How People Misunderstand Growth and Inequality – and the Effects of Correcting Their Beliefs

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Abstract

Income and wealth grow over time, and this leads to changes in the level of inequality in a society. Yet, a large literature in cognitive psychology suggests that individuals often struggle to understand the effect of exponential growth. Misunderstanding how growth influences inequality may lead to biased preferences for policies with long-term effects, from taxation to investments in education. In an incentivised experiment, I examine (i) whether individuals are able to predict how exponential economic growth influences inequality, and (ii) whether informing individuals about the actual development in inequality influences their preferences for redistribution. I find that most people underestimate how much inequality increases in the presence of growth. However, informing individuals about the actual development in inequality does not affect their 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 experiment, taxation, forecasts, exponential growth bias

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1 Introduction

Economic growth is typically perceived as a positive development, often associated with improving living standards and widespread prosperity. Because economic growth tends to be exponential, even modest increases in growth rates can lead to substantial benefits in the long run. However, the distribution of these benefits can vary significantly across different groups in the population, and this has important implications for income and wealth inequality. Notably, even if all incomes grow at the same rate – leaving relative inequality unchanged – the absolute differences between individuals increase.[1] And if larger incomes grow at higher rates, then both relative and absolute inequality increase. In the US, for example, Piketty et al. (2018) show that while pre-tax income growth rates were fairly uniform from 1946 to 1980, both absolute and relative inequality increased drastically from 1980 to 2014, as the bottom 50 percent, the next 40 percent, and the top 10 percent experienced yearly growth rates of 0.03, 1.04, and 2.36 percent, corresponding to overall growths of 1, 42, and 121 percent.

However, people may not realise how growth influences the level of inequality, as exponential developments are inherently difficult to understand (cf. 'exponential growth bias', Wagenaar and Sagaria, 1975). Many citizens are concerned about the effects of policies and social investments with long-term impacts (Neimanns et al., 2018; Christensen and Rapeli, 2021; Busemeyer, 2024). But failing to understand the effect of exponential growth could lead to biases in policy preferences, as perceptions of inequality are often more central to policy support than the actual inequality in society (Marino et al., 2024; Stantcheva, 2024). And biased preferences may translate into biased policies, as voters' preferences for long-term policies have been shown to impact policy-making (see e.g. Busemeyer et al., 2020, for the case of education policy and Schaffer et al., 2022, for climate policy).

Failing to understand developments of inequality could be critical for two reasons: first, with respect to future growth, beliefs about how inequality will develop may influence decisions with long-term effects on incomes and wealth, including taxation, investments in children's equal access to education, and wage negotiations. While it is sometimes feasible to redistribute at a later point when the inequality has already risen, it is often more difficult and expensive to do so (Heckman, 2006; Bhalotra et al., 2017; Hjort et al., 2017; Bütikofer et al., 2019; Schiariti et al., 2021). Second, with respect to past growth, beliefs about how inequality has developed previously will influence individuals' perceptions about the current level of inequality unless people continuously update their beliefs (which is highly unlikely, e.g. due to rational

[1] By definition, absolute inequality is translation-invariant (adding the same amount to all incomes does not change inequality), whereas relative inequality is scale-invariant (multiplying all incomes with the same factor does not change inequality).

inattention, Sims, 2003).

In this paper, I therefore conduct an experiment to examine (i) the accuracy of individuals' inequality forecasts in the presence of exponential economic growth and (ii) how information about the actual development in inequality causally influences preferences for redistribution. I use an experimental approach to ensure that individuals make incentivised choices and 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.

In the experiment, participants predict how inequality develops in a group where all incomes grow exponentially over multiple rounds. In the main treatments, all incomes grow at the same rate (changed in Extension 1). This implies that absolute inequality increases exponentially, whereas relative inequality is constant. Participants' forecasts provide a measure of their ability to anticipate how absolute and relative inequality evolve when incomes grow. Then, participants decide on how to redistribute incomes in the final round with a tax-transfer scheme. I use a between-subjects design to examine the causal effect of informing individuals about the true development of inequality. In particular, some participants are informed about post-redistribution incomes based on their own forecasts (treatment *Forecast*), while others learn the actual incomes (*Realized*). A third treatment (*Ratio*) further examines the effect of the type of forecasting errors individuals make.

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. Importantly, informing individuals about the actual development of inequality influences both their perceived benefits and costs of redistribution: on the one hand, when inequality is larger, inequality averse individuals may perceive a greater need for redistribution. 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 correcting beliefs will thus depend on whether the change in perceptions of benefits or personal costs of redistribution matter the most. The theoretical model assumes that individuals experience increasing marginal disutility from inequality (as detailed in Section 3), and it therefore yields the prediction that individuals who underestimate how much growth influences inequality (e.g. in *Forecast*) vote for less redistribution than they would if they correctly estimated the development in inequality (e.g. in *Realized*).

I find that participants underestimate how much absolute inequality increases in the presence of growth, but they are better at predicting that there is no change in relative inequality. However, informing participants about the development in inequality does not influence their demand for redistribution unless they misperceive their personal costs of redistribution in one of two ways: first, some net contributors make forecasts that wrongly imply that they would gain from redistribution. Providing these individuals with information (*Realized*) leads them to vote for tax rates that are 42 percentage points lower on average, compared to participants in *Forecast* who make the same forecast mistake but do not receive any information. Second, some net contributors wrongly believe that redistribution comes at negligible personal costs. Providing these individuals with information leads them to vote for a tax rate that is 22 percentage points lower on average. The information treatments have no effects on the demand for redistribution for participants who do not make forecasts that lead to one of these two misperceptions of personal costs.

Extension 1 addresses the concern from the main treatments that the information about the development in inequality had limited effects because individuals care about relative and not absolute inequality. Two additional treatments (*ForecastR* and *RealizedR*) have incomes increase at a higher rate the greater the initial incomes are, and this leads to an increase in both absolute and relative inequality. In these treatments, I find that participants underestimate the increase in both absolute and relative inequality. But supporting the results from the main treatments, only individuals who misperceive their personal costs of redistribution are influenced by learning how inequality actually develops.

Extension 2 abstracts from individual forecasts to examine the causal effect of the level of inequality on individuals' preferences for redistribution. Two additional treatments (*ForecastNo* and *RealizedNo*) have participants vote on redistribution in a group without making forecasts. Instead, participants are randomised into different levels of inequality. Consistent with the results from the main treatments, the level of inequality has no causal effect on the preferred tax rates unless the level of inequality is such that some individuals face negligible personal costs of redistribution. This suggests a mechanism for why the information treatments had limited effects: the level of inequality has no causal impact on the preferred level of redistribution.

This study makes an important contribution to the literature on distributional preferences: it examines the accuracy of individuals' beliefs about how economic growth influences the level of inequality, and it examines how information about the actual development in inequality influences preferences for redistribution. By doing so, this study adds a temporal perspective to the large literature that has examined distributional preferences in static voting experiments (e.g. Tyran and Sausgruber, 2006; Messer et al., 2010; Agranov and Palfrey, 2015; Sauermann, 2018). Such a temporal perspective has been applied by researchers exploring other aspects than distributional preferences, including studies on the temporal discounting of altruism and collaboration (e.g. Rogers and Bazerman, 2008; Breman, 2011; Andreoni and Serra-Garcia, 2019; Chopra et al., 2021), cooperativeness in dynamic public goods games (Noussair and Soo, 2008; Cadigan et al., 2011; Gächter et al., 2017; Rockenbach and Wolff, 2019), and the extent to which people habituate to inequality over time (e.g. Lerner, 1980; Roth and Wohlfart, 2018; Mijs, 2021). The study also relates to the prospect of upward mobility hypothesis, which concerns how beliefs about one's future income affects preferences for redistribution (Hirschman and Rothschild, 1973; Benabou and Ok, 2001; Cojocaru, 2014). The current study deviates from this literature by examining instead the role of beliefs about how growth influences the level of inequality. The finding that correcting individuals' biased beliefs does not affect the level of redistribution is important, as it indicates that the biased beliefs do not lead to biased preferences for policies with long-run impacts, such as taxation and investments in education.

This paper also contributes to the literature on people's misperceptions of inequality by examining the effect of information provision in an incentivised and controlled setting. 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). Yet, while providing individuals with information about inequality tends to increase their concerns about inequality, it often 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., 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; Günther and Martorano, 2025; Henkel et al., 2025). Typical explanations for the null effects on preferences in these studies 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, whose incomes make up a larger share of the tax base. Such a 'cost' explanation has not been addressed in the survey-based studies where individuals express their concerns about inequality and their support for redistributive policies without monetary consequences. Acknowledging the role of personal costs is important: it suggests that interventions that e.g. provide information about the effectiveness of policies, strengthen trust in the government, and emphasise the role of luck for succeeding in life might often be inadequate if one wishes to make the electorate responsive to increasing inequality.

This paper proceeds as follows: Section 2 describes the experimental design. Section 3

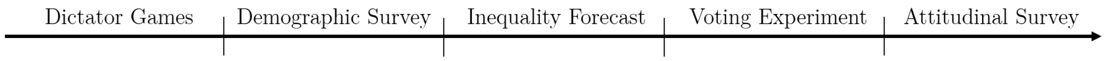


Figure 1: Timeline of the experiment

presents the theoretical framework which gives rise to the main hypotheses. I present data from the main treatments in Section 4. In Section 5, I examine the robustness of the results in an extension where participants earn higher interest rates if they have higher initial incomes. Section 6 presents a second extension where participants vote on redistribution without making any forecasts. I discuss further results in Section 7, and Section 8 concludes. The Supplementary Materials include extensions of the theoretical model as well as further results, tables, and figures.

2 Experimental Design

The experiment consists of five parts that participants complete in a single session online (see Figure 1 for an overview). First, participants play a standard and a modified dictator game to measure their inequality aversion and efficiency concerns, respectively. Second, participants fill in information about their demographics. Third, participants make forecasts about how incomes in a group grow, and this provides an individual forecast of inequality in the group. Fourth, participants are randomly allocated to an income class in the group and vote for redistribution in a tax-transfer scheme. Finally, participants complete an attitudinal survey. In the following, I describe the three main treatments, and I explain the two extensions in Sections 5 and 6. All treatments were pre-registered, and participants were recruited for all treatments simultaneously. The full set of instructions are available online and can be accessed here: https://doi.org/10.17605/OSF.IO/KZQSR.

Throughout the experiment, participants answer a series of control questions to ensure their understanding of the tasks. If participants answer incorrectly, they are informed about this and are asked to try again. They are not allowed to continue before they answer the control questions correctly. Additional screeners ensure that participants provide high-quality data, and I explain these in Appendix A.1.

2.1 Dictator Games

First, participants decide how to allocate earnings in a standard and a modified dictator game. These games provide proxies for inequality aversion and efficiency concerns, and these measures are later used as control variables as well as for subgroup analyses. Using the strategy method in both games (Selten, 1967; Brandts and Charness, 2011), all participants decide as a dictator, and a random draw determines whose decision is implemented. To avoid spillovers to the remaining parts of the experiment, participants are not informed about the outcome of the dictator games before they continue with the experiment.

Standard Dictator Game. In the standard dictator game, participants are randomised into pairs. One person (the dictator) is given USD 1 and decides how much to give to the other person (the recipient), choosing any number between USD 0 and 1.

Modified Dictator Game. The modified dictator game elicits preferences for efficiency relative to equity. Participants are divided into groups of three. One participant (Person C) is the dictator and decides how to allocate USD .9 between the two other participants, 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, similar to 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 see the earnings for Person A and Person B as well as the total earnings. Thus, the dictators decide between the following options (in cents): $(x_A, x_B) = \{(30, 30)$, $(40, 25)$, $(50, 20)$, $(60, 15)$, $(70, 10)$, $(80, 5)$, $(90, 0)\}$.

2.2 Demographic Survey

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

2.3 Inequality Forecast

Setting. In the third part of the experiment, the participants are divided into groups of seven. Two individuals are 'poor', three are 'middle class', and two are 'rich'. The group members are informed about the initial income of individuals in each income class and that all group members will receive an interest of 25 percent on their income for 30 rounds.[2] The group members are not informed about the final incomes. The initial (final) income for each income

[2]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.

class is $1 ($808), $4 ($3,231), and $7 ($5,655), respectively. Note that the 30 rounds only serve as a frame; there is no waiting or delay involved, and participants 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 participants 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, the participants are asked to guess the income of a member of each income class after 30 rounds with compounded interest. To rule out motivated reasoning, participants are not told what their own income will be when making their forecasts. The task is incentivised with participants earning 5 cents for each income class they estimate correctly (with a 10 percent margin of error). I use the income forecasts to calculate the corresponding inequality forecasts as explained in Section 4, and I show in Section 7.1 that forecast bias predicts the extent to which participants misperceive inequality in society.

2.4 Voting on Redistribution

The fourth part of the experiment expands on the subjective inequality forecast. The participants 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, participants are randomly assigned to one of three information treatments (see Table 1 for an overview).

Table 1: Overview of experimental treatments

	Treatment	**Interest Rates**	**Participants make income forecast**	**Participants see true income levels**	**Participants see true income ratios**
Main Treatments	Forecast	Uniform	Yes	No	No
	Ratio	Uniform	Yes	No	Yes
	Realized	Uniform	Yes	Yes	Yes
Extension 1	ForecastR	Unequal	Yes	No	No
	RealizedR	Unequal	Yes	Yes	Yes
Extension 2	ForecastNo	Uniform	No	No	No
	RealizedNo	Uniform	No	Yes	Yes

Tax-Transfer Scheme. Participants 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 is added to ensure that it is costly for the middle-income participants 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 participants' decisions (Beckman et al., 2004; Krawczyk, 2010; Durante et al., 2014; Tepe et al., 2021; see also Supplementary Materials S.8.3).

After all group members state their preferred tax rate, one of these tax rates is chosen at random and implemented ('random dictator procedure'). All participants have an equal chance of being pivotal; therefore, all participants have an incentive to truthfully report their preferred tax rate (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 participants 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 participants' estimates from the forecast task or the actual incomes in the final round. The effect of the tax is shown in terms of the consequences for the incomes in the final round rather than the first round because this simplifies the redistribution decision for the participants: first, participants see the consequences of the tax directly from the table. If the participants saw the post-tax incomes for the first round, they would need to make forecasts for all combinations of tax rates and income groups to understand the final post-tax incomes. Second, seeing the information about final incomes ensures a logical progression from the previous forecast task, and participants do not need to recall their answers to the previous task to make a decision in the voting experiment.

A key feature of the experimental design is that participants 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 information about the development in inequality affects preferences for redistribution.

Treatments. Participants are randomised into different treatments that vary the information available to the participants when they choose a tax rate (see Figure 3). In the *Forecast* treatment, participants see the effect of redistribution based on the level of inequality they estimated in the forecast task. In contrast, participants in the *Realized* treatment see the post-redistribution incomes based on the actual level of inequality in the final round. Comparing these two treatments yields the causal effect of informing individuals about the actual

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 participant in *Forecast* who estimated that the poor, middle income, and rich group members would earn $700, $2,000, and $3,150 in the final round, respectively. The complete instructions are provided here: https://doi.org/10.17605/OSF.IO/KZQSR.

development of inequality in the group.

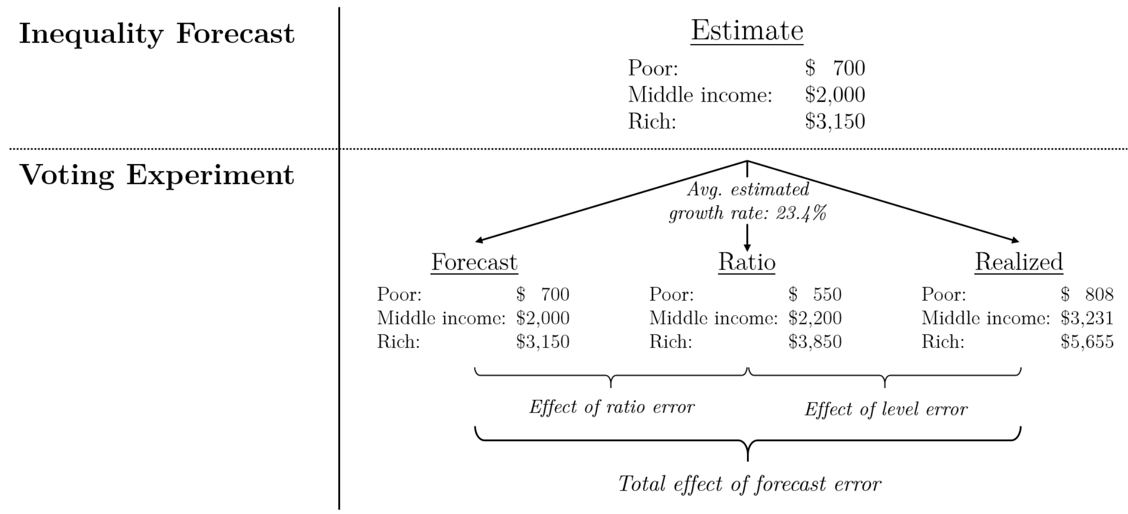
Differences in preferences between the two treatments may be due to two types of forecast errors: participants may wrongly estimate how the incomes develop relative to each other, and they may wrongly estimate the absolute increase in income levels. The *Ratio* treatment disentangles the effects of the two types of errors. In this treatment, participants 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 how correcting forecast errors influences preferences for redistribution. The contrast between *Ratio* and *Realized* tests how correcting beliefs about the absolute level of inequality affects redistributive preferences. And comparing *Forecast* and *Ratio* sheds light on how correcting beliefs about the relative inequality influences redistributive preferences.

2.5 Attitudinal Survey

After completing the voting experiment, participants answer a survey that elicits various attitudes and beliefs. First, participants answer a question about their general risk preferences (Dohmen et al., 2011) and a generalised trust question (Lundmark et al., 2015). Next, participants place themselves on a left-right political scale and report their beliefs about the

Figure 3: Illustration of received information in each treatment



Notes: this figure illustrates the incomes that a participant will see in each of the information treatments given a forecast of \$700, \$2,000, and \$3,150 for the respective income classes and a tax rate of $\tau = 0$. In *Forecast*, participants see their own income forecasts. In *Ratio*, participants see information where all incomes grow at the average of the subjectively forecasted interest rates. Finally, participants in the *Realized* treatment are informed about the actual incomes that arise based on the 25 percent interest rate. Together, the three treatments make it possible to uncover the total effect of correcting erroneous forecasts and to decompose it into the effects of correcting beliefs about absolute or relative inequality.

importance of merit for obtaining success in life (Fong, 2001; Fisman et al., 2017; Haerpfer et al., 2020; Kerschbamer and Müller, 2020). Participants 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 (Norton and Ariely, 2011; Norton et al., 2014; Franks and Scherr, 2019). Afterwards, participants 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. Participants 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, participants answer the 10-item version of the Martin-Larsen Approval Motivation Scale (MLAMS, Martin, 1984), which measures desire for social approval. To allow comparisons 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).[3]

[3]I use POMP scores rather than computing standardised (z) scores because the participants' responses are skewed on the questions regarding trust, political attitudes, meritocratic beliefs, income differences, and the government's responsibility. Such skewness can make 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

2.6 Procedure

For the three main treatments, I recruited 1,584 participants on Amazon's Mechanical Turk (MTurk) between 13 November and 3 December 2021, and the experiment was conducted using 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 total, the screeners led to the exclusion of 10.7 percent of the responses, and the main sample thus consists of 1,415 participants.[4] With this sample size, I expected to have a power of 0.8 to detect an effect size of Hedge's $g = 0.22$, corresponding to a difference in tax rates of 6.6 percentage points (from power simulations, details in Supplementary Materials S.6). 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 participants received a fixed payment of 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 counts time spent off task with the experiment open in the background).

enabling comparisons between studies in a manner that is robust to sampling differences.

[4]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. The results in the current study are qualitatively robust to including all participants who completed the study.

3 Theory

3.1 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 informing individuals about the actual development in inequality influences preferences for redistribution. I now develop a stylised model to form the hypotheses I test in Section 4. The model builds on the quadratic version of the Fehr and Schmidt (1999) model of inequality aversion. 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 I 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 inequality aversion. This model implies that the marginal disutility of inequality is greater for higher levels of inequality, and this has several advantages over the linear version: first, this model corroborates the transfer principle (Pigou, 1912; Dalton, 1920) to yield e.g. the intuitive result that a rich individual would approve of a transfer from the middle class to people living close to the subsistence minimum. Second, the model is consistent with the finding that 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).[5]

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

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

In Supplementary Materials S.1.5, I demonstrate that the predictions derived from the

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

utility function specified in Equation 1 are qualitatively robust to including explicit preferences for efficiency (total surplus).

Social Preferences and Forecast 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), 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 rate of growth (or real interest rate), T is the time horizon, and θ is the forecast bias:

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

To make assumptions about the function f, I draw on an extensive literature that shows how individuals tend to linearise exponential developments. This exponential growth bias (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), which implies that individuals are unlikely to take the necessary steps to alleviate problems caused by EGB. Common theoretical frameworks for EGB assume that the function f does 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.1.1 and S.1.2 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.1.4, 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 once. 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.[6] Thus, the post-redistribution income that individual i receives is $(1-\tau)x_i + \lambda \tau \bar{x}$. In line with the experimental setup, I assume that individuals are only concerned with post-redistribution incomes at time T. That is, individual i expects to receive the following utility at time T:

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

[6] The notion that the tax involves an efficiency loss is a standard simplification used in the literature to describe an equity-efficiency trade-off (Alesina and Giuliano, 2011). It reflects possible distortions to the labour supply caused by income taxation. In the literature, such a distortion is sometimes considered as convex, but I adopt a linear efficiency loss to reflect the more simple experimental design from Section 2 (see e.g. Krawczyk, 2010, or Tepe et al., 2021). Supplementary Materials S.1.6 shows that the qualitative predictions of the model remain the same with convex distortionary costs of taxation.

Supplementary Materials S.1.7 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.

3.2 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. Importantly, the assumption that individuals apply the same multiplicative function f to all incomes implies that individuals always know whether they pay more or less than they get back in transfers; i.e., whether they are net contributors or net recipients from taxation. Poor individuals are net recipients ($x_i < \lambda \bar{x}$); for them, a higher tax rate leads to both higher income and greater equality in the group. So, they prefer the highest tax rate of $\tau_i^b = 1$ as long as the efficiency of the tax is high (as is the case in the current experiment). In contrast, middle-income and rich 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 income growths to an extreme extent ($f \to \infty$). The corner solution of $\tau_i^b = 0$ will only occur for middle-income and rich individuals who are severely biased and/or care very little about inequality ($\phi_i f(r, T, \theta) \leq (x_i - \lambda \bar{x})/2$).

All other middle-income and rich participants prefer an intermediate tax rate when trading off their own income and equality. Their preferred tax rate increases in inequality aversion (α, β) and the efficiency of the tax (λ). Moreover, it increases in the subjective estimate of growth ($f(r, T, \theta)$), which implies that more biased individuals prefer less redistribution, ceteris paribus.

Let τ_i^* denote the optimal tax rate for an individual, corresponding to the preferred tax rate after an unbiased forecast ($\tau_i^b(x_1, \ldots, x_n; \theta = 0)$). It is the relevant benchmark for participants in the *Realized* treatment, as these participants 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 (e.g. in the *Realized* treatment).

3.3 Hypotheses

The first hypothesis tests whether the participants' 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 implies that all individuals provide accurate estimates 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, participants on average underestimate how much absolute inequality increases.*

2. *When interest rates are the same for all individuals, participants 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 informing individuals about the actual development of inequality. As outlined in Section 3.2, the theoretical model predicts that participants who are not in a corner solution prefer a higher tax rate in the *Realized* treatment than in the *Forecast* and *Ratio* treatments. If individuals correctly estimate relative inequality (H1.2), there should be no difference between *Forecast* and *Ratio*. This yields the following hypothesis:

Hypothesis 2 *Comparing individuals with the same degree of inequality aversion, middle-income and rich participants 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.

4 Results

I now analyse participants' inequality forecasts and how informing about the actual development in inequality influences preferences for redistribution in the voting experiment. Throughout, I follow the pre-analysis plan exactly, and all reported p-values are from two-sided tests. Table 2 provides descriptive statistics.

Table 2: Descriptive statistics, main treatments

	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

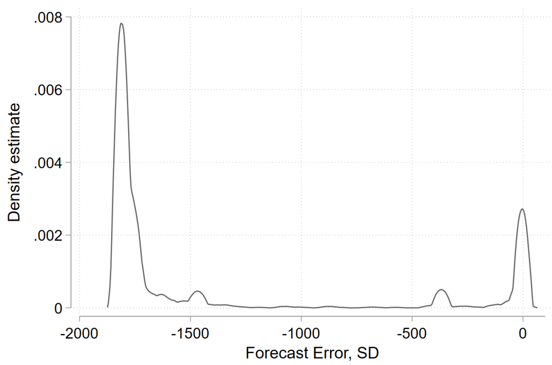
Notes: averages are taken over all participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by participants' 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 led to issues for participants 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 participants are presented in Tables S.15 and S.16.

4.1 H1: Do People Underestimate Changes in Inequality?

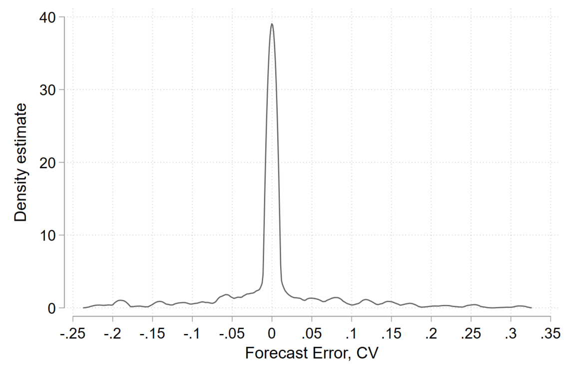
To test H1, I first obtain the level of absolute and relative inequality implied by the participants' forecasts. Specifically, I calculate the standard deviation and Absolute Gini coefficient to examine absolute inequality, and I compute the coefficient of variation and the Gini coefficient as measures of relative inequality.[7]

[7]The model presented in Section 3 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 participant 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, main treatments



(a) Absolute inequality, SD



(b) Relative inequality, CV

Notes: the figures show the kernel density of participants' forecast error (epanechnikov). Figure (a) shows the forecast error of absolute inequality ($bw = 20$) with the standard deviation calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$. Figure (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.1 and S.3 for the full sample.

Supporting H1.1, most participants greatly underestimate the level of absolute inequality in the final round, both in terms of the standard deviation (Figure 4a) and the Absolute Gini coefficient (Figure S.2). The underestimation is statistically significant for both measures ($p < .001$, bootstrapped t-test).

Turning to relative inequality, Figure 4b suggests that many participants have nearly accurate forecasts for the coefficient of variation (similar for the Gini coefficient, see Figure S.4). However, the statistical evidence is mixed. On average, participants 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 participants perform better at forecasting relative than absolute inequality, the data only partially support H1.2. As I show in Section 4.2, the fact that H1.2 is not confirmed has implications for the participants' demand for redistribution, as erroneous beliefs about relative inequality make some participants misperceive whether they gain or lose from taxation.

The data also suggest that a number of participants make exact forecasts for the level of inequality (Figure 4a). Specifically, 129 participants (9 percent) make estimates within $\pm\$1$ of the correct answer for all three income classes. Of the participants who answered correctly, 95 were later randomised into the middle or rich income class. These participants are not influenced by the information treatments in the voting part, as they will see the correct level of inequality regardless of what treatment they are randomised into. The analyses in the next section include these participants (as some individuals in the field do look up statistical information before making decisions), and the analyses are robust to instead excluding these participants (as they are not affected by the information treatments, see Table S.14). There is no difference in the preferred tax rates among the participants who answered correctly and those who did not (treatments combined or analysed separately, all $p's > .656$).

To examine EGB in the data, I follow Stango and Zinman (2009) and calculate 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 participant makes three forecasts (one for each income class), and I use the average value of θ as an estimate of the participants' degree of EGB. Of the participants who did not obtain the correct incomes, I find that 55 of 1,286 participants (4 percent) overestimate growth on average (i.e. $\theta < 0$), whereas 1,231 participants (96 percent) underestimate growth on average (i.e. $0 < \theta < 1$). The average bias is $\theta = 0.46$, which implies that participants on average make forecasts as if the growth rate was only 13 percent rather than 25 percent per round. In Supplementary Materials S.5, I comment on the heterogeneity in the participants' forecast errors, and I show in Section 7.1 that forecast errors predict underestimation of inequality in society.

I sum up the results on H1 below:

Result 1 *When interest rates are the same for all individuals, participants underestimate how much absolute inequality increases with exponential growth. They are markedly better at predicting relative inequality, but there is partial evidence that participants underestimate relative inequality on average.*

Having thus shown that participants misunderstand how exponential economic growth influences inequality, I now turn to the behavioural implications of correcting the participants' beliefs.

4.2 H2: How Does Information Influence Redistribution?

To test H2, I restrict my attention to the choices of middle-income and rich participants (motivated by the theoretical model outlined in Section 3). In Supplementary Materials S.8.3, I show that there are no treatment effects when looking at the behaviour of poor participants. The model predicts that all poor participants vote for complete redistribution in all treatments; yet, this is not always the case, and I provide suggestive evidence that tax aversion may also influence the behaviour of some poor participants.

In the following, I compare preferred tax rates across treatments using tobit regressions, as the tax is bounded between 0 and 100 percent. I estimate regressions (i) without controls, (ii) controlling for dictator giving as a proxy for inequality aversion, (iii) adding demographic controls, and (iv) adding also attitudinal controls. I expect the control variables to explain variation in the preferred tax rates not accounted for by the treatments, leading to more statistically efficient estimates, and the full specification (iv) is therefore the preferred specification. I test for robustness using the semi-parametric Symmetrically Censored Least Squares estimator (SCLS, Powell, 1986) and the nonparametric Mann-Whitney U-test (MWU, Wilcoxon, 1945; Mann and Whitney, 1947).

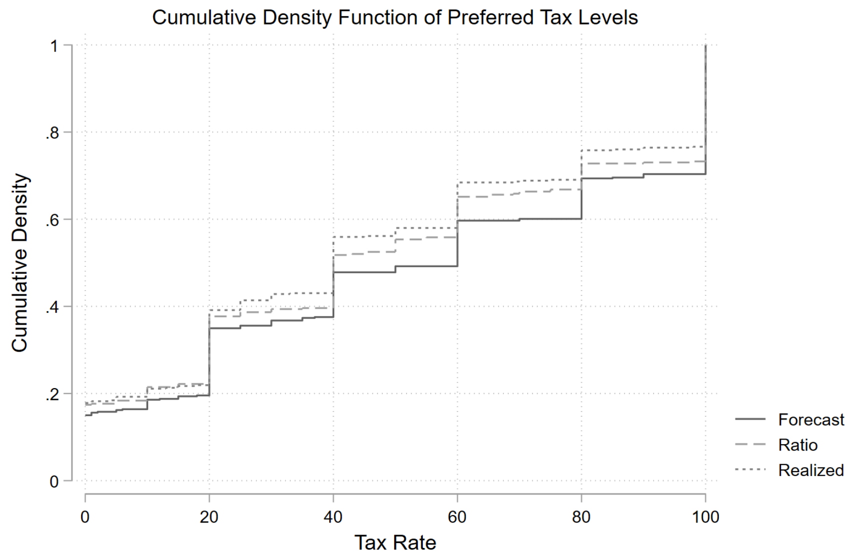
Contrary to H2.1, Figure 5 suggests that the average tax rate is greater in *Forecast* (47.73) than in *Realized* (40.74). This treatment difference is statistically significant, also when adding controls (all $p's < .01$, see Table 3), and it is robust (SCLS: $p = .009$, MWU: $p = 0.022$).

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 ($p = .094$).

Interpretation of Results: Perceived Costs. The above analysis revealed that informing individuals about the actual (higher) level of inequality causes them to vote for less redistribution. Yet, 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 participants grossly underestimate their personal costs of redistributing. This happens in two ways. First, 172 of 605 middle-income participants (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 3, as the model assumes that individuals apply the same (possibly biased) overall growth to all incomes. These individuals think that they gain from redistribution if they are randomised into *Forecast*, but they will learn that redistribution is costly if they are randomised into *Ratio* and *Realized*. Column 5 in Table 3 reports the results of a specification that adds a dummy equal to 1 if the participant made a forecast that would imply personal gains from redistribution,

Figure 5: Preferred tax rate by treatment



Notes: the figure presents the cumulative density function (or empirical distribution function) of the participants' tax decisions by treatment. Participants bunch at the tax rates 0, 20, 40, 60, 80, and 100, which is to be expected because participants observe the post-redistribution incomes for precisely these tax rates (cf. Figure 2), and it confirms that the participants pay attention to the experimental instructions.

and it adds interaction dummies for the case that a person with this type of forecast is in the *Ratio* or *Realized* treatment. When in the *Forecast* treatment, these individuals vote for a tax rate that is 50 percentage points higher than those who do not believe that they will gain from redistribution ($p < .001$). And the individuals who make this type of forecasting mistake vote for a tax rate that is 42 (36) percent lower if they are randomised into *Realized* (*Ratio*) such that they are no longer significantly different from individuals who do not make this forecasting mistake (F-test: $p = .313$ and $p = .091$).

A second way that participants may underestimate their personal costs of redistribution occurs if participants 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).[8] This misperception occurs for 229 of the 1,013 middle-income and rich participants (22.6 percent), and it is directly related to the size of the participants' forecast bias such that EGB is markedly larger for participants who make

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

forecasts that imply very low personal costs (average $\theta = .70$ vs. $\theta = .41$; $r = .40$, $p < .001$). These individuals think that redistribution comes at negligible costs if they are randomised into *Forecast* or *Ratio*, but they will learn that redistribution comes at a considerable cost if they are randomised into *Realized*. If the participants who perceive very low personal costs 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 participants to vote for higher taxes ('weak inequality aversion', Tyran and Sausgruber, 2006). Column 5 in Table 3 reports the results of a specification that adds a dummy equal to 1 if the participant made a forecast that would imply low personal costs as well as its interaction with the *Realized* treatment. It shows that in *Forecast* or *Ratio*, individuals who make forecasts that imply very low personal costs vote for a tax rate that is 13 percentage points higher on average ($p < .001$). But these individuals on average vote for a tax level that is 22 percentage points lower if they are randomised into *Realized* ($p = .013$), such that these individuals do not vote differently in the *Realized* treatment than individuals who make forecasts that imply considerable personal costs (F-test: $p = .240$).

Importantly, the results in Column 5 in Table 3 also show that there are no treatment differences for the individuals who do not make forecasts that grossly underestimate the personal costs of redistribution. The differences are also insignificant when one considers SCLS estimators (all $p's > .116$) and when one conducts MWU-tests excluding participants whose forecasts wrongly imply perceive personal gains or low personal costs (all $p's > .301$). When including these endogenous controls, the coefficients on the treatment dummies and the treatment interactions can only be interpreted as causal within the respective subgroups: information has a causal effect for individuals who misperceive the personal costs of redistribution; it has no causal effect for individuals who understand that redistribution comes at considerable costs.

In Supplementary Materials S.2, I show that EGB predicts the preferred tax rate in *Forecast*, but not when one controls for perceived gains and perceived low personal costs. In *Realized*, EGB does not predict the preferred tax rate. This supports the interpretation that forecasts matter through the information that is available to participants, and it also indicates that the two ways of underestimating personal costs are the channels through which forecast bias matters – not inequality *per se*. In Supplementary Materials S.8, I provide further evidence that the level of inequality does not influence demand for redistribution: perceptions of inequality within the group do not predict participants' tax preferences regardless of how inequality is operationalised.

Together, the findings imply that participants are willing to give up approximately the same share of their income regardless of the level of absolute inequality. 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). Compared to the theoretical model outlined in Section 3, this indicates that the assumption of individuals exhibiting increasing marginal disutility from inequality is refuted by the data: this assumption was key for the prediction that participants would respond to greater absolute inequality by preferring a higher tax rate.

The next result summarises the effect of providing individuals with information about the level of inequality in the final round:

Result 2 *Participants vote for lower tax rates when they are informed about the actual, higher level of inequality. This effect is only driven by participants who based on their own forecasts erroneously believe (i) that they gain from tax-financed redistribution or (ii) that redistribution comes at very low personal costs.*

5 Extension 1: Unequal Growth Rates

The analysis above revealed that individuals underestimate how much (absolute) inequality increases in the presence of uniform growth, but that perceived inequality does not affect the demand for redistribution. Rather, what matters is whether individuals grossly underestimate their personal costs of redistribution. Yet, it is still possible 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 because the distribution of growth can lead to increases in relative inequality as well, depending on the country, time period, and domain (e.g. wage growth vs. stock market returns). To address this concern, two further treatments have participants earn higher interest rates the larger their initial incomes are, thereby leading economic growth to increase both absolute and relative inequality. In what follows, I briefly describe the two treatments and the results; details are provided in Supplementary Materials S.3.

Table 3: Belief Correction and Demand for Redistribution

	(1)	(2)	(3)	(4)	(5)
Ratio	-8.31*	-8.41*	-7.85*	-7.49*	-1.62
	(4.69)	(4.56)	(4.53)	(4.32)	(4.63)
Realized	-11.87***	-12.68***	-12.98***	-13.88***	-2.20
	(4.49)	(4.36)	(4.34)	(4.14)	(4.99)
Forecasted Gains					50.03***
					(8.18)
Forecasted Gains×Ratio					-35.91***
					(11.52)
Forecasted Gains×Realized					-42.28***
					(11.23)
Forecasted Low Costs					13.48***
					(5.11)
Forecasted Low Costs×Realized					-21.94**
					(8.84)
Dictator Giving	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Attitudinal Controls	No	No	No	Yes	Yes
Observations	1013	1013	1013	1013	1013

Notes: tobit regressions with preferred tax rate as the dependent variable, reporting average partial effects. Forecasted Gains is a dummy equal to one if the participant made forecasts that imply that they would gain from taxation. Forecasted Low Costs is a dummy equal to one if the participant made a forecast that would imply that redistribution comes at negligible 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.13 for the full specification. Robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Treatments. In Extension 1, interest rates are different for each income class (24, 26, and 27 percent), leading to vastly different overall growths of 635, 1,026, and 1,301 percent over the 30 rounds of compounded interest. Thus, the initial (final) income levels are $1 ($635), $4 ($4,104), and $7 ($9,104). In the voting part, participants are randomised into either the *RealizedR* or *ForecastR* treatment (the *R* reflects that relative inequality is also affected by growth). Participants in *RealizedR* receive information about the actual post-redistribution incomes, whereas participants in *ForecastR* observe the post-redistribution incomes based on their own forecasts.

In total and after screeners, there were 980 participants in the two treatments (see Tables S.17 and S.18 for summary statistics).

Results. Because the model in Section 3 can be extended to the case with unequal interest rates (see Supplementary Materials S.1.3), the pre-registered hypotheses were that (i) participants would on average underestimate how much absolute and relative inequality increase, and (ii) middle-income and rich participants would on average vote for a higher tax rate in *RealizedR* than in *ForecastR*. The first hypothesis is confirmed, as participants underestimate the increase in both absolute and relative inequality (all $p's < .001$, bootstrapped t-tests).

Looking at the preferred level of redistribution for middle-income and rich participants, however, there are no differences between *ForecastR* (mean: 39.08) and *RealizedR* (mean: 39.83), cf. Figure S.11. 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.2), and this result is robust to using the SCLS estimator ($p = .158$) and the MWU-test ($p = .892$). Instead, forecast bias only matters for the participants who based on their forecasts grossly underestimate their personal costs of redistributing: 95 of the 415 middle-income participants in *ForecastR* or *RealizedR* (23 percent) make forecasts that would imply that they would gain from redistribution. When randomised into the *ForecastR* treatment, these individuals vote for a tax that is 24.6 percentage points larger than individuals who do not make this forecast mistake ($p = .002$). These individuals are influenced by the information, as the individuals vote for a tax rate that is 20 percentage points lower if they are randomised into the *RealizedR* treatment ($p = .080$), making them statistically indistinguishable from participants whose forecasts imply that redistribution is costly (F-test: $p = .552$). Wrongly perceiving the costs as minimal leads to an increase in the preferred tax rate of 10 percentage points, but this difference fails to reach statistical significance ($p = .151$).

I summarise these results as follows:

Result 3 *When interest rates correlate positively with initial incomes, the participants underestimate how much absolute and relative inequality increase with exponential growth.*

Result 4 *When interest rates correlate positively with initial incomes, informing individuals about the actual, higher level of inequality only changes individuals' preferences for redistribution when their forecasts imply that they would gain from redistribution.*

6 Extension 2: Voting Without Forecast

The above analyses demonstrate that participants often misunderstand how inequality develops in the presence of economic growth, but correcting participants' beliefs about how growth influences inequality does not affect their preferences for redistribution. A possible issue concerning the mechanism behind these findings is that the results may capture other effects than inequality beliefs, as participants may be influenced by the act of making a forecast. For instance, participants may be surprised by how much inequality has increased due to exponential growth and react to the surprise rather than to the level of inequality. Participants may also understand that their forecast is uncertain and 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 participants to perceive the poor as well-off even at low tax rates.

To address these concerns, two further treatments have participants engage in the voting part of the experiment without making forecasts. Below, I briefly describe the two treatments and the results; details are in Supplementary Materials S.4.

Treatments. In Extension 2, participants do not engage in the forecast task; instead, they are randomised into one of two treatments that differ only in the income levels of the poor, middle-income, and rich classes. In the *RealizedNo* treatment, participants are informed that the income for an individual from each income class is \$808, \$3,231, and \$5,655, respectively (similar to the *Realized* treatment). In the *ForecastNo* treatment, participants are randomised into a level of inequality that corresponds to one of eight income forecasts from a pilot study (avoiding the most extreme forecasts). Specifically, participants are randomised into one of the following allocations $(x_P; x_M; x_R)$ for the case of a tax rate of zero: $\{(8; 31; 55)$, $(12; 46; 81)$, $(13; 51; 89)$, $(30; 120; 210)$, $(38; 150; 263)$, $(41; 162; 284)$, $(156; 624; 1092)$, $(579; 2315; 4052)\}$. Note that the forecasted incomes were chosen such that all middle-income and rich participants realise that redistribution comes at a personal cost, but some of the forecasted incomes imply very low personal costs for the middle-income group. The post-redistribution incomes correspond to the actual payoffs (again using an exchange rate of 2000:1), thus avoiding any reference to 'forecasts' or 'estimates'. Because individuals are randomised into different allocations, comparing these allocations has a direct casual interpretation.

In total and after screeners, there were 1,094 participants in the two treatments (see Tables S.23 and S.24 for summary statistics).

Results. Because the model in Section 3 assumes that forecasts only matter via beliefs about inequality in the final round, the model is directly applicable to the case where participants simply vote based on different levels of inequality. Thus, the pre-registered hypothesis was that middle-income and rich participants on average vote for a higher tax rate in *RealizedNo* than in *ForecastNo*.

Contrary to this hypothesis, the average preferred tax rate for middle-income and rich participants is slightly higher in *ForecastNo* (52 percent) than in *RealizedNo* (47 percent).

This difference is marginally significant without controls and controlling for dictator givings ($p = .063$ and $p = .067$), and it becomes statistically significant when adding demographic and attitudinal controls (tobit: $p = .009$, MWU: $p = .050$, SCLS: $p = .143$). Yet, corroborating Result 2, this difference becomes insignificant when including a dummy for facing low personal costs ($p = .613$, cf. Table S.4; SCLS: $p = .956$; MWU: $p = .824$). Participants who faced low personal costs on average vote for tax rates that are 29.55 percentage points higher ($p < .001$).

I summarise these results as follows:

Result 5 *When participants do not make forecasts, the level of inequality only influences the demand for redistribution if the incomes are such that participants face very low personal costs of redistributing.*

7 Discussion

The preceding sections show that individuals tend to underestimate how much growth influences the level of inequality, regardless of whether growth is uniformly or unequally distributed across income classes. Yet, informing individuals about the actual, greater inequality in their group does not influence their preferences for redistribution. Instead, information only influences the demand for redistribution if individuals learn that redistribution is costly for themselves. These results go against the predictions of the theoretical model outlined in Section 3 for two reasons: first, the model predicts that individuals know whether they gain or lose from redistribution. This follows the assumptions 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). These assumptions are refuted by the data. Second, the theory predicts that individuals desire more redistribution when inequality is larger. This prediction stems from the assumption that individuals experience increasing marginal disutility from inequality, and this is also refuted by the data.

In the following, I discuss exploratory findings from the experiment. I first show that forecast bias significantly correlates with misperceiving wealth inequality in the US. Then, I discuss the correlation between perceived inequality in society and voting preferences.

In Supplementary Materials S.8, I demonstrate that the current study replicates 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 in the experiment. Then, I discuss how the act of making a forecast influences tax preferences. Finally, I show that the extent of inequality in a group does not predict participants' tax preferences regardless of how inequality is operationalised.

7.1 EGB Predicts Misperceptions of Inequality in Society

The forecast task in the experiment reveals that participants 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 participants (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, participants who exhibit a larger extent of EGB in the current experiment tend to more severely underestimate wealth inequality in the US (Spearman's $\rho = -0.121$, $p < .001$, also significant when adding controls for demographics and attitudinal measures). Moreover, the rank correlation is robust to restricting the sample to participants who did not provide exact forecasts for all income classes, to looking at each treatment separately, using participants' forecasted inequality levels instead of EGB, and to using Kendall's tau instead of Spearman's ρ (all $p's < .030$). 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.

7.2 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: participants 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 in a tobit regression, 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).[9] This shows that people who perceive greater inequality in society are also more concerned about inequality in an abstract environment. Hence, the relation between inequality perceptions and preferences may be complex, as there could be two-way causality between perceptions of inequality and preferences.

8 Conclusion

In this paper, I have shown that individuals tend to underestimate the extent to which exponential economic growth leads to increases in the level of inequality. Yet, providing information about the actual, higher level of inequality does not influence individuals' preferences for redistribution in an incentivised voting experiment. Net contributors who know that redistribution is costly are informed about two things: (i) there is a greater need for redistribution, and (ii) redistribution is more costly because their income makes up a larger share of the tax base. Since preferences for redistribution are not affected by the (perceived) level of inequality, it seems that the increased benefits and costs of redistribution cancel out. Opposingly, net contributors who wrongly believe that redistribution is not costly respond to the information they receive. In particular, learning that redistribution entails personal costs decreases support for redistribution.

These findings are central for understanding individuals' policy preferences and the possible effects of information interventions. First, the fact that correcting individuals' beliefs about how economic growth influences inequality does not affect their demand for redistribution suggests that growth bias might not lead to biased preferences for decisions with long-run impacts. This is important, as there are many such decisions: for example, tax policies not only influence current levels of income and wealth, but also how wealth accumulates over time. Political or parental investments into children's equal access to education influences human

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

capital accumulation and thereby income trajectories. And wage negotiations may involve setting growth rates of wages across occupations for many periods.

Second, the study addresses why providing individuals with information about the level of inequality often does not influence the demand for redistribution. As mentioned in Section 1, previous studies suggest as possible reasons for null effects that citizens might (i) believe that policies are ineffective, (ii) distrust the government, or (iii) believe that inequalities are justified due to differences in merit. This paper finds that information can have limited effects even after ruling out these explanations, pointing instead to the role of personal costs: among the net contributors, greater concerns about inequality are offset by greater personal costs of redistribution. This provides a possible explanation for why interventions that e.g. provide information about the effectiveness of policies, strengthen trust in the government, or emphasise the role of luck for succeeding in life can have little effect on making the electorate responsive to changes in inequality.

Yet, some issues limit the external validity of the current experiment. First, the sample was recruited among US residents using MTurk. MTurk is widely 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 current findings among other populations.

Second, the experiment has participants make decisions and inequality forecasts without external influences. In the field, people are often influenced by peers, experts, media, and organisations that make forecasts about how inequality will develop over time. The current paper does not address what type of information people seek or 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.

An interesting avenue for future research 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 experiment 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 providing information about inequality influences decisions that are more likely to be driven by non-consequentialist motives.

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A.1 Screeners

In the following, I describe the screeners that I applied to ensure high-quality data. Since both the three main treatments and the two extensions were carried out at the same time, all participants first had to pass a VPN/VPS and bot test before being randomly allocated into treatments. Specifically, participants 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 participants 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 who tried to access the study using a VPS, VPN, or other proxy. These participants were not allowed to take part in the study. To detect bots, the survey included two honeypots (coded in JavaScript). Following Moss and Litman (2018a), these honeypots were survey items hidden from human participants, 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.[10] 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.[11]

The study did not involve any attention checks such as the widely used Instructional Manipulation Checks (Oppenheimer et al., 2009). 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 participants' behaviour (Hauser and Schwarz, 2015; Hauser et al., 2018), and that they may result in participants providing responses of lower quality.

Main Treatments. As described in Section 2.6, I screened out 10.6 percent of the 1,584 participants for the three main treatments. First, I excluded participants who made forecasts that failed to rank the three income groups as $poor < middle < rich$. This led to the exclusion of 34 respondents. Second, Wood et al. (2017) show that participants 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).[12] Third, I followed Kennedy et al. (2020) and included a consistency check. In the

[10] Designing bot detection in this way has advantages over using (re)CAPTCHAs, as some bots are 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).

[11] 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 page with 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.

[12] 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

demographic survey, participants were asked about their age, and participants provided their year of birth in the attitudinal survey (see also Zhang et al., 2022). This led to the additional exclusion of 38 participants 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. Participants 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 participants 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 participants who provided such answers, although this screener was not pre-registered. Removing this screener does not change the results of the study.

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

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

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.

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 or 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 participants 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 Supplementary Materials

How People Misunderstand Growth and Inequality – and the Effects of Correcting Their Beliefs

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13 September 2025

S.1 Theoretical Extensions

In the following, I first demonstrate how the theoretical model outlined in Section 3 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 of income-specific real interest rates. I then 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.1.1 Special Case: EGB as Modelled by Stango and Zinman (2009)

In the following, I derive the results presented in Section 3 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 3, 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 forecast bias:

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

As in Section 3, poor individuals ($x_i < \lambda\bar{x}$) prefer the highest tax rate of $\tau_i^b = 1$. Middle-income and rich individuals must be either very averse to inequality or overestimate developments to a large extent to prefer the highest tax rate of $\tau_i^b = 1$ (for $2\phi_i(1+r)^{(1-\theta)T} \to \infty$). 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):

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

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

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

The effect of 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.1.2 Special Case: EGB as Modelled by Levy and Tasoff (2016)

In the following, I examine a special case of the framework presented in Section 3, 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 forecast bias:

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

As in Section 3, poor individuals ($x_i < \lambda\bar{x}$) prefer the highest tax rate of $\tau_i^b = 1$. Middle-income and rich individuals must be either very averse to inequality or overestimate growth to a large extent to prefer the highest tax rate of $\tau_i^b = 1$ (for $2\phi_i\left[(1+(1-\theta)r)^T + \theta T r\right] \to \infty$). 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, in the following I only examine the comparative statics that work through the forecast bias.

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

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

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.1.3 Income-Specific Real Interest Rates

In the following, I abandon the assumption from Section 3 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 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 3, other individuals greatly concerned with inequality will prefer a tax rate of 1. Opposingly, 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 3, denote the optimal tax rate for individual i by τ_i^*. This corresponds to τ_i^b in the absence of bias ($\theta = 0$). For the real interest rates applied in the current experiment, one obtains 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.1.4 Including Aversion to Relative Inequality

In this section, I extend the theoretical framework developed in Section 3 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 3, 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:

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

Hence, the results derived in Section 3 also hold under aversion to relative inequality, provided that this aversion is not excessive. Furthermore, Equation S.10 implies that the requirement for γ_i varies with the level of income. 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.

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.[1] 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 3 hold as long as the individual does not care more about inequality as obtained with the Krtscha measure than they care about their own income.

S.1.5 Including Efficiency Concerns

In the following, I extend the model from Section 3 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:

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

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

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

[1] 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).

As before, individuals with low income will prefer a corner response of $\tau_i^b = 1$ (if $x_i < \lambda\bar{x} - \delta_i y(1-\lambda)$). Moreover, individuals who are sufficiently inequality averse or overestimate growths to a large extent prefer $\tau_i^b = 1$ (if $2\phi_i f(r,T,\theta) \to \infty$. In contrast, 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 3, one may compare the optimal tax rate for individual i (τ_i^*) with the tax rate influenced by the forecast bias (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 implies that the individual becomes more responsive to changes in the efficiency of the tax. 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 partial effects of inequality aversion (α, β) or forecasts (r, T, θ); these effects are analogous to the above cases.

S.1.6 Convex Efficiency Loss

In Section 3, I assume the efficiency loss is linear in the tax, which reflects the experimental design outlined in Section 2.4. 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:[2]

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

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. Restricting again attention to individuals who prefer a tax between 0 and 1, the preferred tax rate is higher for participants with greater concerns about inequality, and it increases with the efficiency of the tax:

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

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

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

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 $T = 0$, longer time horizons yield the general result that individuals who exhibit forecast bias vote for less redistribution than would be in their own long-run interest (i.e. $\tau_i^* \geq \tau_i^b$).

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

S.1.7 Lump-Sum Tax Scheme

In the following, I build on the setup from Section 3, but I examine the individual's preferences under a lump-sum rather than proportional tax scheme. Specifically, I assume that a lump-sum tax is levied on individuals with an income above the mean ($x_i > \bar{x}$). I denote by $p \in (0,1)$ the fraction of the population with such an income, and they each pay τ in tax. I assume furthermore that individuals with an income below the mean receive $\lambda\tau\frac{p}{1-p}$, where $\lambda \in (0,1]$ denotes the efficiency of the redistribution scheme. Also, I assume that the tax preserves the order of the individuals' 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. Focusing on individuals who pay the tax, therefore, individual i's utility is as follows:[3]

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

As individual i maximises Equation S.16 with respect to τ, their preferred tax (influenced by forecast bias) is:

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

Individuals who are very biased and/or care very little about inequality prefer the corner solution of $\tau_i^b = 0$. Looking at individuals who are not at the corner solution, one sees that individuals vote for less redistribution than would be in their long-run interest if they are biased (i.e. $\tau_i^* \geq \tau_i^b$).

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

[3] 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: $x_i - \tau - \left(x_j + \frac{p}{1-p}\lambda\tau\right) = x_i - x_j - \tau\lambda\frac{1}{1-p}$.

not matter for the individuals who earn more than the mean. This is the case because the tax does not affect any comparison between individuals who pay the same lump-sum tax.

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 3), 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 ($\left.\frac{\partial \tau_i^b}{\partial x_k}\right|_{x_k<\bar{x}} = -\frac{1}{\lambda}(1-p) < 0$). The intuition behind the ambiguous result in Section 3 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.2 Mechanism: Forecasts Matter Through Inequality Information

If perceived costs of redistribution is driving the differences in behaviour between *Forecast* and *Realized*, then one would assume that participants' forecast bias in *Forecast* correlates with preferred tax rates, but not if one controls for perceived gains and perceived low personal costs. In contrast, forecast bias should be uncorrelated with the participants' behaviour in *Realized*, where participants are informed about the true level of inequality. I find that this is indeed the case: using the same measure of EGB as in Section 4, I find that EGB is a marginally significant predictor of the preferred tax rate in *Forecast* (tobit: $p = .085$, 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 (tobit: $p = 0.732$; SCLS: $p = .789$; Spearman's ρ: $p = 0.507$).[4]

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

[4] 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.

S.3 Extension 1: Details

In this section, I provide further details about Extension 1, which was briefly described in Section 5.

S.3.1 Experimental Design

S.3.1.1 Experiment

Extension 1 follows the design presented in Section 2. As described in Section 5, the main difference is that interest rates are different for each income class, with the poor, middle-income, and rich participants 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 the three main treatments 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 the three main treatments. In Supplementary Materials S.8.3, I provide evidence that this change in the size of the efficiency loss does not influence the participants' preferred tax rate.

For the voting part, participants are randomised into either the *RealizedR* or *ForecastR* treatment (the R reflects that relative inequality is also affected by growth). As in the main treatments, participants in *RealizedR* receive information about the actual post-redistribution incomes, whereas participants in *ForecastR* observe the post-redistribution incomes based on their subjective forecasts.

S.3.1.2 Procedure

For Extension 1, 1,105 participants were recruited on MTurk. The procedures were identical to those used in the main treatments, and applying the same screeners led to a main sample of 980 participants, as 11.3 percent of the responses were excluded. Similar to the main treatments, 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.17 and S.18 provide a full set of summary statistics.

As in the main treatments, participants 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.

S.3.2 Theory

The model in Section 3 can be extended to the case with unequal interest rates, and it yields the same qualitative predictions (see Supplementary Materials S.1.3). Note that the theoretical framework was not adapted based on the results previously described because the three main treatments and the extensions were pre-registered and carried out at the same time. Thus, the model in Supplementary Materials S.1.3 assumes that individuals experience increasing marginal disutility from absolute inequality, which is the key assumption for the prediction that participants 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.

S.3.2.1 Hypotheses

As in Section 3, the model assumes that individuals underestimate exponential developments for all compounding rates and initial amounts, following the literature on EGB. Because both absolute and relative inequality increase over the 30 rounds, I first test the model's assumptions about perceived growth, which implies that participants underestimate both types of inequality:

Hypothesis 3 *When interest rates correlate positively with initial incomes, participants on average underestimate how much absolute and relative inequality increase.*

Looking at voting behaviour, middle-income and rich participants who exhibit EGB (i.e. in *ForecastR*) are expected to underestimate the extent of inequality in the final round, but they realise that redistribution is costly for themselves. They therefore underestimate the need for redistribution, and the model in Supplementary Materials S.1.3 leads to the following hypothesis:

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

S.3.3 Results

As in the main treatments, I first analyse participants' inequality forecasts and then examine how forecasts influence preferences in the voting experiment. Descriptive statistics are presented in Table S.1.

Table S.1: Descriptive statistics, Extension 1

	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 participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' 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 participants' 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 participants are presented in Tables S.19 and S.20.

Forecasts. As explained in Section 5, the data provide clear support for H3. Figures S.5-S.10 reveal that participants underestimate both absolute and relative inequality across all measures. The underestimation is statistically significant (all $p's < .001$, bootstrapped t-tests).

In Extension 1, 88 participants (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.22), and there is no difference in the preferred tax rates among the participants who answered correctly and those who did not (treatments combined or analysed separately, all $p's > .627$). Of the remaining participants, 45 participants (5 percent) overestimate growth on average (i.e. $\theta < 0$), whereas 847 participants (95 percent) underestimate growth on average (i.e. $0 < \theta < 1$). I discuss heterogeneity in participants' forecasts in Supplementary Materials S.5.

Redistribution. As described in the main text, there are no meaningful differences in preferred tax rates between *ForecastR* (mean: 39.08) and *RealizedR* (mean: 39.83). Nevertheless, an exploratory inspection of the data reveals that providing information matters for the participants who grossly underestimate the personal costs of redistributing.

In Extension 1, 95 of the 415 middle-income participants in *ForecastR* or *RealizedR* (23 percent) make forecasts that would imply that they will gain from redistribution. This type of misperception is not accounted for in the theoretical model presented in Supplementary Materials S.1.3, 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. As explained in the main text, the information treatment only influences the demand for redistribution among these participants (APE = 20 percentage points, $p = .080$); all other participants are unaffected (APE = 6 percentage points, $p = .209$). Furthermore, wrongly perceiving the costs as minimal leads to an increase in the preferred tax rate of 10 percentage points, but this difference fails to reach statistical significance ($p = .151$).

Table S.2: Belief Correction and Demand for Redistribution, Extension 1

	(1)	(2)	(3)	(4)	(5)
RealizedR	1.91	1.57	1.16	1.91	5.94
	(4.17)	(4.03)	(4.02)	(3.88)	(4.73)
Forecasted Gains					24.60***
					(7.97)
Forecasted Gains×RealizedR					-19.84*
					(11.31)
Forecasted Low Costs					10.03
					(6.97)
Forecasted Low Costs×RealizedR					-6.47
					(9.74)
Dictator Givings	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Attitudinal Controls	No	No	No	Yes	Yes
Observations	698	698	698	698	698

Notes: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Forecasted Gains is a dummy equal to one if the participant made forecasts that imply that they would gain from taxation. Forecasted Low Costs is a dummy equal to one if the participant made a forecast that would imply that redistribution comes at negligible 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.21 for the full specification. Robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

To further shed light on the influence of forecast bias, I now examine how EGB correlates with the participants' preferred tax rate within the two treatments. As in the main treatments, I find suggestive evidence that EGB correlates with preferred tax rates in *ForecastR* (tobit: $p = .055$; 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: $p = .077$; SCLS: $p = .182$; Spearman's ρ: $p = .410$).

Surprisingly, EGB is related to the participants' preferred tax rate in *RealizedR* (tobit: $p = .042$; 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 correlate with voting preferences.

S.4 Extension 2: Details

In this section, I provide further details about Extension 1, which was briefly described in Section 5.

S.4.1 Experimental Design

S.4.1.1 Procedure

For Extension 2, 1,186 new participants were recruited on MTurk. All procedures and screeners were identical to those in the main treatments, except for two screeners that were based on the forecast task, as they were not possible to implement in Extension 2. The remaining screeners led to the exclusion of 92 responses (7.8 percent), yielding a main sample of 1,094 participants. The results are qualitatively robust to including all participants. 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.23 and S.24.

Participants 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.

S.4.2 Theory

The theoretical framework presented in Section 3 assumes that subjective forecasts only matter via beliefs about inequality in the final round. Thus, the model is directly applicable to the case where participants simply vote based on forecasts. Again, the theoretical framework was not adapted to the results described in Section 4 because all treatments were pre-registered and carried out at the same time. 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 participants 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).

S.4.2.1 Hypothesis

Participants in *ForecastNo* observe a smaller extent of absolute inequality than participants in *RealizedNo*. This leads to the following hypothesis:

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

S.4.3 Results

In the following, I analyse how inequality influences preferences in the voting experiment. Table S.3 provides descriptive statistics.

Table S.3: Descriptive statistics, Extension 2

	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 participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' 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 participants are presented in Tables S.27 and S.28.

Looking at the middle-income and rich participants, 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 participants in groups with low levels of inequality (see Figure S.12). For these participants, redistribution bears almost no personal costs, indicating that perceiving low costs might be the critical factor, as in the main treatments.

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, no participant wrongly believes that they will gain from redistribution because the forecasted incomes were chosen such that all middle-income and rich participants 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 participant who only cares about their own payoff will be indifferent between different tax rates. Hence, these middle-income participants will vote for greater redistribution even if they are only slightly inequality averse. Accordingly, participants who faced low personal costs on average vote for tax rates that are 29.55 percentage points higher ($p < .001$, cf. Table S.4). When including a dummy for low personal costs, I find no difference between *ForecastNo* and *RealizedNo* ($p = .613$, cf. Table S.4; SCLS: $p = .956$; MWU: $p = .824$). This corroborates the results from the previous treatments, 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.

Table S.4: Belief Correction and Demand for Redistribution, Extension 2

	(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)
Low Personal Cost					29.55***
					(6.04)
Dictator Giving	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Attitudinal Controls	No	No	No	Yes	Yes
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 participant is randomised into an allocation that implies 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.26 for the full specification. Robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

S.5 Heterogeneity in Participants' Forecasts

S.5.1 Main Treatments

First, I look at what factors predict whether participants provide accurate forecasts 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.5). No other factor is significant.

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

Table S.5: Characteristics of participants who answer correctly for all incomes, main treatments

	(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

	(1)	(2)	(3)	(4)
		(0.0624)	(0.0630)	(0.0634)
Income$_p$		0.0642	0.0625	0.0537
		(0.0477)	(0.0475)	(0.0482)
Risk$_p$				0.0142
				(0.0302)
Trust$_p$				-0.0578*
				(0.0305)
Political Right$_p$				-0.0134
				(0.0358)
Meritocracy$_p$				0.0215
				(0.0335)
Inequality Too Large$_p$				0.0503
				(0.0383)
Government Responsibility$_p$				-0.0706**
				(0.0340)
Observations	1415	1415	1415	1415

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.6: Explaining participants' degree of EGB, main treatments

	(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)

	(1)	(2)
Associate degree	0.122	0.130
	(0.174)	(0.179)
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 Extension 1

For Extension 1, I also look at what factors predict whether participants provide accurate forecasts 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.7).

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

Table S.7: Characteristics of participants who answer correctly for all incomes, Extension 1

	(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)
Inequality Too Large_p				-0.0035
				(0.0420)
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 participants with less than high school degree from the regression; they perfectly predict failure and make education inestimable. Robust standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table S.8: Explaining participants' degree of EGB, Extension 1

	(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 Ex-Ante Power Analysis

With the available funding, I aimed to recruit 1,329 participants for the main treatments, 886 participants for Extension 1, and 886 participants for Extension 2. With this sample size, I computed the minimum detectable effect size with a power of 0.8 via simulations (Stata, version 16). Here, I focus solely on testing H2; this only concerns the middle-income and rich participants (5/7 of the sample), and it requires participants to be divided into different treatments. It is therefore the hypothesis that I have the least power to test.[5]

[5] I did not expect every participant to provide useful responses that passed all screeners. For instance, Kennedy et al. (2020) find that 6.8 percent of participants 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 participants to fail one of the screeners that I employed in this 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

In the power analysis, I employ a mean tax rate of 32 percent and a standard deviation of 30, which are the observed values in a pilot study. The simulations show that this yields a power of approximately 0.8 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 2.6, 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 a power of .8 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.

S.7 Attrition

In the following, I examine what factors explain whether participants complete the study (logit regressions, see Table S.9). For this analysis, I only consider participants 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, preventing participants from completing the study. To test differential attrition, I therefore also exclude participants in *Ratio* who began the study during the first two hours. As the attitudinal survey was the last that participants completed, I am not able to examine if attitudes predict whether participants complete the study.

One concern is that participants 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 participants are 2.6 percentage points more likely to complete the study if they are in the rich income group, but this difference fails to reach statistical significance ($p = .072$).

A second concern is that there may be differential attrition by treatment. The only significant difference is that participants in *RealizedNo* are 3.8-7.8 percentage points more likely to complete the study than participants in any other treatment (although the difference with *Forecast* is only marginally significant). Importantly, participants in *RealizedNo* did not have to perform the forecast task, and the experiment was therefore a few minutes shorter for these participants than for participants in the main treatments and Extension 1. 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 participants are more likely to complete the study. Pooling participants with a bachelor's, master's, doctorate, or professional degree

that I invited in total 4,031 participants to participate.

shows that participants 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 participants' misperceptions is conservative in the current paper.

Table S.9: Attrition

	(1)	(2)	(3)	(4)
Middle Class	0.0060	0.0081		
	(0.01)	(0.01)		
Rich	0.0261*	0.0256*		
	(0.01)	(0.01)		
Ratio			-0.0240	-0.0287
			(0.02)	(0.02)
Realized			-0.0404*	-0.0416*
			(0.02)	(0.02)
ForecastR			-0.0270	-0.0270
			(0.02)	(0.02)
RealizedR			-0.0327	-0.0360*
			(0.02)	(0.02)
ForecastNo			-0.0061	-0.0081
			(0.02)	(0.02)
RealizedNo			0.0376*	0.0375*
			(0.02)	(0.02)
Dictator Giving		-0.0006*		-0.0006*
		(0.00)		(0.00)
Efficiency from MDG		-0.0006		-0.0018
		(0.00)		(0.00)
Age		-0.0027***		-0.0031***
		(0.00)		(0.00)
Male		0.0046		0.0134
		(0.01)		(0.01)
Black or African American		-0.0410*		-0.0454**
		(0.02)		(0.02)
Hispanic or Latino		-0.0531*		-0.0695**
		(0.03)		(0.03)
Asian American		-0.0322		-0.0273
		(0.03)		(0.03)
Other ethnicity		-0.0729		-0.1009**

	(1)	(2)	(3)	(4)
		(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
		(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.8 Further Discussion

S.8.1 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 participants provide meaningful answers to the current experimental tasks.

Dictator Giving. In all studies, participants are asked to make a decision as the dictator in a standard dictator game (strategy method). Across all treatments, participants 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, participants 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 treatments, participants 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 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, participants' 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 participants exhibit preferences corresponding to a wealth Gini of .21 and .19, respectively.

Exponential Growth Bias. In the main treatments, participants made forecasts for three income groups with uniform growth rates. With the functional specification of exponential growth bias from Stango and Zinman (2009), participants in this study exhibited an average bias of $\hat{\theta} = .46$. This is close to the average bias of $\hat{\theta} = .49$ in Extension 1, where participants 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

[6] As in the pilot study, some participants 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 participants who provide a monotonic relation. This was the case for 948 participants (67 percent) in the main treatments, 941 participants (65 percent) in Extension 1, and 715 (65 percent) in Extension 2. If the participants who provide valid responses to the task of estimating wealth distributions are more knowledgeable or sophisticated than other participants, then this additional sample restriction implies that my estimate of participants' misperceptions is conservative.

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

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

	Main Treatments	Extension 1	Extension 2	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 participants. DG is the share that participants 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 participants' estimates of the wealth distribution in the US, and Gini-Preferences are calculated from participants' ideal wealth distribution in the US.

S.8.2 The Behaviour of the 'Poor' Individuals

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' participants, the two motivations work in the same direction, leading to a preference for full redistribution. To test whether the theory truly reflects the motivation that participants have in this experiment, one can examine the poor participants. 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 participants. In contrast to the difficulties with testing the corner prediction, it is easy to test the directional prediction that poor participants vote for higher taxes than middle-class and rich participants, and I therefore proceed with this test in the following, where I examine the main treatments and the extensions separately.

In the main treatments, poor participants vote for more redistribution (median: 80) than participants in the middle and rich income classes (medians: 40 and 25), and this difference is statistically significant ($p < .001$). Still, only 43 percent of the poor vote for full redistribution. This suggests that other concerns may influence how participants vote. For example, one participant in *Ratio* noted that she 'felt guilty taking a large amount of someone else's money'.[8]

[8]Another poor participant commented: 'I tried not to tax it too much because that will be taking more money.' Opposingly, other poor participants mentioned self-interest or fairness. For instance, 'I honestly just

It may also be the case that participants 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 participants 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 Extension 1, I again find that the median tax rate is 80 among the poor participants; only 44 percent vote for full redistribution. Importantly, the poor participants on average vote for a tax rate that is 24.57 (31.72) percentage points greater than the middle-class (rich) participants. These differences are statistically significant ($p < .001$) and robust (MWU: $p < .001$). Also in Extension 1, 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 Extension 2, the median tax rate is again 80 among the poor participants, and 47 percent vote for full redistribution. The poor on average vote for a tax that is 15.30 higher than the middle-class participants and 25.01 higher than the rich participants. These differences are statistically significant ($p < .001$) 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.8.3 What Concerns Influence Participants' Preferred Tax Rates?

For the theoretical framework in Section 3, 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.

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.'

Self-Interest. The above analysis shows that participants randomised into the 'poor' income class vote for greater taxes across all treatments, and believing that one gains from the tax leads to a preference for more taxation.[9] Together, these findings demonstrate that self-interest indeed matters in the current voting experiment.

Inequality Aversion. The fact that participants who give more in the standard dictator game also vote for a higher tax rate indicates that inequality aversion matters for the preferred level of redistribution. Yet, giving in the dictator game does not change the participants' responsiveness to increases in inequality: there are no interaction effects between dictator givings and the treatment effects (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 $p's > .183$). Hence, inequality aversion seems to matter for an individual's desired level of redistribution, but it does not change the individual's responsiveness to an increase in inequality.

Efficiency. People with greater preferences for efficiency vote for significantly lower taxes in all treatments (all $p's < .001$), and this result also holds when one controls for dictator givings, demographics, risk preferences, trust, and political attitudes. The effect is economically significant as well: across all treatments, 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 (Extension 1, $APE = -4.11$), which is insignificant according to a Wald chi-square test for coefficients across tobit regressions ($p = .918$; see Table S.31 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.

[9]The importance of self-interest also receives qualitative support by statements from participants. For instance, a middle-income participant 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 participants consider what is fair, participants often trade-off fairness with self-interest. For example, a middle-income participant 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.'

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

I find that image concerns as measured by MLAMS do not correlate with participants' preferred tax rates in any of the treatments (all $p's > .118$). 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 participants' behaviour in the voting experiment suggests that participants are able to make payoff-maximising decisions without compromising their (self-)image. This could, for instance, be the case if participants justify their selfish behaviour by appealing to efficiency preferences, following the literature on how individuals often choose fairness principles in a self-serving manner (Messick and Sentis, 1979; Rodriguez-Lara and Moreno-Garrido, 2012).

S.8.4 Effect of Making a Forecast

The experiment reported in this paper demonstrates that individuals have erroneous beliefs about how growth influences inequality, but informing individuals about the actual development in inequality does not influence their preferences for redistribution beyond changing their beliefs about their personal costs of redistribution. Extension 2 demonstrated that this likely occurs because the level of inequality does not influence preferences for redistribution. A different question is whether first making a forecast and then receiving accurate information leads to different preferences for redistribution than simply receiving accurate information in the first place. There could be different reasons for why the act of forecasting influences behaviour. For instance, the initial distribution or the forecasted distribution may serve as reference points (Charité et al., 2022), leading participants to believe that if all income classes earn more than the reference point, 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).[10]

One can test this reasoning by comparing *Realized* and *RealizedNo*, as the only difference between these treatments is that participants in *Realized* make a forecast while participants in *RealizedNo* do not. If the initial incomes serve as reference points, participants in *Realized* should be less inclined to redistribute, as the poor are well-off in the sense that their final income is much higher than their initial income. Supporting this idea, participants on average vote for a higher tax rate in *RealizedNo* than in *Realized* (9 percentage points, $p = .024$, cf. Figure S.13), and this difference is robust (SCLS: $p = .017$; MWU: $p = .033$).

S.8.5 Inequality Concepts and Preferences for Redistribution

There are many ways of operationalising 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 that informing individuals about true levels of inequality in a group does not influence their preferences for redistribution. This result was corroborated by the evidence from Extension 2, which suggests that the perceived level of inequality does not influence preferences for redistribution. Further evidence suggests that this result is not influenced by how one measures inequality. First, one may compare *Realized* and *RealizedR*. These treatments differ in whether the growth rates are uniform or unequal, and comparing the two therefore sheds light on the importance of an increase in relative inequality. I find that there are no differences between the two treatments, also when controlling for dictator givings, demographics, or (political) attitudes (40.74 vs. 39.83 percent, all $p's > .816$).[11]

Second, one may examine whether perceived inequality predicts redistributive preferences within the treatments *Forecast*, *Ratio*, *ForecastR*, and *ForecastNo*. For completeness, I examine

[10] In principle, the reference point could also be the forecasted income levels for the final round. Yet, since there is no relation between EGB and participants' preferred tax rates in *Realized* (cf. Section 4.2), this seems unlikely.

[11] 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, 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.14). Finally, 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).

here 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.29, none of these conceptualisations are significant predictors of the tax rate for which a participant votes.[12] 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 participant votes (see Table S.30).

Note that the fact that no inequality measure correlates with participants' 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 to an increase in the personal costs of redistribution. According to this explanation, the two effects cancel out, such that the share of their income that participants are willing to give up remains constant.

S.9 Additional Tables and Figures, Main Treatments

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

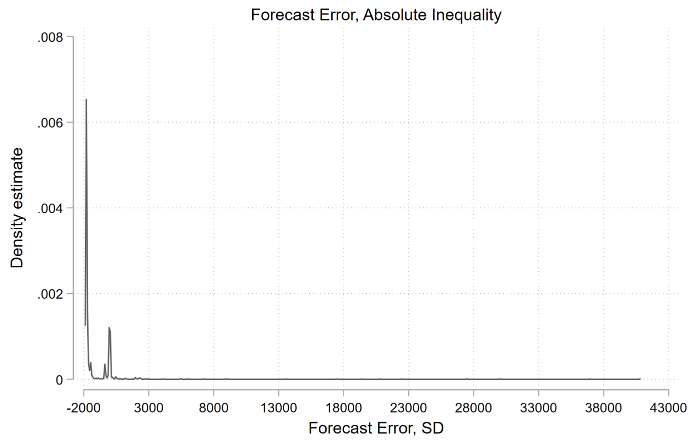
Table S.11: Sample characteristics, main treatments

	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, main treatments

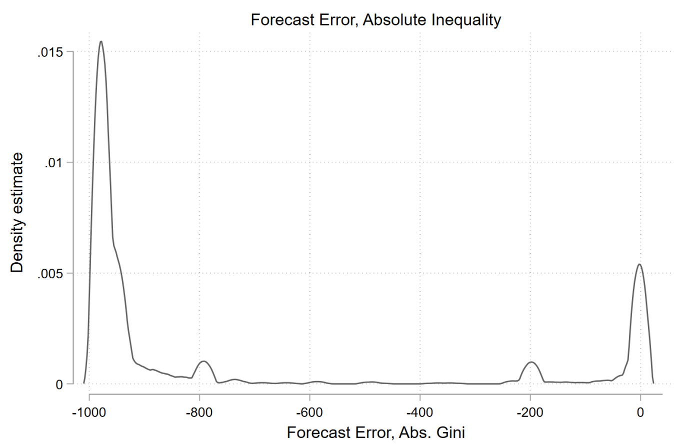
	Forecast	Ratio	Realized	Total
Risk	0.47	0.47	0.46	0.47
Trust	0.53	0.54	0.53	0.53
Political Right	0.44	0.43	0.42	0.43
Belief in Meritocracy	0.63	0.62	0.59	0.62
Inequality Too Large	0.80	0.81	0.80	0.81
Government Responsibility	0.57	0.57	0.59	0.58
MLAMS	0.37	0.38	0.38	0.38
Social Ladder	0.43	0.43	0.42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

Figure S.1: Forecast error of absolute inequality, main treatments, full sample



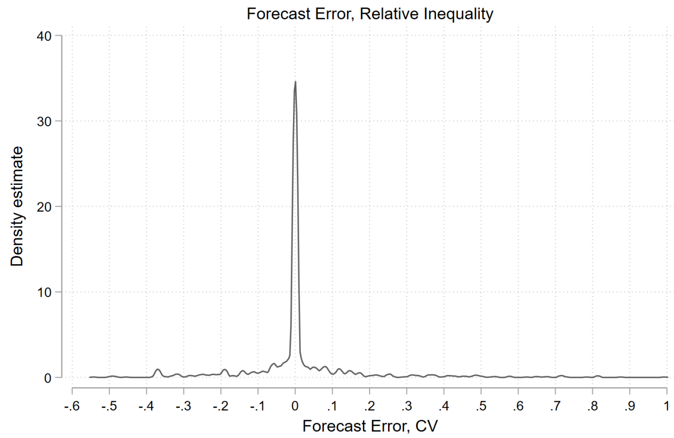
Notes: the figure shows the kernel density of participants' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}\right]^{\frac{1}{2}}$.

Figure S.2: Forecast error of absolute inequality, Abs. Gini, main treatments 1



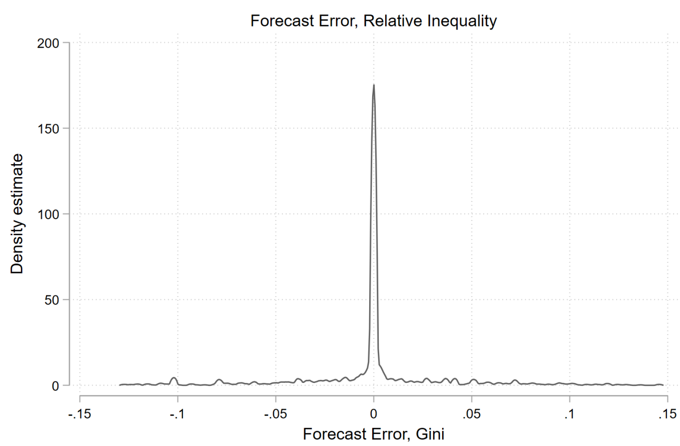
Notes: the figure shows the kernel density of participants' forecast error (epanechnikov, $bw = 10$). The Absolute Gini coefficient is calculated as $AG(\mathbf{x}) = \bar{x}\left(\frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i\right)$, where x_i are ranked-ordered incomes such that $x_i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

Figure S.3: Forecast error of relative inequality, main treatments, full sample



Notes: the figure shows the kernel density of participants' 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.4: Forecast error of relative inequality, Gini, Main Treatments



Notes: the figure shows the kernel density of participants' 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: Belief Correction and Demand for Redistribution, Full Specification

	(1)	(2)	(3)	(4)	(5)
Ratio	-8.31*	-8.41*	-7.85*	-7.49*	-1.62
	(4.69)	(4.56)	(4.53)	(4.32)	(4.63)
Realized	-11.87***	-12.68***	-12.98***	-13.88***	-2.20
	(4.49)	(4.36)	(4.34)	(4.14)	(4.99)
Forecasted Gains					50.03***
					(8.18)
Forecasted Gains×Ratio					-35.91***
					(11.52)
Forecasted Gains×Realized					-42.28***
					(11.23)
Forecasted Low Costs					13.48***
					(5.11)
Forecasted Low Costs×Realized					-21.94**
					(8.84)
Dictator Giving		0.73***	0.70***	0.58***	0.62***
		(0.09)	(0.09)	(0.09)	(0.09)
Age			-0.36**	-0.19	-0.18
			(0.17)	(0.17)	(0.17)
Male			-7.93**	-5.01	-4.91
			(3.75)	(3.64)	(3.57)
Black or African American			-1.40	-2.99	-2.34
			(7.08)	(6.74)	(6.60)
Hispanic or Latino			5.41	2.56	4.68
			(9.39)	(8.88)	(8.70)
Asian American			-1.73	-5.31	-4.77
			(7.58)	(7.25)	(7.13)
Other ethnicity			-2.36	0.71	1.57
			(15.89)	(15.15)	(14.86)
High school degree or equivalent (e.g. GED)			-23.79	-16.01	-18.78
			(24.56)	(23.45)	(22.81)
Some college, no degree			-16.71	-14.65	-17.60
			(24.19)	(23.08)	(22.45)
Associate degree (e.g. AA, AS)			-23.46	-19.14	-25.06
			(24.57)	(23.45)	(22.84)
Bachelor's degree (e.g. BA, BS)			-17.67	-16.47	-19.81
			(24.21)	(23.12)	(22.48)
Master's degree (e.g. MA, MS, MEd)			-9.23	-10.19	-15.52
			(24.58)	(23.47)	(22.84)
Doctorate or pro degree (e.g. MD, DDS, PhD)			-26.05	-26.43	-30.72
			(25.73)	(24.59)	(23.96)
Self-employed			7.24	7.09	5.90
			(5.70)	(5.45)	(5.33)
Unemployed			0.86	0.54	-2.51
			(6.04)	(5.81)	(5.72)
Student			-9.64	-13.12	-14.33
			(9.94)	(9.50)	(9.34)
Retired			3.57	-0.23	-0.67

	(1)	(2)	(3)	(4)	(5)
			(9.77)	(9.38)	(9.18)
Other employment			1.29	7.03	4.15
			(12.58)	(12.11)	(11.78)
Income$_p$			-30.25***	-8.16	-9.91
			(10.83)	(10.78)	(10.60)
Efficiency from MDG				-1.51**	-1.59**
				(0.68)	(0.66)
Risk$_p$				-12.85*	-11.41
				(7.39)	(7.23)
Trust$_p$				16.34**	14.11*
				(7.60)	(7.46)
Political Right$_p$				-25.49***	-21.11***
				(7.88)	(7.73)
Meritocracy$_p$				-14.01*	-13.74*
				(7.46)	(7.30)
Inequality Too Large$_p$				22.50***	20.77**
				(8.34)	(8.15)
Government Responsibility$_p$				10.77	14.04**
				(7.28)	(7.15)
MLAMS$_p$				-7.09	-10.68
				(11.83)	(11.59)
Observations	1013	1013	1013	1013	1013

Notes: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Forecasted Gains is a dummy equal to one if the participant made forecasts that imply that they would gain from taxation. Forecasted Low Costs is a dummy equal to one if the participant made a forecast that would imply that redistribution comes at negligible 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.14: Belief Correction and Demand for Redistribution, Restricted Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Ratio	-9.14*	-9.31*	-8.67*	-7.79*	-1.52	
	(4.89)	(4.79)	(4.77)	(4.54)	(4.90)	
Realized	-11.38**	-12.59***	-12.98***	-14.01***	-0.20	
	(4.69)	(4.60)	(4.59)	(4.37)	(5.42)	
Forecasted Gains					52.25***	
					(8.33)	
Forecasted Gains×Ratio					-35.28***	
					(11.63)	
Forecasted Gains×Realized					-44.78***	
					(11.46)	
Forecasted Low Costs					15.65***	
					(5.25)	
Forecasted Low Costs×Realized					-23.74***	
					(9.06)	
Dictator Giving		0.63***	0.60***	0.48***	0.51***	
		(0.10)	(0.10)	(0.10)	(0.10)	
Age			-0.28	-0.12	-0.12	
			(0.18)	(0.18)	(0.17)	
Male			-7.79*	-5.72	-5.79	
			(3.97)	(3.85)	(3.77)	
Black or African American			-0.24	-2.03	-0.98	
			(7.15)	(6.80)	(6.63)	
Hispanic or Latino			4.87	1.75	4.41	
			(9.94)	(9.37)	(9.14)	
Asian American			-3.91	-7.79	-7.90	
			(8.23)	(7.88)	(7.74)	
Other ethnicity			-2.45	0.64	2.36	
			(16.00)	(15.23)	(14.89)	
High school degree or equivalent (e.g. GED)			-12.06	-2.93	-4.80	
			(26.44)	(25.34)	(24.58)	
Some college, no degree			-5.25	-2.07	-4.38	
			(26.10)	(24.99)	(24.25)	
Associate degree (e.g. AA, AS)			-15.32	-9.37	-14.56	
			(26.47)	(25.34)	(24.62)	
Bachelor's degree (e.g. BA, BS)			-5.33	-2.50	-4.96	
			(26.14)	(25.03)	(24.30)	
Master's degree (e.g. MA, MS, MEd)			2.98	4.44	-1.54	
			(26.54)	(25.43)	(24.69)	
Doctorate or pro degree (e.g. MD, DDS, PhD)			-14.98	-15.77	-19.66	
			(27.74)	(26.56)	(25.82)	
Self-employed			3.87	3.66	2.48	
			(5.94)	(5.68)	(5.54)	
Unemployed			-1.09	-0.72	-3.67	
			(6.23)	(5.98)	(5.86)	
Student			-9.04	-12.35	-12.98	
			(10.05)	(9.60)	(9.41)	
Retired			2.68	-1.61	-1.54	

	(1)	(2)	(3)	(4)	(5)	(6)
			(10.15)	(9.73)	(9.49)	
Other employment			0.35	8.03	4.12	
			(13.27)	(12.78)	(12.39)	
Income_p			-30.66***	-9.83	-12.12	
			(11.51)	(11.42)	(11.19)	
Efficiency from MDG				-1.77**	-1.88***	
				(0.71)	(0.70)	
Risk_p				-10.55	-9.11	
				(7.74)	(7.55)	
Trust_p				19.22**	16.96**	
				(7.99)	(7.82)	
Political Right_p				-23.23***	-17.85**	
				(8.30)	(8.12)	
Meritocracy_p				-12.83*	-12.99*	
				(7.78)	(7.60)	
$\text{Inequality Too Large}_p$				25.01***	22.94***	
				(8.80)	(8.58)	
$\text{Government Responsibility}_p$				12.20	16.36**	
				(7.70)	(7.54)	
MLAMS_p				-11.09	-15.48	
				(12.29)	(12.01)	
Observations	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 participants who do not provide correct answers for all inequality forecasts. Forecasted Gains is a dummy equal to one if the participant made forecasts that imply that they would gain from taxation. Forecasted Low Costs is a dummy equal to one if the participant made a forecast that would imply that redistribution comes at negligible 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.15: Descriptive statistics for poor participants, main treatments

	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 poor participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' 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 participants' 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.16: Descriptive statistics for middle-income and rich participants, main treatments

	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 participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' 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 participants' 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 participants in the *Ratio* treatment during the first two hours of the data collection, and this explains why there are fewer observations in this treatment.

S.10 Additional Tables and Figures, Extension 1

Table S.17: Sample characteristics, Extension 1

	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.18: Summary statistics by treatment, Extension 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

Table S.19: Descriptive statistics for poor participants, Extension 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

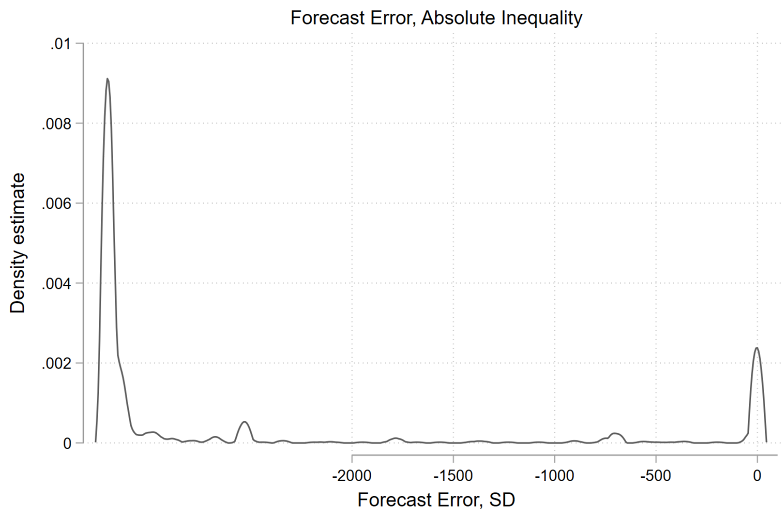
Notes: averages are taken over all poor participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by participants' 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.20: Descriptive statistics for middle-income and rich participants, Extension 1

	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

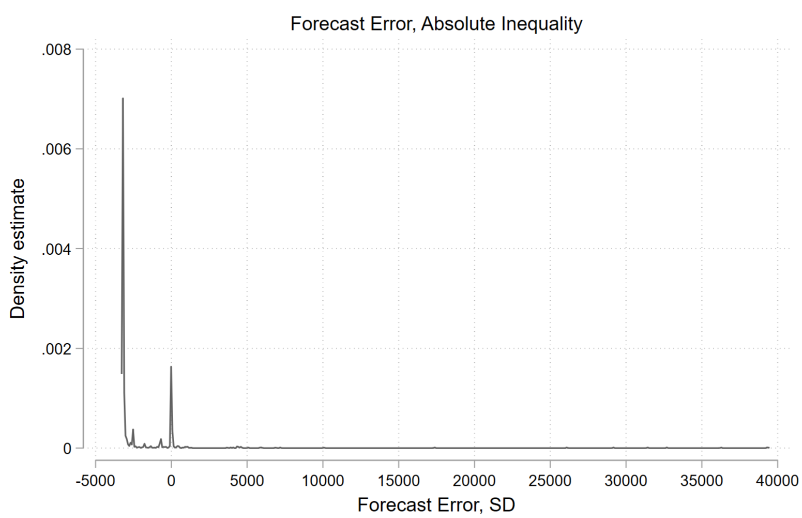
Notes: averages are taken over all middle-income and rich participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency). SD (F) and CV (F) are the average standard deviation and coefficient of variation that are implied by participants' 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.5: Forecast error of absolute inequality, Extension 1



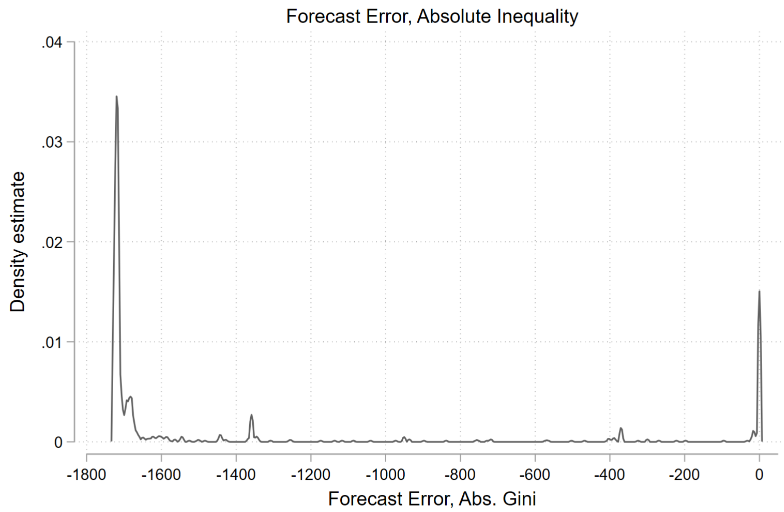
Notes: the figure shows the kernel density of participants' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}} \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N} \right]^{\frac{1}{2}}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors. For the full sample, see Figure S.6.

Figure S.6: Forecast error of absolute inequality, Extension 1, full sample



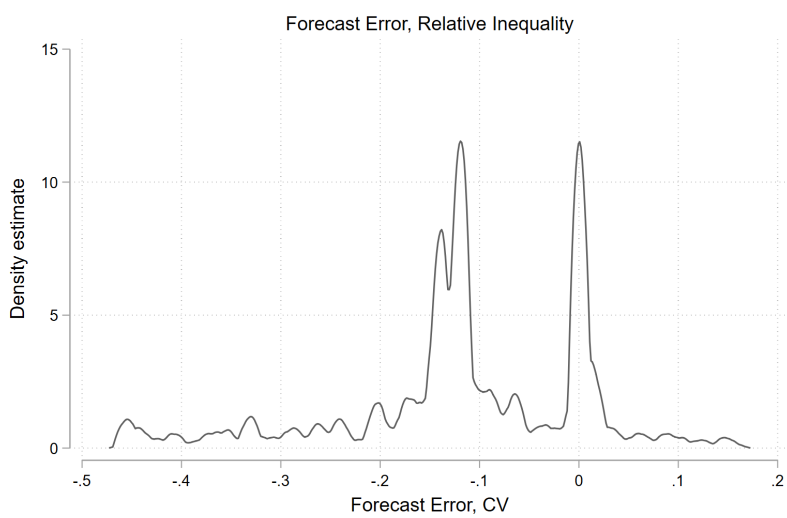
Notes: the figure shows the kernel density of participants' forecast error (epanechnikov, $bw = 20$). The standard deviation is calculated as $SD(\mathbf{x}) = \left[\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N} \right]^{\frac{1}{2}}$.

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



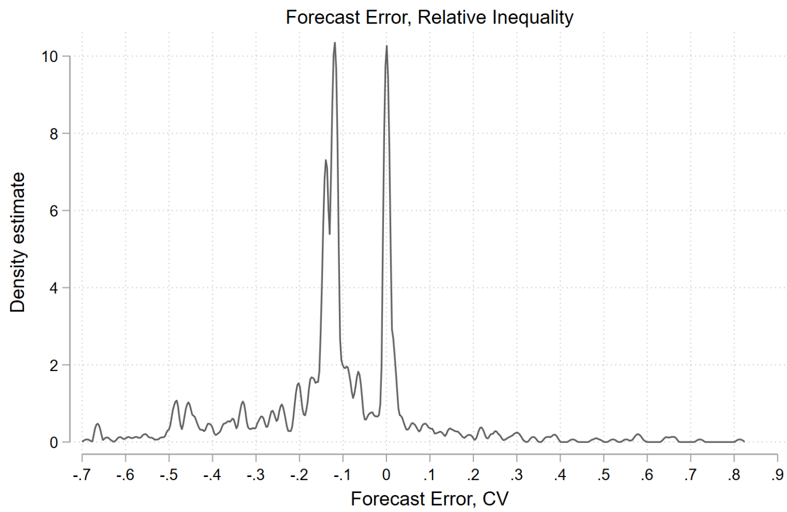
Notes: the figure shows the kernel density of participants' forecast error (epanechnikov, $bw = 10$). The Absolute Gini coefficient is calculated as $AG(\mathbf{x}) = \bar{x}\left(\frac{N+1}{N} - \frac{2}{N^2\bar{x}}\sum_{i=1}^{N}(N+1-i)x_i\right)$, where x_i are ranked-ordered incomes such that $x_i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

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



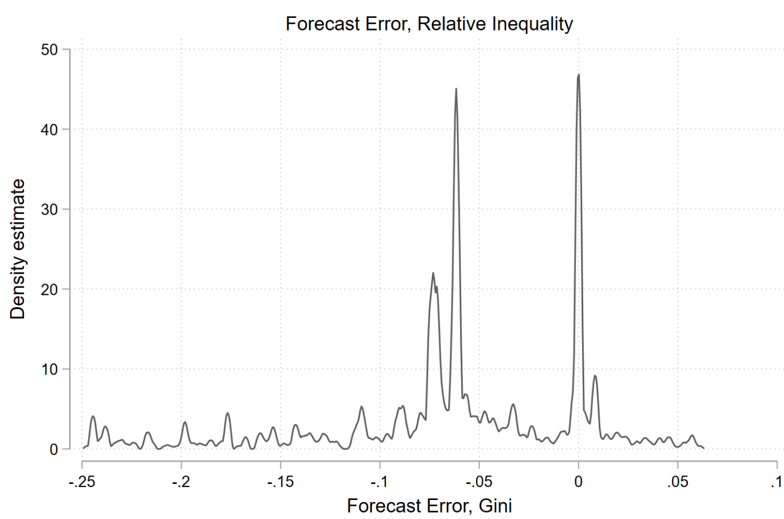
Notes: the figure shows the kernel density of participants' forecast error (epanechnikov, $bw = 0.005$). The coefficient of variation is calculated as $CV(\mathbf{x}) = \frac{1}{\bar{x}}\left[\sum_{i=1}^{N}\frac{(x_i-\bar{x})^2}{N}\right]^{\frac{1}{2}}$. For illustrative purposes, the figure excludes the 5 percent smallest and largest errors. For the full sample, see Figure S.9.

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



Notes: the figure shows the kernel density of participants' 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, Extension 1



Notes: the figure shows the kernel density of participants' 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.21: Belief Correction and Demand for Redistribution, Full Specification, Extension 1

	(1)	(2)	(3)	(4)	(5)
RealizedR	1.91	1.57	1.16	1.91	5.94
	(4.17)	(4.03)	(4.02)	(3.88)	(4.73)
Forecasted Gains					24.60***
					(7.97)
Forecasted Gains×RealizedR					-19.84*
					(11.31)
Forecasted Low Costs					10.03
					(6.97)
Forecasted Low Costs×RealizedR					-6.47
					(9.74)
Dictator Giving		0.77***	0.79***	0.68***	0.68***
		(0.10)	(0.10)	(0.10)	(0.10)
Age			-0.15	-0.05	-0.08
			(0.19)	(0.18)	(0.18)
Male			-1.62	-1.66	-1.50
			(4.16)	(4.06)	(4.04)
White or Caucasian American			0.00	0.00	0.00
			(.)	(.)	(.)
Black or African American			-5.83	-6.45	-5.66
			(6.73)	(6.62)	(6.58)
Hispanic or Latino			17.87*	14.99	13.38
			(10.01)	(9.69)	(9.63)
Asian American			-5.47	-3.95	-3.51
			(7.76)	(7.59)	(7.54)
Other ethnicity			23.72	26.88*	27.52*
			(14.79)	(14.59)	(14.47)
Less than a high school diploma			0.00	0.00	0.00
			(.)	(.)	(.)
High school degree or equivalent (e.g. GED)			25.23	40.66	36.64
			(50.41)	(48.59)	(48.19)
Some college, no degree			33.67	48.01	42.96
			(50.25)	(48.45)	(48.05)
Associate degree (e.g. AA, AS)			26.90	39.44	33.89
			(50.34)	(48.52)	(48.13)
Bachelor's degree (e.g. BA, BS)			43.13	56.16	50.96
			(50.16)	(48.35)	(47.95)
Master's degree (e.g. MA, MS, MEd)			50.64	60.07	55.46
			(50.40)	(48.58)	(48.20)
Doctorate or pro degree (e.g. MD, DDS, PhD)			65.53	71.03	66.19
			(51.23)	(49.38)	(48.98)
Employed (part or full time)			0.00	0.00	0.00
			(.)	(.)	(.)
Self-employed			3.43	3.04	2.93
			(6.07)	(5.95)	(5.90)
Unemployed			13.13*	13.62*	14.15*
			(7.54)	(7.33)	(7.29)
Student			4.79	-1.77	-0.17

	(1)	(2)	(3)	(4)	(5)
			(11.28)	(10.98)	(10.97)
Retired			-6.49	-9.60	-9.81
			(10.50)	(10.28)	(10.21)
Other employment			-3.50	-4.30	-3.98
			(12.70)	(12.38)	(12.32)
Income_p			1.27	10.61	11.47
			(11.91)	(11.91)	(11.83)
Efficiency from MDG				-2.36***	-2.38***
				(0.78)	(0.78)
Risk_p				-4.85	-4.79
				(8.12)	(8.08)
Trust_p				22.98***	21.62**
				(8.77)	(8.71)
Political Right_p				-2.34	-3.78
				(8.78)	(8.73)
Meritocracy_p				-19.90**	-20.52**
				(8.55)	(8.50)
Inequality Too Large_p				5.15	5.36
				(9.80)	(9.74)
Government Responsibility_p				20.14**	18.70**
				(8.07)	(8.01)
MLAMS_p				-1.29	0.86
				(13.48)	(13.40)
Observations	698	698	698	698	698

Notes: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Forecasted Gains is a dummy equal to one if the participant made forecasts that imply that they would gain from taxation. Forecasted Low Costs is a dummy equal to one if the participant made a forecast that would imply that redistribution comes at negligible 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.22: Belief Correction and Demand for Redistribution, Restricted Sample, Extension 1

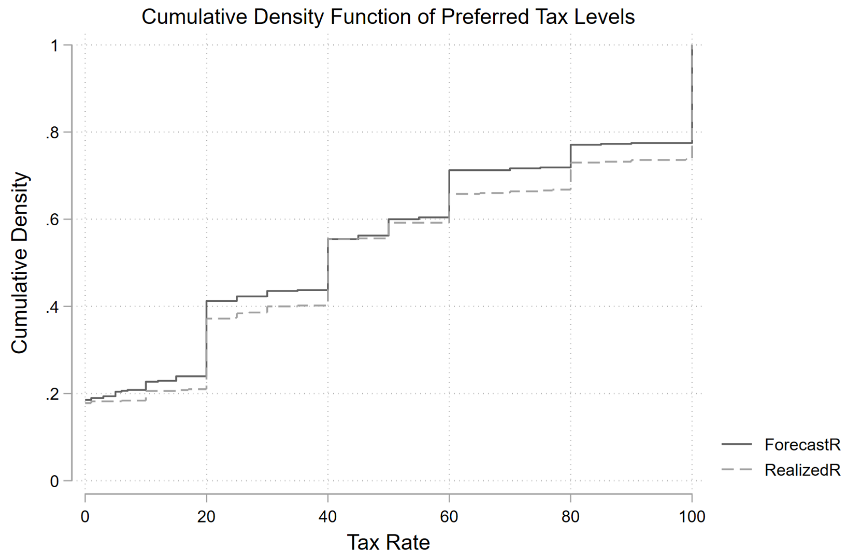
	(1)	(2)	(3)	(4)	(5)
RealizedR	2.75	1.80	1.04	1.28	6.10
	(4.34)	(4.20)	(4.18)	(4.04)	(5.04)
Forecasted Gains					25.10***
					(8.03)
Forecasted Gains×RealizedR					-20.60*
					(11.37)
Forecasted Low Costs					10.76
					(7.03)
Forecasted Low Costs×RealizedR					-7.34
					(9.83)
Dictator Giving		0.76***	0.78***	0.68***	0.68***
		(0.11)	(0.11)	(0.11)	(0.11)

	(1)	(2)	(3)	(4)	(5)
Age			-0.14	-0.03	-0.05
			(0.20)	(0.19)	(0.19)
Male			-2.59	-2.88	-2.90
			(4.38)	(4.28)	(4.25)
Black or African American			-6.82	-8.11	-7.04
			(6.89)	(6.79)	(6.74)
Hispanic or Latino			14.08	11.46	9.63
			(10.59)	(10.26)	(10.19)
Asian American			-6.68	-5.92	-5.28
			(8.03)	(7.84)	(7.78)
Other ethnicity			25.47*	29.46*	30.12**
			(15.38)	(15.20)	(15.05)
High school degree or equivalent (e.g. GED)			24.65	41.19	36.60
			(50.06)	(48.18)	(47.73)
Some college, no degree			33.00	49.03	43.23
			(49.91)	(48.05)	(47.61)
Associate degree (e.g. AA, AS)			26.86	40.00	33.48
			(50.02)	(48.12)	(47.70)
Bachelor's degree (e.g. BA, BS)			42.36	57.59	51.46
			(49.82)	(47.95)	(47.52)
Master's degree (e.g. MA, MS, MEd)			52.38	63.65	57.95
			(50.10)	(48.22)	(47.79)
Doctorate or pro degree (e.g. MD, DDS, PhD)			65.34	72.49	66.91
			(50.99)	(49.06)	(48.61)
Self-employed			2.91	3.19	3.10
			(6.27)	(6.13)	(6.07)
Unemployed			11.03	12.07	12.73*
			(7.70)	(7.48)	(7.44)
Student			5.06	-1.48	0.42
			(11.48)	(11.17)	(11.15)
Retired			-7.33	-10.33	-10.52
			(10.76)	(10.57)	(10.50)
Other employment			-9.58	-10.19	-9.71
			(13.00)	(12.68)	(12.60)
Income$_p$			-3.46	8.07	8.61
			(12.46)	(12.51)	(12.41)
Efficiency from MDG				-2.35***	-2.37***
				(0.82)	(0.81)
Risk$_p$				-6.44	-5.99
				(8.39)	(8.34)
Trust$_p$				20.24**	19.07**
				(9.07)	(9.00)
Political Right$_p$				1.11	-0.39
				(9.01)	(8.94)
Meritocracy$_p$				-19.45**	-19.62**
				(8.96)	(8.89)
Inequality Too Large$_p$				6.22	6.09
				(10.05)	(9.98)
Government Responsibility$_p$				24.61***	23.22***

	(1)	(2)	(3)	(4)	(5)
				(8.29)	(8.22)
MLAMS$_p$				-0.57	2.04
				(13.88)	(13.80)
Observations	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 participants who do not provide correct answers to all inequality forecasts. Forecasted Gains is a dummy equal to one if the participant made forecasts that imply that they would gain from taxation. Forecasted Low Costs is a dummy equal to one if the participant made a forecast that would imply that redistribution comes at negligible 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.11: Preferred tax rate by treatment



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

S.11 Additional Tables and Figures, Extension 2

Table S.23: Sample characteristics, Extension 2

	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.24: Summary statistics by treatment, Extension 2

	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.25: 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, participants in *RealizedNo* faced an absolute inequality of $SD = 1832$ (as in the main treatments). See Figure S.12 for an illustration of the average preferred tax rate across all levels of absolute inequality.

Table S.26: Belief Correction and Demand for Redistribution, Full Specification, Extension 2

	(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)
Low Personal Cost					29.55***
					(6.04)
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

	(1)	(2)	(3)	(4)	(5)
			(12.90)	(12.57)	(12.41)
Efficiency from MDG				-3.03***	-3.28***
				(0.85)	(0.84)
Risk_p				12.87	13.33
				(8.91)	(8.78)
Trust_p				11.74	11.41
				(9.45)	(9.30)
Political Right_p				-17.31*	-16.53*
				(9.23)	(9.09)
Meritocracy_p				-17.99**	-21.12**
				(8.52)	(8.41)
$\text{Inequality Too Large}_p$				29.21***	27.06***
				(10.26)	(10.11)
$\text{Government Responsibility}_p$				22.14**	26.00***
				(8.95)	(8.86)
MLAMS_p				-13.51	-11.54
				(14.10)	(13.90)
Observations	785	785	785	785	785

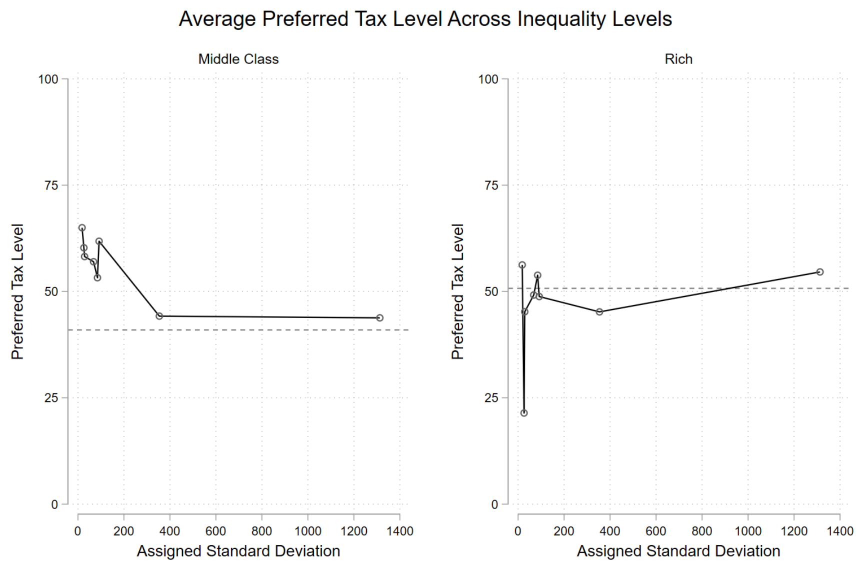
Notes: tobit regressions with preferred tax rate as dependent variable, reporting average partial effects. Low Personal Cost is a dummy equal to one if the participant is randomised into an allocation that implies that redistribution comes at negligible personal costs ($3, corresponding to a payment 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.27: Descriptive statistics for poor participants, Extension 2

	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 poor participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency).

Figure S.12: Tax across inequality levels



Notes: the figure shows average preferred tax levels in *ForecastNo* for each of the inequality information treatments in Extension 2, separated by middle-income and rich participants. The dash line is the average preferred tax rate in *RealizedNo*.

Table S.28: Descriptive statistics for middle-income and rich participants, Extension 2

	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 participants in a treatment. DG is the share that participants give as dictators in the standard dictator game. Efficiency corresponds to participants' allocations in the modified dictator game, ranging from 1 (max equity) to 7 (max efficiency).

S.12 Additional Tables and Figures, Discussion

Table S.29: 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

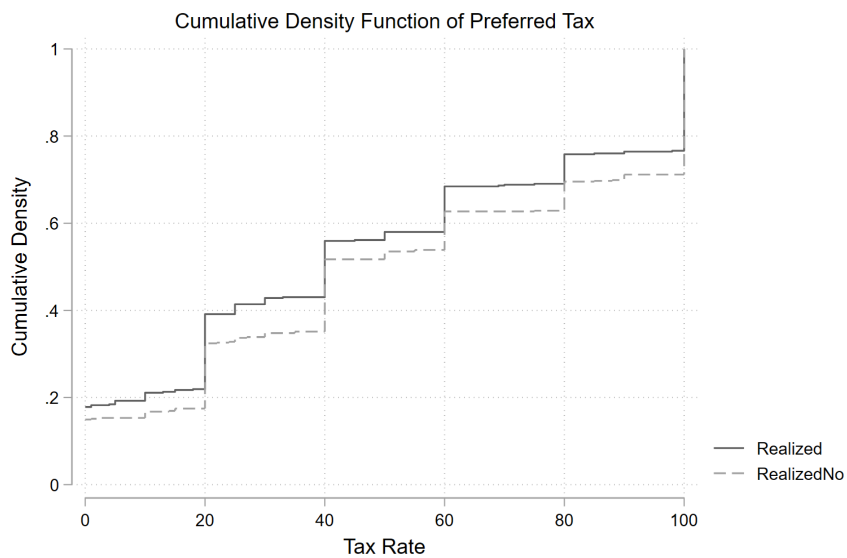
Notes: p-values from tobit regressions with preferred tax rate as the dependent variable. All regressions control for dictator givings and a perceived gains dummy equal to one if the participant mistakenly believes they will gain from taxation.

Table S.30: 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 participant mistakenly believes they will gain from taxation.

Figure S.13: Preferred tax rate by treatment, Extension 2



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

Table S.31: 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

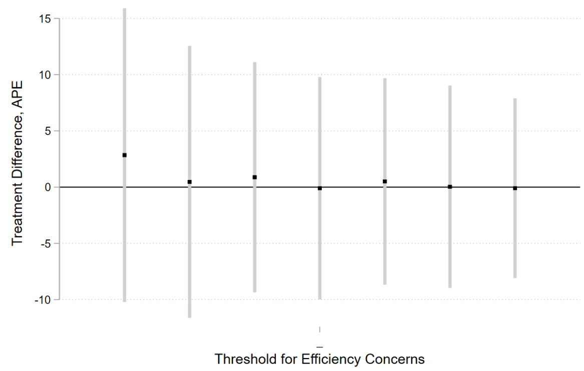
Table S.32: Tax preferences across growth paradigms

	(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.14: Comparing Realized and RealizedR for different efficiency concerns



Notes: the figure presents the average partial effect from tobit regressions with the participants' preferred tax rate as the dependent variable. Each point $i = \{1, 2, \ldots, 7\}$ on the plot represents a different sample restriction, such that only participants with efficiency concerns $e \leq i$ are included.

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