that voters may also want to express a preference for a certain outcome (Brennan and Buchanan, 1984), maintain a positive self-image (Bénabou and Tirole, 2006), 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). Thus, future research should explore how different non-consequentialist motives are affected by inequality information.

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A.1 Screeners

In the following, I describe the screeners that I applied in each of the studies to ensure high-quality data. 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 bot tests. In all three studies, subjects were informed that they were only allowed to participate from the US and without using a VPN or VPS. This is to alleviate any concern about poor data quality from so-called 'farmers' (Moss and Litman, 2018b), who participate from outside the US and mask their location. That subjects did not use VPN or VPS 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.[16] There were six 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.[17]

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).

A.1.1 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

[16]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).

[17]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.

poor $<$ *middle* $<$ *rich*. This led to the exclusion of 34 respondents. Second, Wood et al. (2017) show that subjects who answer more than one item per second provide responses of poor quality. I therefore excluded two additional respondents based on their response times in the attitudinal survey (see also Aguinis et al., 2021).[18] 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 incomes 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.1.2 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.

[18] 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.1.3 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, two 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.

A.1.4 Online Fora

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., 2021). 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. 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 because the expected hourly wage of this study was somewhat larger than most other studies on MTurk.

Online Supplement

Growth and Inequality: Experimental Evidence on How Misperceptions Affect Redistribution

Jonas Pilgaard Kaiser

9 August 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 income 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 Supplementary Materials S.7.2).

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 Supplementary Materials S.7.2).

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 interest 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 because 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 weekday 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 led 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 Supplementary Materials S.7.1 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 because I still considered it useful for exploratory purposes, and removing the least sophisticated subjects bias the underestimation of inequality in a conservative direction.

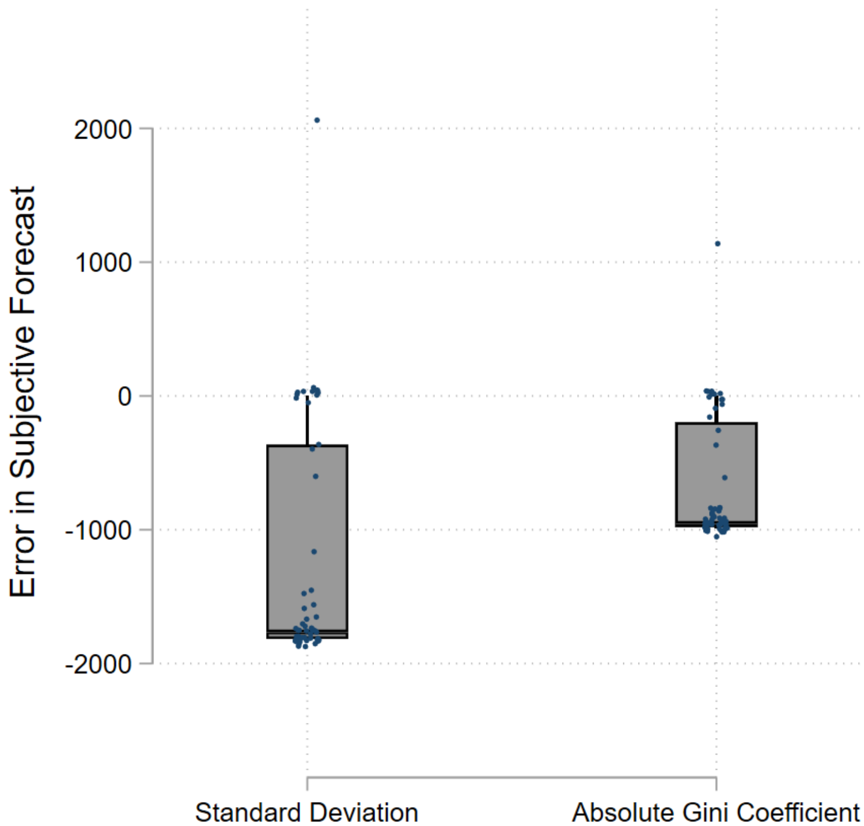
Table S.1: Sample characteristics, Study 1 pilot

	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	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.

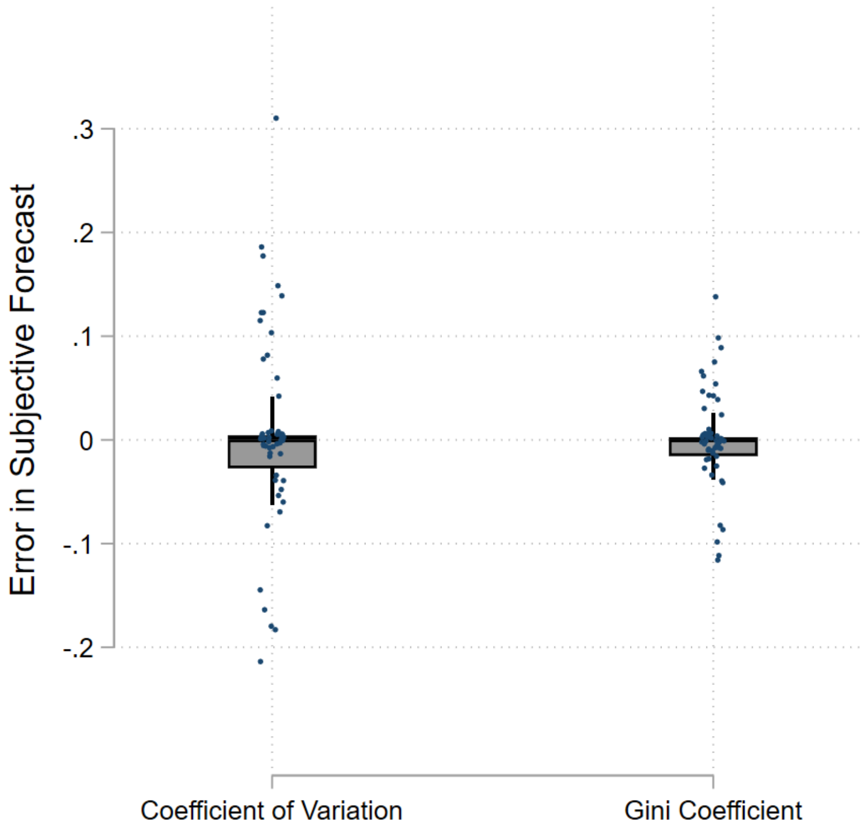
Voting Behaviour. Figure S.3 shows the subjects' tax decision. Here, I only examine responses by middle-income and rich subjects, and I separate responses by treatment. Thus, there are too few data points to draw any conclusions from this sample.

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



Notes: 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



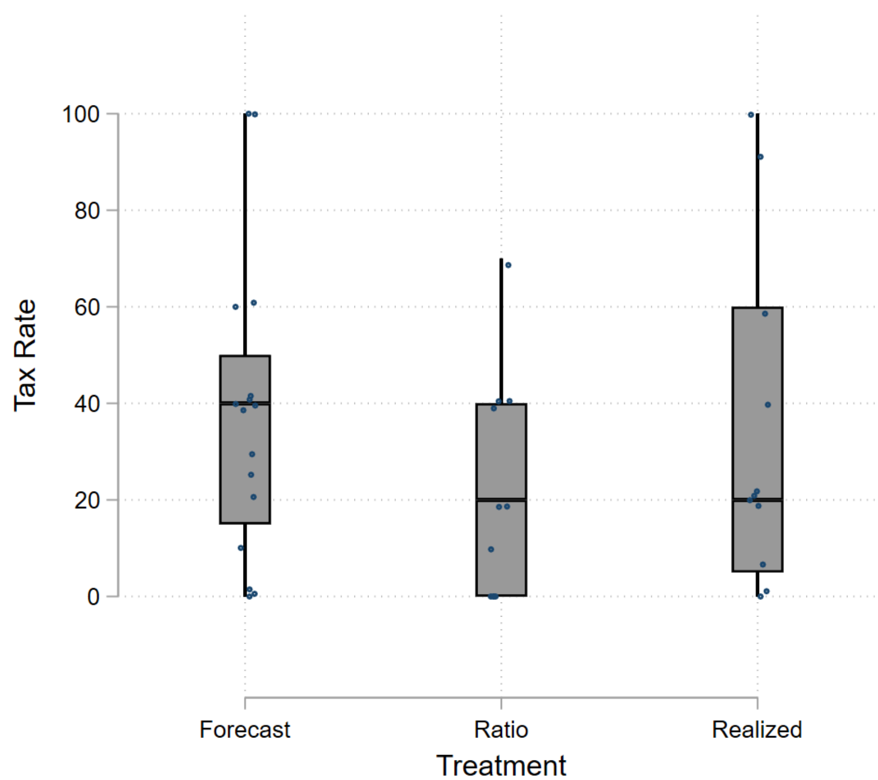
Notes: box plots show the median as well as upper and lower quartiles of forecast errors (raw data). Spikes extend to the largest or smallest values within 1.5 times the upper or lower quartiles, respectively. The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$,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

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

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



Notes: 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 (Supplementary Materials 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.

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	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

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, 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.

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

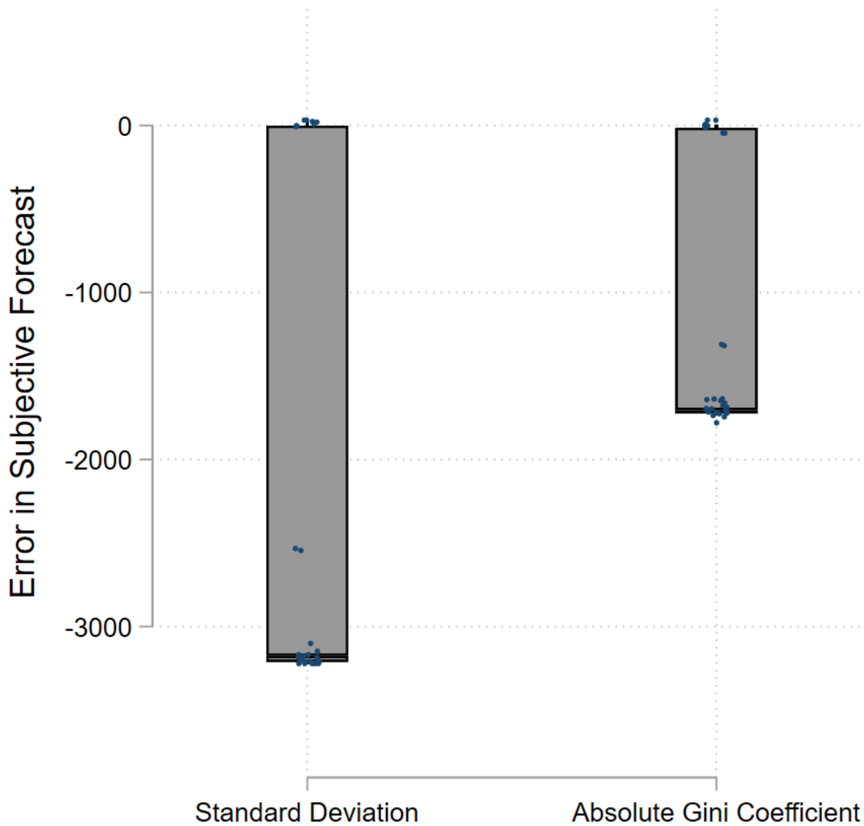
	N	Tax	DG	Actual SD	SD (F)	Actual CV	CV (F)	EGB
ForecastR	11	46.91	27.27	3223.32	614.09	0.71	0.59	0.58
RealizedR	12	20.83	29.17	3223.32	1683.51	0.71	0.68	0.34
Total	23	33.30	28.26	3223.32	1172.05	0.71	0.64	0.45

Notes: 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).

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.

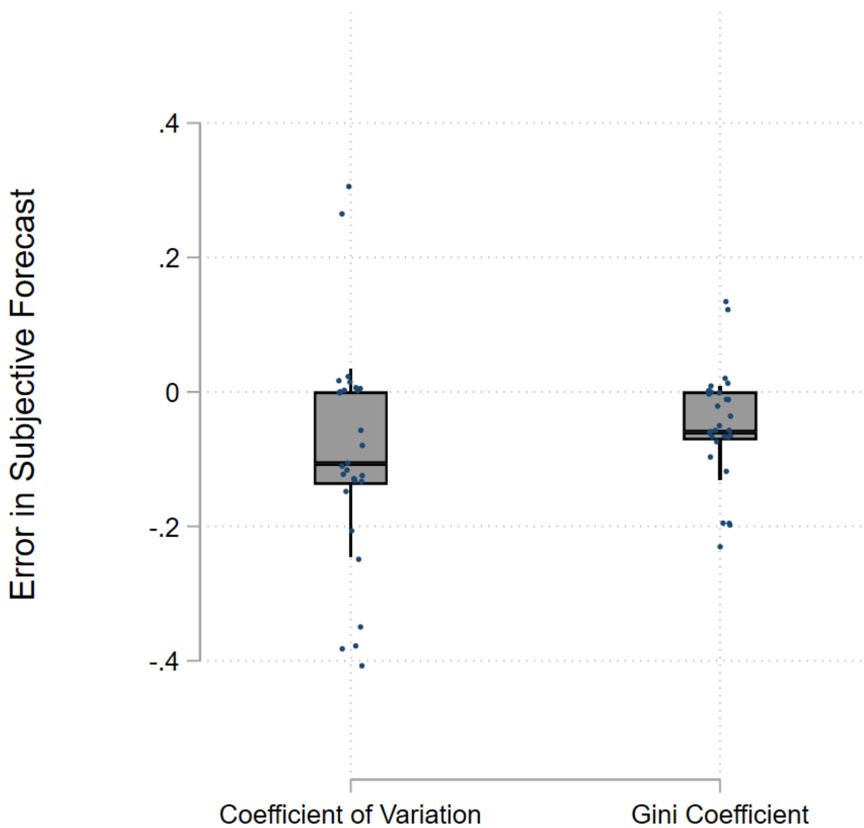
Voting Behaviour. Figure S.6 shows subjects' tax decision by treatment, and I again refrain from drawing any conclusions from this due to the small sample size.

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



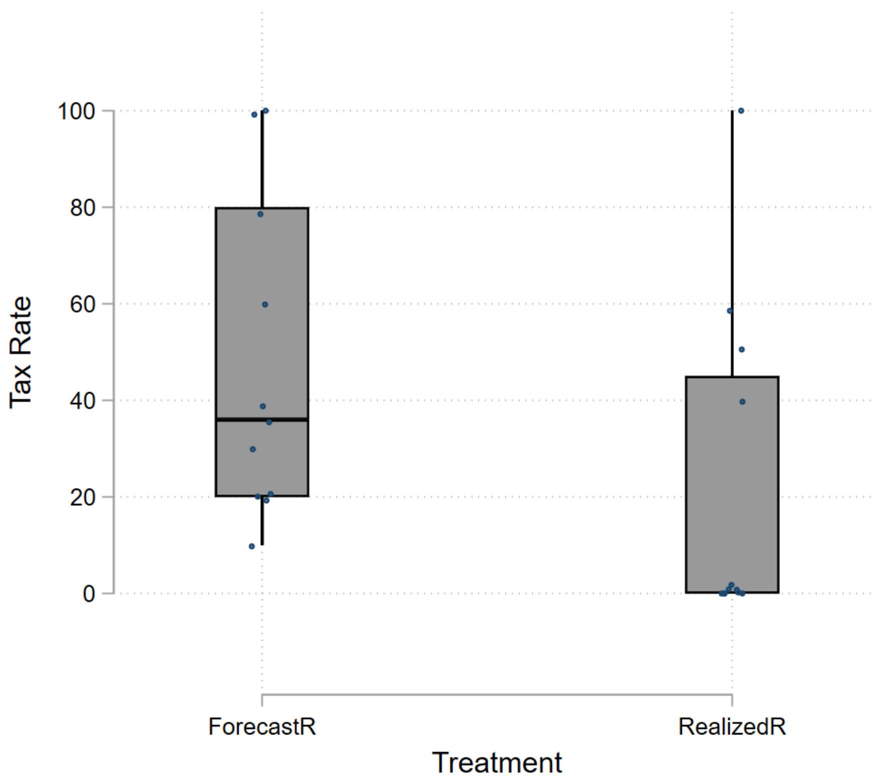
Notes: box plots show the median as well as upper and lower quartiles of forecast errors (raw data). Spikes extend to the largest or smallest values within 1.5 times the upper or lower quartiles, respectively. The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$, 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



Notes: 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



Notes: 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:

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

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

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

As in Section 2.2, poor individuals ($x_i < \lambda\bar{x}$) prefer the highest tax rate of $\tau_i^b = 1$; middle-income and rich individuals who are severely biased and/or care very little about inequality ($\phi_i(1+r)^{(1-\theta)T} \leq (x_i - \lambda\bar{x})/2$) prefer the lowest tax rate of $\tau_i^b = 0$.

For other individuals, one can see from the partial derivatives of τ_i^b that the preferred tax level increases in inequality aversion (α, β) and the efficiency of the tax (λ):

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

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

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

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:

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

$$x_k < x_i: \ \frac{\partial \tau_i^b}{\partial x_k} = \frac{\frac{1}{n}\lambda\phi_i - (x_i - \lambda\bar{x})2\beta_i(x_i - x_k)}{2(1+r)^{(1-\theta)T}\phi_i^2} < 0$$

$$\text{if } (1-\tau_i^b)2\beta_i(x_i - x_k) > \frac{\lambda}{(1+r)^{(1-\theta)T}} \frac{n-1}{2n}$$

The effect of the individual i's own income 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 x_i 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:

$$FV = PV \left[(1 + (1-\theta) r)^T + \theta T r \right] \tag{S.3}$$

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

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

Maximising the utility from Equation S.4 with respect to the tax rate yields the preferred tax level τ_i^b under the influence of EGB:

$$\tau_i^b(x_1, \ldots, x_N; \theta) = 1 - \frac{x_i - \lambda \bar{x}}{2\phi_i \left[(1 + (1-\theta) r)^T + \theta T r \right]} \tag{S.5}$$

As in Section 2.2, poor individuals ($x_i < \lambda \bar{x}$) prefer the highest tax rate of $\tau_i^b = 1$; middle-income and rich individuals who are severely biased and/or care very little about inequality ($\phi_i \left[(1 + (1-\theta) r)^T + \theta T r \right] \leq (x_i - \lambda \bar{x})/2$) prefer the lowest tax rate of $\tau_i^b = 0$.

I now look at individuals who prefer an intermediate tax rate. As the tax rate specified in Equation S.5 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:

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

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

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

In contrast to the analysis that draws 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 income obtain a higher interest rate, resembling the empirical case of unequal income growth. Formally, denote individual i's forecast of their own income by $f_i(r, T, \theta)$ and their forecast of individual j's income by $f_j(r, T, \theta)$. Then, $f_i(r, T, \theta) \geq f_j(r, T, \theta)$ iff $x_i \geq x_j$. I assume – similar to standard frameworks such as the ones presented by Stango and Zinman (2009) and Levy and Tasoff (2016) – that the forecast bias matters in such a way that the degree of underestimation is proportional to the interest rate.

The average income 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 income, 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, they estimate that they will obtain the following utility, where I suppress the arguments for the function f to simplify notation:

$$\begin{aligned} U_i(x_1,...,x_n) = (1-\tau)x_i f_i + \lambda\tau\bar{x}\tilde{f} \\ - \alpha_i \frac{1}{n-1}(1-\tau)^2 \sum_{j\neq i}(\max\{x_j f_j - x_i f_i, 0\})^2 \\ - \beta_i \frac{1}{n-1}(1-\tau)^2 \sum_{j\neq i}(\max\{x_i f_i - x_j f_j, 0\})^2 \end{aligned} \tag{S.6}$$

Maximising Equation S.6 with respect to τ yields individual i's preferred tax rate:

$$\tau_i^b(x_1,\ldots,x_N;\theta) = 1 - \frac{x_i f_i - \lambda\bar{x}\tilde{f}}{2\frac{1}{n-1}\left[\alpha_i \sum_{j\neq i}(\max\{x_j f_j - x_i f_i, 0\})^2 + \beta_i \sum_{j\neq i}(\max\{x_i f_i - x_j f_j, 0\})^2\right]} \tag{S.7}$$

For any individual i with $x_i f_i \leq \lambda\bar{x}\tilde{f}$, increasing the tax rate leads to both higher income and more equality in the group, leading to the corner solution of a tax rate of 1. Similar to the model from Section 2.2, individuals with very little concern for inequality will prefer the corner solution of a tax rate of 0. I therefore look at individuals who prefer intermediate tax rates 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 S.7 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 their 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 S.7. In other words, I compare how much redistribution individuals prefer in the case where everyone obtains the same real interest on their income (f) to the case where interest rates and incomes are positively related (f_i).

Intuitively, there are two effects of making interest rates positively correlated with income. 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 S.7) 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 S.7 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 earns less 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 income 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 earns much less 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:

$$
\begin{aligned}
U_i(x_1, ..., x_n) = {} & [(1-\tau)x_i + \tau\bar{x}] \cdot f(r, T, \theta) \\
& - \alpha_i \frac{1}{n-1}(1-\tau)^2 f(r, T, \theta)^2 \sum_{j\neq i}(\max\{x_j - x_i, 0\})^2 \\
& - \beta_i \frac{1}{n-1}(1-\tau)^2 f(r, T, \theta)^2 \sum_{j\neq i}(\max\{x_i - x_j, 0\})^2 \\
& - \gamma_i \frac{1}{\bar{x}}\left[\sum_{i=1}^{n}\frac{(1-\tau)^2(x_i-\bar{x})^2}{n}\right]^{\frac{1}{2}}
\end{aligned}
\tag{S.8}
$$

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

$$
\tau_i^b(x_1, \ldots, x_N; \theta) = 1 - \frac{x_i - \bar{x} - \gamma_i \frac{1}{n\bar{x}f(r,T,\theta)}\left(\sum_{i=1}^{n}(x_i-\bar{x})^2\right)^{\frac{1}{2}}}{2\phi_i f(r, T, \theta)}
\tag{S.9}
$$

By comparing Equation S.9 with Equation 5, one can see that introducing aversion towards relative inequality leads, ceteris paribus, to preferences for more redistribution. As before, I focus on the case where individuals are not initially in a corner solution of either $\tau_i^b = 1$ or $\tau_i^b = 0$.

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:

$$
\frac{x_i - \bar{x}}{2\frac{1}{n\bar{x}}\left(\sum_{i=1}^{n}(x_i-\bar{x})^2\right)^{\frac{1}{2}}} \geq \gamma_i
\tag{S.10}
$$

This implies that the requirement for γ_i varies with the level of income. Specifically, the left-hand side of Equation S.10 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 $\lim_{T\to 0}\tau_i^b = \tau_i^*$. 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 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.[2] Extending the model with inequality aversion in the form of the Krtscha measure yields the following utility for individual i:

$$\begin{aligned} U_i(x_1, ..., x_n) = & [(1-\tau)x_i + \tau\bar{x}] \cdot f(r, T, \theta) \\ & - \alpha_i \frac{1}{n-1}(1-\tau)^2 f(r, T, \theta)^2 \sum_{j \neq i} (\max\{x_j - x_i, 0\})^2 \\ & - \beta_i \frac{1}{n-1}(1-\tau)^2 f(r, T, \theta)^2 \sum_{j \neq i} (\max\{x_i - x_j, 0\})^2 \\ & - \gamma_i \frac{1}{n\bar{x}}(1-\tau)^2 \sum_{i=1}^{n} (x_i - \bar{x})^2 \end{aligned} \tag{S.11}$$

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 they care about their 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:

$$\begin{aligned} U_i(x_1, ..., x_n) = & [(1-\tau)x_i + \lambda\tau\bar{x}] \cdot f(r, T, \theta) \\ & + \delta_i y(1 - \tau(1-\lambda)) \cdot f(r, T, \theta) \\ & - \alpha_i \frac{1}{n-1}(1-\tau)^2 f(r, T, \theta)^2 \sum_{j \neq i} (\max\{x_j - x_i, 0\})^2 \\ & - \beta_i \frac{1}{n-1}(1-\tau)^2 f(r, T, \theta)^2 \sum_{j \neq i} (\max\{x_i - x_j, 0\})^2 \end{aligned} \tag{S.12}$$

[2]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 in which income is measured. 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).

Maximising equation S.12 yields the preferred tax level of individual i:

$$\tau_i^b(x_1, \ldots, x_N; \theta) = 1 - \frac{x_i - \lambda\bar{x} + \delta_i y(1-\lambda)}{2\phi_i f(r, T, \theta)} \tag{S.13}$$

As before, some individuals will prefer a corner response of $\tau_i^b = 1$ (if $x_i > \lambda\bar{x} - \delta_i y(1-\lambda)$), while those who are sufficiently biased and/or care very little about inequality ($\phi_i f(r,T,\theta) \leq (x_i - \lambda\bar{x} + \delta_i y(1-\lambda))/2$) prefer the corner response of $\tau_i^b = 0$. In the following, I focus on individuals who prefer an intermediate tax rate.

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 S.13 that $\tau_i^* \geq \tau_i^b$.

By comparing Equation S.13 with Equation 5, one can see that introducing concerns for efficiency leads, ceteris paribus, to a preference for less redistribution (as this involves an efficiency loss). Naturally, the more individuals value efficiency, the lower is their 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 tiebreaker for incomes in the middle class. However, 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:[3]

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

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

$$\tau_i^b(x_1, \ldots, x_N; \theta) = \frac{\bar{x} - x_i + 2\phi_i f(r, T, \theta)}{2(1-\lambda)\bar{x} + 2\phi_i f(r, T, \theta)} \tag{S.15}$$

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. Assuming again that individual i prefers a tax between 0 and 1, 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 they are. 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. $\tau_i^* \geq \tau_i^b$).

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 proportional tax scheme. Specifically, I assume

[3] 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 $\tau\bar{x} - (1-\lambda)\tau\bar{x} = \lambda\tau\bar{x}$.

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' incomes.

The preferred tax for individuals with incomes below the mean is trivially set to the largest possible lump sum, as these obtain greater utility both from increased income and decreased inequality. In addition, individuals who are very biased and/or care very little about inequality prefer the corner solution of $\tau_i^b = 0$. Hence, I here examine the redistributive preferences for an individual i who prefers an intermediate tax level. is not too indifferent to inequality . For any time t with T remaining time periods, individual i's utility is then:[4]

$$\begin{aligned} U_i(x_i, ..., x_n) = &(x_i - \tau) \cdot f(r, T, \theta) \\ &- \alpha_i \frac{1}{n-1} f(r, T, \theta)^2 \sum_{j \neq i} (\max\{x_j - x_i, 0\})^2 \\ &- \beta_i \frac{1}{n-1} f(r, T, \theta)^2 \left[\sum_{x_j \geq \bar{x}} (\max\{(x_i - x_j), 0\})^2 + \sum_{x_j < \bar{x}} \left(x_i - x_j - \lambda\tau \frac{1}{1-p} \right)^2 \right] \end{aligned} \tag{S.16}$$

As individual i maximises Equation S.16 with respect to τ, their preferred tax under bias becomes:

$$\tau_i^b(x_1, \ldots, x_N; \theta) = \frac{1}{\lambda} \sum_{x_j < \bar{x}} (x_i - x_j) - \frac{1-p}{2\beta_i \frac{1}{n-1} f(r, T, \theta) \lambda^2} \tag{S.17}$$

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 individuals prefer a higher tax rate when they are 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.

[4]With this tax scheme, note that the tax does not influence disutility from inequality arising from the comparison with 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 because 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 ($\frac{\partial \tau_i^b}{\partial x_k}\Big|_{x_k<\bar{x}} = -\frac{1}{\lambda}(1-p) < 0$). 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.[5]

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 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.

[5] I did not expect every subject to provide useful responses that passed 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 expected 10 percent of subjects to fail one of the screeners that I employed in this study, corresponding roughly to what I found in the pilot study. In addition, it is common that many respondents opt out of the study without payment. I thus followed the recommendation by i.a. Aguinis et al. (2021) and over-recruited by 30 percent on MTurk, such that I invited in total 4,031 subjects to participate.

S.4 Attrition

In the following, I use logit regressions to examine what factors explain whether subjects complete the study (see Table S.5). For this analysis, I only consider subjects who pass all screeners until the point where they drop out. Moreover, 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 difference 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 because 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 S.5: 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

	(1)	(2)	(3)	(4)
			(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		0.1122		0.0846
		(0.11)		(0.11)
Some college, no degree		0.1591		0.1345
		(0.11)		(0.11)
Associate degree		0.1222		0.0989
		(0.11)		(0.11)
Bachelor's degree		0.1846*		0.1678
		(0.11)		(0.10)
Master's degree		0.1935*		0.1891*
		(0.11)		(0.11)
Doctorate or pro degree		0.2400**		0.2228**
		(0.11)		(0.11)
Self-employed		0.0195		0.0207
		(0.02)		(0.02)
Unemployed		0.0241		0.0134

	(1)	(2)	(3)	(4)
		(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

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

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

S.5 Heterogeneity in Subjects' Forecasts

S.5.1 Study 1

First, I look at what factors predict whether subjects answer correctly for all three income classes. A logit model reveals that men are 5 percentage points more likely to answer correctly, while Black or African American participants are 7 percentage points less likely to do so (see Table S.6). 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.7). 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.6: Characteristics of subjects who answer correctly for all incomes, 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		-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		-0.0965	-0.0952	-0.1020
		(0.1409)	(0.1393)	(0.1341)
Bachelor's degree		-0.0597	-0.0583	-0.0629
		(0.1405)	(0.1388)	(0.1335)
Master's degree		-0.0235	-0.0214	-0.0234
		(0.1424)	(0.1407)	(0.1356)
Doctorate or pro degree		-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

	(1)	(2)	(3)	(4)
		(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

Notes: 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.7: 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	0.162	0.164
	(0.173)	(0.178)
Some college, no degree	0.141	0.151
	(0.173)	(0.178)
Associate degree	0.122	0.130
	(0.174)	(0.179)

	(1)	(2)
Bachelor's degree	0.089	0.099
	(0.173)	(0.178)
Master's degree	0.084	0.096
	(0.174)	(0.179)
Doctorate or pro degree	-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
		(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

	(1)	(2)

Notes: 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.5.2 Study 2

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

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.9).

Table S.8: Characteristics of subjects who answer correctly for all incomes, 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)

	(1)	(2)	(3)	(4)
Some college, no degree		0.0287	0.0279	0.0269
		(0.0282)	(0.0277)	(0.0300)
Associate degree		0.0194	0.0208	0.0209
		(0.0329)	(0.0326)	(0.0351)
Bachelor's degree		0.0510*	0.0527**	0.0509*
		(0.0264)	(0.0259)	(0.0277)
Master's degree		0.1323***	0.1323***	0.1175***
		(0.0408)	(0.0401)	(0.0395)
Doctorate or pro degree		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
		(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)
$\text{Inequality Too Large}_p$				-0.0035
				(0.0420)
$\text{Government Responsibility}_p$				-0.0261
				(0.0340)
Observations	978	978	978	978

	(1)	(2)	(3)	(4)

Notes: 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.9: 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)
High school degree or equivalent	0.312*	0.311*
	(0.170)	(0.163)
Some college, no degree	0.305*	0.308*
	(0.169)	(0.163)
Associate degree	0.342**	0.348**
	(0.170)	(0.163)
Bachelor's degree	0.270	0.277*
	(0.169)	(0.162)
Master's degree	0.284*	0.297*
	(0.171)	(0.164)
Doctorate or pro degree	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

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

Notes: 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.6 Mechanisms

S.6.1 Study 2

S.6.1.1 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.28; 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.28; 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.29; 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.

S.7 Further Discussion

S.7.1 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.10). 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.[6] Across all three studies, subjects tend to underestimate wealth inequality, with their answers implying a Gini coefficient of .58 compared to the true value of .72 (2019, World Inequality Database).[7] Such underestimation

[6]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 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.

[7]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,

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.

Table S.10: 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

Notes: 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.

S.7.2 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.

respectively.

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 preference for more taxation.[8] Together, these findings demonstrate that self-interest indeed matters in the current voting experiment.

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.20, S.31, and S.40). 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 the 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 (Studies 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.47 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. To examine the influence of image concerns, I use the answers to the 10-item Martin-Larsen Approval Motivation Scale (MLAMS). This scale asks subjects to rate on a 5-point Likert scale from 'Disagree Strongly' to 'Agree Strongly' items such as 'I

[8]The importance of self-interest also receives qualitative support by statements from subjects. For instance, a middle-income subject in *RealizedR* states: 'I chose a tax rate of 0% because the table indicated that would result in the highest amount for me.' Even when subjects consider what is fair, subjects often trade-off fairness with self-interest. For example, a middle-income subject in *ForecastR* underestimated the personal costs of redistribution and stated as follows: '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.'

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).

I find that image concerns as measured by MLAMS do not correlate with subjects' preferred tax rates in any of the current studies (all $p's > .118$, cf. Tables S.21, S.32, and S.41). 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). 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).

Other Concerns. As explained in the main part of the paper, the theory assumes that individuals are motivated by their own income and equality in the group. For 'poor' subjects, 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 can examine the poor subjects. Testing the corner prediction of full redistribution is difficult, 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.

Across all treatments, 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 S.13). Still, only 43 percent of the poor vote for full redistribution. This suggests that other concerns may influence how subjects vote. For example, one subject in *Ratio* noted that she 'felt guilty taking a large amount of someone else's money'.[9] 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). As a proxy for tax aversion, I use right-wing political attitudes in a tobit regression that controls for givings in a dictator game (as political preferences also correlate with social preferences, Kerschbamer and Müller, 2020). Here, I find that moving from the extreme left to the extreme right predicts a decrease in the preferred tax rate of 44 percentage points among the poor ($p < .001$). Finally, some poor subjects might be concerned with the total income 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: $p = .160$).

Looking at Study 2, I find that 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.30) and robust (MWU: $p < .001$). Also in Study 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 the preferred tax rate among the poor (67 percentage points, $p < .001$). Efficiency concerns are once more statistically insignificant ($p = .466$).

In Study 3, the median tax rate is again 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.39) 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 the preferred tax rate among the poor of 42 percentage points ($p = .003$). Efficiency concerns are again not statistically significant ($p = .686$).

S.7.3 Effect of Making a Forecast

The analyses of Studies 1-3 demonstrate that the level of inequality generally has no effect on subjects' preferences for redistribution, regardless of whether subjects first engage in the forecasting task. A different question concerns whether preferences for redistribution are in-

[9] 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.'

fluenced by witnessing the level of inequality in round 1. For instance, the initial distribution may serve as a reference point, leading subjects to believe that if all income classes earn more than the initial levels, there is no need for redistributing income. 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 subjects in *Realized* make a forecast while subjects in *RealizedNo* do not. If the initial incomes serve as reference points, subjects in *Realized* should be less inclined to redistribute, as the poor are well-off in the sense that their income is 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.19), and this difference is robust (additional controls in Table S.44; SCLS: $p = .017$; MWU: $p = .033$).

S.7.4 Inequality Concepts and Preferences for Redistribution.

There are many ways to conceive and operationalise inequality (Kolm, 1976; Cowell, 2016), making inequality an essentially contested concept (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 incomes 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.48, 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.20). 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.42, none of these conceptualisations are significant predictors of the tax rate for which a subject votes.[10] 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 for which a subject votes (see Table S.43).

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 willingness-to-pay 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.8 Additional Tables and Figures, Study 1

[10] 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 × (4 treatments + pooling of treatments).

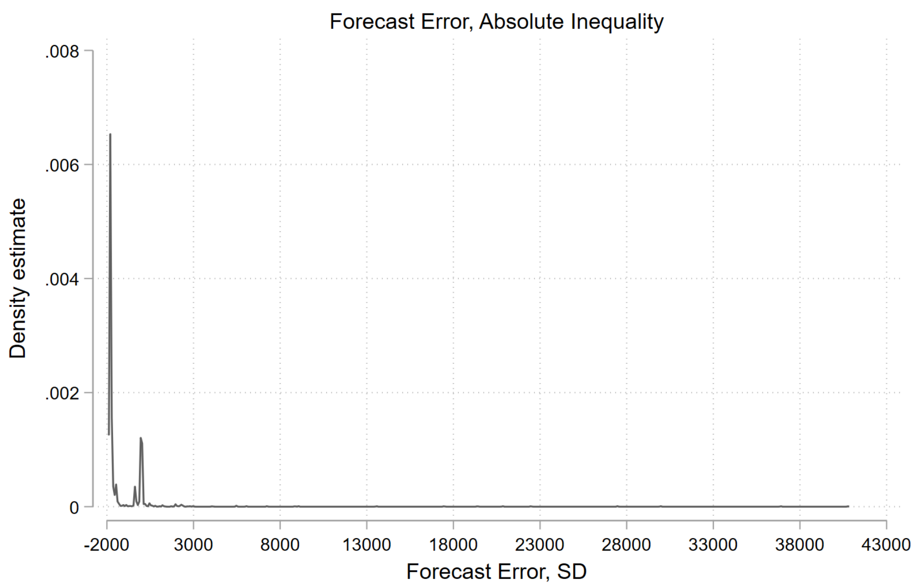
Table S.11: Sample characteristics, Study 1

	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 S.12: Summary statistics by treatment, Study 1

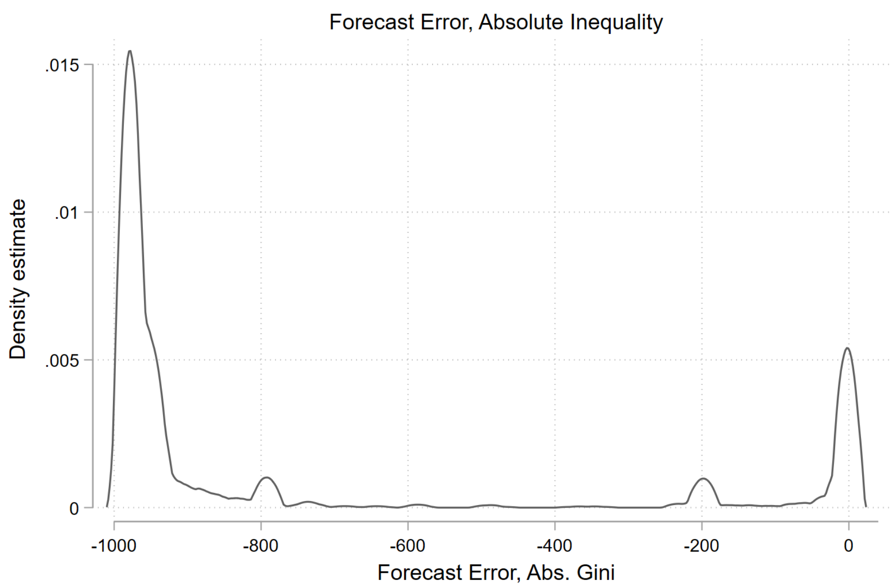
	Forecast	Ratio	Realized	Total
Risk	0.47	0.47	0.46	0.47
Trust	0.53	0.54	0.53	0.53
Political Right	0.44	0.43	0.42	0.43
Belief in Meritocracy	0.63	0.62	0.59	0.62
Inequality Too Large	0.80	0.81	0.80	0.81
Government Responsibility	0.57	0.57	0.59	0.58
MLAMS	0.37	0.38	0.38	0.38
Social Ladder	0.43	0.43	0.42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

Figure S.7: Forecast error of absolute inequality, Study 1, full sample



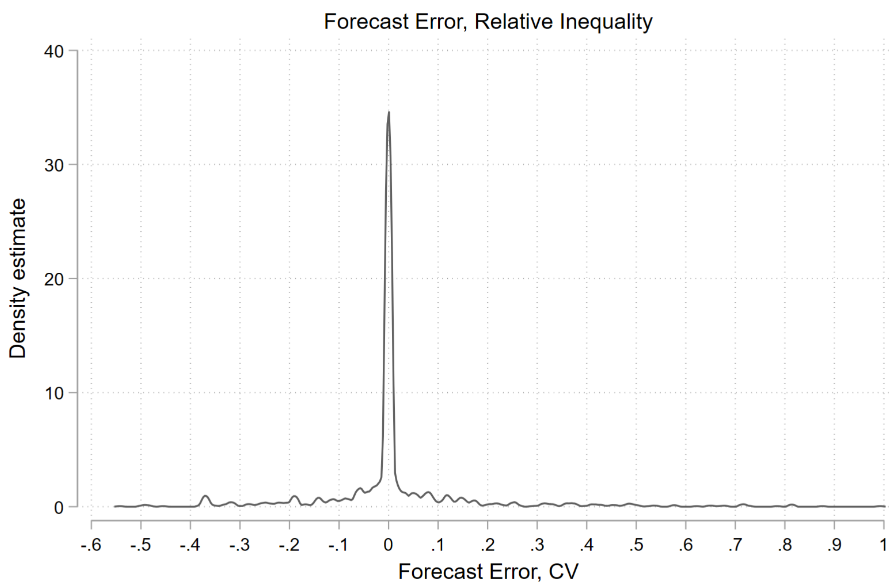
Notes: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$.

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



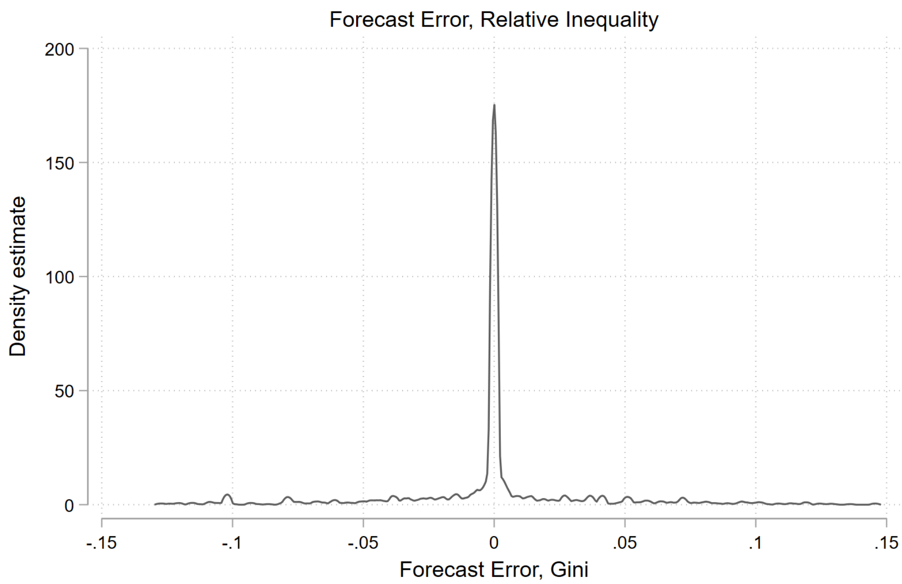
Notes: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 10$). The Absolute Gini coefficient is calculated as $AG(\mathbf{x}) = \bar{x}\left(\frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i\right)$, where x_i are ranked-ordered incomes such that $x_i \leq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

Figure S.9: Forecast error of relative inequality, Study 1, full sample



Notes: 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.10: Forecast error of relative inequality, Gini, Study 1



Notes: 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.13: 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

Notes: 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.14: EGB and tax inconsistency, Study 1

	(1)	(2)	(3)	(4)	(5)	(6)
Ratio	-8.31*	-8.41*	-7.85*	-7.49*	0.08	-1.23
	(4.69)	(4.56)	(4.53)	(4.32)	(4.44)	(4.44)
Realized	-11.87***	-12.68***	-12.98***	-13.88***	-6.53	-2.58
	(4.49)	(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			-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			-23.46	-19.14	-26.34	-25.14
			(24.57)	(23.45)	(23.07)	(22.86)
Bachelor's degree			-17.67	-16.47	-21.13	-19.67
			(24.21)	(23.12)	(22.72)	(22.51)
Master's degree			-9.23	-10.19	-16.05	-15.28

	(1)	(2)	(3)	(4)	(5)	(6)
			(24.58)	(23.47)	(23.08)	(22.86)
Doctorate or pro degree			-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
			(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	1013

Notes: 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 they 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.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.15: EGB and tax inconsistency, restricted sample in Study 1

	(1)	(2)	(3)	(4)	(5)	(6)
Ratio	-9.14*	-9.31*	-8.67*	-7.79*	0.67	-0.89
	(4.89)	(4.79)	(4.77)	(4.54)	(4.68)	(4.68)
Realized	-11.38**	-12.59***	-12.98***	-14.01***	-5.77	-0.62
	(4.69)	(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)
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			-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			-15.32	-9.37	-16.82	-14.59
			(26.47)	(25.34)	(24.90)	(24.65)
Bachelor's degree			-5.33	-2.50	-7.20	-4.73
			(26.14)	(25.03)	(24.58)	(24.33)
Master's degree			2.98	4.44	-2.25	-1.07
			(26.54)	(25.43)	(24.97)	(24.71)
Doctorate or pro degree			-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)	(6)
				(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	918

Notes: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The sample is restricted to those subjects who do not provide correct answers for all inequality forecasts. Perceived gains is a dummy equal to one if the subject mistakenly believes they 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.16: 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)
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)

	(1)	(2)	(3)	(4)	(5)
High school degree or equivalent		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		-0.39	-7.70	-16.30	-12.20
		(31.90)	(30.28)	(29.00)	(28.98)
Bachelor's degree		-0.69	-11.85	-14.12	-9.77
		(31.30)	(29.74)	(28.45)	(28.46)
Master's degree		13.33	-3.41	-9.56	-6.08
		(32.15)	(30.54)	(29.23)	(29.17)
Doctorate or $_p$pro degree		-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*
			(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)

	(1)	(2)	(3)	(4)	(5)
Low Personal Cost					11.17
					(7.86)
N	349	349	349	349	349

Notes: 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 they 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.17: 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)
Other ethnicity		59.86	81.62**
		(39.47)	(39.75)
High school degree or equivalent		-85.13**	-76.89*
		(40.96)	(39.89)
Some college, no degree		-81.70**	-79.43**
		(40.67)	(39.46)
Associate degree		-73.79*	-71.52*
		(41.45)	(40.30)
Bachelor's degree		-75.38*	-74.04*
		(40.82)	(39.65)

	(1)	(2)	(3)
Master's degree		-76.05*	-74.06*
		(41.41)	(40.18)
Doctorate or $_p$pro degree		-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
			(11.33)
MLAMS$_p$			-16.26
			(18.61)
N	359	359	359

Notes: 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.18: Descriptive statistics for poor subjects, Study 1

	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

Notes: 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.19: 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

Notes: 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). 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.

Table S.20: 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)
Male			-7.26*	-5.01
			(3.74)	(3.64)

	(1)	(2)	(3)	(4)
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			-25.58	-16.01
			(24.39)	(23.45)
Some college, no degree			-17.25	-14.65
			(24.01)	(23.08)
Associate degree			-24.21	-19.14
			(24.40)	(23.45)
Bachelor's degree			-18.54	-16.47
			(24.04)	(23.12)
Master's degree			-10.19	-10.19
			(24.41)	(23.47)
Doctorate or pro degree			-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***
				(7.88)
Meritocracy_p				-14.01*
				(7.46)
$\text{Inequality Too Large}_p$				22.50***
				(8.34)

	(1)	(2)	(3)	(4)
Government Responsibility$_p$				10.77
				(7.28)
MLAMS$_p$				-7.09
				(11.83)
Observations	1013	1013	1013	1013

Notes: 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.21: 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)
High school degree or equivalent			-23.59	-16.01
			(24.57)	(23.45)
Some college, no degree			-16.32	-14.65
			(24.20)	(23.08)
Associate degree			-23.15	-19.14
			(24.58)	(23.45)

	(1)	(2)	(3)	(4)
Bachelor's degree			-17.55	-16.47
			(24.22)	(23.12)
Master's degree			-9.25	-10.19
			(24.59)	(23.47)
Doctorate or pro degree			-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)
Observations	1013	1013	1013	1013

Notes: 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.9 Additional Tables and Figures, Study 2

Table S.22: 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.23: 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.24: 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

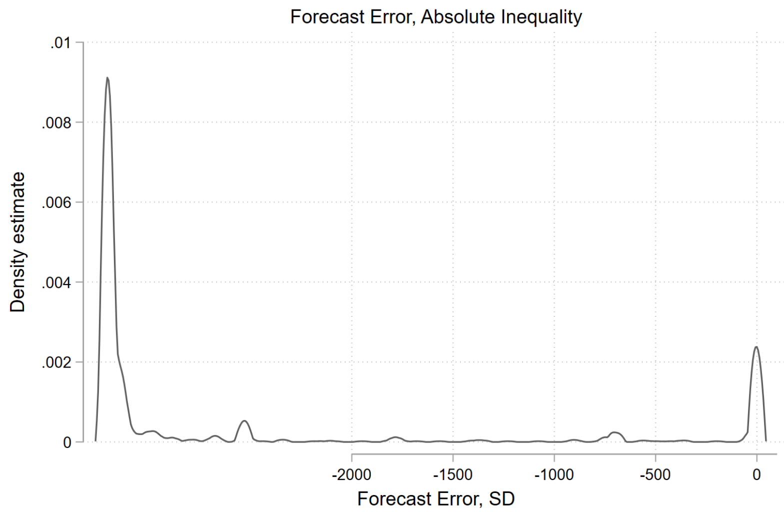
Notes: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. Efficiency corresponds to subjects' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and 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.25: 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

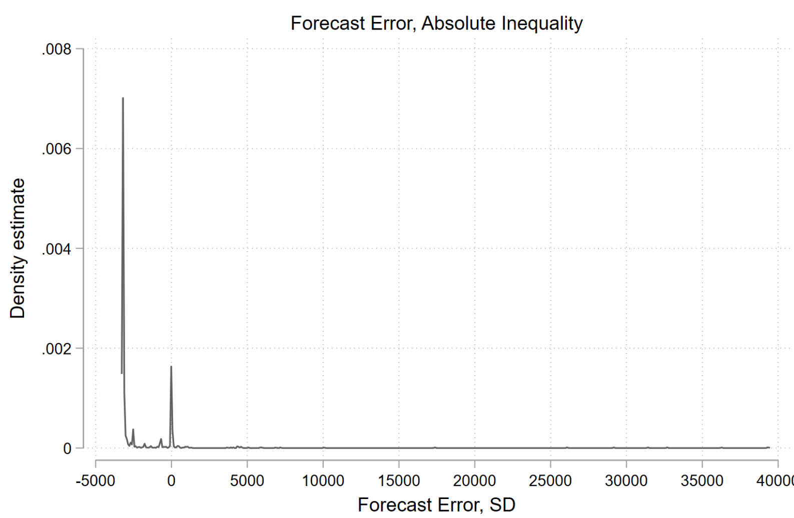
Notes: averages are taken over all middle-income and rich subjects in a treatment. DG is the share that subjects give as dictators in the standard dictator game. Efficiency corresponds to subjects' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and 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.11: Forecast error of absolute inequality, Study 2



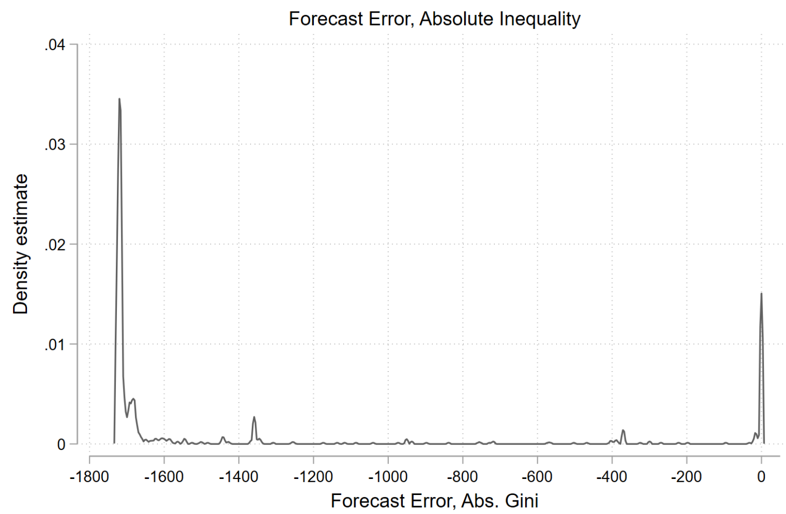
Notes: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors. For the full sample, see Figure S.12.

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



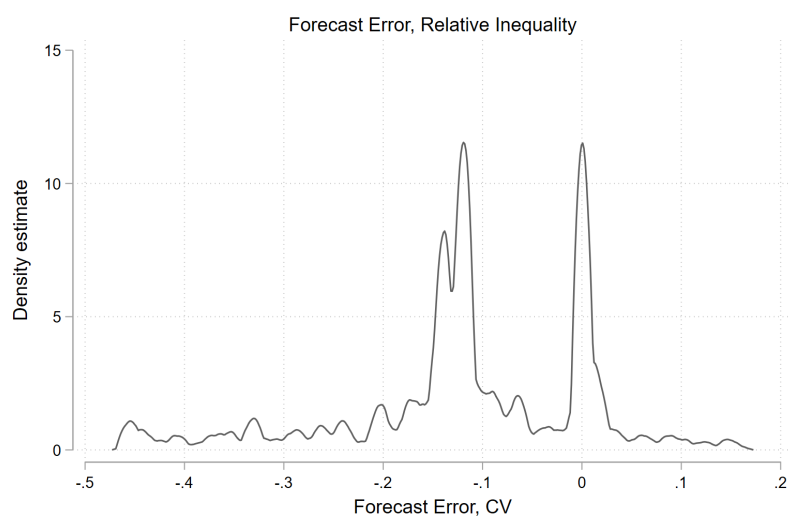
Notes: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$.

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



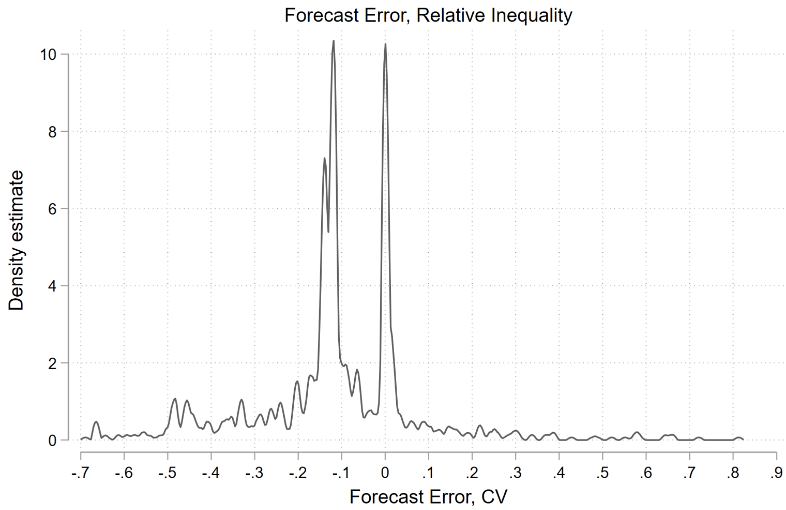
Notes: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 10$). The Absolute Gini coefficient is calculated as $AG(\mathbf{x}) = \bar{x}\left(\frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i\right)$, where x_i are ranked-ordered incomes such that $x_i \leq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

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



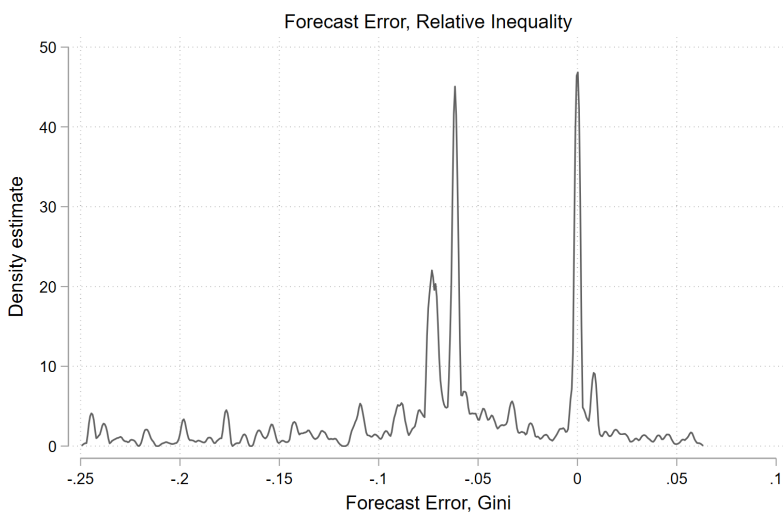
Notes: the figure shows the kernel density of subjects' forecast error (epanechnikov, $bw = 0.005$). The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors. For the full sample, see Figure S.15.

Figure S.15: Forecast error of relative inequality, Study 2, full sample



Notes: 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.16: Forecast error of relative inequality, Gini, Study 2



Notes: 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.26: EGB and tax inconsistency, Study 2

	(1)	(2)	(3)	(4)	(5)	(6)
RealizedR	1.91	1.57	1.16	1.91	4.94	7.30*
	(4.17)	(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)
High school degree or equivalent			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			26.90	39.44	36.31	33.56
			(50.34)	(48.52)	(48.16)	(48.14)
Bachelor's degree			43.13	56.16	53.24	50.74
			(50.16)	(48.35)	(47.99)	(47.97)
Master's degree			50.64	60.07	57.94	55.04
			(50.40)	(48.58)	(48.22)	(48.20)
Doctorate or pro degree			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

	(1)	(2)	(3)	(4)	(5)	(6)
				(8.78)	(8.72)	(8.71)
Meritocracy$_p$				-19.90**	-19.77**	-20.49**
				(8.55)	(8.49)	(8.50)
Inequality Too Large$_p$				5.15	4.77	5.50
				(9.80)	(9.73)	(9.73)
Government Responsibility$_p$				20.14**	18.97**	18.82**
				(8.07)	(8.02)	(8.01)
MLAMS$_p$				-1.29	0.76	1.18
				(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	698

Notes: 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 they 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.
* $p < .10$, ** $p < .05$, *** $p < .01$.

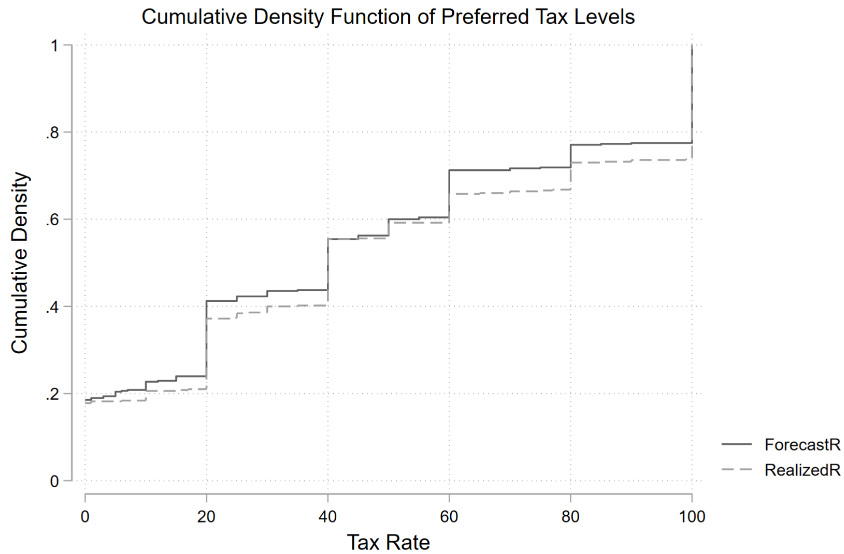
Table S.27: EGB and tax inconsistency, restricted sample in Study 2

	(1)	(2)	(3)	(4)	(5)	(6)
RealizedR	2.75	1.80	1.04	1.28	4.68	7.52
	(4.34)	(4.20)	(4.18)	(4.04)	(4.18)	(4.57)
Dictator Giving		0.76***	0.78***	0.68***	0.67***	0.67***
		(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Age			-0.14	-0.03	-0.03	-0.04
			(0.20)	(0.19)	(0.19)	(0.19)
Male			-2.59	-2.88	-3.16	-2.84
			(4.38)	(4.28)	(4.25)	(4.25)
Black or African American			-6.82	-8.11	-7.35	-7.02
			(6.89)	(6.79)	(6.74)	(6.73)
Hispanic or Latino			14.08	11.46	9.67	9.39
			(10.59)	(10.26)	(10.18)	(10.18)
Asian American			-6.68	-5.92	-4.83	-5.25
			(8.03)	(7.84)	(7.79)	(7.78)
Other			25.47*	29.46*	29.74**	30.06**
			(15.38)	(15.20)	(15.07)	(15.06)
High school degree or equivalent			24.65	41.19	38.93	36.40
			(50.06)	(48.18)	(47.79)	(47.74)
Some college, no degree			33.00	49.03	46.20	43.07
			(49.91)	(48.05)	(47.66)	(47.63)
Associate degree			26.86	40.00	36.40	33.07
			(50.02)	(48.12)	(47.74)	(47.71)

	(1)	(2)	(3)	(4)	(5)	(6)
Bachelor's degree			42.36	57.59	54.22	51.18
			(49.82)	(47.95)	(47.56)	(47.53)
Master's degree			52.38	63.65	61.04	57.49
			(50.10)	(48.22)	(47.82)	(47.80)
Doctorate or pro degree			65.34	72.49	69.54	66.70
			(50.99)	(49.06)	(48.67)	(48.62)
Self-employed			2.91	3.19	3.18	3.14
			(6.27)	(6.13)	(6.08)	(6.07)
Unemployed			11.03	12.07	13.34*	12.59*
			(7.70)	(7.48)	(7.44)	(7.44)
Student			5.06	-1.48	0.78	1.21
			(11.48)	(11.17)	(11.10)	(11.09)
Retired			-7.33	-10.33	-10.33	-10.42
			(10.76)	(10.57)	(10.49)	(10.48)
Other			-9.58	-10.19	-8.38	-9.63
			(13.00)	(12.68)	(12.58)	(12.59)
Income_p			-3.46	8.07	7.55	8.41
			(12.46)	(12.51)	(12.41)	(12.40)
Efficiency from MDG				-2.35***	-2.37***	-2.39***
				(0.82)	(0.81)	(0.81)
Risk_p				-6.44	-5.32	-5.88
				(8.39)	(8.33)	(8.33)
Trust_p				20.24**	18.86**	18.93**
				(9.07)	(9.01)	(8.99)
Political Right_p				1.11	-0.08	-0.07
				(9.01)	(8.94)	(8.93)
Meritocracy_p				-19.45**	-18.89**	-19.54**
				(8.96)	(8.89)	(8.89)
Inequality Too Large_p				6.22	5.52	6.16
				(10.05)	(9.98)	(9.98)
Government Responsibility_p				24.61***	23.43***	23.35***
				(8.29)	(8.23)	(8.22)
MLAMS_p				-0.57	1.80	2.39
				(13.88)	(13.80)	(13.79)
Perceived Gains					22.41***	25.16***
					(7.83)	(8.03)
Low Personal Cost						10.80
						(7.03)
Observations	634	634	634	634	634	634

Notes: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. The sample is restricted to those subjects who do not provide correct answers to all inequality forecasts. Perceived gains is a dummy equal to one if the subject mistakenly believes they 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.17: Preferred tax rate by treatment



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

Table S.28: 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		24.63	45.47	44.18	42.18
		(48.39)	(46.65)	(46.00)	(45.72)

	(1)	(2)	(3)	(4)	(5)
Some college, no degree		28.16	47.41	45.15	41.42
		(47.86)	(46.18)	(45.53)	(45.28)
Associate degree		18.86	34.25	31.47	27.03
		(48.13)	(46.34)	(45.70)	(45.47)
Bachelor's degree		39.77	58.19	55.67	51.94
		(47.75)	(46.06)	(45.42)	(45.17)
Master's degree		53.79	69.24	68.28	63.05
		(48.26)	(46.57)	(45.92)	(45.70)
Doctorate or $_p$pro degree		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)
Perceived Gains				20.80***	23.27***
				(7.44)	(7.50)
Low Personal Cost					15.33**
					(7.23)

	(1)	(2)	(3)	(4)	(5)
N	345	345	345	345	345

Notes: 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 they 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.29: 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		10.22	9.91
		(14.14)	(13.70)
Bachelor's degree		17.03	14.33
		(11.46)	(11.18)
Master's degree		19.98	13.43
		(13.04)	(12.82)
Doctorate or pro degree		39.10**	29.19*
		(17.26)	(16.86)

	(1)	(2)	(3)
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

Notes: 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.30: 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

Notes: 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.31: 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			29.04	40.66
			(49.93)	(48.59)
Some college, no degree			37.55	48.01
			(49.77)	(48.45)
Associate degree			29.63	39.44
			(49.86)	(48.52)
Bachelor's degree			47.79	56.16

	(1)	(2)	(3)	(4)
			(49.69)	(48.35)
Master's degree			53.68	60.07
			(49.92)	(48.58)
Doctorate or pro degree			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)
Inequality Too Large_p				5.15
				(9.80)
Government Responsibility_p				20.14**
				(8.07)
MLAMS_p				-1.29
				(13.48)
Observations	698	698	698	698

Notes: 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.32: 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			25.59	40.66
			(50.41)	(48.59)
Some college, no degree			34.07	48.01
			(50.25)	(48.45)
Associate degree			27.40	39.44
			(50.35)	(48.52)
Bachelor's degree			43.31	56.16
			(50.15)	(48.35)
Master's degree			50.72	60.07
			(50.39)	(48.58)
Doctorate or pro degree			65.68	71.03
			(51.22)	(49.38)
Self-employed			3.47	3.04
			(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

	(1)	(2)	(3)	(4)
			(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)
$\text{Inequality Too Large}_p$				5.15
				(9.80)
$\text{Government Responsibility}_p$				20.14**
				(8.07)
Observations	698	698	698	698

Notes: 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.10 Additional Tables and Figures, Study 3

Table S.33: 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	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.34: 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.35: 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	539	100.0

Notes: for comparison, subjects in *RealizedNo* faced an absolute inequality of SD = 1832 (as in Study 1). See Figure S.18 for an illustration of the average preferred tax rate across all levels of absolute inequality.

Table S.36: EGB and tax inconsistency, Study 3

	(1)	(2)	(3)	(4)	(5)
RealizedNo	-8.67*	-8.32*	-8.89**	-10.95***	2.47
	(4.66)	(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			-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			-50.36	-45.86	-43.24
			(31.55)	(29.29)	(29.07)
Bachelor's degree			-47.04	-50.74*	-47.46*
			(31.09)	(28.90)	(28.70)
Master's degree			-51.50	-54.36*	-49.55*
			(31.46)	(29.28)	(29.07)
Doctorate or pro degree			-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
			(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***

	(1)	(2)	(3)	(4)	(5)
				(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	785

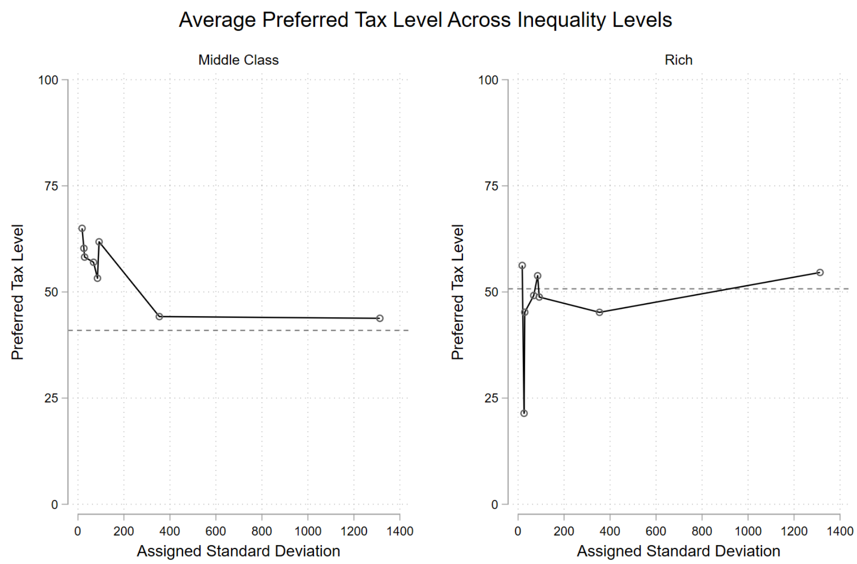
Notes: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Low Personal Cost is a dummy equal to one if the subject mistakenly believes that redistribution will come at almost no personal costs ($3, corresponding to a payment of USD 0.0015). Variables with 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.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.37: Descriptive statistics for poor subjects, Study 3

	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

Notes: 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).

Figure S.18: Tax across inequality levels



Notes: 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.38: 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

Notes: 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.39: 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

Notes: 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.40: 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			-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			-58.57*	-45.86
			(31.85)	(29.29)
Bachelor's degree			-56.56*	-50.74*
			(31.39)	(28.90)
Master's degree			-57.31*	-54.36*
			(31.77)	(29.28)
Doctorate or pro degree			-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

Notes: 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.41: 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			-47.06	-46.19
			(31.46)	(29.17)
Some college, no degree			-40.63	-41.08
			(31.13)	(28.86)
Associate degree			-49.71	-45.86
			(31.59)	(29.29)
Bachelor's degree			-46.10	-50.74*
			(31.15)	(28.90)
Master's degree			-50.46	-54.36*
			(31.54)	(29.28)
Doctorate or pro degree			-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)
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

	(1)	(2)	(3)	(4)
			(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)
$\text{Inequality Too Large}_p$				29.21***
				(10.26)
$\text{Government Responsibility}_p$				22.14**
				(8.95)
Observations	785	785	785	785

Notes: 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.11 Additional Tables and Figures, Discussion

Table S.42: 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
$(\text{Rich-Poor})^2$	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

Notes: 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 they will gain from taxation.

Table S.43: 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

Notes: 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 they will gain from taxation.

Table S.44: Effect of making a forecast

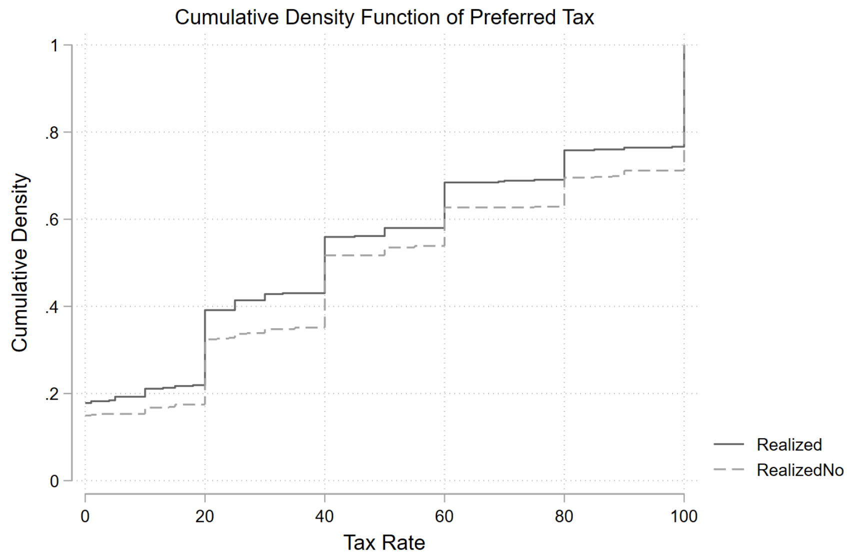
	(1)	(2)	(3)
Realized	-9.26**	-8.34**	-6.45*
	(4.09)	(4.07)	(3.86)
Dictator Giving	0.77***	0.77***	0.68***
	(0.10)	(0.10)	(0.10)
Age		-0.30	0.02
		(0.20)	(0.19)
Male		-5.72	-2.09
		(4.14)	(4.00)
Black or African American		-8.34	-8.68
		(7.75)	(7.32)
Hispanic or Latino		-5.23	-6.29
		(10.35)	(9.88)
Asian American		4.56	0.16
		(7.98)	(7.55)
Other ethnicity		39.34**	38.85**
		(16.25)	(15.51)
High school degree or equivalent		-48.38*	-43.92*
		(26.34)	(24.84)
Some college, no degree		-36.06	-35.26
		(25.88)	(24.37)
Associate degree		-39.42	-36.92
		(26.44)	(24.94)
Bachelor's degree		-40.27	-41.27*
		(25.90)	(24.38)
Master's degree		-39.19	-41.31*
		(26.28)	(24.75)
Doctorate or pro degree		-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***

	(1)	(2)	(3)
			(0.77)
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

Notes: 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.

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

Figure S.19: Preferred tax rate by treatment, Study 3



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

Table S.45: 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		0.0202	0.0336
		(0.0760)	(0.0730)
Some college, no degree		0.0377	0.0428
		(0.0755)	(0.0725)

	(1)	(2)	(3)
Associate degree		0.0397	0.0439
		(0.0760)	(0.0730)
Bachelor's degree		0.0532	0.0578
		(0.0753)	(0.0724)
Master's degree		0.0685	0.0650
		(0.0759)	(0.0730)
Doctorate or pro degree		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)
$\text{Inequality Too Large}_p$			0.0937***
			(0.0179)
$\text{Government Responsibility}_p$			-0.0615***
			(0.0161)
MLAMS_p			-0.0341
			(0.0233)
Constant	-0.1191***	-0.1105	-0.1001
	(0.0059)	(0.0767)	(0.0778)
Observations	1589	1589	1589

	(1)	(2)	(3)

Notes: 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.46: 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		-0.0221	-0.0061
		(0.0791)	(0.0758)
Some college, no degree		0.0008	0.0094
		(0.0786)	(0.0754)
Associate degree		0.0023	0.0098
		(0.0792)	(0.0759)
Bachelor's degree		0.0147	0.0219
		(0.0785)	(0.0753)
Master's degree		0.0388	0.0382
		(0.0791)	(0.0760)
Doctorate or pro degree		0.0164	0.0173
		(0.0803)	(0.0771)
Self-employed		0.0196*	0.0166
		(0.0118)	(0.0116)
Unemployed		-0.0136	-0.0131
		(0.0130)	(0.0129)
Student		0.0318	0.0280

	(1)	(2)	(3)
		(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)
$\text{Inequality Too Large}_p$			0.1034***
			(0.0189)
$\text{Government Responsibility}_p$			-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

Notes: OLS regressions with Gini error (Gini-Belief - Actual Gini) as dependent variable. The sample is restricted to those subjects who do not provide correct answers to all 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.
* $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.47: Differences in effect of efficiency concerns across treatments, Wald chi-square tests

	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

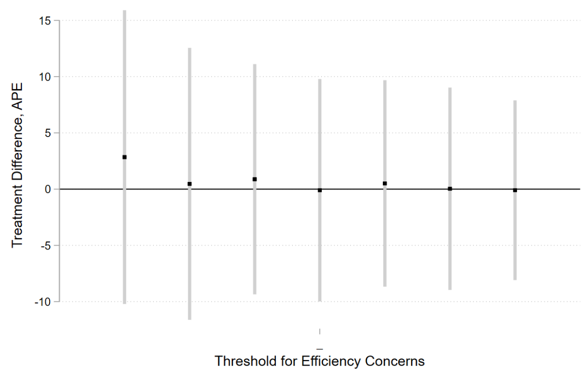
Table S.48: Tax preferences across growth paradigms

	(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		-91.10**	-81.67**
		(40.80)	(39.69)
Some college, no degree		-85.65**	-79.72**
		(40.62)	(39.50)
Associate degree		-82.05**	-74.98*
		(40.94)	(39.82)
Bachelor's degree		-78.68*	-73.52*
		(40.59)	(39.46)
Master's degree		-78.26*	-74.62*
		(40.91)	(39.73)
Doctorate or pro degree		-62.62	-67.50*
		(41.70)	(40.51)
Self-employed		7.64	7.14

	(1)	(2)	(3)
		(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

Notes: 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.20: Comparing Realized and RealizedR for different efficiency concerns



Notes: 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.49: 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***
				(3.15)
Black or African American				-7.59
				(5.80)

	(1)	(2)	(3)	
Hispanic or Latino				2.70
				(7.70)
Asian American				-7.78
				(6.67)
Other ethnicity				17.07
				(11.01)
High school degree or equivalent				-47.27**
				(22.23)
Some college, no degree				-41.12*
				(21.98)
Associate degree				-55.22**
				(22.36)
Bachelor's degree				-41.06*
				(21.97)
Master's degree				-38.30*
				(22.27)
Doctorate or pro degree				-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

	(1)	(2)	(3)

Notes: 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 they 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.12 Instructions

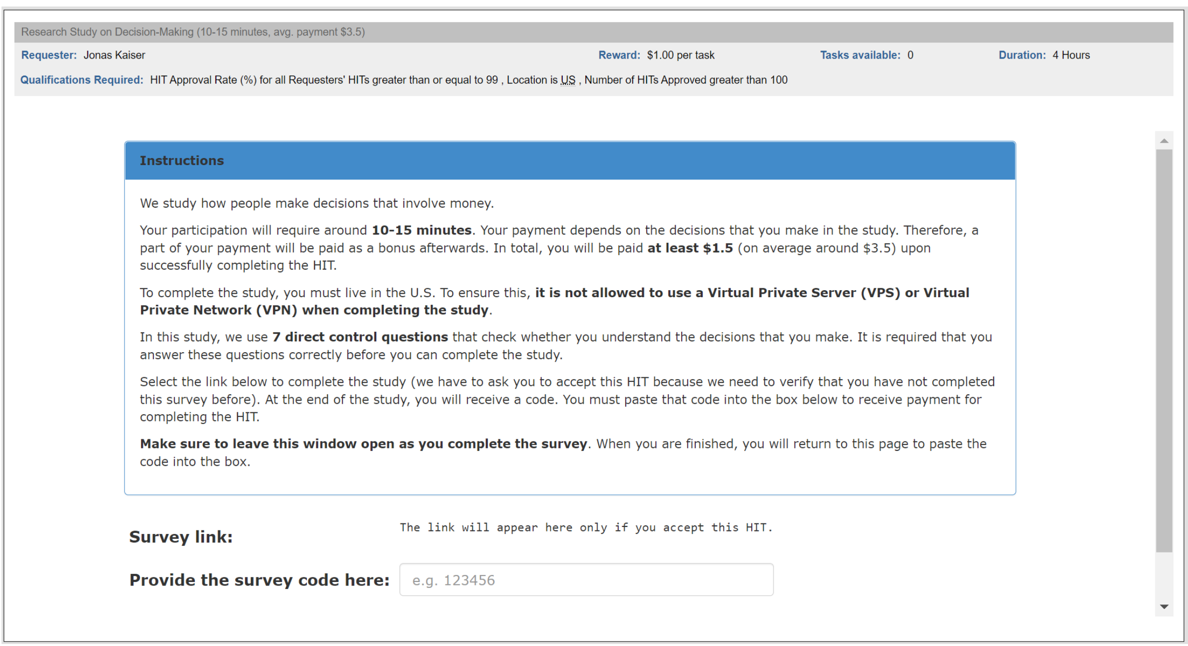
On the following pages, I include the instructions for a subject who is randomly assigned to the *Forecast* treatment and the middle income class. Afterwards, I include the instructions that vary across the different treatments, using again the example of a subject randomised into the middle class for 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 B: 30 cents Total: 60 cents	A: 40 cents B: 25 cents Total: 65 cents	A: 50 cents B: 20 cents Total: 70 cents	A: 60 cents B: 15 cents Total: 75 cents	A: 70 cents B: 10 cents Total: 80 cents	A: 80 cents B: 5 cents Total: 85 cents	A: 90 cents B: 0 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 %

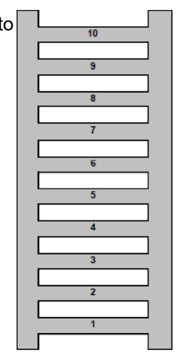
To what extent do you agree or disagree with the following statement: **Differences in income in the United States are too large.**

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.

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Total	$22619	$22528	$22437	$22346	$22258	$22169

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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.