Social Justice Research (2020) 33:137–194
https://doi.org/10.1007/s11211-019-00343-7

Mind the Income Gaps? Experimental Evidence of Information's Lasting Effect on Redistributive Preferences

Bastian Becker[1]

Published online: 4 September 2019

Abstract
Individuals reject economic inequality if they believe it to result from unequal opportunities. This paper argues income gaps between groups determined at birth, based on sex, race, or family background, can serve people as an indication of unequal opportunities. Findings from a survey experiment show Americans underestimate these gaps. When confronted with accurate information, participants correct their perceptions and adjust redistributive preferences. A follow-up survey finds these effects to last for over one year. In sum, this paper contributes to political economy scholarship that links individual preferences to objective characteristics of the income distribution. Focusing on income gaps offers new ways to explore the political consequences of structural economic change.

Keywords Inequality · Redistribution · Distributive justice · Experiment

Introduction

Rising income inequality in the USA, and elsewhere, has not led to the public outcry many anticipated. A prominent explanation for this phenomenon is that most people misperceive the extent of income inequality, usually underestimating it (Evans & Kelley, 2004; Norton & Ariely, 2011; Osberg & Smeeding, 2006). However, recent experimental studies find little evidence that learning about inequality affects support for governmental redistribution (Kuziemko, Norton, Saez, & Stantcheva, 2015; Trump, 2017). I argue that this finding is unsurprising if one considers that people, especially Americans, usually take issue with unequal opportunities but not with unequal outcomes. Income differences should therefore only spur opposition if they reflect unequal opportunities.

✉ Bastian Becker
bastian.becker@uni-bremen.de

1 Research Center on Inequality and Social Policy (SOCIUM), University of Bremen, Mary-Somerville-Straße 3, 28359 Bremen, Germany

Distributive justice theories concerned with equality of opportunity emphasize the distinction between factors beyond and within individual control. Whereas the influence of factors beyond individual control on outcomes violates equality of opportunity, the influence of factors within individual control does not (Roemer, 1998). Empirical research has shown that people share this understanding, rejecting economic inequality as unfair if they believe it to result from factors beyond rather than within individual control (Alesina, Stantcheva, & Teso, 2018; Fong, 2001; Linos & West, 2003). However, studies rarely connect such beliefs to objective characteristics of the income distribution. I argue that income gaps between groups of different birth circumstances, such as sex, race, or family background, can serve people as a signal of how strongly income is affected by factors beyond individual control.[1] If people adhere to a distributive understanding of equality of opportunity, they should become more supportive of redistribution, the larger they perceive income gaps to be.

Political actors in the USA are well aware of people's desire for equality of opportunity. Appeals to it, often in terms of the "American Dream," are commonplace in political speeches and campaigns, with policies being promoted as solutions to "gaps" between groups that differ by birth circumstances. As such, they appeal to the same understanding of equality of opportunity distributive justice scholarship is commonly concerned with. The most prominent example in recent years is the White House's *equal pay* campaign under then-President Barack Obama. The campaign frequently referred to governmental statistics showing that women earn only 79% of what men receive for the same kind of work.[2] Leaving disputes over the accuracy of such numbers aside, it is not clear whether such factual information is well suited to rally voters' support. The findings of this study affirm the effectiveness of such information, in particular income gaps.

The findings presented here are based on a survey experiment, which asked Americans about their perception of income gaps corresponding to gender, race, and family background.[3] Most respondents strongly underestimate the size of the income gaps. When treated with factual information, those who underestimated the income gaps become more likely to support redistribution, and those who overestimated them become less likely. A follow-up survey after one year shows that the effects persist. In sum, this paper develops and provides evidence for a new mechanism about how objective characteristics of the income distribution, i.e., income gaps, affect redistributive preferences. The paper also shows that—contra to frequent criticism of survey experiments—informational effects can be long-lasting.

[1] It is common to focus on birth circumstances as they are clearly beyond individual control.

[2] See https://obamawhitehouse.archives.gov/issues/equal-pay, accessed on September 1, 2017.

[3] Note that surveys most commonly elicit gender, not sex, which is why I refer to the respective gap as gender income gap.

Inequality, Information, and Redistribution

This section introduces political economy scholarship on the relationship between economic inequality and redistributive preferences. The two main camps hold that opposition to inequality arises from material self-interest or fairness concerns. While earlier works have not connected fairness concerns with the actual income distributions, more recent research has done so by focusing on intergenerational mobility, which indicates how strongly children's economic attainment depends on that of their parents. I expand on this literature to argue that income gaps between groups determined at birth constitute a signal of the influence of a wider set of factors beyond individual control, including sex and race, on income.

While income gaps constitute objective characteristics of the income distribution, recent literature emphasizes that individuals often misperceive such characteristics and points at the role information plays in shaping perceptions and related preferences. In the following I introduce research on the relationship between the income distribution, perceptions thereof, and corresponding preferences, and discuss its implications for the present study.

Opposing Inequality, Demanding Redistribution

Political economy scholarship often posits that people demand more redistribution in the face of growing inequalities. One popular account holds that redistribution, due to its inequality-reducing effect, decreases work incentives. Lower incentives to work in turn reduce economic prosperity and thus the pool of income that can be taxed and redistributed. This effect is most detrimental in low-inequality environment, which makes it in people's self-interest to oppose redistribution (Meltzer & Richard, 1981; Romer, 1975). Numerous empirical studies provide evidence for such a positive relationship between inequality and demand for redistribution (Dimick, Rueda, & Stegmueller, 2017; Finseraas, 2009; Rueda, Stegmueller, & Idema, 2014; Schmidt-Catran, 2016). Rueda et al. (2014) offer an alternative account to explain the observed relationship. The authors argue it is fear of crime that leads to greater support of redistribution as inequality grows.

However, not all scholars agree that it is concerns about work incentives, or fear of crime, that drive the relationship between inequality and support for redistribution. Instead, support might depend on whether inequality is regarded to be "fair." Fong (2001) explores how beliefs about the determinants of poverty and wealth influence preferences for redistribution. She finds that those who believe effort to be most decisive oppose redistribution, while those who believe circumstances and luck to be more consequential support it. In the same vein, Linos and West (2003) show that individuals reject outcome differences due to factors beyond individual control and accept differences resulting from factors within individual control. In other words, people are seen to adhere to a distributive understanding of equality of opportunity. These findings are echoed by McCall (2013), who analyzes American public opinion over the past 30 years, a time during which inequality increased strongly. She argues that Americans growing opposition to inequality is not a concern about inequality itself but rather about narrowing opportunities (see also McCall & Kenworthy, 2009).

One limitation of this scholarship on economic fairness is its focus on subjective beliefs, detaching it from objective characteristics of the income distribution. However, two recent studies posit that the two are related (Alesina et al., 2018; Jaime-Castillo & Marqués-Perales, 2014). These studies focus on intergenerational mobility, which describes how strongly the socioeconomic standing of parents and their offspring coincides. Intergenerational mobility is most commonly quantified through transition probabilities, for example, between the lowest- and highest-income group (from parent to child). As parental standing is a factor beyond individual control, transition probabilities can be seen as an objective indicator of how strongly an outcome is affected by factors beyond individual control. In line with the earlier scholarship using subjective beliefs, the two studies show that individuals who perceive mobility to be low are more likely to support redistributive policies than those who perceive mobility to be high.

Underestimating Inequality

General patterns in misperceptions about objective characteristics of the income distribution are well established. Studies across a range of countries show that most people, including the poor and the rich, perceive themselves to be middle class and their incomes close to the national average (Cruces, Truglia, & Tetaz, 2012; Evans & Kelley, 2004; Fernández-Albertos & Kuo, 2015; Kuziemko et al., 2015). Similarly, perceptions of inequality are usually below its actual level (Norton & Ariely, 2011; Osberg & Smeeding, 2006). With regard to intergenerational mobility, Americans tend to overestimate it, whereas Europeans are prone to underestimating it (Alesina et al., 2018).

One explanation of misperceptions is limited, and biased, information (Weatherford, 1983). A number of recent experimental studies show that misperceptions, and corresponding preferences, can—though not always—be corrected through the provision of factual information (Nyhan & Reifler, 2010). Several studies explore the effect of information about individuals' position in the income distribution. They find that those who previously overestimated their position become more supportive of redistribution (Cruces et al., 2012; Karadja, Mollerstrom, & Seim, 2016), especially through progressive taxation (Fernández-Albertos & Kuo, 2015), and vice versa.

Moving beyond individuals' own position in the income distribution, Kuziemko et al. (2015) confront American respondents with an "omnibus treatment" that contains information about the extent of inequality and its recent growth. They find that the treated adjust perceptions and various beliefs about inequality but only narrowly increase their support of government redistribution. The effect of information about inequality is further put into question by Trump (2017), who finds that individuals adjust their willingness to accept inequality rather than to adjust redistributive preferences. This is in line with earlier observational studies that attest to a similar relationship (García-Sánchez et al., 2018; Schröder, 2017). Information also plays an important role when it comes to perceptions of intergenerational mobility. The study by Alesina et al. (2018) features a large-scale, comparative survey experiment. Providing factual information about intergenerational mobility leads to a correction of misperceptions and, at least among left-leaning individuals, to greater support of redistribution. In sum, these studies underline that information conditions perceptions of the income distribution but only provide support for some perceptions to affect redistributive preferences.

Contribution

As discussed above, it is well established in the literature that individuals reject outcome differences if they believe them to be the result of factors beyond individual control. However, few political economists have explored how such beliefs relate to the actual income distribution. One exception is research on intergenerational mobility. This research has shown that objective information about chances of upward mobility is an important determinant of mobility perceptions and related beliefs (Alesina et al., 2018). Of course, parents' economic standing is of central importance for children's economic opportunities, but there are many other consequential factors beyond individual control, in particular those determined at birth, like sex or race. Since birth circumstances are invariably unaffected by individual choices, they constitute factors beyond individual control. As for intergenerational mobility, income differences corresponding to birth characteristics, or income gaps for short, indicate how strongly income is affected by factors beyond individual control.

What do people know about income gaps and how does it influence their redistributive preferences? Prior research shows that people tend to underestimate income differences, i.e., the distance of their income from the national average and the extent of inequality in general, and overestimate the extent of intergenerational mobility (at least Americans). I expect the same to hold for income gaps, *people tend to underestimate income gaps* (Hypothesis 1). Furthermore, as income gaps indicate differences corresponding to factors beyond individual control, I expect that *people who perceive income gaps to be larger are more likely to support redistribution* (Hypothesis 2). Finally, if perceptions of income gaps are constrained by available information, new information should lead to a correction of misperceptions and an adjustment of redistributive preferences. Concerning income gap perceptions, I propose that *information about income gaps leads people who underestimated them to correct their perceptions upwardly and those who overestimated to correct them downwardly* (Hypothesis 3). In particular, I contend that people incorporate the new information through Bayesian updating, which implies that posterior perceptions constitute a weighted average of prior perceptions and the new information (Gerber & Green, 1999; Griffiths, Kemp, & Tenenbaum, 2008).[4] With regard to redistributive preferences, I propose that *information about income gaps makes people who underestimated them more likely to support redistribution and those who*

[4] For critical views of Bayesian updating, see Bartels (2002) and Taber and Lodge (2006).

overestimated them less likely (Hypothesis 4). The contribution is strengthened by exploring both the immediate and lasting effect of information.

Beyond the literature introduced above, the hypotheses relate to other important scholarly accounts of the link between inequality and preference formation. Political economy research commonly identifies self-interest as the most prominent explanatory factor of redistributive preferences. This not only applies to research on inequality between individuals but also between groups. Individuals who see their own interest aligned with the standing of their group might therefore seek to promote their own group at the expense of others (Alesina & La Ferrara, 2005; Alt & Iversen, 2016; Finseraas, 2012).With regard to income gaps, self-interest can lead those advantaged by gaps not to be concerned with—or even to promote—income gaps, whereas those who are disadvantaged should demand their undoing.

Social psychology is another strand in the literature that offers important insights on inequality and redistribution (see Hegtvedt & Isom, 2015, for an overview). One prominent account holds that preferences for redistributive policies, and social policies more generally, are driven by stereotypes about the policy's direct beneficiaries. What people attribute poverty to and who they regard as deserving are key questions in this line of work. Although negative stereotypes mainly affect persons of color and women, the role of stereotypes in preference formation is not fixed. They can be triggered and changed through deservingness cues and frames (Gilens, 2000; Katz, 2013; Likki & Staerklé, 2015). When it comes to the relationship between income gaps and redistributive preferences, it should therefore be less decisive what people think about their size but what stereotypes they hold about the groups disadvantaged by the gap, i.e., the potential beneficiaries of redistributive policies.

The next section lays out how the hypotheses developed above are to be tested using a survey experiment. In the experiment respondents are treated with information about income gaps that reflect how labor market returns differ by gender, race, and parental education.

The Income Gaps Experiment

Survey experiments that explore the causal effects of factual information have grown increasing popular in the social sciences. For example, Kuklinski et al. (2000) investigate people's perceptions of the amount of welfare expenditure and Hopkins et al. (2018) perceptions of the size of the immigrant population. However, most such survey experiments do focus on people's perception of the income distribution (Cruces et al., 2012; Fernández-Albertos & Kuo, 2015; Kuziemko et al., 2015; Trump, 2017).

While such experiments can be incorporated into face-to-face or telephone surveys, they are increasingly conducted over the internet, in particular the online time sharing platform Amazon Mechanical Turk (MTurk) (e.g., Kugler, Cooper, & Nosek, 2010; Kuziemko et al., 2015; Trump, 2017). Through this platform "requesters" can offer tasks for pay to a pool of registered "workers." Academics use this platform to recruit participants for their online studies. The advantages of such online studies are not only speed and affordability, but online platforms often provide a broader pool of respondents than the more commonly used student samples. Furthermore, numerous evaluation studies of MTurk show that established findings of experimental studies and economic games can be reliably replicated (Berinsky, Huber, & Lenz, 2012; Clifford, Jewell, & Waggoner, 2015; Mullinix, Leeper, Druckman, & Freese, 2015).

An important challenge for survey experiments that use information treatment is the interpretation of revealed effects. While a properly implemented experiment provides evidence for the absence or presence of a treatment effect, it is not necessarily the factual information itself that underlies the effect. One concern is social desirability, which describes how participants adjust their behavior and responses to what they think is expected and appropriate (McDermott, 2002). This risk is high for research about contentious topics, such as inequality. Another concern is priming. Rather than considering its factual content, an informational treatment can lead people to think about subsequent choices and answer in a particular way. For example, information about income differences primes economic concerns rather than ideological ones and this affects people's subsequently stated preferences for redistribution (Kuklinski et al., 2000). As such, priming and social desirability potentially confound any revealed treatment effects and thus threaten the internal validity of survey experiments interested in effects of factual information.

These threats to internal validity can be overcome by adjusting the research design and analysis. As factual information about income differences cannot avoid priming economic considerations, it is important to equally prime those in the control group who receive no factual information. Most survey experiments on inequality do this by asking all participants about their perception of the economic fact under study (i.e., their own standing or inequality in general). Correct information is then only provided to those in the treatment group. Since asking all participants about their prior perception gives all of them an idea of what the survey is about, doing so has the additional advantage of minimizing social desirability biases. Further precautions can be taken during the analysis. To understand how this is done, it is important to consider that treatment effects should depend on participants' prior perceptions. Those in the treatment group who learn most from the factual information should adjust their perceptions and preferences most strongly. Hence, the presence of such an interaction effect in the analysis is a strong indication that it is the factual content of the treatment that explains its effect (Kuklinski et al., 2000; Lenz, 2009). As most above-mentioned experimental studies of economic inequality, the present paper follows these best practices to avoid confounding through priming or social desirability biases.

Another point of contention for survey experiments is the duration of treatment effects. As follow-up surveys are rare, scholars are skeptical that effects last (Gaines, Kuklinski, & Quirk, 2007). Kuziemko et al. (2015) constitute one such exception. One month after their experiment, which included an "omnibus treatment" with numerous facts about income and wealth disparities, they conduct a follow-up survey. Encouragingly, they still discover statistically significant differences between both groups for most variables of interest, including support of governmental redistribution. However, one concern about their findings is the low response rate of the second survey; only 14% of the original respondents participated. If response patterns differ between experimental conditions, so-called attrition bias, comparisons of control and treatment group cannot be interpreted causally anymore. Kuziemko et al. (2015) identify such attrition bias in their sample and are careful in drawing strong conclusions.

Treatment

In specifying the income gap treatment, this study focuses on three social divides that are frequently subject to academic and public debates: gender, race, and family background. The latter divide is akin to intergenerational mobility, which has been the subject of earlier survey experiments, and I thus refer to it as intergenerational income gap. Here, the intergenerational income gap distinguishes the incomes of those who have at least one university-educated parent and those who do not. The race income gap indicates income differences between whites and non-whites.

Income gaps constitute an imprecise signal for the importance of factors beyond individual control, and thus violations of distributive equality of opportunity.[5] This is because the groups to which the income gap refers might not only differ in factors beyond individual control but also in factors which are believed to be within individual control. In order to best test whether people oppose income gaps corresponding to factors beyond individual control, it is important to reduce the potentially confounding effect of factors within individual control. In this paper, I increase the precision of the signal by focusing on income gaps only among individuals that are currently employed. These income gaps give an indication of the influence of factors beyond individual control in the labor market and thus cover the larger part of the adult population. At the same time, excluding incomes of those who are currently unemployed reduces the impact of factors within individual control, such as lack of effort, skills, or choices to abstain from the labor market.[6]

As discussed above, perceptions of income gaps in the labor market are elicited for respondents in treatment and control group. Figure 1 shows the interface which is used for this purpose; respondents can drag the slider to any multiple of 250 between US$0 and 37,500. Once respondents indicate their perception, those in the control group immediately proceed to the post-treatment questions, whereas those in the treatment group are presented with the factual information before proceeding. They are presented with the information in the same interface by additional dots on the sliders; the gender income gap amounts to US$27,300, the race income gap to US$17,800, and the intergenerational income gap to US$18,700.[7] These dots are

[5] This imprecision equally applies to earlier studies on intergenerational mobility (e.g., Alesina et al. 2018; Jaime-Castillo and Marqués-Perales 2014).

[6] Future studies might want to use more precise signals by accounting for factors such as education, occupation, or working hours. However, a less precise signal was chosen here in order to keep the presentation of the informational treatment as simple as possible.

[7] The size of the income gaps has been calculated for the year 2010, which constitutes the reference year of the project this study was part of *(1 citation removed for masked review)*, based on data from the Panel Study of Income Dynamics. In order to reflect labor market differences and not the redistributive effects of taxation, before-tax income data were used. These data include income from both employment and self-employment, but not income from property or other investments. All incomes were adjusted for life-

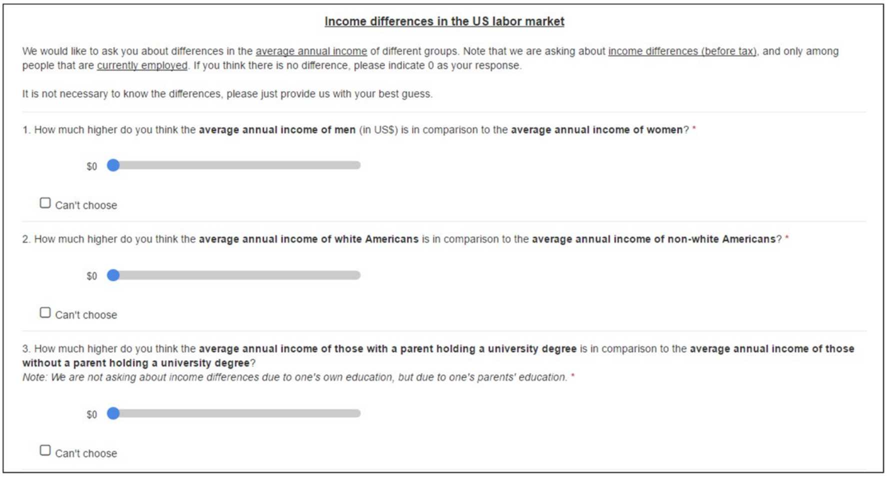

Fig. 1 Interface to elicit perceptions of income gaps. *Note*: Respondents can indicate any multiple of 250 between US$0 and 37,500

red if the respondent underestimated the income gap and green if she overestimated the respective gap. This is complemented by a short text above each slider stating whether the respondent's indicated perception was below or above the actual value.

Follow-Up Survey

To explore whether information about income gaps has a lasting effect on redistributive preferences, I conducted a follow-up survey with respondents after one year. In order to increase the response rate, respondents who volunteered their e-mail address in the initial survey were invited to a paid follow-up survey. This strategy proved successful, leading to a high response rate and no detectable attrition bias (details below). Following questions about redistributive preferences, the second survey also asks respondent to again indicate their perception of the size of income gaps. The results section shows that the treatment does indeed have a lasting effect on income gap perceptions as well as redistributive preferences.

Re-surveying respondents after one year has further advantages. Most workers on MTurk complete academic surveys on a frequent basis (Stewart et al. 2015). In contacting those who provided their contact information, any explicit reference to the initial survey was avoided. Recipients are only informed that they are being contacted because they "previously participat[ed] in one of our surveys." The only information recipients could use to connect the message to the earlier survey is the e-mail

Footnote 7 (continued)
cycle variations by correcting for systematic differences based on a cubic regression of income on age to account for potential compositional differences across groups. As it is common in the USA to indicate income in annual values, the same time reference is used here.

Table 1 Descriptive statistics of respondent sample (initial survey)

	Mean	SD	Min.	Max.
Treatment	0.52	0.50	0.00	1.00
Age	36.38	11.67	19.00	71.00
Male	0.54	0.50	0.00	1.00
Household size	2.67	1.49	0.00	10.00
Children	0.80	1.27	0.00	10.00
Income	3.71	3.58	0.05	19.50
University-educated parent	0.59	0.49	0.00	1.00
Race				
White	0.80	0.40	0.00	1.00
Black	0.06	0.23	0.00	1.00
Other	0.14	0.35	0.00	1.00
Employment status				
Unemployed	0.08	0.27	0.00	1.00
Full-time	0.64	0.48	0.00	1.00
Part-time	0.13	0.34	0.00	1.00
Keeping house	0.04	0.21	0.00	1.00
Retired	0.04	0.19	0.00	1.00
Student	0.05	0.21	0.00	1.00
Other	0.02	0.15	0.00	1.00
Education				
Less than high school	0.01	0.07	0.00	1.00
High school	0.35	0.48	0.00	1.00
University	0.64	0.48	0.00	1.00

Income in US$10,000. University-educated parent is a dummy variable indicating whether respondent has at least one parent with a university degree. Education refers to respondent level of education

address through which they are contacted. This seems very unlikely. As a result, it is equally unlikely that any priming effects or social desirability biases induced by the initial treatment are still at work during the follow-up.

Respondent Pool

The initial survey was conducted in two rounds, May and June 2016, and received a total of 441 responses. Due to duplicate IP addresses, failed attention checks, lack of permanent residence in the USA, and missing data, the analysis is restricted to a sample of 364 of them. Randomization led to 189 of these respondents being in the treatment group and 175 in control. While the pool of MTurk workers cover a wide range of socio-demographics, samples drawn from it are not representative of the US population. Table 1 shows the composition of the present sample. Similar to related studies, participants are disproportionately white, young, university-educated, non-religious, and have fewer children than the average American. More importantly though, these covariates are well balanced across both conditions (not shown here).

Respondents who provided their e-mail address during the initial survey were contacted in August 2017. They were offered to participate in the follow-up survey for pay. In case they had quit the MTurk platform, the e-mail invited them to complete the follow-up without pay. Of the 312 respondents who earlier provided their e-mail address, 114 filled in the paid survey and 29 participated in the unpaid one. This equals a response rate of 45.8%.[8] After removing respondents with missing data in the follow-up survey, a total of 136, 69 of which received the treatment in the initial survey, could be retained for the analysis (37.4% of the initial sample). Descriptive statistics of the follow-up sample are presented in Appendix 1, Table 3.

Methods

In the following analysis the central variables are redistributive preferences, perceptions of income gaps, and treatment status. Treatment status is a simple binary variable, indicating whether a respondent belonged to the treatment group or not. The other two variables need to be explained in more detail.

Redistributive Preferences

Preferences for redistribution indicate how much a person wants the government to reduce income inequality. Similar to the phrasing common in social surveys, respondents are asked about their agreement with the following statement, "The government should redistribute more from the rich to the poor, even if it means increasing taxes." Answers can be indicated on a seven-point scale, ranging from "Strongly disagree" over "Neither agree nor disagree" to "Strongly agree." For the main analysis, responses are dichotomized to distinguish those who support increased governmental redistribution from those who do not.[9] "Neither agree nor disagree" responses are categorized as not supporting redistribution. In the initial survey, 58.2% of respondents agree with redistribution, and 59% agree with it in the follow-up. The distribution of the raw values is shown in Appendix 1, Table 4. From a statistical point of view, the main advantage of dichotomizing redistributive preferences is that it avoids having to make the assumption that respondents interpret all seven answer categories of the survey question in the same way (Gerber & Green, 2012).[10]

[8] This is the percentage of those who provided their e-mail in the first survey. A small amount of messages was returned due to incorrect or expired addresses.

[9] Different from its use here, some social justice research uses this item to measure the latent concept of "egalitarian ideology".

[10] Results regarding the proposed hypotheses are not driven by this specification. Table 14 in Appendix 1 shows that they hold if redistribution preferences are treated as a continuous variables (although in one specification only for a low level of statistical significance).

Income Gap Perception

I have hypothesized above that effects of information about income gaps should depend on prior perceptions of these gaps. Therefore, participants are asked before the treatment about their perception of each income gap. Their responses are coded as *Prior(Gender gap)*, *Prior(Race Gap)*, and *Prior(Intergenerational gap)*. In analyzing the causal effects of the treatment, it is important to consider that the treatment contains information on all three income gaps. As such, causal effects are only identified for an appropriate aggregate measure of all three prior income gap perceptions. I use the average estimate, *Prior(Gaps)*, as such a measure, as it gives equal weight to each perception.

Statistical Estimation

Different statistical models and robustness checks are used to estimate and ascertain the effect of the treatment. All results in the main text that concern the effect on redistributive preferences are based on linear probability models (LPM). Estimated model coefficients of LPMs in combination with dichotomous dependent variables can be easily interpreted, here as percentage point changes in the probability of agreeing with redistribution. The functional form of LPMs also fits the theoretical conjecture that individuals are Bayesian updaters.[11] All models control for a *round dummy* which indicates whether the respondent was recruited in May or June 2016.

In each model, I condition the treatment estimate on prior income gap perceptions. As discussed above, causal interpretation requires the conditioning on an aggregate measure, here *Prior(Gaps)*. However, I also estimate additional models conditioning the treatment estimate on each of the prior income gap perceptions separately. Doing so allows me to verify that the treatment effect is not driven by any one of the perceptions alone. Furthermore, I estimate each model on different subsets defined by respondents' gender, race, and their parents' education. This allows me to consider a number of alternative explanations.

As a robustness check, all models are estimated with and without controls to account for small imbalances in the sample. The following variables are included as controls; one dummy each to indicate whether the respondent is male or not (gender), white or not (race), fully employed or not (employment), and whether they hold a university degree or not (education). Further I account for age, number of children, and personal income. Descriptive statistics of these variables are included in Table 1. However, it should be noted that the inclusion of control variables in analyses of experimental data can undermine the benefits of randomization and invalidate the causal identification. Furthermore, the models presented in the main text are re-estimated without dichotomization of the redistribution variable, using

[11] Alternative link function which is often used for dichotomous dependent variables, such as logit or probit, would due to their nonlinearity necessitate different assumptions about how individuals incorporate new information, i.e., not Bayesian updating. That being said, the results are robust to using these alternative link functions.

ordinary least squares (OLS) estimation (see Appendix 1; Tables 14, 15). To assess the durability of the effects, I also estimate the just-described models for the data elicited in the follow-up survey.

Repeated surveys risk inducing attrition bias. Such bias can result from differential drop-out patterns in treatment and control groups. However, as I demonstrate in Appendix 4, there is no evidence of such a bias present here. Nevertheless, I implement an estimation strategy using inverse probability weights to account for potential covariate imbalances from attrition. To do so, I first estimate a model predicting the probability of each respondent to participate in the follow-up survey. Second, the inverse of the predicted probabilities is applied as weights in the estimation of treatment effects. This process gives more weight to those respondents in the follow-up survey that have similarities with those who dropped out before. Hence, this approach accounts for covariate imbalances, even if they are only minor (Gerber & Green, 2012). Both types of models, with and without inverse probability weighting, lead to the same substantive results regarding the treatment effects. However, including the weights leads to marginally larger effect estimates.

Two further robustness checks are implemented. First, underlying LPMs is the assumption of a linear relationship between income gap perceptions and the strength of the treatment effect. To determine whether this assumption is appropriate, I split the sample based on deciles of respondents' average income gap perception, *Prior(Gaps)*, in the initial survey. For each decile I separately estimate the effect of the treatment on redistributive preferences. The results presented in Appendix 3 largely corroborate the linearity assumption. Second, I estimate a set of panel models to further ascertain the duration of the treatment effect on redistributive preferences. For these models responses from the initial and follow-up survey are pooled. The results show that interactions between the survey round and coefficients relating to treatment effects are statistically insignificant (see Appendix 1, Table 16).

Finally, I estimate a set of models to determine whether the treatment had a lasting effect on respondents' income gap perceptions. In those models, the dependent variable is the average income gap perception in the follow-up survey, *Posterior(Gaps)*, respectively each individual income gap perception, *Posterior(Gender gap)*, *Posterior(Race gap)*, and *Posterior(Intergenerational gap)*. As income gap perceptions are continuous variables, OLS estimation is suited best.

Results

Initial Survey

Income Gap Perceptions

Earlier studies have found that Americans underestimate the extent of economic disparities with regard to their own relative position in the income distribution and inequality in general. Confirming Hypothesis 1, perceptions of income gaps turn out to be no different. As Fig. 2 shows, most participants recruited for this study vastly underestimate income gaps in the labor market. A total of 98.4% of

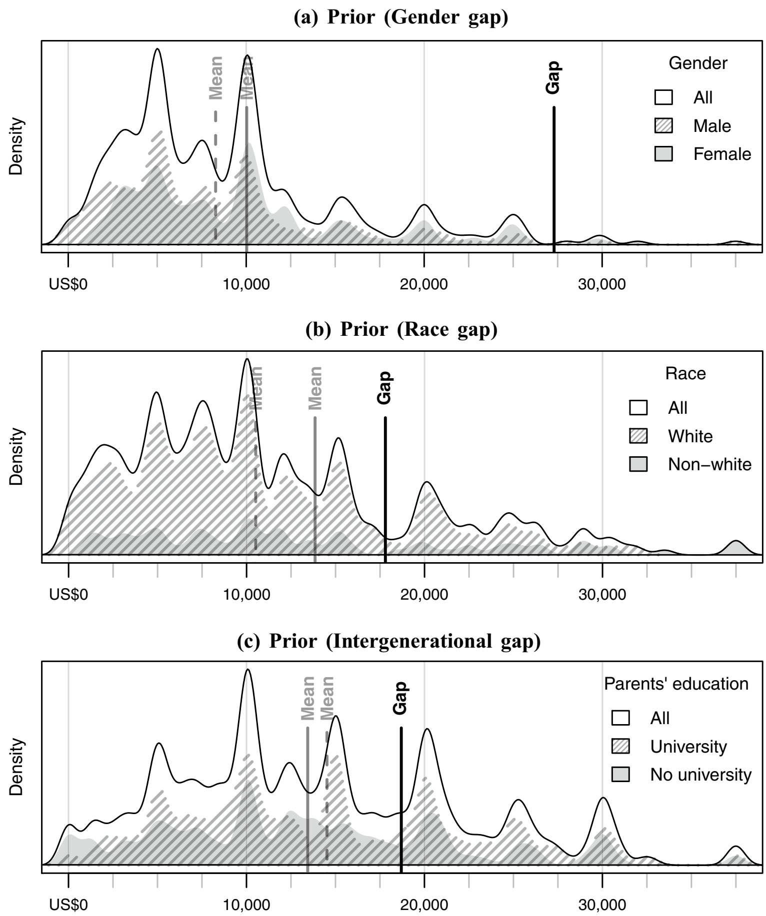

Fig. 2 Perceptions of income gaps before treatment (initial survey). *Note* Density plots for total sample and subsamples as indicated by shading (Gaussian kernel, bandwidth = 500). Gray "Mean" lines indicate mean income gap perception of subsamples (dashed lines correspond to subsample with hatched shading). Black "Gap" lines indicate actual income gaps; gender income gap, US$27,300; race gap, US$17,800; intergenerational income gap, US$18,700 (own computations based on data from the Panel Study of Income Dynamics, see footnote 7 for details)

respondents underestimate the gender income gap, 80.8% the race income gap, and 70.1% the intergenerational income gap. Perceptions are least inaccurate for the intergenerational income gap, that is income differences corresponding to parents' education, where the mean income gap perception amounts to US$14,074 (SD = 6470). The mean perception of the race income gap is US$11,183

(SD = 7953) and US$9084 (SD = 8226) for the gender income gap. One-sided t-tests confirm that the underestimation of all gaps is statistically significant.[12]

Are those advantaged by income gaps more likely to underestimate them? The top panel shows that this holds true for the gender income gap. Men more strongly underestimate the gap than women; the mean income gap perceptions are US$8,282 (SD = 6345), respectively, US$10,030 (SD = 6507). Similarly, whites' mean perception of the race income gap is US$10,515 (SD = 7266) as opposed to a mean perception of US$13,849 (SD = 9860) among non-whites. The pattern for the intergenerational income gap is reversed; the mean perception of those with university-educated parents, US$14,523 (SD = 8156), exceeds that of those who do not, US$13,442 (SD = 8309). One-sided Welch t-tests reveal that the differences are statistically significant for the gender income gap ($t = -2.584$, $p = 0.005$) and race income gap ($t = -2.711$, $p = 0.004$), but not the intergenerational income gap ($t = 1.233$, $p = 0.8907$). As laid out in the methods section, in the following the average income gap perception of all three gaps, *Prior(Gaps)*, serves as indicator of each respondent's prior perception of the income gaps. The distribution of the average income gap perception in the initial survey has a mean of US$11,447 (SD = 6114).

Effects on Redistributive Preferences

In this section I present the main results from the initial survey regarding the relationship between income gap perceptions and redistributive preferences. The results are summarized in Table 2 which includes the output from regression models with redistribution as the dependent variable and the treatment status and its interaction with the income gap perceptions as independent variables. Each model is presented with and without the inclusion of control variables.[13]

Above I formulated the expectation that perceptions of income gaps and support for redistribution should be positively related (Hypothesis 2). Models 1 and 2, whereby the second includes socio-demographic controls, estimate how the probability to agree with redistribution depends on respondents' average prior income gap perception, *Prior(Gaps)*, and whether they received new information (treatment). The empirical support for Hypothesis 2 can be assessed by turning to the *Prior(Gaps)* estimate, which indicates how income gap perceptions and support for redistribution are related in the control group, and thus without the influence of new information. In line with the hypothesis, perceived larger income gaps are associated with greater support for redistribution. Model 1 indicates that respondents who on average perceive income gaps to be US$10,000 larger are 17 percentage points more likely to support redistribution. When controls are included (model 2), the same estimate amounts to 14.4 percentage points. This results holds if the model is estimated with *Prior(Gender gap)* (models 3–4) and *Prior(Race gap)* (models 5–6) instead of

[12] Gender income gap, t = 26.779, $p = 1.000$; race income gap, $t = 26.826$, $p = 1.000$; intergenerational income gap, $t = 32.641$, $p = 1.000$).

[13] All models control for a *round dummy* indicating whether the respondent was recruited in May or July round of 2016.

Table 2 LPM results, effects on agreement with redistribution (initial survey)

	Dependent variable:							
	Redistribution (agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.349**	0.308**	0.292**	0.250**	0.225**	0.208**	0.303**	0.265**
	(0.109)	(0.108)	(0.088)	(0.088)	(0.088)	(0.087)	(0.102)	(0.101)
Prior(Gaps)	0.170**	0.144*						
	(0.058)	(0.058)						
Treatment × Prior(Gaps)	− 0.217*	− 0.192*						
	(0.084)	(0.083)						
Prior(Gender gap)			0.165**	0.128*				
			(0.055)	(0.055)				
Treatment × Prior(Gender gap)			− 0.209**	− 0.176*				
			(0.079)	(0.078)				
Prior(Race gap)					0.148**	0.138**		
					(0.047)	(0.047)		
Treatment × Prior(Race gap)					− 0.114+	− 0.110+		
					(0.064)	(0.063)		
Prior(Intergenerational gap)							0.060	0.047
							(0.043)	(0.043)
Treatment × Prior(Intergen. gap)							− 0.146*	−0.128*
							(0.063)	(0.062)
Constant	0.329***	0.577***	0.369***	0.631***	0.363***	0.606***	0.440***	0.670***
	(0.080)	(0.134)	(0.069)	(0.126)	(0.068)	(0.124)	(0.076)	(0.134)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
Observations	364	364	364	364	364	364	364	364

Table 2 (continued)

	Dependent variable:							
	Redistribution (agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R^2	0.035	0.092	0.036	0.090	0.038	0.097	0.026	0.086
Adjusted R^2	0.025	0.064	0.025	0.062	0.028	0.069	0.015	0.057

Note: LPM = Linear probability models. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

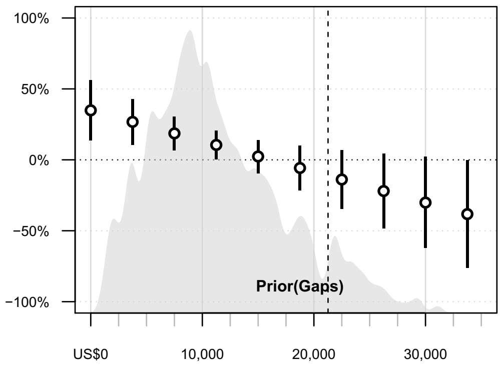

Fig. 3 Treatment effect on agreement with redistribution conditional on average prior perception of income gaps (initial survey). *Note*: Conditional average treatment effects (vertical axis) as change in predicted probability (percentage points) of agreeing with redistribution, based on model 1 (Table 2). *Prior(Gaps)* refers to the mean prior income gap perception. Confidence intervals (95%) based on bootstrapped model parameters (N = 100,000). Gray shading indicates distribution of prior perceptions (kernel density estimation, bandwidth = 500). The average of the actual income gaps is indicated by the dashed vertical line

the average income gap perception. The intergenerational income gap constitutes an exception (models 7–8). While the coefficient points in the expected direction, it is not statistically significant.

To determine whether there is a causal relationship between perceptions and preferences (Hypothesis 4), it is necessary to determine how individuals adjust preferences in response to new information about income gaps. These conditional average treatment effects (CATE) can be determined based on the *Treatment* coefficient and its interaction with *Prior(Gaps)* (see Table 2). Put differently, CATE indicates the difference in the probability to agree with redistribution between those who received the treatment and those who did not, given a certain income gap perception. As such, the treatment coefficient alone corresponds to the treatment effect on respondents with prior perceptions equal to zero. Model 1 indicates that respondents with *Prior(Gaps)* equal to zero become 34.9 percentage points more likely to agree with redistribution when treated with information. The interaction coefficient indicates that the treatment effect is smaller for respondent with higher *Prior(Gaps)*, 21.7 percentage points smaller for every US$10,000 to be precise.[14]

Again, these results are supported by models estimated with each perception separately (Table 2, models 3-8). *Treatment* is statistically significant in all models; the interaction with the prior perceptions is too, but only at a 10%-level for *Prior(Race gap)* (Table 2, models 5-6). While it is important to keep in mind that coefficients in these models are not causally identified, their results strongly suggest that the causal

[14] Table 2 shows that the design-only model (1) explains about 3.5% of the variation in the dependent variable (see R^2). Removing the treatment indicator and its interaction with *Prior(Gaps)* reduces the explained variance to 0.7% (see Table 5, Model 1). This implies that new information is not only statistically significant but also substantively important. For further comparisons between the results presented in the main text and baseline models the reader can refer to Table 5.

effects estimated in models 1 and 2 (Table 2) are not driven by a single gap but rather by how much the respondents' perception is off on average.

To better illustrate the size of the treatment effects, Fig. 3 presents them visually. Respondents who underestimated the income gaps most are on the very left. As predicted by Hypothesis 4, they respond most strongly to the treatment, becoming over 30 percentage points more likely to agree with redistribution when confronted with accurate information. Also as expected, this effect diminishes among those who were closer to the accurate size of the income gaps (indicated by the dashed line). Respondents with an average income gap perception of US$10,000 still become about 13.2 percentage points more likely to agree with redistribution when confronted with accurate information, but the effect vanishes among those with an average perception around US$15,000. Interestingly, there is even some indication that respondents who overestimated the income gaps become less likely to agree with redistribution when learning about their accurate size. As such, learning about the actual size of income gaps can both increase and decrease support for redistribution depending on what a person's initial perceptions are.[15]

Competing Accounts

As discussed above, two prominent accounts suggest alternative hypotheses of how individuals might respond to new information about income gaps. The first refers to individual's self-interest. Self-interest implies that those who are disadvantaged by a specific income gap should increase their support for redistribution if they learn that it is larger than they thought. Those who are advantaged by a specific income gap should show no, or even the inverse, response. The second account proposes the deservingness hypotheses. If deservingness considerations are at work here, information about income gaps should trigger stereotypes about the disadvantaged group. This triggering effect should be most pronounced among members of the advantaged group who underestimated the income gaps. As such, those who are advantaged by a specific income gap should decrease their support for redistribution if they learn that it is larger than they thought. No response is expected by disadvantaged individuals. Most importantly, both self-interest and deservingness hypothesis suggest that responses to information about income gaps should depend on how one is positioned with regard to a specific gap. Instead, the explanation offered in this paper, opposition to unequal opportunities, implies no such interaction.

To test whether these competing accounts hold explanatory power here, I separately estimate regression models for each combination of income gap and social group. Figure 4 summarizes the main model results by showing the CATEs for each income gap perception, with samples split based on the respective covariate, e.g.,

[15] One might be concerned that this reversal in the effect might is driven by extrapolation. I address this issue in Appendix 3 by separately estimating treatment effect for each *Prior(Gaps)* decile. It turns out that the decile with the highest perceptions indeed responds negatively to treatment (although not at statistically significant levels); thus, the reversal is not driven by extrapolation.

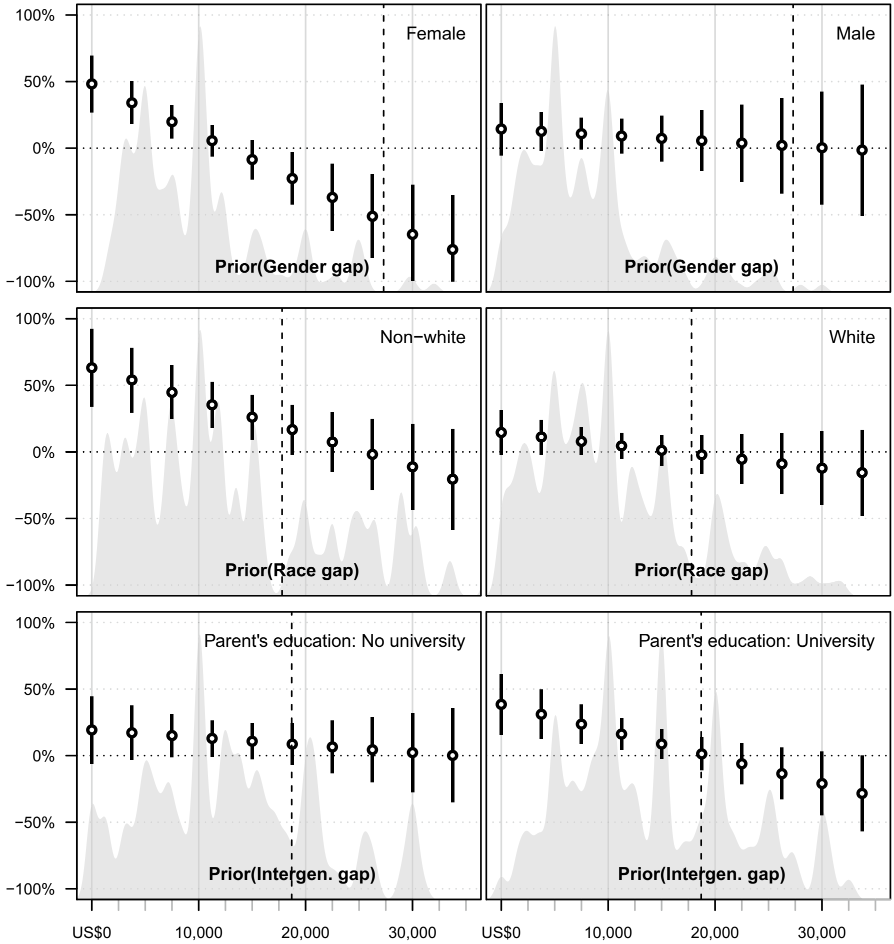

Fig. 4 Treatment effect on agreement with redistribution conditional on prior perceptions of income gaps, by group (initial survey). *Note*: Conditional average treatment effects (vertical axis) as change in predicted probability (percentage points) of agreeing with redistribution. Top panels based on Table 6 (models 3–4), middle panels on Table 8 (models 5–6), and bottom panels on Table 10 (models 7–8) (see Appendix 1). Confidence intervals (90%) based on bootstrapped model parameters (N = 100,000). Gray shading indicates distribution of prior perceptions of the different income gaps (kernel density estimation, bandwidth = 500). The actual income gaps are indicated by the dashed vertical lines

female and male for the gender income gap. The full set of regression tables can be found in Appendix 1.

The top panels of Fig. 4 show the treatment effects, separately estimated for female and male respondents, conditional on prior perceptions of the gender income gap (see Appendix 1; Table 6, models 3–4, for details). As suggested by the self-interest hypothesis, women are much more responsive to information about the gender income gap and demand more redistribution the smaller they initially perceived the gap to be. Men show no statistically significant response to information about the gender income gap, a potential indication of their self-interest. However, men do also not reduce their support for redistribution when they learn the gap is larger than they initially perceived it, thus providing no support for the deservingness hypothesis.

The middle panels compare the treatment effect on non-whites and whites, given their prior perception of the race income gap (see Appendix 1; Table 8, models 5–6, for details). The pattern is very similar to that of the gender income gap. It is largely in line with the self-interest hypothesis but reveals no support for the deservingness hypothesis. The bottom panels display the treatment effect on respondents without respectively with a university-educated parent, conditional on their prior perception of the intergenerational income gap (see Appendix 1; Table 10, models 7–8, for details). The pattern here deviates from the other two gaps. Whereas no treatment effect can be discerned for those without a university-educated parent (the advantaged side), those who have a university-educated parent display a strong response (the disadvantaged side). In particular, those who perceived the intergenerational income gap to be smaller than it actually is become more supportive of redistribution after learning about the actual size of the gap. Neither the self-interest nor the deservingness hypothesis can account for this result.

I have argued above that the self-interest and deservingness hypotheses imply differential responses for those on either side of the gap. At the same time, they imply no differential responsiveness of those advantaged or disadvantaged by one income gap to information on another gap, e.g., men and women should respond equally to information about the race income gap. The results presented in the appendix show that this is not the case. Women not only respond more strongly to the gender income gap (shown above), they also respond more strongly to the race income gap (see Appendix 1; Table 6, models 5–6). At the same time, men are more responsive to the intergenerational income gap (see Appendix 1; Table 6, models 7–8). While whites respond to information on all gaps, the response of non-whites is more pronounced for all of them (see Appendix 1; Table 8), not only the race income gap (also shown above). Similarly, when it comes to parental education, those with a parent who graduated from university are not only more responsive to the intergenerational income gap, but in fact all gaps (see Appendix 1; Table 10). This additional evidence sheds doubt on both the self-interest and deservingness hypothesis, as it appears that each social group is responsive to certain income gaps, independent of whether they find themselves on the advantaged or disadvantaged side of that gap.[16]

The estimation of treatment effects by gaps and groups shows that there is substantial variation in how strongly participants respond to new information. While some of the heterogeneity might by accounted for by deservingness cues, and especially self-interest, the explanatory power of these alternative accounts is far from encompassing. Similarly, if adherence to distributive equality of opportunity was the sole determinant of the responses, one would expect no such heterogeneity across groups. However, one would expect new information to be most effective among those who

[16] The results discussed here are based on models without controls. Models with controls are presented in Tables 7, 9, and 11. There are few substantive differences between these model specifications.

largely underestimate income gaps and less effective among those with more accurate perceptions. Although statistical significance is not always attained, the basic pattern is present for all combinations of gaps and groups.

While decomposing treatment effects, as done in this section, can provide important insights, it is important to note that causal interpretations are not warranted. On the one hand, splitting the sample into different subgroups undermines the randomization of the treatment as it potentially introduces bias in other observable as well as unobservable covariates. On the other, information about the income gaps is provided jointly, and as perceptions are highly correlated, results of separately estimated models are necessarily confounded. As such, many findings discussed in this section have to be regarded as tentative and can only be addressed conclusively by adequate future experimental studies.

Follow-up Survey

Learning About Gaps

An advantage of re-surveying respondents is the possibility to check whether the treatment effectively and lastingly manipulated income gap perceptions. Therefore, the follow-up survey again asked respondents about their perception of the three income gaps, here referred to as posterior perceptions. If respondents are Bayesian updaters, their perception in the follow-up survey should be closer to the actual income gap than it was in the initial survey. This effect should be most pronounced among those whose initial perception was most different from the actual income gaps.

Treatment effects on posterior perceptions are estimated with OLS models, both with and without socio-demographic controls, and additionally, accounting for attrition probabilities (see Appendix 1; Table 12, models 1–3), even if there was no evidence of any attrition bias (see Appendix 4). All three models reveal the same pattern, and Fig. 5a displays the CATEs of the design-only model. Those who initially perceived all gaps to be zero, indicate perceptions in the follow-up survey, *Posterior(Gaps)*, that are about US$5000 higher on average. This correction diminishes among respondents with more accurate prior perceptions, and those who overestimated them correct their perceptions downward. It should be noted that most treatment-relevant model coefficients are only statistically significant at a low level of confidence (10%). The results of separate regressions for each income gap hint at a potential explanation (see Appendix 1; Table 12, models 4–12). In particular, learning might be limited to perceptions of the gender and race income gap; all relevant coefficients in the intergenerational gap specification are insignificant. As such, the evidence for a lasting treatment effect on income gap perceptions is mixed but mostly in favor of Hypothesis 3.

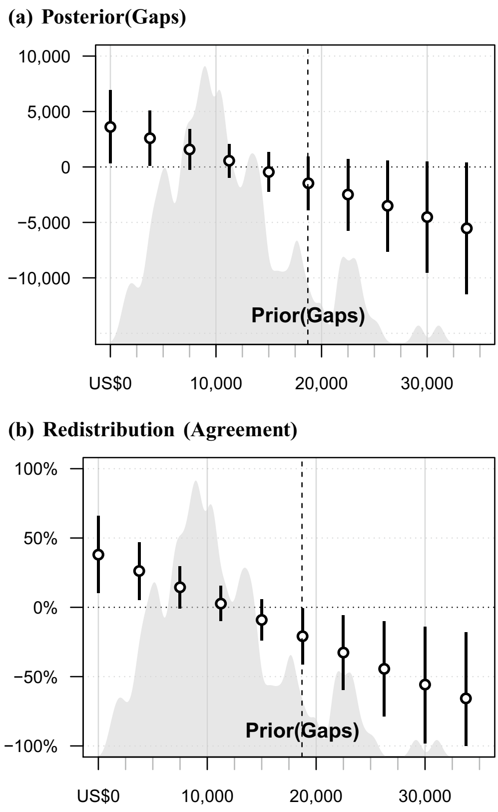

Fig. 5 Treatment effects conditional on average prior perception of income gaps (follow-up survey). *Note* **a** Conditional average treatment effect (vertical axis) on mean posterior income gap perception, *Posterior(Gaps)*, based on Table 12, model 1, **b** conditional average treatment effects (vertical axis) as change in predicted probability (percentage points) of agreeing with redistribution, based on Table 13, model 1. *Prior(Gaps)* refers to the mean prior income gap perception (in initial survey). Confidence intervals (90%) based on bootstrapped model parameters ($N = 100{,}000$). Gray shading indicates distribution of prior income gap perceptions (kernel density estimation, bandwidth = 500). The average of the actual income gaps is indicated by the dashed vertical lines

Lasting Preference Change

Even if information about income gaps has been internalized by the respondents, it is far from obvious that this should also lead to lasting changes in redistributive preferences. To address this question, I estimate the same set of model as in the initial round, now using redistributive preferences in the follow-up survey as dependent variables, and an attrition-adjusted model (see Appendix 1; Table 13). The results strongly speak to lasting treatment effects, lending further support to Hypothesis 4. As Fig. 5b evinces, the long-term effect of information about income gaps is still strongest among those who perceived the income gaps to be smallest. Those who perceived all gaps to equal zero and were treated with information about the actual size of the gaps are—one year later—still 38 percentage points more likely to agree with redistribution than their counterparts in the control group. This effect is statistically indistinguishable from the immediate treatment effect in the initial survey. As in the initial survey, the effect is less pronounced among those whose perceptions were more accurate. And again, those who overestimated the income gaps and received information about the actual gaps are still less supportive of redistribution than those who received no information.

These findings are robust to estimating separate models for each income gap perception (see Appendix 1; Table 13) and to treating redistribution as a continuous rather than a dichotomous variable (see Appendix 1; Table 14). In sum, the results show that the information provided to the treated did lead them to correct their perceptions about income gaps. What is more, the revealed patterns coincide with how respondents adjust preferences for redistribution. This supports the argument that the treatment effect on redistribution preferences is the result of updated income gap perceptions.

Discussion

This study has explored perceptions of the income distribution, in particular income gaps between groups of different gender, race, and family background, and their effects on preferences for redistribution. The large majority of participants in this study perceive income gaps to be smaller than they actually are. What is more, these perceptions have been shown to have a strong impact on redistributive preferences. The larger participants in this study perceived income gaps to be, the more supportive they were of redistribution. Due to the experimental manipulation of these perceptions through the provision of accurate information to half of the participants, it was demonstrated that this relationship is—to considerable degree—causal. Those who underestimated the income gaps become more supportive of redistribution when treated with accurate information, and those who overestimated them become less supportive. These effects were long-lasting as evinced by a follow-up survey after one year. The follow-up survey also showed that the informational treatment had a lasting effect on the income gap perceptions, with participants in the treatment group still expressing more accurate perceptions than those in the control group.

The finding that study participants underestimated income gaps is maybe the least surprising. It echoes earlier work on other perceptions of the income distribution (i.e., relative positions, inequality, and mobility). It is often argued that one reason for this pattern is people's immediate social environment, which is usually economically more homogeneous than society at large. However, this cannot easily explain the finding that men are more likely than women to underestimate the gender income gap, and why the same is true for whites regarding the race income gap, but not for those with university-educated parents regarding the intergenerational income gap. While one might want to explore more closely the specific social environments of each of these groups, other explanations should also be considered.

One of them is motivated beliefs and reasoning. Most commonly, this motivation is rooted in a need to justify the system a person lives in or to justify one's own position in society. As such, individuals facing disadvantage develop beliefs that legitimate the status quo, thus reducing related distress and relieving them from a need to demand change (Jost, Pelham, Sheldon, & Ni Sullivan, 2003). Similarly, advantaged individuals form beliefs that accommodate the privileges they experience and allow them to ignore group-related injustices (Miron, Warner, & Branscombe, Miron et al. 2011). Factual perceptions are not seen to play an important role as they are overwhelmed by individual justification processes. Trump (2017) has shown this to be the case for inequality perceptions and similar processes might explain why, as this study has shown, advantaged individuals are less responsive to gender and race gaps than disadvantaged individuals.

A further explanation relates to education. As education affects how individuals process information, and as parents impress some of their education on their children, it could lead their children to process information faster (Mérola & Hitt, 2016). This might explain why those with university-educated parents tend to respond more strongly to new information about income gaps. Last but not least, the extent to which individuals hold egalitarian or pro-social attitudes might differ by groups, for example due to shared experiences of disadvantage or socialization more generally (Auspurg, Hinz, & Sauer, 2017; O'Grady, 2017), but also genetic endowments (Batricevic & Littvay, 2017).

In addition to various robustness checks, the analysis above also shed light on different details of the informational treatment. Specifically, effects on redistributive preferences have been analyzed for each of the income gaps separately as well as by splitting the sample into subgroups based on the definition of the income gaps. While the corresponding findings cannot strictly speaking be causally interpreted, they can inform further research. Most importantly, the treatment effect does not appear to be driven by any one income gap alone. However, treatment effects do vary considerably across different subgroups. Women, people of color, and those with a university-educated parent are more responsive to information about income gaps, though this cannot simply be accounted for by their own positionality with regard to a specific gap. It is a promising endeavor for future research to further explore which groups respond to what gaps and whether this can be explained by differences in perceptions and information processing, or possibly, different normative assessments of income gaps.

The findings presented in this paper come with the same caveats that apply to similar experiments. First of all, even though the respondents in this experiment cover a wide range of socio-demographics, they constitute a convenience sample. Findings are thus limited in their generalizability. Therefore, repeating the experiment on different samples or in representative surveys is of utmost importance. Furthermore, Barabas and Jerit (2010) have shown that informational effects are contingent on levels of exposure. Hence, survey experiments usually find stronger effects than more realistic natural or field experiments. Ideally, future experiments on the mechanism revealed in this paper will make use of such designs. As earlier studies have found that the relationship between objective indicators and inequality perceptions can depend on context (e.g., Loveless & Whitefield, 2011), further studies should also expand beyond the USA. Despite these shortcomings, the present paper also overcame a major criticism of earlier survey experiments, the durability of effects. While this finding similarly calls for replications, the fact that treatment effects persisted for well over one year should be encouraging for other scholars interested in the effect of information on individual perceptions and preferences.

The treatment in this study was designed to connect the insight of earlier research on distributive justice that people tend to oppose income differences that result from factors beyond individual control, but not those resulting from factors within individual control, to an objective characteristic of the income distribution. I have argued that income gaps constitute such a characteristic, indicating how strongly income is affected by factors beyond individual control. However, I also pointed out that income gaps can reflect factors that are believed to be within individual control and therefore income gaps constitute an imprecise signal. While I have increased the precision of the signal by focusing only on income gaps among the employed, future studies might seek to account for other factors, such as education, occupation, or working hours. There is a long-standing debate on how to best measure gender and race gaps to explore factors such as discrimination and self-selection, in particular concerning the adjustment of confounding factors (Blinder, 1973; Blau & Kahn, 2017). How differently measured income gaps can affect individual preferences and public opinion is an important question for future research. More generally, studies might want to look at what determines the salience of different income gaps, also in relation to their measurement, and whether people employ certain strategies to justify income gaps.

Conclusion

The mechanism revealed in this paper connects two strains of political economy scholarship. One of them argues that it is beliefs about equality of opportunity or economic fairness that are decisive for redistributive preferences, but—with the exception of a few works on intergenerational mobility—this scholarship does not link preferences and objective characteristics of the income distribution. The second line of scholarship focuses on how such objective characteristics influence preferences. However, that scholarship has found it difficult to determine what it is about inequality that people reject, unless it is aligned with their material self-interest. This paper argues that income gaps—an objective characteristic of the income distribution—can serve people as an indication for the presence of unequal opportunities. As such, the paper opens a new avenue to explore how changes in the income distribution and demand for government redistribution relate.

Great care was taken in the experimental design to ascertain that any revealed effect can indeed be attributed to the informational content of the treatment. Still, some questions about the underlying mechanism remain. While I argued that it is a desire for distributive equality of opportunity that underpins the effect, one can also argue that respondents use the information to update other perceptions or beliefs that are relevant to redistributive preferences. For example, people might form preferences according to the Rawlsian difference principle and use the provided information to update their beliefs about the well-being of less advantaged groups. Alternatively, learning that income gaps are different from what one thought might lead individuals to update perceptions of national income averages and thus also their own relative economic standing. While this implies a rather complex mechanism, the possibility of some hidden self-interest, beyond the naive version considered here, cannot be fully excluded. It is up to future research to better discriminate between these mechanisms.

In addition to replications and extensions of the presented experiment, it is important to study how information about the income distribution spreads in the real world. An interesting starting point is work by Iversen and Soskice (2015) who argue that inequality and lack of information about it are reinforcing. They show that increases in inequality are associated with institutional change, like decreasing union density and access to education, which simultaneously undermine the availability of political information to the poor. It is possible that similar dynamics are at work with regard to income gaps. In particular, income gaps can limit the resources disadvantaged groups have at their avail to contest such gaps and inform the public about them.

Another avenue forward relates research on intergroup contact. This line of work points to direct contact between members of different groups to explain group-related preferences and beliefs. Under favorable conditions intergroup contact can reduce prejudice and allow groups to work toward common ends (Pettigrew, 1998). In the absence of favorable conditions contact can produce the opposite, leading to resentment between groups (Semyonov, Raijman, & Gorodzeisky, 2006). Newman (2014) has shown that direct contact across economic groups can be an important determinant of inequality-related beliefs and redistributive preferences. With its focus on prejudice, research on intergroup contact is rarely concerned with the factual information transmitted in such exchanges. The findings presented here suggest that closer attention to this dimension of intergroup contact would be a promising way forward, in particular with regard to income gaps.

A final comment on the use of information on income gaps in political communication is warranted. While this paper has demonstrated that such information can effectively and lastingly increase support for redistribution, it has also shown that the effects are contingent on other factors, such as prior knowledge and group identity. In order to use such information it is thus necessary to consider the composition of targeted audiences. In the probably rare case of audiences who overestimate income gaps, information might even have the inverse effect, reducing support for redistribution.

Acknowledgments Bastian Becker has received a research grant from the Central European University (Grant No. 2014/2015/1/RSS)

Compliance With Ethical Standards

Conflicts of interest No conflict of interest is declared.

Human Participants All procedures performed involving human participants were approved by the Central European University's Ethical Research Committee.

Appendix 1

See Tables 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15 and 16.

Table 3 Descriptive statistics of respondent sample (follow-up survey)

	Mean	SD	Min.	Max.
Treatment	0.51	0.50	0.00	1.00
Age	39.46	13.08	21.00	71.00
Male	0.49	0.50	0.00	1.00
Household size	2.57	1.42	1.00	9.00
Children	0.89	1.22	0.00	5.00
Income	3.79	3.62	0.05	19.50
University-educated parent	0.59	0.49	0.00	1.00
Race				
White	0.82	0.38	0.00	1.00
Black	0.07	0.25	0.00	1.00
Other	0.11	0.31	0.00	1.00
Employment status				
Unemployed	0.07	0.25	0.00	1.00
Full-time	0.67	0.47	0.00	1.00
Part-time	0.10	0.30	0.00	1.00
Keeping house	0.03	0.17	0.00	1.00
Retired	0.07	0.26	0.00	1.00
Student	0.04	0.21	0.00	1.00
Other	0.02	0.15	0.00	1.00
Education				
Less than high school	0.00	0.00	0.00	0.00
High school	0.28	0.45	0.00	1.00
University	0.72	0.45	0.00	1.00

Income in US$10,000. University-educated parent is a dummy variable indicating whether respondent has at least one parent with a university degree. Education refers to respondent level of education

Table 4 Agreement with redistribution, raw distributions (initial and follow-up survey)

	Strongly disagree	Disagree	Slightly disagree	Neither	Slightly agree	Agree	Strongly agree
Initial survey	55	28	31	36	52	71	91
	(15.1%)	(7.7%)	(8.5%)	(9.9%)	(14.3%)	(19.5%)	(25%)
Follow-up survey	18	10	8	6	31	30	33
	(13.2%)	(7.4%)	(5.9%)	(4.4%)	(22.8%)	(22.1%)	(24.3%)

Pooled distribution of responses in treatment and control group as elicited in the initial and follow-up survey

Table 5 LPM results (baseline), effects on agreement with redistribution (initial and follow-up survey)

	Dependent variable:									
	Redistribution (agreement)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment		0.101^{+}		0.087^{+}		0.021		0.056		0.038
		(0.052)		(0.051)		(0.079)		(0.079)		(0.079)
Prior(Gaps)	0.064	0.066	0.050	0.052	0.171^{*}	0.172^{*}	0.165^{**}	0.168^{**}	0.118^{+}	0.120^{+}
	(0.042)	(0.042)	(0.042)	(0.042)	(0.066)	(0.066)	(0.063)	(0.063)	(0.066)	(0.067)
Paid follow-up									0.078	0.077
									(0.098)	(0.098)
Constant	0.504^{***}	0.448^{***}	0.753^{***}	0.706^{***}	0.486^{***}	0.474^{***}	0.504^{***}	0.471^{***}	0.670^{**}	0.647^{**}
	(0.060)	(0.066)	(0.120)	(0.123)	(0.092)	(0.103)	(0.093)	(0.104)	(0.206)	(0.212)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
IPW							✓	✓		
Controls			✓	✓					✓	✓
Survey	Initial	Initial	Initial	Initial	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up
Observations	364	364	364	364	136	136	136	136	136	136
R^2	0.007	0.017	0.070	0.078	0.050	0.050	0.050	0.054	0.152	0.153
Adjusted R^2	0.001	0.009	0.047	0.052	0.035	0.029	0.036	0.032	0.084	0.078

LPM Linear probability models, *IPW* inverse probability weighting. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 6 LPM results, effects on agreement with redistribution, by gender (initial survey)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.463**	0.257+	0.483***	0.144	0.289*	0.158	0.251	0.327*
	(0.158)	(0.148)	(0.130)	(0.119)	(0.121)	(0.127)	(0.152)	(0.137)
Prior(Gaps)	0.252**	0.082						
	(0.080)	(0.083)						
Treatment × prior(Gaps)	− 0.305*	− 0.137						
	(0.119)	(0.117)						
Prior(Gender gap)			0.268***	0.024				
			(0.073)	(0.084)				
Treatment × Prior(Gender gap)			− 0.379***	− 0.047				
			(0.109)	(0.114)				
Prior(Race gap)					0.233***	0.056		
					(0.061)	(0.073)		
Treatment × Prior(Race gap)					− 0.179*	− 0.047		
					(0.087)	(0.094)		
Prior(Intergenerational gap)							0.033	0.070
							(0.065)	(0.057)
Treatment × Prior(Intergen. gap)							− 0.111	−0.159+
							(0.094)	(0.084)
Constant	0.315**	0.356**	0.333***	0.428***	0.371***	0.385***	0.558***	0.351***
	(0.111)	(0.114)	(0.095)	(0.097)	(0.086)	(0.106)	(0.114)	(0.102)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓
Controls								
Sample	Female	Male	Female	Male	Female	Male	Female	Male

Table 6 (continued)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Observations	167	197	167	197	167	197	167	197
R^2	0.071	0.019	0.098	0.012	0.098	0.015	0.021	0.030
Adjusted R^2	0.048	− 0.002	0.076	− 0.008	0.076	− 0.006	− 0.003	0.010

LPM Linear probability models. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 7 LPM results (with controls), effects on agreement with redistribution, by gender (initial survey)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.435^{**}	0.186	0.457^{***}	0.083	0.257^{*}	0.122	0.246	0.273^{*}
	(0.161)	(0.148)	(0.134)	(0.119)	(0.123)	(0.128)	(0.155)	(0.137)
Prior(Gaps)	0.259^{**}	0.036						
	(0.081)	(0.084)						
Treatment × Prior(Gaps)	-0.300^{*}	− 0.075						
	(0.121)	(0.116)						
Prior(Gender gap)			0.264^{***}	− 0.025				
			(0.074)	(0.083)				
Treatment × Prior(Gender gap)			-0.372^{**}	0.023				
			(0.111)	(0.113)				
Prior(Race gap)					0.234^{***}	0.027		
					(0.062)	(0.072)		
Treatment × Prior(Race gap)					-0.168^{+}	− 0.017		
					(0.088)	(0.093)		
Prior(Intergenerational gap)							0.046	0.049
							(0.066)	(0.059)
Treatment × Prior(Intergen. gap)							− 0.122	− 0.123
							(0.096)	(0.085)
Constant	0.370^{+}	0.642^{***}	0.365^{*}	0.718^{***}	0.414^{*}	0.660^{***}	0.598^{**}	0.587^{**}
	(0.189)	(0.185)	(0.181)	(0.169)	(0.175)	(0.172)	(0.191)	(0.181)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sample	Female	Male	Female	Male	Female	Male	Female	Male

Table 7 (continued)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Observations	167	197	167	197	167	197	167	197
R^2	0.091	0.094	0.113	0.093	0.118	0.093	0.041	0.103
Adjusted R^2	0.033	0.045	0.056	0.044	0.062	0.044	− 0.021	0.054

LPM Linear probability models. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 8 LPM results, effects on agreement with redistribution, by race (initial survey)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.816^{***}	0.262^{*}	0.684^{***}	0.193^{+}	0.634^{***}	0.146	0.642^{**}	0.243^{*}
	(0.219)	(0.126)	(0.168)	(0.102)	(0.177)	(0.103)	(0.236)	(0.114)
Prior(Gaps)	0.254^{*}	0.149^{*}						
	(0.097)	(0.072)						
Treatment × Prior(Gaps)	-0.377^{*}	-0.192^{+}						
	(0.145)	(0.102)						
Prior(Gender gap)			0.183^{*}	0.160^{*}				
			(0.089)	(0.068)				
Treatment × Prior(Gender gap)			-0.402^{**}	-0.155				
			(0.140)	(0.094)				
Prior(Race gap)					0.235^{**}	0.115^{+}		
					(0.076)	(0.059)		
Treatment × Prior(Race gap)					-0.248^{*}	-0.089		
					(0.104)	(0.080)		
Prior(Intergenerational gap)							0.111	0.051
							(0.083)	(0.052)
Treatment × Prior(Intergen. gap)							-0.204	-0.142^{+}
							(0.128)	(0.073)
Constant	0.161	0.369^{***}	0.322^{*}	0.387^{***}	0.204	0.413^{***}	0.311^{+}	0.463^{***}
	(0.159)	(0.093)	(0.126)	(0.080)	(0.129)	(0.080)	(0.173)	(0.086)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓
Controls								

Table 8 (continued)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	Non-white	White	Non-white	White	Non-white	White	Non-white	White
Observations	73	291	73	291	73	291	73	291
R^2	0.200	0.019	0.203	0.022	0.216	0.017	0.139	0.017
Adjusted R^2	0.153	0.005	0.156	0.008	0.170	0.003	0.088	0.004

LPM Linear probability models. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 9 LPM results (with controls), effects on agreement with redistribution, by race (initial survey)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.761**	0.237^{+}	0.625**	0.180^{+}	0.607**	0.140	0.625*	0.215^{+}
	(0.234)	(0.124)	(0.191)	(0.101)	(0.186)	(0.102)	(0.242)	(0.112)
Prior(Gaps)	0.224*	0.126^{+}						
	(0.099)	(0.071)						
Treatment × Prior(Gaps)	− 0.338*	− 0.168^{+}						
	(0.151)	(0.100)						
Prior(Gender gap)			0.135	0.125^{+}				
			(0.096)	(0.068)				
Treatment × Prior(Gender gap)			− 0.335*	− 0.139				
			(0.155)	(0.093)				
Prior(Race gap)					0.222**	0.109^{+}		
					(0.076)	(0.058)		
Treatment × Prior(Race gap)					− 0.236*	− 0.081		
					(0.105)	(0.079)		
Prior(Intergenerational gap)							0.111	0.039
							(0.083)	(0.052)
Treatment × Prior(Intergen. gap)							− 0.198	−0.120^{+}
							(0.129)	(0.073)
Constant	0.005	0.565***	0.171	0.592***	0.110	0.603***	0.092	0.633***
	(0.327)	(0.147)	(0.318)	(0.139)	(0.295)	(0.138)	(0.350)	(0.143)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Sample	Non-white	White	Non-white	White	Non-white	White	Non-white	White

Table 9 (continued)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Observations	73	291	73	291	73	291	73	291
R^2	0.278	0.076	0.263	0.076	0.304	0.077	0.238	0.075
Adjusted R^2	0.161	0.043	0.144	0.043	0.192	0.044	0.115	0.042

LPM Linear probability models. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 10 LPM results, effects on agreement with redistribution, by parental education (initial survey)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.224	0.454**	0.208	0.364**	0.189	0.256*	0.194	0.385**
	(0.156)	(0.152)	(0.130)	(0.124)	(0.129)	(0.122)	(0.152)	(0.138)
Prior(Gaps)	0.184*	0.155+						
	(0.079)	(0.085)						
Treatment × Prior(Gaps)	− 0.090	− 0.310**						
	(0.120)	(0.118)						
Prior(Gender gap)			0.169*	0.153+				
			(0.070)	(0.090)				
Treatment × Prior(Gender gap)			− 0.096	− 0.306*				
			(0.108)	(0.119)				
Prior(Race gap)					0.162*	0.138*		
					(0.067)	(0.067)		
Treatment × Prior(Race gap)					− 0.067	− 0.142		
					(0.095)	(0.089)		
Prior(Intergenerational gap)							0.076	0.048
							(0.065)	(0.058)
Treatment × Prior(Intergen. gap)							− 0.057	− 0.199*
							(0.097)	(0.083)
Constant	0.332**	0.331**	0.384***	0.365***	0.362***	0.357***	0.441***	0.440***
	(0.115)	(0.112)	(0.097)	(0.100)	(0.102)	(0.093)	(0.115)	(0.102)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓
Controls								

Table 10 (continued)

	Dependent variable:							
	Redistribution (Agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample+	No Uni.	University	No Uni.	University	No Uni.	University	No Uni.	University
Observations	151	213	151	213	151	213	151	213
R^2	0.055	0.042	0.056	0.041	0.063	0.030	0.023	0.043
Adjusted R^2	0.029	0.023	0.030	0.023	0.037	0.011	− 0.004	0.024

LPM Linear probability models. Prior income gap perceptions in US$, ten thousands. (+) Sample split by parents' highest level of education. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 11 LPM results (with controls), effects on agreement with redistribution, by parental education (initial survey)

	Dependent variable:							
	Redistribution (agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.189	0.432**	0.187	0.314*	0.162	0.251*	0.151	0.388**
	(0.161)	(0.151)	(0.137)	(0.122)	(0.134)	(0.120)	(0.158)	(0.137)
Prior(Gaps)	0.168*	0.138+						
	(0.084)	(0.083)						
Treatment × Prior(Gaps)	− 0.074	− 0.285*						
	(0.122)	(0.117)						
Prior(Gender gap)			0.145*	0.114				
			(0.073)	(0.088)				
Treatment × Prior(Gender gap)			− 0.085	− 0.243*				
			(0.111)	(0.117)				
Prior(Race gap)					0.155*	0.130*		
					(0.071)	(0.065)		
Treatment × Prior(Race gap)					− 0.057	− 0.134		
					(0.097)	(0.087)		
Prior(Intergenerational gap)							0.062	0.048
							(0.069)	(0.057)
Treatment × Prior(Intergen. gap)							− 0.039	− 0.196*
							(0.100)	(0.083)
Constant	0.406*	0.662***	0.461*	0.737***	0.425*	0.725***	0.539*	0.742***
	(0.203)	(0.193)	(0.190)	(0.182)	(0.189)	(0.176)	(0.207)	(0.188)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Table 11 (continued)

	Dependent variable:							
	Redistribution (agreement)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample$^+$	No Uni.	University	No Uni.	University	No Uni.	University	No Uni.	University
Observations	151	213	151	213	151	213	151	213
R^2	0.086	0.129	0.083	0.123	0.097	0.121	0.060	0.133
Adjusted R^2	0.014	0.081	0.010	0.075	0.026	0.072	− 0.015	0.086

LPM Linear probability models. Prior income gap perceptions in US$, ten thousands. ($^+$) Sample split by parents' highest level of education. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^+p < 0.1$; $^*p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 12 OLS Model results, effects on perceptions of income gaps (follow-up survey)

	Dependent variable:											
	Posterior(Gaps)			Posterior(Gender gap)			Posterior(Race gap)			Posterior(Intergenerational gap)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	0.362^{+}	0.412^{*}	0.363^{+}	0.414^{*}	0.457^{*}	0.478^{*}	0.427^{*}	0.516^{*}	0.470^{*}	0.005	0.034	− 0.138
	(0.202)	(0.205)	(0.207)	(0.185)	(0.182)	(0.189)	(0.197)	(0.202)	(0.201)	(0.263)	(0.276)	(0.262)
Prior(Gaps)	0.630^{***}	0.588^{***}	0.604^{***}									
	(0.102)	(0.099)	(0.107)									
Treatment × Prior(Gaps)	-0.272^{+}	-0.265^{+}	-0.280^{+}									
	(0.157)	(0.152)	(0.161)									
Prior(Gender gap)				0.534^{***}	0.465^{***}	0.543^{***}						
				(0.107)	(0.095)	(0.110)						
Treatment × Prior(Gender gap)				-0.373^{*}	-0.325^{*}	-0.425^{*}						
				(0.161)	(0.148)	(0.164)						
Prior(Race gap)							0.729^{***}	0.669^{***}	0.746^{***}			
							(0.104)	(0.101)	(0.107)			
Treatment × Prior(Race gap)							-0.362^{*}	-0.365^{**}	-0.397^{**}			
							(0.142)	(0.140)	(0.145)			
Prior(Intergenerational gap)										0.409^{***}	0.436^{***}	0.334^{**}
										(0.111)	(0.119)	(0.114)
Treatment × Prior(Intergen. gap)										0.035	0.013	0.116
										(0.170)	(0.174)	(0.170)
Paid follow-up			0.051			0.095			− 0.105			0.133
			(0.120)			(0.137)			(0.148)			(0.158)
Constant	0.357^{*}	0.361^{*}	0.581^{*}	0.327^{*}	0.298^{*}	0.095	0.328^{*}	0.307^{+}	0.318	0.701^{***}	0.732^{***}	1.713^{***}
	(0.147)	(0.151)	(0.281)	(0.146)	(0.144)	(0.305)	(0.155)	(0.159)	(0.316)	(0.190)	(0.208)	(0.364)

Table 12 (continued)

	Dependent variable:											
	Posterior(Gaps)			Posterior(Gender gap)			Posterior(Race gap)			Posterior(Intergenerational gap)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
IPW		✓			✓			✓			✓	
Controls			✓			✓			✓			✓
Observations	136	136	136	136	136	136	136	136	136	136	136	136
R^2	0.283	0.268	0.323	0.194	0.193	0.249	0.337	0.304	0.374	0.172	0.173	0.263
Adjusted R^2	0.261	0.246	0.257	0.169	0.168	0.176	0.317	0.283	0.313	0.147	0.148	0.191

Prior variables refer to income gap perceptions in initial survey, posterior variables to perception in follow-up survey, all in US$, ten thousands. OLS = ordinary least squares. IPW = inverse probability weighting. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 13 LPM results, effects on agreement with redistribution (follow-up survey)

	Dependent variable:											
	Redistribution (Agreement)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	0.380^{*}	0.513^{**}	0.370^{*}	0.279^{*}	0.341^{*}	0.288^{*}	0.240^{+}	0.346^{*}	0.247^{+}	0.323^{+}	0.458^{**}	0.279^{+}
	(0.168)	(0.166)	(0.168)	(0.134)	(0.131)	(0.133)	(0.134)	(0.136)	(0.132)	(0.165)	(0.165)	(0.163)
Prior(Gaps)	0.305^{***}	0.330^{***}	0.245^{**}									
	(0.085)	(0.080)	(0.086)									
Treatment × Prior(Gaps)	-0.314^{*}	-0.382^{**}	-0.291^{*}									
	(0.131)	(0.123)	(0.131)									
Prior(Gender gap)				0.278^{***}	0.265^{***}	0.235^{**}						
				(0.077)	(0.069)	(0.078)						
Treatment × Prior(Gender gap)				-0.270^{*}	-0.281^{**}	-0.262^{*}						
				(0.116)	(0.107)	(0.116)						
Prior(Race gap)							0.247^{***}	0.261^{***}	0.213^{**}			
							(0.070)	(0.068)	(0.070)			
Treatment × Prior(Race gap)							-0.200^{*}	-0.251^{**}	-0.188^{+}			
							(0.097)	(0.094)	(0.095)			
Prior(Intergenerational gap)										0.136^{+}	0.195^{**}	0.085
										(0.070)	(0.072)	(0.071)
Treatment × Prior(Intergen. gap)										-0.228^{*}	-0.292^{**}	-0.182^{+}
										(0.106)	(0.104)	(0.106)
Constant	0.310^{*}	0.259^{*}	0.444^{+}	0.383^{***}	0.377^{***}	0.481^{*}	0.393^{***}	0.361^{**}	0.526^{*}	0.481^{***}	0.390^{**}	0.659^{**}
	(0.122)	(0.122)	(0.228)	(0.106)	(0.104)	(0.216)	(0.105)	(0.108)	(0.208)	(0.119)	(0.125)	(0.227)
Round dummy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
IPW		✓			✓			✓			✓	

Table 13 (continued)

	Dependent variable:											
	Redistribution (Agreement)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Controls			✓			✓			✓			✓
Observations	136	136	136	136	136	136	136	136	136	136	136	136
R^2	0.090	0.119	0.186	0.091	0.104	0.191	0.090	0.103	0.192	0.039	0.067	0.152
Adjusted R^2	0.062	0.092	0.107	0.063	0.076	0.113	0.062	0.076	0.113	0.009	0.039	0.069

LPM Linear probability models, *IPW* inverse probability weighting. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 14 Main OLS model results, effects on agreement with redistribution (initial and follow-up survey)

	Dependent variable:				
	Redistribution (1-7)				
	(1)	(2)	(3)	(4)	(5)
Treatment	1.178*	0.929*	1.873*	2.344**	1.719*
	(0.474)	(0.463)	(0.745)	(0.721)	(0.751)
Prior(Gaps)	0.725**	0.599*	1.375***	1.565***	1.179**
	(0.253)	(0.248)	(0.377)	(0.348)	(0.387)
Treatment × Prior(Gaps)	− 0.800*	− 0.643^{+}	− 1.560**	− 1.767**	− 1.425*
	(0.365)	(0.355)	(0.579)	(0.536)	(0.585)
Paid follow-up					0.419
					(0.434)
Constant	3.525***	5.151***	3.005***	2.751***	3.196**
	(0.350)	(0.578)	(0.540)	(0.530)	(1.019)
Round dummy	✓	✓	✓	✓	✓
Controls		✓			✓
IPW				✓	
Survey	Initial	Initial	Follow-up	Follow-up	Follow-up
Observations	364	364	136	136	136
R^2	0.028	0.110	0.102	0.147	0.173
Adjusted R^2	0.017	0.082	0.067	0.115	0.093

OLS ordinary least squares, *IPW* inverse probability weighting. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 15 Further OLS model results, effects on agreement with redistribution (Initial survey)

	Dependent variable:					
	Redistribution (1-7)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1.133^{**}	0.888^{*}	0.628	0.522	0.995^{*}	0.760^{+}
	(0.384)	(0.378)	(0.384)	(0.376)	(0.445)	(0.435)
Prior(Gender gap)	0.785^{**}	0.608^{*}				
	(0.241)	(0.237)				
Treatment × Prior(Gender gap)	-0.951^{**}	-0.754^{*}				
	(0.344)	(0.336)				
Prior(Race gap)			0.613^{**}	0.565^{**}		
			(0.206)	(0.201)		
Treatment × Prior(Race gap)			−0.341	−0.308		
			(0.281)	(0.272)		
Prior(Intergenerational gap)					0.219	0.155
					(0.188)	(0.186)
Treatment × Prior(Intergen. gap)					-0.532^{+}	−0.413
					(0.274)	(0.268)
Constant	3.618^{***}	5.285^{***}	3.688^{***}	5.263^{***}	4.047^{***}	5.610^{***}
	(0.299)	(0.539)	(0.298)	(0.531)	(0.332)	(0.576)
Round dummy	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Observations	364	364	364	364	364	364
R^2	0.035	0.112	0.035	0.119	0.016	0.101
Adjusted R^2	0.025	0.085	0.024	0.092	0.005	0.073

OLS Ordinary Least Squares. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Table 16 Panel models results (LPM and OLS), effects on agreement with redistribution

	Dependent variable:					
	Redistribution (Agreement)			Redistribution (1-7)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.574^{**}	0.726^{***}	0.565^{**}	2.011^{*}	2.745^{**}	1.837^{*}
	(0.180)	(0.182)	(0.196)	(0.854)	(0.871)	(0.895)
Prior(Gaps)	0.256^{***}	0.300^{***}	0.207^{**}	1.123^{**}	1.327^{**}	0.935^{**}
	(0.071)	(0.067)	(0.077)	(0.342)	(0.434)	(0.363)
Follow-up survey	0.046	0.092	0.046	0.031	0.189	0.031
	(0.067)	(0.081)	(0.067)	(0.289)	(0.348)	(0.289)
Treatment × Prior(Gaps)	-0.449^{**}	-0.511^{***}	-0.426^{**}	-1.626^{**}	-1.958^{**}	-1.466^{*}
	(0.137)	(0.144)	(0.147)	(0.611)	(0.631)	(0.632)
Treatment × Follow-up survey	− 0.191	− 0.212	− 0.191	− 0.140	− 0.401	− 0.140
	(0.126)	(0.171)	(0.126)	(0.497)	(0.600)	(0.497)
Prior(Gaps) × Follow-up survey	0.049	0.030	0.049	0.252	0.237	0.252
	(0.050)	(0.055)	(0.050)	(0.214)	(0.250)	(0.214)
Treatment × Prior(Gaps) × Follow-up survey	0.133	0.129	0.133	0.068	0.191	0.068
	(0.132)	(0.184)	(0.132)	(0.487)	(0.639)	(0.487)
Paid follow-up			0.048			0.313
			(0.084)			(0.369)
Constant	0.254^{*}	0.165	0.365^{+}	2.981^{***}	2.563^{***}	3.220^{**}
	(0.117)	(0.118)	(0.213)	(0.563)	(0.639)	(1.027)
Round dummy	✓	✓	✓	✓	✓	✓
IPW		✓			✓	
Controls			✓			✓
Observations	136(2)	136(2)	136(2)	136(2)	136(2)	136(2)
R^2	0.094	0.132	0.176	0.085	0.128	0.177
Adjusted R^2	0.067	0.105	0.124	0.057	0.102	0.126

OLS Ordinary Least Squares, *LPM* linear probability models, *IPW* inverse probability weighting. Prior income gap perceptions in US$, ten thousands. Controls included in the respective models are dummy variables for gender, race, education (university), employment status, parental education (university), and continuous variables for age, income, and number of children. Standard errors clustered at individual level. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

Appendix 2

See Figs. 6, 7 and 8

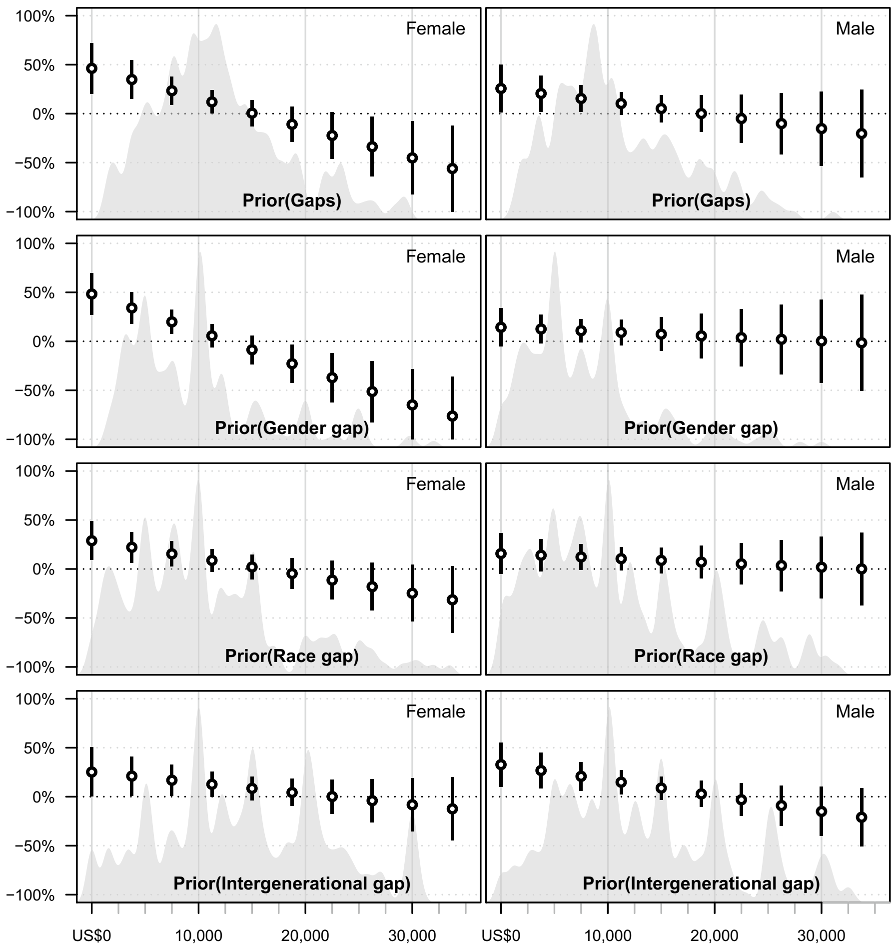

Fig. 6 Average treatment effect on agreement with redistribution conditional on prior perceptions of income gaps, by gender (initial survey). *Note*: Conditional average treatment effects (vertical axis) as change in predicted probability (percentage points) of agreeing with redistribution, based on separate models 1-8 (Table 6). *Prior(Gaps)* refers to the mean prior income gap perception. Confidence intervals (90%) based on bootstrapped model parameters (N=100,000). Gray shading indicates distribution of prior perceptions (kernel density estimation, bandwidth=500)

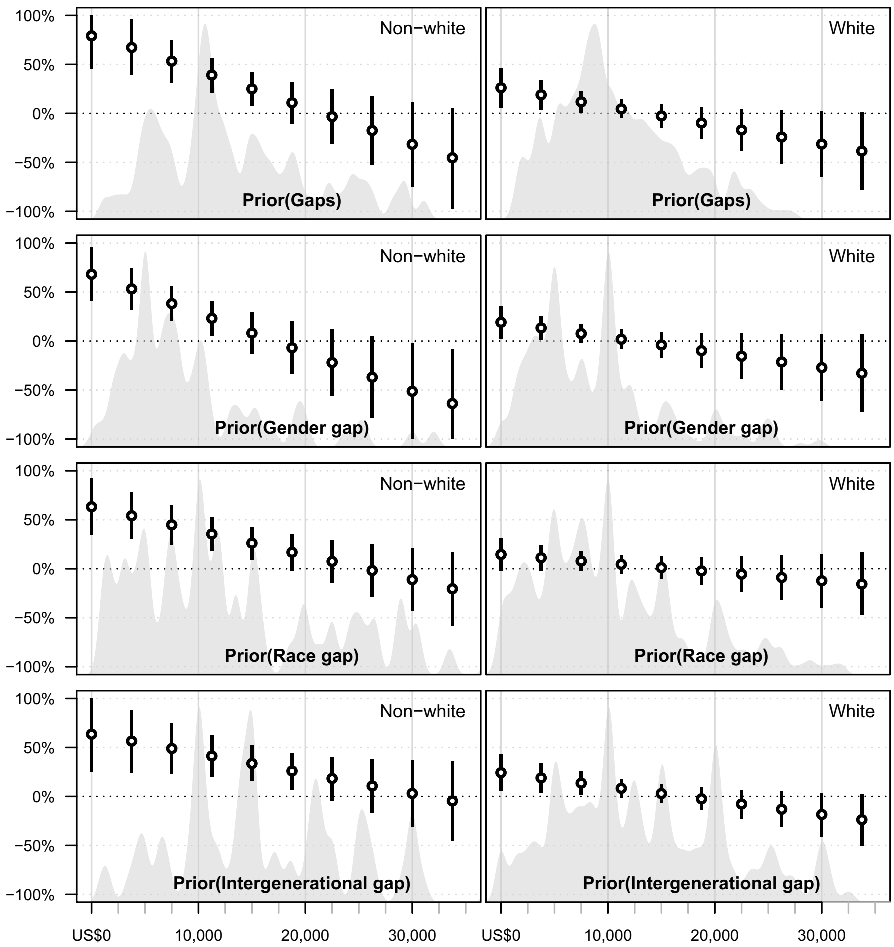

Fig. 7 Average treatment effect on agreement with redistribution conditional on prior perceptions of income gaps, by race (initial survey). *Note*: Conditional average treatment effects (vertical axis) as change in predicted probability (percentage points) of agreeing with redistribution, based on separate models 1–8 (Table 8). *Prior(Gaps)* refers to the mean prior income gap perception. Confidence intervals (90%) based on bootstrapped model parameters (N=100,000). Gray shading indicates distribution of prior perceptions (kernel density estimation, bandwidth=500)

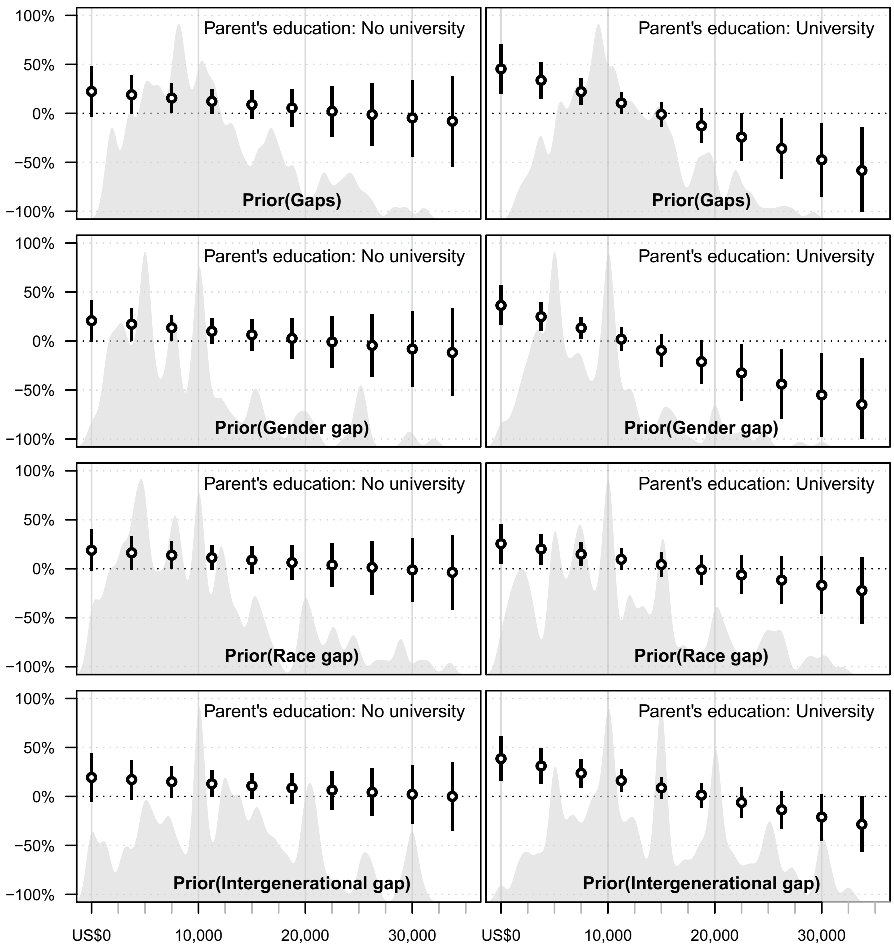

Fig. 8 Average treatment effect on agreement with redistribution conditional on prior perceptions of income gaps, by parental education (initial survey). *Note*: Conditional average treatment effects (vertical axis) as change in predicted probability (percentage points) of agreeing with redistribution, based on separate models 1–8 (Table 10). *Prior(Gaps)* refers to the mean prior income gap perception. Confidence intervals (90%) based on bootstrapped model parameters ($N = 100,000$). Gray shading indicates distribution of prior perceptions (kernel density estimation, bandwidth = 500)

Appendix 3

Linearity of Treatment Effects

Above analysis posited a linear relationship between prior perceptions and the strength of the treatment effect. Other specifications would require stronger

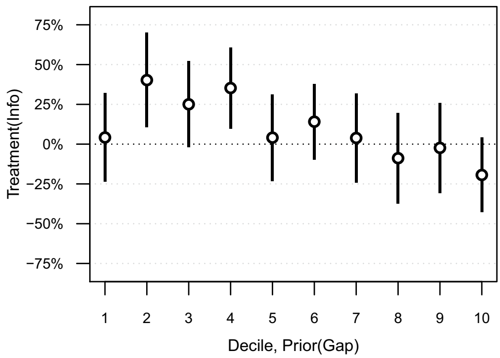

Fig. 9 LPM results, effects on agreement with redistribution, by average prior perception deciles (initial survey). *Note*: Local average treatment effects (vertical axis) as change in predicted probability (percentage points) of agreeing with redistribution. Horizontal axis indicates Prior(Gap) decile based on which OLS regression models were estimated. Models include treatment status and round dummy. Confidence intervals (90%) based on bootstrapped model parameters ($N = 100,000$)

assumptions about how individuals process information. To determine whether positing a linear relationship is warranted I estimate separate regression models for each decile of the *Prior(Gaps)* distribution. Figure 9 displays the treatment effects estimated for each decile. Confidence intervals are wide as the number of observations for each regression is only one-tenth of the total sample. The figure attests to clear deviations from a perfectly linear, or even monotonous, relationship. In particular, the treatment effect falls off among those in the lowest decile, whose average prior perception is below US$4000. One explanation might be that respondents with such low income gap perceptions might find information on the actual extent of income gaps hard to believe. Another deviation from a linear relationship is the steep decline in the treatment effect between the fourth and fifth decile. The difference between the two deciles accounts for much of the decline observed over all deciles (Fig. 9).

Another important aspect of Fig. 9 is that the treatment effect for the tenth decile strongly points into a negative direction. These respondents become more likely to disagree with redistribution when confronted with factual information. This is in fact what would be expected. With average prior perceptions of US$20,000 or higher, these respondents overestimated the actual size of the gaps. Therefore, confronting them with factual information should reduce their concern about income gaps and hence demand for redistribution. This corroborates the findings presented in Fig. 3 and affirms that the changing sign of the treatment effect is not driven by extrapolation. Overall, the separate regressions attest to a decline in the treatment effect that is sufficiently steady to assume a linear relationship between prior perceptions and the effect of factual information.

Appendix 4

Attrition Analysis and Inverse Probability Weighting

Experiments that stretch longer time periods unavoidably face attrition, which can lead to bias in the estimation of treatment effects. The high response rate to the second survey is not sufficient to exclude the possibility of such a bias. Therefore, it is important to check for indications of attrition bias. This is done similarly to how researchers check for covariate balance in single-shot experiments. Just as in the case of covariate balance, it is only possible to check for attrition bias based on

Table 17 OLS model results, determinants of attrition (DV: participation in follow-up survey)

	Control group		Treatment group		Difference (C–T)	
	Estimate	SE	Estimate	SE	Estimate	SE
Intercept	0.404*	0.037	0.383*	0.035	– 0.022	0.051
Male	– 0.158*	0.073	– 0.007	0.07	0.152	0.101
Age	0.009*	0.003	0.009*	0.003	0	0.004
Household size	– 0.027	0.025	– 0.016	0.024	0.011	0.034
Children	0.024	0.026	0.022	0.032	– 0.002	0.041
Income	– 0.005	0.009	0.002	0.011	0.007	0.015
University-educated parent	0.044	0.074	0.002	0.072	– 0.043	0.103
Prior(Gaps)	– 0.018	0.058	– 0.03	0.06	– 0.012	0.084
Redistribution	0	0.017	– 0.021	0.017	– 0.021	0.024
Duration (Survey 1)	– 0.003	0.007	0.005	0.007	0.008	0.009
Race						
White	0.135	0.088	0.014	0.091	– 0.121	0.127
Black	– 0.005	0.161	0.125	0.145	0.13	0.216
Other	– 0.166	0.098	– 0.089	0.108	0.077	0.146
Employment status						
Unemployed	– 0.14	0.153	– 0.036	0.115	0.104	0.192
Full-time	0.073	0.078	– 0.041	0.072	– 0.114	0.106
Part-time	– 0.13	0.108	0.066	0.104	0.196	0.15
Keeping house	– 0.162	0.178	– 0.008	0.176	0.154	0.251
Retired	0.444*	0.202	0.253	0.175	– 0.191	0.268
Student	– 0.074	0.205	– 0.02	0.152	0.053	0.254
Other	0.098	0.249	– 0.135	0.247	– 0.233	0.351
Education						
Less than high school	– 0.407	0.494	– 0.385	0.489	0.022	0.695
High school	– 0.14	0.075	– 0.052	0.074	0.088	0.105
University	0.149*	0.075	0.06	0.073	– 0.089	0.105

OLS Ordinary Least Squares. Each row corresponds univariate regression (first row, intercept-only), estimated separately for each covariate, for control (columns 2 and 3, $N = 175$) and treatment group (columns 4 and 5, $N = 189$), and jointly with interaction effects between treatment condition and covariate (columns 6 and 7, $N = 364$). (*$p < 0.05$)

Table 18 Logit model results, estimating probability of participation in follow-up survey

	Dependent variable: Participation, follow-up survey
Treatment (Info)	0.540
	(1.389)
Redistribution (Survey 1)	− 0.009
	(0.081)
Prior(Gap)	− 0.031
	(0.277)
Round	0.142
	(0.337)
Duration (Survey 1)	− 0.051
	(0.037)
Male	− 0.706^{+}
	(0.372)
White	0.122
	(0.468)
Full-time employment	0.929*
	(0.426)
Children	− 0.010
	(0.125)
Income	− 0.071
	(0.050)
University degree	0.364
	(0.373)
Age	0.053**
	(0.018)
Treatment (Info) × Redis. (Survey 1)	− 0.064
	(0.113)
Treatment (Info) × Pr. (Gap)	− 0.248
	(0.393)
Treatment (Info) × Round	− 0.131
	(0.468)
Treatment (Info) × Duration (Survey 1)	0.058
	(0.048)
Treatment (Info) × Male	0.794
	(0.501)
Treatment (Info) × White	− 0.560
	(0.636)
Treatment (Info) × Univ.-educated parent	0.047
	(0.514)
Treatment (Info) × Full-time employment	− 0.992^{+}
	(0.559)

Table 18 (continued)

	Dependent variable: Participation, follow-up survey
Treatment (Info) × Children	− 0.074
	(0.207)
Treatment (Info) × Income	0.044
	(0.079)
Treatment (Info) × University degree	0.074
	(0.514)
Treatment (Info) × Age	− 0.005
	(0.024)
Constant	− 2.019*
	(1.010)
Observations	364
Log Likelihood	− 225.394
Akaike Inf. Crit.	498.787

Binary dependent variable indicating participation in follow-up survey. Estimation based on generalized linear model with logit link function. ($^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$)

observables. While the absence of such a bias for observables can increase our confidence in the unbiasedness of results, it is no guarantee (Tables 17, 18).

The most basic source of attrition bias is differential response rates across experimental conditions. Furthermore, it would be worrisome if attrition patterns based on covariates differed between control and treatment group. In order to assess these sources of bias, I run separate regressions for both experimental groups. I begin with an intercept-only model and continue with univariate regressions for the main sociodemographic covariates elicited in the initial survey. Results are shown in Table 17. The response rates of both groups are not exactly the same, 40.7% for the control group and 38.7% for the treatment group. However, as the right-most columns show, the difference is not statistically significant.

There is some evidence, especially in the control group, that respondents who are male, retired, and/or older are more likely to drop out. However, in no case is this pattern significantly different between both groups. There is also no evidence that respondents who took more time for the first survey are more likely to drop out. Finally, it is possible to check for attrition based on the two central variables in this study, respondents' prior perceptions of income gaps and preferences for redistribution. As these variables are constitutive of the causal mechanism explored here, attrition bias would be detrimental. Fortunately, there is no indication of any bias with regard to either variable.

As discussed above (see The Income Gaps Experiment), one approach to address imbalances due to attrition is inverse probability weighting. These weights are based on each respondent's probability to participate in the follow-up survey. Table 18 presents the model based on which these probabilities are estimated.

References

Alesina, A., & La Ferrara, E. (2005). Preferences for redistribution in the land of opportunities. *Journal of Public Economics*, *89*(5–6), 897–931. https://doi.org/10.1016/j.jpubeco.2004.05.009.

Alesina, A., Stantcheva, S., & Teso, E. (2018). Intergenerational mobility and preferences for redistribution. *American Economic Review*, *108*(2), 521–554. https://doi.org/10.1257/aer.20162015.

Alt, J., & Iversen, T. (2016). Inequality, labor market segmentation, and preferences for redistribution. *American Journal of Political Science*,. https://doi.org/10.1111/ajps.12264.

Auspurg, K., Hinz, T., & Sauer, C. (2017). Why should women get less? Evidence on the gender pay gap from multifactorial survey experiments. *American Sociological Review*, *82*(1), 179–210. https://doi.org/10.1177/0003122416683393.

Barabas, J., & Jerit, J. (2010). Are survey experiments externally valid? *American Political Science Review*, *104*(02), 226–242. https://doi.org/10.1017/S0003055410000092.

Bartels, L. M. (2002). Beyond the running tally: Partisan bias in political perceptions. *Political Behavior*, *24*(2), 117–150. https://doi.org/10.1023/A:1021226224601.

Batricevic, N., & Littvay, L. (2017). A genetic basis of economic egalitarianism. *Social Justice Research*, *30*(4), 408–437. https://doi.org/10.1007/s11211-017-0297-y.

Berinsky, A. J., Huber, G. A., & Lenz, G. S. (2012). Evaluating online labor markets for experimental research: Amazon.com's mechanical turk. *Political Analysis*, *20*(3), 351–368. https://doi.org/10.1093/pan/mpr057.

Blau, F. D., & Kahn, L. M. (2017). The gender wage gap: Extent, trends, and explanations. *Journal of Economic Literature*, *55*(3), 789–865. https://doi.org/10.1257/jel.20160995.

Blinder, A. S. (1973). Wage discrimination: Reduced form and structural estimates. *The Journal of Human Resources*, *8*(4), 436–455. https://doi.org/10.2307/144855.

Clifford, S., Jewell, R. M., & Waggoner, P. D. (2015). Are samples drawn from mechanical turk valid for research on political ideology? *Research & Politics*,. https://doi.org/10.1177/2053168015622072.

Cruces, G., Truglia, R. P., & Tetaz, M. (2012). Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment. *Journal of Public Economics*. Retrieved 11 13, 2013, from http://www.sciencedirect.com/science/article/pii/S004727271200117X.

Dimick, M., Rueda, D., & Stegmueller, D. (2017). The Altruistic Rich? Inequality and other-regarding preferences for redistribution. *Quarterly Journal of Political Science*, *11*(4), 385–439.

Evans, M., & Kelley, J. (2004). Subjective social location: Data from 21 nations. *International Journal of Public Opinion Research*, *16*(1), 3–38. https://doi.org/10.1093/ijpor/16.1.3.

Fernández-Albertos, J., & Kuo, A. (2015). Income perception, information, and progressive taxation: Evidence from a survey experiment. *Political Science Research and Methods*,. https://doi.org/10.1017/psrm.2015.73.

Finseraas, H. (2009). Income inequality and demand for redistribution: A multilevel analysis of European public opinion. *Scandinavian Political Studies*, *32*(1), 94–119. https://doi.org/10.1111/j.1467-9477.2008.00211.x.

Finseraas, H. (2012). Poverty, ethnic minorities among the poor, and preferences for redistribution in European regions. *Journal of European Social Policy*,. https://doi.org/10.1177/0958928711433655.

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

Gaines, B. J., Kuklinski, J. H., & Quirk, P. J. (2007). The logic of the survey experiment reexamined. *Political Analysis*, *15*(01), 1–20. https://doi.org/10.1093/pan/mpl008.

García-Sánchez, E., Willis, G. B., Rodríguez-Bailón, R., Palacio Sañudo, J., David Polo, J., & Rentería Pérez, E. (2018). Perceptions of economic inequality and support for redistribution: The role of existential and utopian standards. *Social Justice Research*,. https://doi.org/10.1007/s11211-018-0317-6.

Gerber, A., & Green, D. (1999). Misperceptions about perceptual bias. *Annual Review of Political Science*, *2*(1), 189–210. https://doi.org/10.1146/annurev.polisci.2.1.189.

Gerber, A., & Green, D. (2012). *Field Experiments: Design, Analysis, and Interpretation*. New York: W. W. Norton & Company.

Gilens, M. (2000). *Why Americans Hate Welfare: Race, Media, and the Politics of Antipoverty Policy*. Chicago: University of Chicago Press.

Griffiths, T. L., Kemp, C., & Tenenbaum, J. B. (2008). Bayesian models of cognition. In R. Sun (Ed.), *The Cambridge Handbook of Computational Psychology* (pp. 59–100). Cambridge: Cambridge University Press.

Hegtvedt, K. A., & Isom, D. (2015). Inequality: A matter of justice? In *Handbook of the social psychology of inequality* (pp. 65–94). New York: Springer.

Hopkins, D. J., Sides, J., & Citrin, J. (2018). The muted consequences of correct information about immigration. *The Journal of Politics, 81*(1), 315–320. https://doi.org/10.1086/699914.

Iversen, T., & Soskice, D. (2015). Information, inequality and mass polarization: Ideology in advanced democracies. *Comparative Political Studies*, *48*(13), 1781–1813. https://doi.org/10.1177/0010414015592643.

Jaime-Castillo, A. M., & Marqués-Perales, I. (2014). Beliefs about social fluidity and preferences for social policies. *Journal of Social Policy*, *43*(03), 615–633. https://doi.org/10.1017/S0047279414000221.

Jost, J. T., Pelham, B. W., Sheldon, O., & Ni Sullivan, B. (2003). Social inequality and the reduction of ideological dissonance on behalf of the system: Evidence of enhanced system justification among the disadvantaged. *European Journal of Social Psychology*, *33*(1), 13–36. https://doi.org/10.1002/ejsp.127.

Karadja, M., Mollerstrom, J., & Seim, D. (2016). Richer (and Holier) than thou? The effect of relative income improvements on demand for redistribution. *The Review of Economics and Statistics*, *99*(2), 201–212. https://doi.org/10.1162/REST_a_00623.

Katz, M. B. (2013). *The Undeserving Poor: America's Enduring Confrontation with Poverty: Fully Updated and Revised*. USA: Oxford University Press.

Kugler, M. B., Cooper, J., & Nosek, B. A. (2010). Group-based dominance and opposition to equality correspond to different psychological motives. *Social Justice Research*, *23*(2), 117–155. https://doi.org/10.1007/s11211-010-0112-5.

Kuklinski, J. H., Quirk, P. J., Jerit, J., Schwieder, D., & Rich, R. F. (2000). Misinformation and the currency of democratic citizenship. *Journal of Politics*, *62*(3), 790–816.

Kuziemko, I., Norton, M. I., Saez, E., & Stantcheva, S. (2015). How elastic are preferences for redistribution? Evidence from randomized survey experiments. *American Economic Review*, *105*(4), 1478–1508. https://doi.org/10.1257/aer.20130360.

Lenz, G. S. (2009). Learning and opinion change, not priming: Reconsidering the priming hypothesis. *American Journal of Political Science*, *53*(4), 821–837. https://doi.org/10.2307/20647953.

Likki, T., & Staerklé, C. (2015). Welfare support in Europe: Interplay of dependency culture beliefs and meritocratic contexts. *International Journal of Public Opinion Research*, *27*(1), 138–153. https://doi.org/10.1093/ijpor/edu014.

Linos, K., & West, M. (2003). Self-interest, social beliefs, and attitudes to redistribution. Re-addressing the issue of cross-national variation. *European Sociological Review*, *19*(4), 393–409. https://doi.org/10.1093/esr/19.4.393.

Loveless, M., & Whitefield, S. (2011). Being unequal and seeing inequality: Explaining the political significance of social inequality in new market democracies. *European Journal of Political Research*, *50*(2), 239–266. https://doi.org/10.1111/j.1475-6765.2010.01929.x.

McCall, L. (2013). *The Undeserving Rich: American Beliefs about Inequality, Opportunity, and Redistribution*. Cambridge: Cambridge University Press.

McCall, L., & Kenworthy, L. (2009). Americans' social policy preferences in the era of rising inequality. *Perspectives on Politics*, *7*(03), 459–484. https://doi.org/10.1017/S1537592709990818.

McDermott, R. (2002). Experimental methodology in political science. *Political Analysis*, *10*(4), 325–342.

Meltzer, A. H., & Richard, S. F. (1981). A rational theory of the size of government. *The Journal of Political Economy*, *89*(5), 914–927.

Miron, A. M., Warner, R. H., & Branscombe, N. R. (2011). Accounting for group differences in appraisals of social inequality: Differential injustice standards. *British Journal of Social Psychology*, *50*(2), 342–353. https://doi.org/10.1111/j.2044-8309.2010.02009.x.

Mullinix, K. J., Leeper, T. J., Druckman, J. N., & Freese, J. (2015). The generalizability of survey experiments. *Journal of Experimental Political Science*, *2*(02), 109–138. https://doi.org/10.1017/XPS.2015.19.

Mérola, V., & Hitt, M. P. (2016). Numeracy and the persuasive effect of policy information and party cues. *Public Opinion Quarterly*, *80*(2), 554–562. https://doi.org/10.1093/poq/nfv051.

Newman, B. J. (2014). My poor friend: Financial distress in one's social network, the perceived power of the rich, and support for redistribution. *The Journal of Politics*, *76*(1), 126–138. https://doi.org/10.1017/S0022381613001138.

Norton, M. I., & Ariely, D. (2011). Building a better America—One wealth quintile at a time. *Perspectives on Psychological Science*, *6*(1), 9–12. https://doi.org/10.1177/1745691610393524.

Nyhan, B., & Reifler, J. (2010). When corrections fail: The persistence of political misperceptions. *Political Behavior*, *32*(2), 303–330. https://doi.org/10.1007/s11109-010-9112-2.

O'Grady, T. (2017). How do economic circumstances determine preferences? Evidence from long-run panel data. *British Journal of Political Science.*,. https://doi.org/10.1017/S0007123417000242.

Osberg, L., & Smeeding, T. (2006). "Fair" inequality? Attitudes toward pay differentials: The United States in comparative perspective. *American Sociological Review*, *71*(3), 450–473. https://doi.org/10.1177/000312240607100305.

Pettigrew, T. F. (1998). Intergroup contact theory. *Annual Review of Psychology*, *49*(1), 65–85.

Roemer, J. E. (1998). *Equality of Opportunity*. Cambridge: Harvard University Press.

Romer, T. (1975). Individual welfare, majority voting, and the properties of a linear income tax. *Journal of Public Economics*, *4*(2), 163–185. https://doi.org/10.1016/0047-2727(75)90016-X.

Rueda, D., Stegmueller, D., & Idema, T. (2014). *The Effects of Income Expectations on Redistribution Preferences in Western Europe*. Washington, DC: American Political Science Association.

Schmidt-Catran, A. W. (2016). Economic inequality and public demand for redistribution: Combining cross-sectional and longitudinal evidence. *Socio-Economic Review*, *14*(1), 119–140. https://doi.org/10.1093/ser/mwu030.

SchrÖder, M. (2017). Is income inequality related to tolerance for inequality? *Social Justice Research*, *30*(1), 23–47. https://doi.org/10.1007/s11211-016-0276-8.

Semyonov, M., Raijman, R., & Gorodzeisky, A. (2006). The rise of anti-foreigner sentiment in European societies, 1988–2000. *American Sociological Review*, *71*(3), 426–449. https://doi.org/10.1177/000312240607100304.

Stewart, N., Ungemach, C., Harris, A. J., Bartels, D. M., Newell, B. R., Paolacci, G., et al. (2015). The average laboratory samples a population of 7300 Amazon mechanical turk workers. *Judgment and Decision Making*, *10*(5), 479.

Taber, C. S., & Lodge, M. (2006). Motivated skepticism in the evaluation of political beliefs. *American Journal of Political Science*, *50*(3), 755–769. https://doi.org/10.1111/j.1540-5907.2006.00214.x.

Trump, K.-S. (2017). Income inequality influences perceptions of legitimate income differences. *British Journal of Political Science*,. https://doi.org/10.1017/S0007123416000326.

Weatherford, M. S. (1983). Evaluating economic policy: A contextual model of the opinion formation process. *The Journal of Politics*, *45*(4), 866–888. https://doi.org/10.2307/2130416.