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YOUR PLACE IN THE WORLD:
THE DEMAND FOR NATIONAL AND GLOBAL REDISTRIBUTION

Dietmar Fehr
Johanna Mollerstrom
Ricardo Perez-Truglia

Working Paper 26555
<http://www.nber.org/papers/w26555>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2019, Revised March 2020

We are thankful for comments from several colleagues and seminar discussants. We would like to thank Jose Felipe Montano-Campos for superb research assistance. We are grateful to Bettina Zweck (Kantar Public Germany), David Richter (DIW Berlin), and Carsten Schroeder (DIW Berlin) for their support in implementing the project. This project received financial support from the German Research Foundation (DFG) through individual grant FE 1452/3-1 (Fehr) and from the German Institute for Economic Research (DIW Berlin, Mollerstrom). The authors declare that they have no relevant or material financial interests that relate to the research described in this paper. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 26555
December 2019, Revised March 2020
JEL No. C83,C91,D63,D83,D91,H23

ABSTRACT

Some of today's most heated policy debates about Brexit, trade wars, climate change abatement, and migration involve redistribution of resources within a given country (national redistribution) and between countries (global redistribution). Nevertheless, theories and evidence on preferences for redistribution have focused almost exclusively on national redistribution. In this paper, we study preferences for global redistribution. The workhorse model in political economy predicts that individuals with incomes in the upper levels of the national income distribution are less supportive of national redistribution than those with incomes in the lower levels. Applied to the global arena, the model predicts that individuals who are richer in the global income distribution will be less supportive of global redistribution. We test this hypothesis using a two-year, face-to-face survey of a representative sample of German households. We show that respondents are misinformed about their positions in the national and global income distributions, and we provide novel evidence that those misperceptions are meaningful. Consistent with previous studies, we find support for the political economy model in the national arena: the correlational and experimental estimates indicate that the demand for national redistribution decreases with national relative income. However, the political economy model does not hold in the global arena: support for global redistribution does not depend on global relative income.

Dietmar Fehr
University of Heidelberg
Bergheimer Str 58
Heidelberg 69115
Germany
dietmar.fehr@awi.uni-heidelberg.de

Ricardo Perez-Truglia
Anderson School of Management
University of California, Los Angeles
110 Westwood Plaza
Los Angeles, CA 90095
and NBER
ricardo.truglia@anderson.ucla.edu

Johanna Mollerstrom
Vernon Smith Hall 5028
George Mason University
3434 Washington Blvd
Arlington, VA 22201
jmollers@gmu.edu

A data appendix is available at <http://www.nber.org/data-appendix/w26555>

1 Introduction

In recent years, discussions have intensified around the issue of how economic resources should be distributed. One reason for this is that inequality in many Western democracies has become more pronounced (Piketty, 2014; OECD, 2015; Alvaredo et al., 2018). However, what constitutes “too much” inequality differs widely between individuals and countries. This has led to different interpretations about (and implementations of) the appropriate role and size of the public sector and the welfare state (see e.g. Alesina and Glaeser, 2004).¹

The debate around redistribution has focused on how to allocate resources between individuals from a given country. This emphasis may not be surprising, as there are multiple institutions and policy levers to redistribute resources domestically. By contrast, at the global level, comparable institutions and policies are scarce.² Nonetheless, the differences between the world’s poorest and most affluent citizens are staggering, and awareness about these differences is increasing as information flows more freely across the globe (OECD, 2015; Milanovic, 2015, 2016). At the same time, many of the most pressing policy issues, such as Brexit, trade wars, climate change abatement, and migration, involve redistribution of resources across countries. Despite abundant research about the demand for national redistribution, little is known about preferences for global redistribution and support for institutions mandated to implement such redistribution.

In this paper, we use evidence from a survey experiment to take a first step toward understanding the drivers of preferences for global redistribution. There is a large research agenda aimed at understanding how preferences for national redistribution are shaped at the individual level. Seminal theoretical papers, such as Romer (1975) and Meltzer and Richard (1981), predict that preferences for redistribution will be a decreasing function of one’s relative income. In these models, richer individuals are less excited about redistribution for selfish reasons, as they expect to contribute more than they receive. Consistent with this basic prediction, other studies have documented a negative correlation between survey measures of preferences for redistribution and individual relative income (see e.g., Fong, 2001; Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Mollerstrom and Seim, 2014). However, individual perceptions about relative income often differ from the actual distribution. As a result, preferences for redistribution tend to be determined not so much by whether individuals are rich or poor, but whether they perceive themselves to be rich or poor (Cruces et al., 2013; Karadja et al., 2017).³ Applying the logic of a

¹Experimental and observational research document that people, in general, do not approve of situations where there is “too much” inequality, but they also do not prefer resources to be completely equally distributed (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Norton and Ariely, 2011).

²There are exceptions, however. For example, in a recent referendum in 2019 in Zurich, Switzerland, about 70 percent of voters supported an initiative to increase funds for alleviating global poverty up to one percent of the city’s tax revenue in a given year (ea-foundation.org/media/).

³The empirical work also highlights how other factors beyond one’s (perceived) relative position in society affect how much redistribution one desires. In particular, an individual’s perception about the respective role of controllable and uncontrollable factors (e.g., effort and luck) in determining individual economic success is an important determinant of that individual’s redistributive preferences. Those who believe in the importance of effort

relative income model to global income redistribution predicts that individuals who believe their households are relatively richer in the global income distribution will be less supportive of global redistribution.

We designed and conducted a survey experiment to test this prediction using the German Socio-Economic Panel (SOEP), a longitudinal study of German households that started in 1984. This panel contains an innovation sample (SOEP-IS) that is representative of the German population. The panel offers opportunities to implement tailor-made survey modules as well as incentivized measures and experiments. The SOEP-IS is administered face-to-face by trained interviewers who visit respondents in their homes each year. Our research design exploits some unique advantages that the SOEP-IS has over other survey modes (e.g. phone and online surveys). For example, due to its infrastructure, the SOEP can survey the same respondents again a year later, with little attrition. The SOEP also can survey all household members, and guarantee that each member completes the survey independently without communicating with other members. Moreover, the face-to-face interviews minimize the risk of non-response to specific survey items, as well as misunderstandings that may arise about experimental tasks. It also prevents respondents from using the Internet to look up information while they are completing the survey.

In the baseline survey, we elicited respondents' perceptions about their household's position in the national and global income distributions. To encourage participant attention, we rewarded respondents for accurate responses. Comparing their prior perceptions to our own best estimates, we quantified the degree of their misperceptions. We then introduced an information-provision experiment to create exogenous variations in perceptions. This allowed us to measure the causal effect of these perceptions on demand for redistribution. Individuals were randomly assigned to a control group that received no information or to a treatment group that received easy-to-digest information about their true positions in both the national and global income distributions. Providing information about both national and global relative income allowed us to disentangle the effect of national-versus-global relative income. The reason is that if individuals learn that they are richer, on a global scale, than they previously thought, they may infer from that information that they are also richer than they thought on the national scale, and vice-versa. Measuring and providing information about both national and global relative incomes help us avoid this problem. Later, we elicited two main outcomes of interest: preferences for national redistribution, and preferences for global redistribution.

The information-provision experiment allows us to measure the causal effect of perceptions about relative income on these outcomes. For example, take a pair of individuals who underestimated their global relative incomes by ten percentage points. The individual who was not assigned to information should remain biased, and the individual who was assigned to the information should reduce or perhaps even eliminate her bias. The information-provision thus creates a positive shock

generally have low demand for redistribution (e.g., Alesina and Giuliano, 2011; Mollerstrom and Seim, 2014; Alesina et al., 2018b; Gärtner et al., 2019).

to the individual's perceived global relative income. We use a simple econometric model that allows us to aggregate across pairs of individuals who start out with different biases to disentangle the effects of global-versus-national relative income. The main hypothesis is straightforward: individuals who learn that they are higher (lower) in the global income distribution than they originally thought should become less (more) supportive of global income redistribution. On the other hand, individuals who learn that they are higher (lower) in the national income distribution than they originally thought, should become less (more) supportive of national redistribution.

We also collected data on secondary outcomes: support for the formation of a global organization in charge of redistributing income across countries, support for generous immigration policies, and support for globalization. We predict that these outcomes may be perceived as forms of global redistribution and thus may be affected by perceptions about relative global income. Moreover, we included two incentivized measures to validate the questions on preferences for redistribution: a decision to share money with a household at the bottom of the national income distribution and a decision to share money with a household at the bottom of the global income distribution. One year after the baseline survey, we conducted a follow-up survey. Among other things, we re-elicited respondents' perceptions about their relative incomes, again incentivized for accuracy. This approach allowed us to measure the long-term effect of information on perceptions. The follow-up survey also provided additional measurements, such as respondents' willingness to pay for information about their global and relative incomes, using standard incentive-compatible methods (Becker, DeGroot, and Marschak, 1964).

The first set of findings covers respondents' significant misperceptions about their relative positions in the national and global income distributions. In our representative sample of Germans, the absolute size of misperceptions about national and global relative positions are similar, with a mean absolute error of 23 percentage points for both. Nevertheless, there are some notable differences in the distribution of global and national misperceptions. Respondents are on average correct about their national relative positions, with approximately an equal number of respondents over- and under-estimating their positions. However, they significantly underestimate their positions in the global income distribution.

Using various unique features of our research design, we provide evidence that the above misperceptions are robust and meaningful, as opposed to merely reflecting measurement error, inattention, and disinterest. We show that these misperceptions persist despite significant rewards for guessing correctly. The misperceptions are consistent across time and across different members of the same household. Providing information to individuals affects on their perceptions a year later, implying that individuals truly incorporate the information. Moreover, giving information to one household member not only affects that same household member a year later; it also affects other household members. This evidence suggests that individuals care enough about the information to share it voluntarily with family members. Finally, using an information-acquisition assessment, we find that individuals are willing to pay non-trivial amounts for information about their global

and national income ranks.

The second set of results cover preferences for redistribution at the global and national levels. We find similar variation in preferences for global redistribution as in the preferences for national redistribution. These two preferences are significantly correlated to one another. Moreover, many of the usual correlates of national redistribution preferences (e.g., political orientation, luck beliefs, effort beliefs) have similar associations with global redistribution preferences. We also show that these unincentivized survey measures are significantly correlated to behavior in simple distributional tasks with real stakes.

Our next results relate to the relationship between relative income and redistribution preferences. The raw data show that the perceived national relative income is negatively correlated to demand for national and global redistribution. Consistent with prior work, we find strong heterogeneity by ideological orientation (Cruces et al., 2013; Kuziemko et al., 2015; Karadja et al., 2017; Fernandez-Albertos and Kuo, 2018; Alesina et al., 2018b). Specifically, these correlations are driven almost entirely by left-of-center individuals, who comprise approximately one-third of the sample. By contrast, the global relative income is not correlated to preferences for global or national redistribution.

Using the information-provision experiment to investigate these relationships further corroborates the correlational findings, both qualitatively and quantitatively. Information about national relative income affects demand for national and global redistribution in the predicted direction, but only for left-of-center respondents. By contrast, information about global relative income seems to be irrelevant for both national and global redistribution preferences. Thus, we find support for the political economy model in the national arena but no support for the political economy model in the global arena.

Our study relates to various strands of literature. First, it is related to the literature on the role of misperceptions in preferences for redistribution. Evidence indicates that individuals tend to misperceive their relative incomes and that correcting those misperceptions tends to influence preferences for redistribution (Cruces et al., 2013; Karadja et al., 2017). Other research documents misperceptions about other features relevant for income redistribution, such as wealth inequality (Norton and Ariely, 2011; Kuziemko et al., 2015), income mobility (Alesina et al., 2018b; Fehr et al., 2019; Gärtner et al., 2019), and immigration (Alesina et al., 2018a; Haaland and Roth, 2019) and show that correcting those misperceptions can change redistributive preferences.

We contribute to this literature in two ways. Our first contribution relies on expanding the literature on national redistribution to include global income redistribution. Our second contribution relates to the nature of misperceptions about relative income. Misperceptions about national relative income have been documented in several studies and contexts (Cruces et al., 2013; Karadja et al., 2017; Poppitz, 2016; Bublitz, 2017; Fernandez-Albertos and Kuo, 2018); misperceptions have also been documented about other factors relevant for redistributive preferences such as income inequality, and income mobility. Still, questions remain about the interpretation of these misper-

ceptions. For example, a significant fraction of survey respondents' misperceptions may be due to their lack of attention to the survey, confusion about what the survey question is trying to elicit, or reactions to information due to anchoring or experimenter-demand effects. Our survey experiment can address those questions in several ways. We show that individuals have substantial misperceptions even when facing significant monetary incentives to guess correctly; that individuals who receive information retain it twelve months later; that information is endogenously shared with other household members; that individuals are willing to pay non-trivial amounts to acquire information about relative income; and that misperceptions are consistent across time and across members of the same household.

Second, this paper relates to international aid and migration research in political science, as well as in sociology and economics. Some literature on international aid argues that it is driven primarily by strategic considerations of the giving nation rather than need in the recipient country (see e.g., Alesina and Dollar, 2000; Kuziemko and Werker, 2006; Dreher et al., 2009). However, there is growing interest in questions regarding public opinion about foreign aid (Kinder and Kam, 2010; Bauhr et al., 2013; Milner and Tingley, 2013; Eichenauer et al., 2018; Nair, 2018). The work of Nair (2018) is probably most related to our paper, as it explores the link between global relative income and support for foreign aid. Immigration seems to be another powerful force for redistribution of income across countries (see e.g., Lucas, 2005; Weyl, 2018). The importance of migration as a redistributive tool is on the rise, and migration streams are expected to continue to increase (OECD, 2014, 2017). Voters and policy makers in most Western democracies and throughout the world struggle with questions about how many migrants to welcome and how to facilitate their integration. This paper contributes to this literature by measuring how preferences for both national and global redistribution relate to attitudes and opinions on immigration.

The paper continues as follows. Section 2 outlines our research design and describes our data. Section 3 documents our results on misperception of national and global income, and Section 4 discusses our results on the demand for both national and global redistribution. Section 5 concludes.

2 Survey Design and Implementation

We collected data in cooperation with the German Socio-Economic Panel (SOEP) and made use of their Innovation Sample (SOEP-IS). The SOEP-IS is a longitudinal study that surveys a representative sample of the German population on a wide range of topics once a year.⁴ All household members over the age of 16 are assessed in computer-assisted face-to-face interviews performed by trained professionals (for more detailed information on the SOEP-IS, see Richter and Schupp, 2015).

⁴The SOEP-IS draws on the same pool of questions as the SOEP and makes use of the same professional survey company (see Goebel et al., 2018, for more details).

We designed two tailor-made survey modules, including a randomized information treatment, and incentivized belief and outcome measures. The modules were implemented in two consecutive waves of the SOEP-IS: a baseline survey in 2017 and a follow-up survey in 2018 (see the Online Appendix for our survey modules). Interviews were conducted in private with each member of a household (i.e., there was no communication possible between household members during and between the interviews). An interview with a household member lasted for about 45-60 minutes, out of which our modules comprised on average 8-10 minutes.

2.1 Survey Design: Baseline

The baseline survey has the following structure: i) pre-treatment questions; ii) assessment of perceived position in the income distribution; iii) randomized treatment providing truthful and accurate information about location in the income distribution; iv) primary outcome measures on preferences for redistribution; and v) secondary outcome measures on globalization and immigration. The outcome measures on preferences for redistribution (in part iv) were assessed both in the national (i.e. German) context, and in the global context. Consequently, we asked respondents in (ii) to state their perceived location in both the national and global income distributions. We randomized whether respondents saw the national or the global question first. To ease presentation and comprehension, the randomization was done across respondents, in the sense that a person who saw the national level question first in (ii) would see information about the national level first in (iii) (if randomly selected to the treatment group) and would be asked the question about national redistribution first in part (iv).

The pre-treatment part (i) included two questions about how respondents perceive the role of luck and effort in economic success in the national and global context (luck/effort beliefs). These beliefs are typically strong predictors of individual demand for redistribution at the national level (see e.g. Piketty, 1995; Alesina and Angeletos, 2005; Benabou and Tirole, 2006 for seminal theoretical work, and Fong, 2001; Mollerstrom and Seim, 2014; Karadja et al., 2017; Gärtner et al., 2019 for empirical evidence). We also use these two questions as a falsification test, as we should not find treatment effects on a variable that was measured before the information treatment.

We placed our module after the questions about political attitudes that are routinely included in the SOEP-IS. The reason is that prior research has found that individual views about redistribution are subject to strong heterogeneity in political orientation. Karadja et al. (2017), for instance, document that individuals to the left and to the right of center on the political spectrum react differently to information about relative income. Even though they do not directly report left-right heterogeneity, the findings by Cruces et al. (2013) and Fernandez-Albertos and Kuo (2018) indicate that informing people of their true economic placement affects support for redistribution only from those who learn they are poor (who tend to be left-wing). This substantial heterogeneity by left-right spectrum goes beyond information-provision experiments on relative income. For

example, Kuziemko et al. (2015) find that providing information about inequality only affects individuals with sufficiently high levels of trust in the government (and these respondents tend to overwhelmingly place themselves to the left of the political spectrum), and Alesina et al. (2018b) find that only left-wing respondents increase support for redistribution in reaction to pessimistic information on social mobility. Further, Alesina et al. (2018a) document that people on the right of center react significantly stronger and change their opinions on immigration more when information about numbers of immigrants, their education level, etc., is provided. Consequently, we use the information on political orientation for analyzing heterogeneity in response to our treatment.

Estimates of the global income distribution predominantly rely on per-capita pre-tax household income (see e.g., Milanovic, 2015, 2016). Therefore, before asking respondents for their perceptions of their relative national and global income in part (ii) of the survey module, we highlighted their absolute, per-capita pre-tax household income. We then asked them to state their position in the national (German) and global income distributions. Both relative income questions were incentivized for accuracy, and respondents were informed that they would receive 20 Euro for each assessment that was correct to the closest percentile (ensuring that it was optimal for them to answer in a way that elicited the true mode of their beliefs).

After stating the perceived location in the national and global income distribution, respondents answered several questions unrelated to our research (these questions were, among other things, related to the respondents' civil status, their siblings, and their children, and did not vary by treatment). Subsequently, our module continued. In part (iii) we randomized half of the respondents into a treatment which provided them with information about their true location in the national and global income distributions. The information revealed how many people are poorer at the national and global levels, based on their stated pre-tax per-capita household income, and additionally visualized this information using customized graphs to make it easier to understand and digest. See Figure 1 for a sample of the information treatment (this one corresponds to the national relative income, but the structure of the information about global relative income was identical). The other half of respondents received no information. Then, in part (iv), we asked both groups for their views on national and global redistribution, respectively. The answers to these questions were given on 1-10 scales with 1 indicating no demand for redistribution and 10 indicating a desire for complete redistribution that equalizes post-redistribution income between citizens.

In part (v), we complemented these outcome measures with two incentivized assessments of altruism. To this end, we used two simple distribution tasks with real stakes in a national and a global context, respectively. More precisely, respondents were asked to: a) distribute 50 Euro between themselves and a poor German household; and b) distribute another 50 Euro between themselves and a poor global household. German households were drawn from the bottom ten percent of the income distribution of SOEP-IS households that are not in our sample.⁵

⁵The SOEP-IS consists of several independent samples that are each representative of the German population.

To facilitate transfers to a poor global household, we used GiveDirectly, a well-established non-profit charity that provides cash transfers to poor households in Kenya and Uganda, and whose eligibility criteria ensures that recipient households belong to the bottom ten percent of the global income distribution (Haushofer and Shapiro, 2016). We randomly selected one in seven respondents and implemented their distribution decision in one randomly selected task (i.e. either the national or the global distribution decision). The money that a respondent allocated to herself was given to her immediately after completing the survey, while national recipient households received their transfers (the exact amount given by the respondent) with a cover letter explaining the transfer after the data collection for this SOEP-IS wave was completed. Finally, part (v) contained questions about whether the respondent supports globalization, and if she would appreciate Germany implementing a generous immigration policy that would allow more people from poor countries to live and work in Germany. We also asked whether the respondent would support an international institution with a mandate to implement global redistribution. The answers to these questions were given on a 1-10 scale, with 1 (10) indicating no (full) support for globalization, a generous immigration policy, and an international, redistributive institution.

2.2 Survey Design: Follow-Up

We designed a follow-up survey that we implemented in the same sample of respondents a year later. The purpose of this survey is to test whether the information provided to the survey participants persisted a year later, as well as to measure how much they value information on income ranks.

We began by highlighting information about the respondents' absolute per-capita pre-tax household income, and again asked them to state their rank in the national and global income distributions. We rewarded each accurate prediction with ten Euro. Additionally, we asked respondents about how certain they were about each of their answers, on a 0-10 point-scale (emulating steps of ten percent: being completely uncertain, ten percent certain, 20 percent certain, ..., 100 percent certain). This time, however, we did not provide information on the true rank in either context. Instead, after answering several SOEP-IS questions unrelated to our research (again among the respondents' civil status, siblings, children etc.), all participants answered the same questions about demand for redistribution, globalization, and immigration as in the baseline survey.

In a next step we elicited respondents' willingness to pay (WTP) for information about their true rank in the national and the global income distributions. To do so, we used a list-price version of the Becker-DeGroot-Marschak method (see e.g., Andersen et al., 2006). The list presents, separately for each income distribution, five scenarios in which respondents must choose between receiving information about their true rank in the corresponding income distribution, or receiving monetary compensation. The amount of money was predetermined and ranged from 0.1 Euro in Scenario 1 to ten Euro in Scenario 10, in increasing increments (0.1, 1, 2.5, 5, and 10 Euro). We informed respondents that one of the overall ten scenarios would be randomly selected and

implemented.⁶ To avoid respondents paying for this information for strategic reasons, we took care to assure respondents that we would not ask any more incentivized questions about their relative income rank, either later in the survey, or in later waves of the survey.

2.3 Survey Implementation

We implemented our two survey modules in the 2017 and 2018 waves of the SOEP-IS, which ran from September through December in each case. A total of 1,392 respondents took part in the baseline survey, while 1,167 participated in the follow-up survey (84 percent of the 1,392 respondents in the baseline survey).

In Table 1, we show that the treatment and control groups are balanced on observable characteristics. Since we will consider heterogeneity by left-of-center and center/right individuals, the table also shows the balance within each of those groups. In general, we see in Table 1 that the observable characteristics are balanced over our samples. For one pre-treatment characteristic, age, we see a small and marginally significant difference. Yet, this is well below what we would expect when running a total of 36 tests. Nevertheless, to be conservative, this characteristic is included in the set of control variables in the regression analysis (in addition to age and sex of the respondent, the vector of control variables also include a set of indicator variables for education and the respondent’s political affiliation, an indicator for whether the respondent is disabled, retired, and unemployed as well as an indicator for East Germany). One potential concern with using data from the follow-up survey as outcome measures is that the treatment may have affected the decision to participate in the follow-up survey. This is not a significant concern for two reasons. First, attrition is not high, as about 84 percent of the participants in the baseline survey participated in the follow-up survey one year later.⁷

Second, and most importantly, there is no significant difference in the attrition rates between individuals who were in the control group (15 percent attrition), and individuals who were in the treatment group in the baseline survey (17 percent attrition, $p=0.247$ for t-test of proportions). In Appendix C.5, we provide further evidence that attrition was random.

3 Results: Misperceptions

In this section, we describe respondents’ misperceptions about their relative income positions in the national and global contexts, and provide novel evidence that those misperceptions are meaningful.

⁶The instructions for the elicitation procedure, which we adapted from the elicitation task employed in Fuster et al. (2019), were tested for understanding with cognitive interviews.

⁷Attrition in our follow-up survey is substantially lower than in other studies, which typically also consider shorter time spans between surveys: for example, 80 percent of first-time respondents participate in the second survey of Karadja et al. (2017) three months later, while 78 percent of participants completed the first survey and only 14 percent responded to a follow-up one month later in Kuziemko et al. (2015).

3.1 Global vs. National Misperceptions

Figure 2 outlines the distribution of perceptions vs. reality in our sample. It shows substantial discrepancies, both for national (panel a) and global relative income (panel b). Figure 3.a shows the histograms of misperceptions: i.e., the difference between prior beliefs and reality.⁸ Here, a positive (negative) number indicates that the individual respondent over-estimates (under-estimates) her own rank. For example, 0.3 means that the respondent believes that she is 30 percentage points higher on the relative income scale than she actually is, and a -0.1 would indicate that the respondent’s relative income position is in fact ten percentage points lower than she believes.⁹

A visual inspection of Figure 3.a indicates a much smaller average bias for national than for global rank, and it is indeed the case that the average bias for national rank is close to zero ($M=-0.01$, $SD=0.29$). Moreover, there are roughly the same number of people over-estimating their national rank as under-estimating it. This is not true for global rank: respondents under-estimate their relative position in the global income distribution by an average of 15 percentage points ($SD=0.26$, $p<0.01$ for a paired t-test of differences in means). Despite these different average errors in national and global relative income perceptions, we observe quite pronounced individual biases that are similar in magnitude at the national and global levels. We compare the accuracy of national and global relative income perceptions using the mean absolute error, which is very similar for national and global beliefs (23 percentage points in both cases). In other words, at the individual level, Germans are as (in)accurate about their national income rank as they are about their global income rank.

The distribution of misperceptions about national relative income is consistent with a middle-class bias that would be expected under assortativity neglect (i.e., that the poor overestimate their position and the rich underestimate it, as in Cruces et al., 2013).¹⁰ On the other hand, the evidence for global misperceptions suggests a lack of such assortativity-neglect when Germans think about the global income distribution (see also Figure 4). Figure 3.b shows the relationship between the national and the global biases. They are significantly (albeit not perfectly) correlated with a correlation coefficient of 0.61 ($p<0.01$), implying that if a respondent over-estimates her position relative to other Germans chances are that she will also over-estimate how relatively rich she is globally. This, in turn, may imply that respondents are, to some extent, extrapolating their beliefs about their national relative position to the global arena.

We also test to what extent misperceptions of national and global relative income differ by

⁸In Appendix C.1, we also show the distribution of the gap between the information provided to the individuals and the prior beliefs.

⁹One potential concern is that part of the misperceptions may be due to the fact that individuals do not know their absolute, rather than relative, income. However, Appendix C.2 shows this is not a significant source of concern. Indeed, we provide suggestive evidence that respondents know their absolute income well, which is consistent with the results reported in Karadja et al. (2017) that self-reported income is highly correlated with administrative data.

¹⁰Frick et al. (2019) formalize how this assortativity neglect may arise more generally. Theoretically, a middle-class bias may also lead to more inequality, in particular, if the middle class can redistribute resources to themselves and are richer than the poor (Acemoglu et al., 2015).

subgroups. As discussed above, misperceptions about national income rank are highly correlated with misperceptions about global income rank. Moreover, the two types of biases have similar correlates (results presented in Appendix C.3).

3.2 The Misperceptions are Meaningful

Self-reported data on misperceptions come with certain challenges. One is that findings may simply reflect measurement error. For instance, some respondents may not be paying attention to the question, or they may have difficulty translating unconscious knowledge about their rank to a number. Another challenge is that respondents may be uninformed simply due to the fact that they do not care about the general topic that is under investigation. These concerns are not specific to relative income, but may be applicable to many types of misperceptions that have been reported, e.g. misperceptions about the inflation rate (Cavallo et al., 2017), housing prices (Fuster et al., 2019), cost of living (Bottan and Perez-Truglia, 2017), and income mobility (Alesina et al., 2018b; Fehr et al., 2019; Gärtner et al., 2019). In other words, the reason why survey data does not match well with reality may lie in the survey nature of the data.

In this section, we take advantage of our unique data and SOEP-specific features to address concerns of measurement error and lack of interest. We start by noting that misperceptions exist even though we provide significant rewards for the respondents to correctly state their national and global position in the relative income distribution. The incentives, at least to some extent, reduce the concerns about measurement error as we are giving people an incentive to pay attention, and to think harder than they would under non-incentivized conditions. In the remainder of this section, we provide additional evidence that the misperceptions are indeed meaningful.

3.2.1 Consistency Across Time and Household Members

The data from the follow-up survey help us to assess the consistency (or lack thereof) of misperceptions. If biases are pure measurement error, there should be no correlation between the bias in one wave of the survey and the next. On the other hand, if individuals are truly biased, their misperceptions should be correlated over time. Figure 5 shows that the persistence is significant: for national ranks, for each one percentage-point bias in the first wave, a respondent is biased in the same direction by 0.4 percentage points in the follow-up survey ($p < 0.01$). Results are similar in magnitude for global rank (correlation is 0.28, $p < 0.01$). The existence of such a persistence is even more remarkable given that there are some factors working against it – in particular, individuals are changing their absolute income over time, which often causes their true position to change as well (for details, see Appendix C.4).

We further document that misperceptions are quite consistent within households. If misperceptions are pure measurement errors, they should be independent across household members. If, on the other hand, they reflect real, meaningful biases, we should expect them to be correlated

across household members. We find that within-household variance is 41.8 percent for national rank and 58.2 percent for global rank. To put this in perspective, we use the standard SOEP-IS question about the number of household members as a natural benchmark. In principle, household members should have a high degree of agreement on this factual question. We observe that for the reported number of household members, 10.8 percent of variation is within-household. Despite this not being zero (which we would expect if all households were perfectly consistent), it is quite low.¹¹ This implies that household members are less consistent in their assessments of the household’s income rank than about how many members the household has. However, the variance is far from 100 percent, which is what we would expect if misperceptions were only measurement errors generated at the individual level. Thus, there is little reason to believe that misperceptions are measurement errors.

3.2.2 Persistence of Learning

Another way of testing whether the misperceptions are spurious or not, is by measuring the persistence of the information provided in the experiment (see e.g., Cavallo et al., 2017). If misperceptions are due to measurement error, anchoring, experimenter demand or lack of attention/interest, we would not expect the effects of providing information to be long-lasting. We explore this in a regression framework, using a specification from Karadja et al. (2017). Let T_i be an indicator variable indicating whether the individual was randomly selected into the treatment group and received relative-income information. Let $r_{i,nat}^{prior}$ denote the perceived national rank in the baseline survey (i.e., the prior belief, before receiving information), $r_{i,nat}^{signal}$ denotes the signal that could have been given (or not given, depending on whether the individual was in the treatment group or not) as feedback, and $r_{i,nat}^{t+1}$ denotes the perceived national rank in the follow-up survey.

$$r_{i,nat}^{t+1} = \alpha_{nat} \cdot (r_{i,nat}^{signal} - r_{i,nat}^{prior}) \cdot T_i + \beta_1 \cdot (r_{i,nat}^{signal} - r_{i,nat}^{prior}) + \varepsilon_i, \quad (1)$$

The coefficient α_{nat} tells us the rate of pass-through between the information given, and subsequent beliefs (for example, a coefficient of 0.1 would indicate that for each percentage point shock in information given, the posterior belief a year later is higher by 0.1 percentage points). Note that we should not expect a perfect pass-through rate (i.e., a rate of 1), because Bayesian individuals would, for example, take an average between the provided signal and their prior beliefs. Empirically, even when beliefs are re-elicited immediately (which is not the case here, but has been done in other work), the pass-through rate tends to be closer to 0.5, and falls significantly over a few months (see e.g., Cavallo et al., 2017; Bottan and Perez-Truglia, 2017; Fuster et al., 2019). Moreover, we expect a limited pass-through in this context as a respondent’s actual relative income

¹¹Note that such small inconsistencies may come from misreporting, e.g., a typo, or they may come from borderline cases where, for example, one spouse includes a child currently at college as a member of the household while the other spouse does not.

can change from one year to the other, so what she learned about her relative income one year ago may only be of limited help when she assesses her current income rank.

The results of estimating the pass-through rate are presented in Table 2. Column (1) suggests a pass-through coefficient of 0.14, i.e., for each percentage point that the treatment corrected a respondent’s misperception, a year later she reports beliefs that have moved 0.14 percentage points closer to accurate beliefs. This suggests that the respondents have at least some interest in the information – as they otherwise would not be likely to remember what they learned.

We provide several robustness checks. Column (5) presents the results from a falsification test where the dependent variable is the belief in the baseline survey (i.e., before they or the other household members actually received the information). We should expect no effect on this prior belief, which is also what we find: this fake rate of pass-through is close to zero (0.017), statistically insignificant and precisely estimated. Appendix C.5 provides an additional robustness check, using attrition to the follow-up survey as the dependent variable, to show that the findings are not driven by selective attrition.

As complementary evidence, we can also use data on the certainty of beliefs a year later. In the follow-up survey, we ask respondents to state how confident they are in their answers about their position in the income distributions. Figure 6.a shows that, on average, individuals are aware that they do not know their position in the income distributions well (only about four (eight) percent of respondents report to be 90 to 100 percent certain about their national (global) relative position assessment, respectively). Moreover, Figure 6.b shows the relationship between respondents’ confidence in their answer and their accuracy, and we see evidence of some self-awareness in particular in the case of global rank: here biases are significantly smaller among people who felt more certain, and whereas the bias is around 32 percentiles for those who are completely uncertain or only 10 percent sure, it is around 12 percentiles for those who report to be 90 or 100 percent sure.

Finally, if an individual truly learned from the information, we would expect her to feel more certain about her answer when assessing her income rank a year later. The results in Table 3, for national rank (column 1) and global rank (column 3) confirm this conjecture. The evidence suggests that receiving information about one’s true income rank increased belief certainty in national rank by 0.421 ($p < 0.01$) and in global rank by 0.583 ($p < 0.01$) in the follow-up one year later.

3.2.3 Information Diffusion within Households

Due to the fact that we randomized the information treatment at the individual level, sometimes an individual received information, while other members of the same household did not. We exploit this feature to measure intra-household information diffusion, and find that providing information to one respondent affects the beliefs of the other members of her household one year later. If

individuals take the time to discuss the information they receive with other household members, they presumably find it interesting or useful. We reiterate that any sharing of information among household members must take place after the baseline survey, as each interview was conducted in private and communication between household members was not allowed.

In this analysis, we extend specification (1):

$$r_{i,nat}^{t+1} = \alpha_{nat} \cdot (r_{i,nat}^{signal} - r_{i,nat}^{prior}) \cdot T_i + \alpha_{nat}^{peer} \cdot (r_{i,nat}^{signal} - r_{i,nat}^{prior}) \cdot T_i^{peer} + \beta_1 \cdot (r_{i,nat}^{signal} - r_{i,nat}^{prior}) + \beta_2 D_i + \varepsilon_i, \quad (2)$$

Here the variable T_i^{peer} takes the value 1 if the individual did not receive the information, but at least one other member of her household did. D_i represents a set of indicator variables for the number of respondents in the household. This is important to control for as a member of a larger household faces a higher probability that another household member will be randomly assigned to the treatment. In other words, assignment to the peer treatment is only random after conditioning on household size. The coefficient α_{nat}^{peer} tells us the rate of pass-through between the information we gave to a respondent's household peer(s) to her own beliefs one year later. The results are presented in column (2) of Table 2, and suggest that there is significant diffusion of information within households. The coefficient of 0.145 implies that for each percentage point shock in information given to another member of a respondent's household, her posterior belief a year later is higher by 0.145 percentage points. Moreover, accounting for this spillover of information is important for correctly understanding the long-term effects on beliefs: once we control for potential peer information, the pass-through of own information to own beliefs rises from 0.137 in column (1) to 0.179 in column (2). The comparisons between the pass-through for own information versus peer information suggests that 81 percent ($= 0.145 / 0.179$) of the information travels to other people in the household. This is a very high degree of information diffusion. We reproduce the analysis for the global rank in column (4). The rate of pass-through is somewhat lower (0.099), and we cannot reject the null hypothesis of no difference ($p=0.12$); however we also cannot reject that it is equal to the corresponding rate for national rank ($p=0.51$). The comparisons between the pass-through for own information versus peer information suggest that 67 percent ($=0.099/0.148$) of the information about global income rank makes its way to other members of the household.¹²

Columns (2) and (4) of Table 3 explore the effects of information diffusion to other members of the household on the certainty of beliefs a year later. If a respondent obtained information from another household member, we would expect her to feel more certain when answering the question about income rank a year later. The results are presented for national and global rank, in columns

¹²We can run the same falsification test as discussed above, where the dependent variable is the belief in the baseline survey (i.e., before they or the other household members actually received the information). These results are presented in columns (6) and (8) of Table 2 and are as expected.

(2) and (4), respectively. The evidence is mixed: The household peer treatment increased belief certainty in national rank by just 0.056 and this effect is statistically insignificant. However, given that this point estimate is not precisely estimated (the 95 percent confidence interval is between -0.38 and 0.49), we cannot rule out large positive effects. For global rank, the evidence is clearer: the household peer treatment increased own belief certainty by 0.513 ($p < 0.05$), which is not only statistically significant but also almost as large in magnitude as the effect of own treatment.

3.2.4 Demand for Information

A last piece of evidence that points to respondents caring about the topic and paying attention is that most of them are willing to pay to receive information about their relative income at the end of the follow-up survey. To analyze the demand for information, we can start by looking at whether the responses people gave are consistent across scenarios. Around five percent of respondents provided inconsistent responses in at least one of the two WTP questions.¹³ This level of consistency is in the upper end of the range of other studies using similar methods to elicit the WTP for information.¹⁴

The distribution of WTP, for the 95 percent of the respondents who provided consistent answers, is shown in Figure 7.a, which indicates a significant WTP for information. We estimate the mean WTP using an interval regression model and find that this is €6.17 (SD=0.26) for national rank and €6.12 (SD=0.27) for global rank.¹⁵ Figure 7.b shows the relationship between the WTP for national vs. global rank. On average, the WTP is almost identical, and the two are highly correlated. But the correlation is not perfect; that is, some respondents are more interested in acquiring information about their national than their global rank, and vice versa.

Given that the maximum WTP is €10, the average WTP seems fairly high, also taking into account that the information provided is in principle something respondents could find out online by themselves. In that sense, this WTP is giving a lower bound on how much respondents care about the information, as many who are interested in acquiring the information are probably deciding whether to pay for it in the survey, or to search for it on their own later. We can also compare the median WTP in our study with the results from other papers that elicit WTP for information using similar methods. We find that individuals value information on their national and global relative income rank more than they value, for example travel information (\$0.40, Khattak et al., 2003), food certification information (\$0.80, Angulo et al., 2005), home energy reports (\$3, Allcott

¹³For example, they chose five Euro instead of information, but then chose information instead of ten Euro. Those who reported inconsistent responses to one piece of information, e.g. national rank, were almost always inconsistent in the other piece of information, i.e. about global rank. This suggests these individuals were not paying attention or they had trouble understanding the instructions.

¹⁴For instance, the share of inconsistent respondents was about two percent in Allcott and Kessler (2019), five percent in Fuster et al. (2019), and 15 percent in Cullen and Perez-Truglia (2018).

¹⁵This model assumes that the latent WTP is normally distributed. The constant in this model can be interpreted as the mean WTP under the implicit assumption that WTP can take negative values; if instead we were to assume that the WTP must be non-negative, then the mean would be even higher.

and Kessler, 2019) and future national home prices (\$4.16, Fuster et al., 2019).¹⁶

Taken together, we have documented significant misperceptions not only about national but also about global relative income and demonstrated that they are not just statistically significant, but also meaningful and persistent.

4 Results: Preferences for Redistribution

4.1 Variation in Demand for Redistribution

We now turn to preferences for redistribution at the national and the global level. The two main outcome variables, demand for national and global redistribution measured in the baseline survey are presented in Figure 8. Panel a of Figure 8 reveals significant variation as to how much redistribution individuals want at both the national and the global level. Figure 8.b shows that even though the two preferences are correlated (correlation coefficient 0.70, $p < 0.01$), the correlation is not perfect: there are some respondents who want extensive national redistribution but very little global redistribution, and vice versa.

Table 4 investigates the correlates of preferences for national and global redistribution, using data from the baseline control group only (i.e., individuals who did not receive any feedback from us regarding their true relative income rank). The two left columns look at the extent to which our measure of national demand for redistribution is correlated with personal characteristics that have previously been shown to correlate with demand for redistribution (Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Mollerstrom and Seim, 2014; Karadja et al., 2017; Gärtner et al., 2017, 2019). In column (1) each covariate enters a bivariate regression with demand for redistribution as the dependent variable, whereas the model in column (2) has all covariates entering simultaneously. We partly confirm previous findings. For example, the demand for national redistribution is decreasing in income, and in the extent to which the respondent believes that effort (rather than luck) is the driver of economic success. Demand for national redistribution is also higher among those who define themselves as being to the left on the political spectrum. We fail to find support for some previously documented findings however. We see, for instance, no gender difference in the demand for national redistribution (in other work, women are generally found to demand more redistribution than men), nor a relation between age and the demand for redistribution. The two right columns in Table 4 display the results of the corresponding correlational analysis for demand for global redistribution. There are some differences compared to the correlates of demand for national redistribution. Most notably, there is no relation between a respondent's income and her demand for global redistribution. The fact that those to the left on the political spectrum want

¹⁶In contrast, the information about income rank is not as valuable as the information about peer salaries, reported in Cullen and Perez-Truglia (2018). That information, however, is not available online and is also potentially profitable from the perspective of career choice and salary negotiations.

more redistribution remains however, as does the correlation with luck/effort beliefs.

A challenge with these two outcome measures is that they are unincentivized self-reports. To test whether respondents put their money where their mouth is, we also conducted two distribution tasks where they could donate part of a real-stake endowment to a household which is poor in the national context (a German household at or below the tenth percentile of the national income distribution) or in the global context (a Kenyan household at or below the tenth percentile of the global income distribution). If we observe that respondents who, on average, report a higher demand for national redistribution also donate more to the German poor, and that respondents who have a higher demand for global redistribution donate more to the global poor, we can conclude that our main outcome measures are at least somewhat robust to incentives. Figure 9 documents that this is indeed the case - the correlation between the respective demand for redistribution measure (national and global), and the respective donation are highly statistically significant ($p < 0.01$ for both). More precisely, the estimated relationship reveals that going from the lowest to the highest demand for redistribution is associated with an increase in the share of giving to a poor German household of ten percentage points, whereas the increase in the share of giving to poor Kenyan household is 17 percentage points.¹⁷

We also included a number of secondary outcome variables aimed at better understanding demand for global redistribution. The first of these is most directly related to preferences for global redistribution and assesses to what extent the respondent supports the creation of an international institution mandated with the implementation of redistribution across countries. The other two secondary outcomes – support for globalization and for immigration of poor people to Germany – are indirectly related to global redistribution. Table 5 documents that demand for both national and global redistribution is positively correlated with support for immigration, with support for an international redistributive organization, and (to some extent) with support for globalization. As expected, people who believe that it is effort (rather than luck) that determine an individual’s economic success in the global arena are less supportive of immigration, of an international redistributive organization, and of globalization. Luck/effort beliefs at the national level, however, are uncorrelated with these opinions.¹⁸

¹⁷Alternatively, a one standard deviation increase in demand for national redistribution increases given to a poor German household by 2 percentage points and to a poor Kenyan household by 4 percentage points. We also find that, in general, left-of-center respondents share significantly more than center/right-of-center respondents.

¹⁸It is also interesting to note that respondents are in wide agreement that luck plays a more important role in generating individual global economic success than in generating individual national economic success. The average answer on the luck/effort scale is 6.41 (SD=1.68) for the national and 5.81 (SD=1.94) for the global context ($p < 0.01$). For more results on the correlates of the secondary outcome variables, see Table C.2 in the Online Appendix.

4.2 Relative Income Perceptions and the Demand for Redistribution

Before presenting the experimental results, we explore the raw correlations between respondents' relative income perceptions and their preferences for redistribution. The results are presented in Table 6, and again based only on individuals in the baseline control group. The first two columns display the results for the full control group. They suggest that perceived global rank is not related to demand for redistribution, either at the national or global level, while perceived national rank is related to demand for national, but not global redistribution.

Previous work has shown significant polarization along political orientation with respect to providing information on relative income and income inequality. To test for possible heterogeneous effects along this dimension, we use a SOEP-IS question on the self-assessment in political left-right spectrum and thus placed our survey module after this question. Columns (3) through (6) of Table 6 show the results from this analysis, by splitting the sample into left (respondents who picked from 0 to 4 on the 1-10 scale) and center/right (the rest). In line with the previous literature, we do find significant heterogeneity. While demand for both national and global redistribution are significantly correlated with a respondents' perceived national (but not global) income rank for those with political opinions to the left-of-center, neither correlation is significant for center/right respondents.

Next, we use our information experiment to investigate the causal relation between relative income and demand for redistribution. We use the following specification:

$$Y_i = \alpha_{nat} \cdot (r_{i,nat}^{signal} - r_{i,nat}^{prior}) \cdot T_i + \alpha_{int} \cdot (r_{i,int}^{signal} - r_{i,int}^{prior}) \cdot T_i + \beta_1 \cdot (r_{i,nat}^{signal} - r_{i,nat}^{prior}) + \beta_2 \cdot (r_{i,int}^{signal} - r_{i,int}^{prior}) + \beta_3 X_i + \varepsilon_i, \quad (3)$$

where T_i is the treatment indicator variable, indicating whether the individual was treated with information about her actual relative income, or not. The two key parameters are α_{nat} and α_{int} , where $\frac{\alpha_{nat}}{100}$ shows the causal effect of a respondent receiving information implying that her national rank is 1 pp. higher than she previously thought. Correspondingly, $\frac{\alpha_{int}}{100}$ shows the causal effect of a respondent being told that her global rank is 1 pp. higher than she believed it to be. The variables $r_{i,nat}^{signal} - r_{i,nat}^{prior}$ and $r_{i,int}^{signal} - r_{i,int}^{prior}$ control for the non-random variation in prior misperceptions: i.e., they guarantee that α_{nat} and α_{int} are identified by random variation in information provision. Last, X_i is a set of additional controls.¹⁹

There are two features of this specification worth mentioning. First, we use the center/right group for the sake of brevity: in Appendix C.6 we show that the results are similar in the two subgroups ("Center" and "Right"). Second, this regression specification is quite demanding in that

¹⁹In Appendix C.7, we present results for the average effects of receiving information (i.e., regardless of whether the feedback was above or below the prior belief).

it simultaneously includes in the regression two variables that are significantly correlated: perceptions of national and global relative income. In Appendix C.6 we present alternative specifications which only include perceptions of national *or* global ranks. The results from these less demanding specifications are not only robust, but also more precisely estimated.

The experimental results are presented in Table 7 and line up relatively well with the raw correlations in Table 6. The first two columns of Table 7 present the average treatment effects and indicate that preferences for redistribution (national and global) decrease with national relative income, but the magnitude is small and the coefficients are imprecisely estimated and statistically insignificant. The second set of columns shows that the effects are large and significant for the left-leaning respondents: communicating information to left-of-center respondents that their national income rank is 10 percentage points higher than they previously believed decreases their support for national redistribution by around 0.08 standard deviations, while the effects of national rank on global redistribution are slightly higher in magnitude (0.09 standard deviations). In contrast, there are no significant effects of information about global rank on the demand for global (or national) redistribution. The third set of columns shows a null-effect for center/right respondents: the point estimates are close to zero, precisely estimated and statistically insignificant.

Figure 10 provides a graphical representation of the effects of information on national rank using binned scatterplots. In the interest of maximizing power, and based on the results presented above, we use a specification that ignores the misperceptions about global income. Figure 10.a depicts this relation for the left-of-center respondents, and indicates that the results are not driven by outliers, and that assuming a linear relationship is reasonable. Figure 10.b shows center/right respondents, and confirms that the effects of the information intervention are robustly null across the entire distribution.

Next, we consider the effects on the secondary outcomes. These results are presented in Table 8. As before, we consider the effects on the full sample (panel a), for left-of-center respondents (panel b), and center/right respondents (panel c). In each panel, the first column reproduces, for comparison, the effect of information provision on the demand for global redistribution. Columns 2-4 in each panel present the results for the questions about support for an international, redistributive institution, support for immigration, and support for globalization, respectively. The outcome measure in the fifth column is a standardized index of all four measures of global redistribution, following the methodology of Kling et al. (2007).²⁰

Considering first the left-of-center sample shown in panel b), we see that receiving information that one has a higher relative income in Germany than previously believed, causally decreases support for a redistributive international institution (column 2). The coefficient for this outcome (-1.035, $p < 0.05$) is similar in magnitude and statistical significance as the coefficient on the main

²⁰In Appendix C.8, we present the effects on the behavior in the dictator games, which took place after the information provision stage and in Appendix C.9 we present the effects on the redistributive preferences elicited in the follow-up survey.

outcome on global redistribution (-0.929, $p < 0.05$). The point estimates for the support for globalization and immigration outcomes (-0.499 and -0.521 in columns (3) and (4), respectively) are also negative although somewhat smaller in magnitude than the other coefficients and statistically insignificant. The effects on the index (column 5) are similar in magnitude to the main outcome (-0.759) and highly statistically significant ($p\text{-value} < 0.01$). For the center/right sample (panel c), the effects are again precisely estimated nulls.

We present further robustness checks in Appendix C.6. First, we provide a falsification test of the information intervention: we use the two questions about the respondent's belief in the importance of effort versus luck for individual economic success both at the national the global level. Despite these variables being related to demand for redistribution, we expect no treatment effect as they were measured before the information-provision. The results confirm this expectation: the coefficients are close to zero, statistically insignificant and precisely estimated. Last, we provide a graphical analysis of the effects of information, using histograms to break down the treatments into positive, neutral, and negative news about the income ranks. This exercise confirms our results reported above.

5 Conclusions

This study presents evidence on the determinants of demand for national and global redistribution using a large-scale, longitudinal, survey experiment. We start by studying misperceptions about individual relative income, at both the national and global levels, in a representative sample of the German population. We document that the misperceptions are significant in magnitude. We also provide strong evidence suggesting that the misperceptions are meaningful. For example, we show that misperceptions persist despite significant rewards for guessing correctly. We show that misperceptions are consistent across time and across different household members. We also show that providing information to individuals affects the perceptions of those same individuals a year later, and affects the perceptions of other members of the individuals' household. We find that individuals are willing to pay non-trivial amounts for information about their global and national income ranks. Although the absolute sizes of the misperceptions are similar for national and global biases, their distributions are different: while Germans, on average, are correct about their national relative income rank, they tend to underestimate their global income rank by about 13 percentage points. Likewise, there is evidence of assortativity neglect at the national level but not at the global level.

We also investigate the demand for national and global redistribution. Using both correlational and experimental estimates, we find evidence that relative national income is a significant determinant of preferences for national redistribution. This finding aligns with the predictions of seminal political economy models (e.g., Meltzer-Richard), which predict that the relatively rich demand less redistribution than the relatively poor, as the rich receive fewer monetary benefits

from redistribution. We observe significant heterogeneity in this relation, however. For the political left, the relation between relative national income rank and demand for national redistribution is pronounced and robust, but for the political center and right it is not. This suggests that they are more open to redistribution as they are also more elastic to information as to whether they are richer or poorer than they thought.

Our findings indicate that a Meltzer-Richard or similar model should not be extrapolated and used to explain the demand for global redistribution: global relative income rank is neither correlated with nor causally related to demand for global redistribution. The fact that global relative income does not affect demand for global redistribution in our sample could be due to poor Germans failing to realize that more extensive global redistribution would redistribute their income to other parts of the world where people are even poorer. Another potential interpretation relies on reference group effects. For example, Germans may care about their relative income when compared to other Germans but not when compared to others around the globe. In line with the finding that demand for global redistribution is unrelated to global rank, we also document that demand is unrelated to support for immigration and globalization. In the global income distribution, Germans are affluent, yet we still observe significant support for global redistribution. Our findings thus indicate that demand for global redistribution in Germany may be higher than it would have been if the main determinant had been global relative income.

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Figure 1: Screenshot of a Sample of the Information Treatment

We would now like to give you information about the distribution of per-capita gross household income in Germany and worldwide. This information is based on representative and independently collected data from scientifically well-recognized institutions, such as the Panel Study "Living in Germany", the World Bank, and the Luxembourg Income Study Center.

In Germany, 50% of people are poorer than you, which means they have a lower per capita gross household income than you.

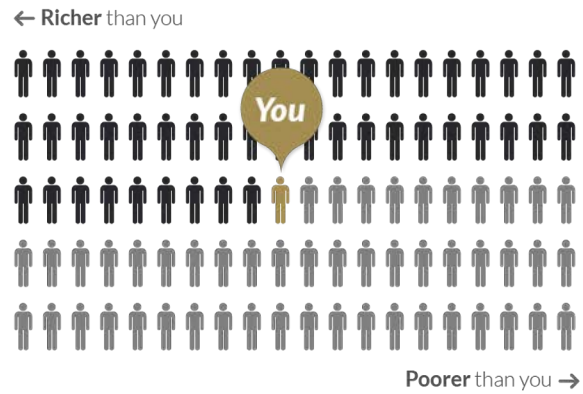
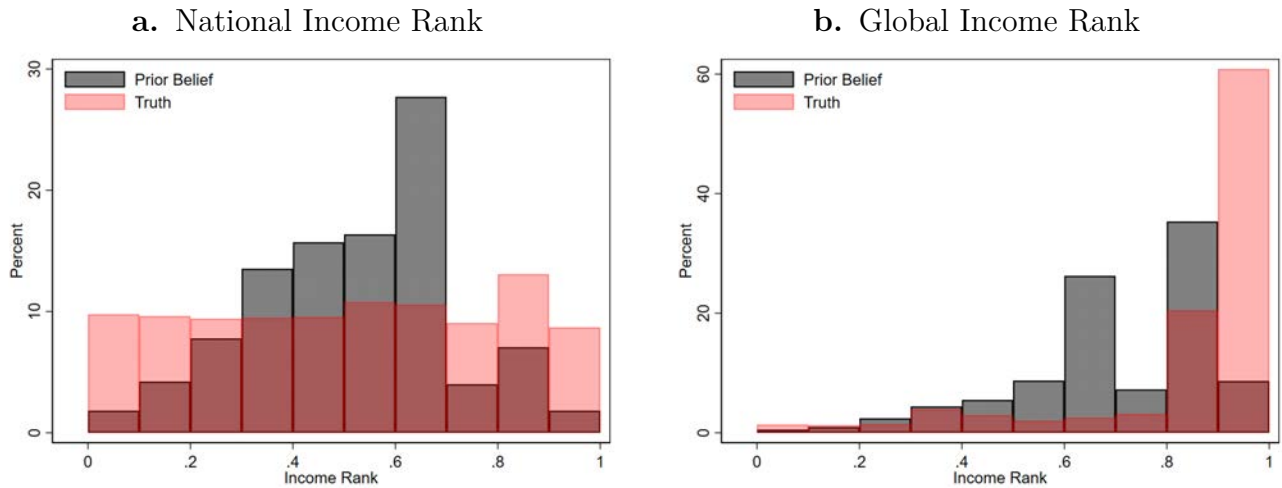
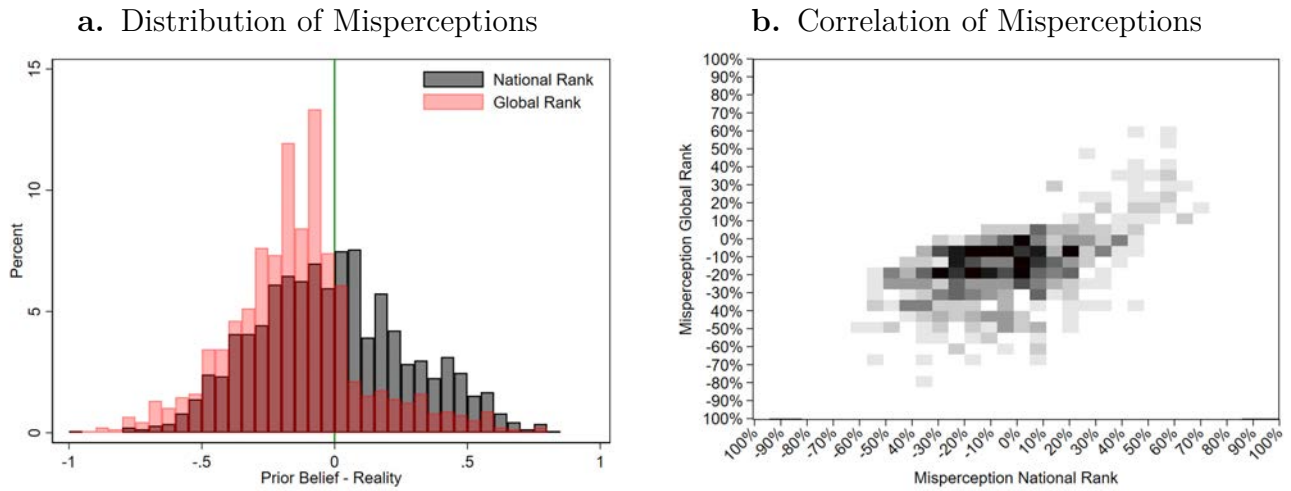


Figure 2: Prior Beliefs about Income Rank vs. True Income Rank



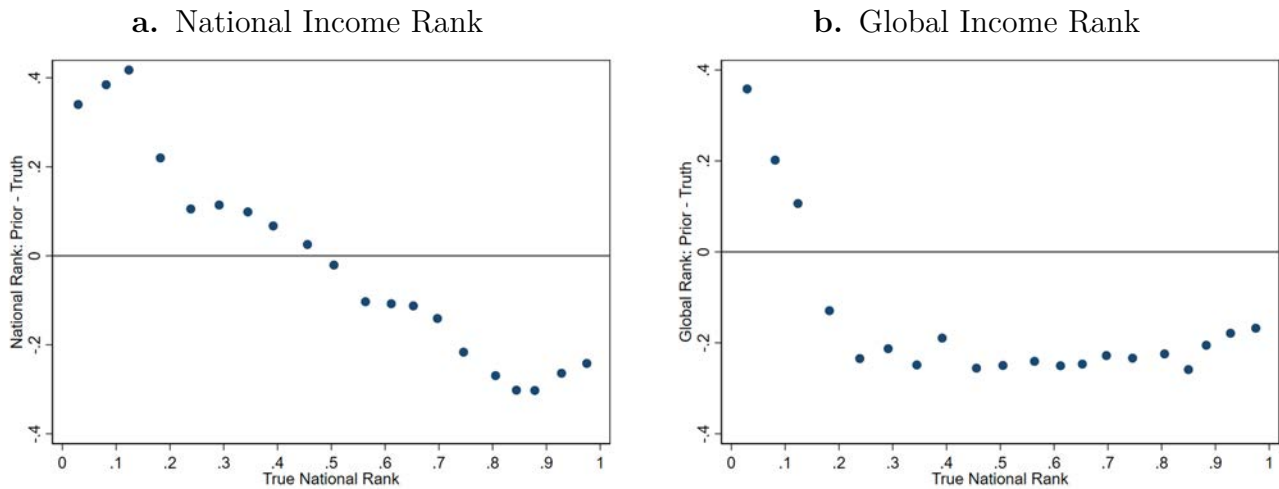
Notes: Distribution of prior beliefs about own income rank and the true income rank at the national level (panel a.) and global level (panel b.). Data from baseline survey.

Figure 3: Misperceptions



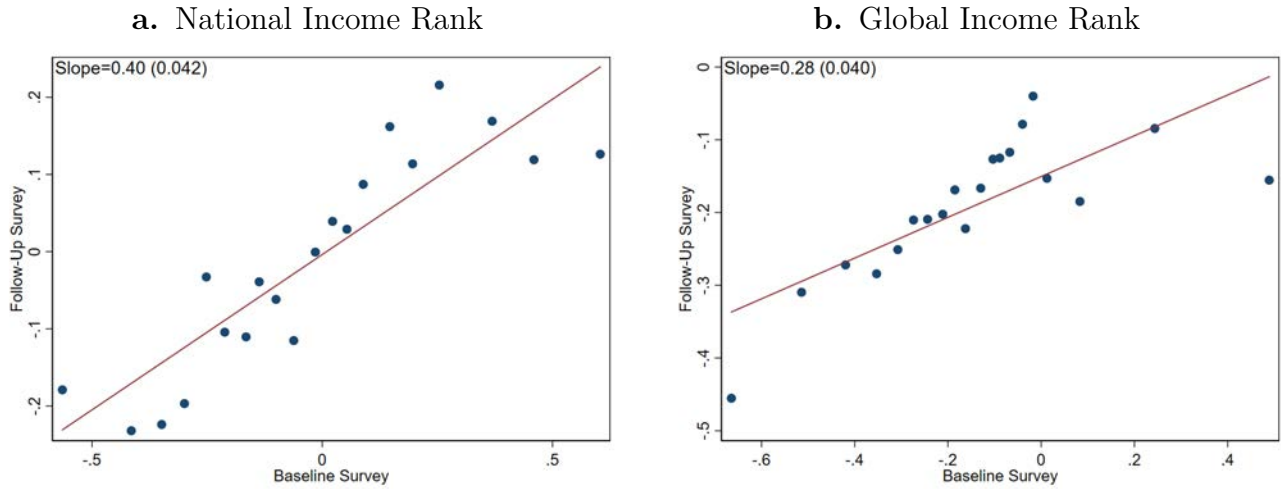
Notes: Distribution of misperceptions at the national and global level (panel a.) and their correlation (panel b.). Misperceptions are calculated as difference between prior beliefs about income rank and true income rank. Positive (negative) differences correspond to overestimation (underestimation) of own income rank. Data from baseline survey.

Figure 4: Middle-Class Bias



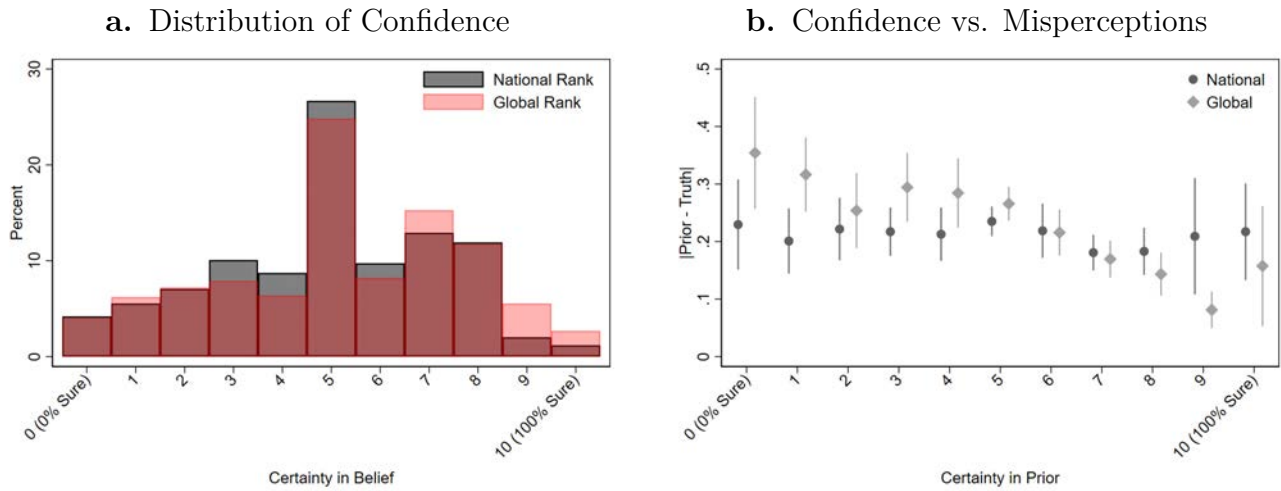
Notes: Binned scatterplots showing the relationship between true income rank and misperceptions at the national level (panel a.) and global level (panel b.). Data from baseline survey

Figure 5: Year-over-year Persistence of Misperceptions



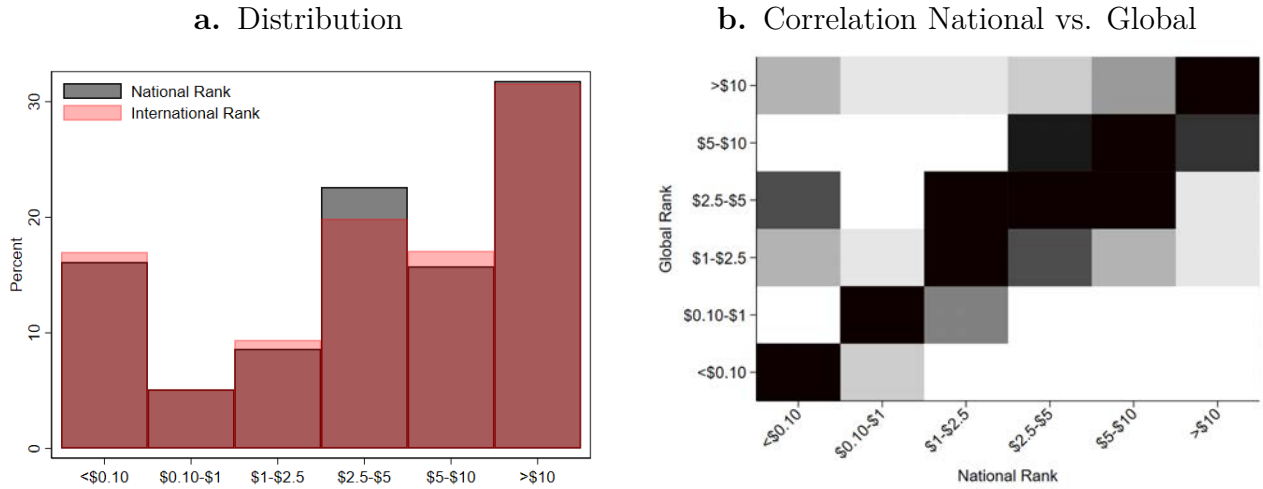
Notes: Binned scatterplots showing the persistence of misperceptions between the baseline and the follow-up survey (one year later) for national income rank (panel a.) and global income rank (panel b.). Estimates based on control groups in the baseline and follow-up survey only.

Figure 6: Confidence in Beliefs about Income Rank



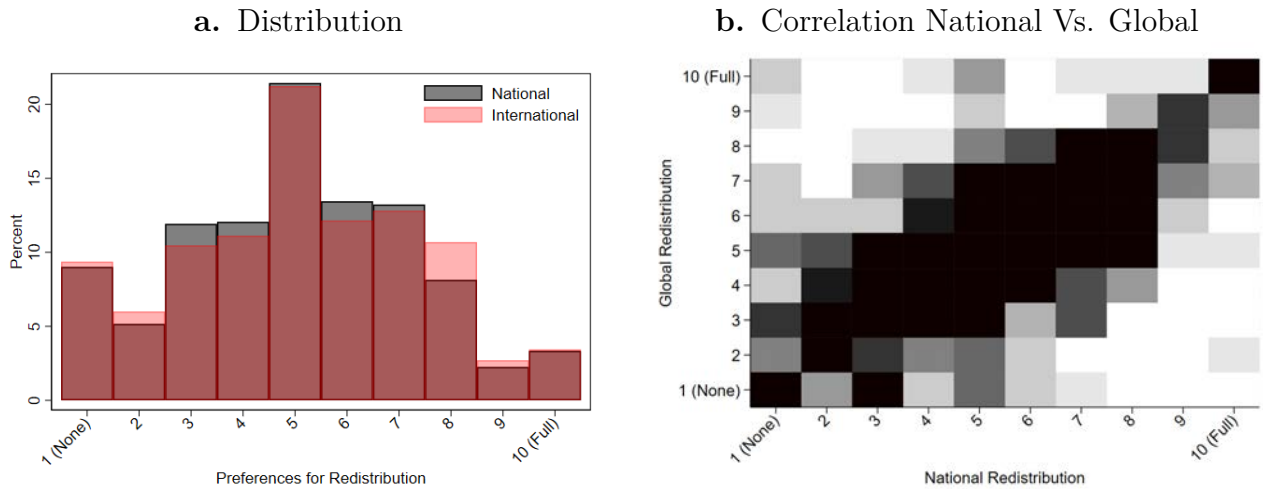
Notes: Distribution of reported confidence in beliefs about income rank in follow-up survey (panel a.) and relationship between confidence and misperceptions for both national and global income rank (panel b.). Estimates in panel b.) based on follow-up control group only.

Figure 7: WTP for Information



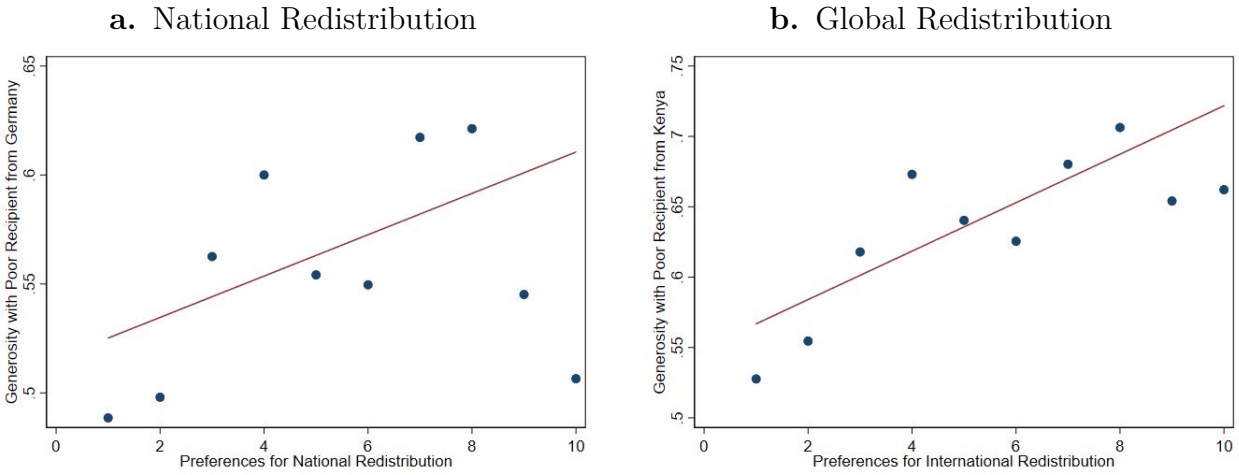
Notes: Distribution of willingness to pay (WTP) for information on national and global income rank (panel a.) and their correlation (panel b.) in the follow-up survey.

Figure 8: Preferences for Redistribution



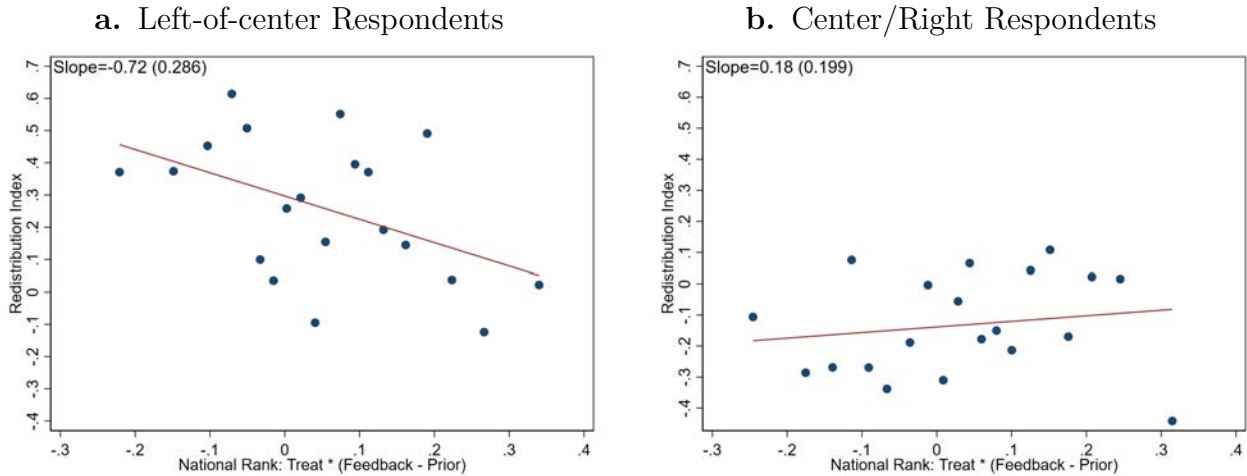
Notes: Distribution of preferences for national and global redistribution (panel a.) and their correlation (panel b.) in the baseline survey.

Figure 9: Preferences for Redistribution vs. Generosity in Distributional Tasks



Notes: Binned scatterplots showing the relationship between preference for national redistribution and share of giving to a poor German household (panel a.) and the relationship between preference for global redistribution and share of giving to a poor global household (panel b.). Data from baseline survey.

Figure 10: Effects of Information Provision on Demand for Redistribution



Notes: Binned scatterplots showing the effect of information on true income rank on demand for redistribution for left-of-center respondents (panel a.) and center/right respondents (panel b.) in the baseline survey. Demand for redistribution is a standardized index of national and global demand for redistribution. Analysis conditional on a set of standard controls.

Table 1: Randomization Balance

	All			Left-Of-Center			Center/Right-Wing		
	Treat (1)	Control (2)	P-value (3)	Treat (4)	Control (5)	P-value (6)	Treat (7)	Control (8)	P-value (9)
HH Income (EUR 1,000s)	43.64 (1.91)	43.54 (2.28)	0.97	52.40 (3.45)	45.61 (2.52)	0.12	39.09 (2.26)	42.54 (3.14)	0.37
No. of Household Members	2.34 (0.04)	2.28 (0.05)	0.35	2.37 (0.07)	2.35 (0.09)	0.90	2.32 (0.06)	2.24 (0.06)	0.31
Female (=1)	0.54 (0.02)	0.55 (0.02)	0.79	0.54 (0.03)	0.58 (0.03)	0.42	0.55 (0.02)	0.54 (0.02)	0.82
Age	54.58 (0.71)	56.44 (0.69)	0.06	52.67 (1.20)	55.51 (1.20)	0.09	55.58 (0.87)	56.88 (0.84)	0.28
Education: upper secondary	0.63 (0.02)	0.60 (0.02)	0.23	0.56 (0.03)	0.54 (0.03)	0.67	0.66 (0.02)	0.62 (0.02)	0.20
Education: college	0.22 (0.02)	0.23 (0.02)	0.61	0.33 (0.03)	0.31 (0.03)	0.70	0.16 (0.02)	0.19 (0.02)	0.24
Disabled (=1)	0.13 (0.01)	0.15 (0.01)	0.18	0.12 (0.02)	0.11 (0.02)	0.90	0.14 (0.02)	0.17 (0.02)	0.11
Unemployed (=1)	0.03 (0.01)	0.04 (0.01)	0.70	0.02 (0.01)	0.02 (0.01)	0.91	0.04 (0.01)	0.05 (0.01)	0.75
Retired (=1)	0.34 (0.02)	0.35 (0.02)	0.72	0.32 (0.03)	0.31 (0.03)	0.92	0.36 (0.02)	0.37 (0.02)	0.65
SPD Supporter (=1)	0.13 (0.01)	0.16 (0.01)	0.14	0.22 (0.03)	0.25 (0.03)	0.35	0.09 (0.01)	0.11 (0.01)	0.16
CDU/CSU Supporter (=1)	0.22 (0.02)	0.24 (0.02)	0.30	0.10 (0.02)	0.09 (0.02)	0.71	0.28 (0.02)	0.31 (0.02)	0.26
East Germany (=1)	0.23 (0.02)	0.23 (0.02)	0.99	0.25 (0.03)	0.27 (0.03)	0.76	0.22 (0.02)	0.22 (0.02)	0.86
Observations	705	687		241	222		464	465	

Notes: Averages for a standard set of observables, p-value is for test of difference for treatment and control.

Table 2: Effects of Information Provision on Beliefs One Year Later

	Beliefs in Follow-Up Survey				Beliefs in Baseline Survey			
	(1) National	(2) National	(3) Global	(4) Global	(5) National	(6) National	(7) Global	(8) Global
National Rank: Treat*(Feedback - Prior)	0.137*** (0.039)	0.179*** (0.044)			0.017 (0.025)	-0.001 (0.030)		
National Rank: Peer Treatment*(Feedback - Prior)		0.144** (0.057)				-0.059* (0.036)		
Global Rank: Treat*(Feedback - Prior)			0.114*** (0.043)	0.148*** (0.047)			-0.043 (0.027)	-0.036 (0.029)
Global Rank: Peer Treatment*(Feedback - Prior)				0.099 (0.063)				0.023 (0.041)
Observations	1,150	1,150	1,135	1,135	1,224	1,224	1,147	1,147

Notes: ***p < 0.01, **p < 0.05, *p < 0.1. OLS regressions estimating the effect of information provision on beliefs about income rank one year later (in the follow-up survey). Standard errors clustered at the household level in parentheses. Analysis conditional on number of household members, and household gross income.

Table 3: Effects of Information Provision on Belief Certainty One Year Later

	Certainty in Follow-Up Survey			
	(1) National	(2) National	(3) Global	(4) Global
Treatment	0.421*** (0.136)	0.441** (0.171)	0.583*** (0.146)	0.770*** (0.181)
Peer Treatment		0.056 (0.222)		0.513** (0.233)
Observations	1,152	1,152	1,138	1,138

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions estimating the effect of information provision on confidence of belief statements about income rank one year later (in the follow-up survey). Standard errors clustered at the household level in parentheses. Analysis conditional on number of household members, and household gross income.

Table 4: Correlates of Demand for Redistribution

	National Redistribution		Global redistribution	
	(1)	(2)	(3)	(4)
Age	-0.008*	-0.003	-0.016***	-0.010**
	(0.005)	(0.005)	(0.005)	(0.005)
Female	-0.047	0.007	-0.065	-0.081
	(0.173)	(0.174)	(0.179)	(0.182)
Education: upper secondary	-0.085	-0.142	-0.239	-0.402
	(0.177)	(0.254)	(0.185)	(0.270)
Education: college & more	-0.076	-0.287	-0.075	-0.638**
	(0.198)	(0.304)	(0.213)	(0.322)
Unemployed	0.456	0.053	0.522	0.571
	(0.453)	(0.440)	(0.423)	(0.428)
Monthly net income (log)	-0.459**	-0.658***	0.013	-0.152
	(0.197)	(0.218)	(0.199)	(0.216)
Political orientation: left-of-center	1.014***	1.016***	1.117***	1.073***
	(0.170)	(0.175)	(0.181)	(0.196)
East Germany	0.527***	0.262	-0.028	-0.155
	(0.203)	(0.217)	(0.208)	(0.232)
German citizenship	-0.697*	-0.466	-0.786**	-0.521
	(0.362)	(0.367)	(0.371)	(0.384)
Catholic	-0.063	-0.145	-0.293	-0.380
	(0.194)	(0.234)	(0.201)	(0.239)
Protesant	-0.418**	-0.421**	-0.104	-0.286
	(0.184)	(0.214)	(0.199)	(0.230)
Luck vs. Effort National	-0.174***	-0.086	-0.086	-0.014
	(0.054)	(0.063)	(0.056)	(0.071)
Luck vs. Effort Global	-0.174***	-0.114**	-0.121**	-0.071
	(0.047)	(0.058)	(0.050)	(0.065)
Risk aversion	-0.041	-0.037	-0.079*	-0.098**
	(0.038)	(0.039)	(0.041)	(0.042)
Observations		657		655

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions with robust standard errors in parentheses using data from the baseline control group. The dependent variable is national demand for redistribution (columns 1 and 2) and global demand for redistribution (columns 3 and 4). Uneven-numbered columns display coefficients from separate regressions for each covariate, while even-numbered columns report multivariate regressions including all covariates at once. The dependent variables are measured on a 0–10 scale with 0 indicating “no redistribution” and 10 indicating “full redistribution.” All covariates are defined as binary variables (except age, monthly net income, luck vs. effort, and risk aversion).

Table 5: Correlates of Support of International Organization, Immigration, and Globalization

	Support Int. Organization		Support Immigration		Support Globalization	
	(1)	(2)	(3)	(4)	(5)	(6)
Luck vs. Effort National	-0.100 (0.065)	-0.010 (0.060)	-0.031 (0.053)	0.081 (0.061)	0.030 (0.048)	0.101* (0.057)
Luck vs. Effort Global	-0.128** (0.055)	-0.019 (0.053)	-0.138*** (0.044)	-0.162*** (0.050)	-0.075* (0.040)	-0.120** (0.048)
National Redistribution	0.610*** (0.044)	0.177*** (0.056)	0.121*** (0.039)	-0.145*** (0.048)	0.042 (0.039)	-0.093* (0.054)
Global Redistribution	0.717*** (0.032)	0.598*** (0.049)	0.281*** (0.035)	0.378*** (0.044)	0.115*** (0.035)	0.172*** (0.050)
Observations	671		682		680	

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions with robust standard errors in parentheses using data from the baseline control group. The dependent variable is support for an international organization to implement global redistribution (columns 1 and 2), support for immigration from poor countries (columns 3 and 4), and support for globalization (columns 5 and 6). Uneven-numbered columns display coefficients from separate regressions for each covariate, while even-numbered columns report multivariate regressions including all covariates at once. All are measured on a 0–10 scale with 0 indicating “no support” and 10 indicating “full support.”

Table 6: Correlation between Prior Beliefs and Preferences for Redistribution

	All		Left-Of-Center		Center/Right	
	(1)	(2)	(3)	(4)	(5)	(6)
	National	Global	National	Global	National	Global
National Rank ⁽ⁱ⁾	-0.557** (0.245)	-0.190 (0.235)	-1.009*** (0.369)	-1.008*** (0.372)	-0.503 (0.311)	0.102 (0.300)
Global Rank ⁽ⁱⁱ⁾	0.085 (0.237)	0.127 (0.235)	0.573 (0.366)	0.643 (0.392)	-0.137 (0.283)	-0.094 (0.281)
P-value (i)=(ii)	0.137	0.446	0.017	0.013	0.484	0.706
Observations	683	679	235	235	448	444

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions with standard errors clustered at the household level in parentheses using data from the baseline control group. The dependent variables are the standardized national (National) and global demand for redistribution (Global). Columns 1–2 use data for the whole baseline control sample, columns 3–4 display results for left-of-center respondents and columns 5–6 display results for center/right respondents. Left-of-center is defined as below median on the self-assessment scale for political orientation from left to right, whereas center/right subsumes respondents at or above the median on this scale. P-value reports the result from a Wald test for equivalence of the two reported coefficients.

Table 7: Effects of Information Provision on Preferences for Redistribution

	All			Left-Of-Center			Center/Right		
	(1) National	(2) Global	(3) Index	(4) National	(5) Global	(6) Index	(7) National	(8) Global	(9) Index
National Rank: Treat*(Feedback - Prior)	-0.190 (0.250)	-0.230 (0.244)	-0.201 (0.225)	-0.777* (0.462)	-0.929** (0.392)	-0.857** (0.382)	0.078 (0.295)	0.102 (0.301)	0.108 (0.272)
Global Rank: Treat*(Feedback - Prior)	-0.001 (0.261)	0.114 (0.245)	0.038 (0.235)	0.190 (0.500)	0.150 (0.446)	0.171 (0.433)	0.002 (0.298)	0.174 (0.285)	0.061 (0.271)
Observations	1,350	1,341	1,351	454	452	454	896	889	897

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions with standard errors clustered at the household level in parentheses using data from the baseline survey. The dependent variables are the standardized national (National) and global demand for redistribution (Global) and an equally-weighted index of the two. Columns 1–2 use data for the whole sample, columns 3–4 display results for left-of-center respondents and columns 5–6 display results for center/right respondents. Left-of-center is defined as below median on the self-assessment scale for political orientation from left to right, whereas center/right subsumes respondents at or above the median on this scale. Analysis conditional on standard set of control (age, indicator variables for sex, education, disabled, unemployed, retired respondents, region (East Germany), and party affiliation of the respondent). P-value reports the result from a Wald test for equivalence of the two reported coefficients.

Table 8: Effects of Information Provision on Secondary Outcomes about Global Redistribution

All					
Panel a)	(1) Global Red.	(2) Supp. Global	(3) Immigration	(4) Globalization	(5) Index
National Rank: Treat*(Feedback - Prior)	-0.230 (0.244)	-0.307 (0.245)	-0.138 (0.242)	0.012 (0.266)	-0.194 (0.165)
Global Rank: Treat*(Feedback - Prior)	0.114 (0.245)	0.172 (0.241)	0.102 (0.240)	-0.243 (0.258)	0.087 (0.160)
P-value (i)=(ii)	0.440	0.280	0.585	0.599	0.344
Observations	1,341	1,325	1,358	1,345	1,364
Left-Of-Center					
Panel b)	(1) Global Red.	(2) Supp. Global	(3) Immigration	(4) Globalization	(5) Index
National Rank: Treat*(Feedback - Prior)	-0.929** (0.392)	-1.035** (0.460)	-0.499 (0.410)	-0.521 (0.473)	-0.759*** (0.273)
Global Rank: Treat*(Feedback - Prior)	0.150 (0.446)	0.540 (0.473)	0.701 (0.457)	-0.110 (0.512)	0.308 (0.321)
P-value (i)=(ii)	0.155	0.063	0.131	0.654	0.049
Observations	452	447	454	454	458
Center/Right					
Panel c)	(1) Global Red.	(2) Supp. Global	(3) Immigration	(4) Globalization	(5) Index
National Rank: Treat*(Feedback - Prior)	0.102 (0.301)	0.073 (0.288)	-0.006 (0.291)	0.269 (0.315)	0.087 (0.200)
Global Rank: Treat*(Feedback - Prior)	0.174 (0.285)	0.068 (0.277)	-0.031 (0.267)	-0.330 (0.282)	0.043 (0.176)
P-value (i)=(ii)	0.894	0.992	0.961	0.273	0.899
Observations	889	878	904	891	906

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. OLS regressions with standard errors clustered at the household level in parentheses using data from the baseline survey. The dependent variables are the standardized global demand for redistribution, support for an international organization for global redistribution, support for immigration from poor countries and support for globalization as well as an equally-weighted index of these four variables. Panel a) uses data for the whole sample, panel b) displays results for left-of-center respondents and panel c) displays results for center/right respondents. Left-of-center is defined as below median on the self-assessment scale for political orientation from left to right, whereas center/right subsumes respondents at or above the median on this scale. Analysis conditional on standard set of control (age, indicator variables for sex, education, disabled, unemployed, retired respondents, region (East Germany), and party affiliation of the respondent). P-value reports the result from a Wald test for equivalence of the two reported coefficients.