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YOUR PLACE IN THE WORLD: RELATIVE INCOME AND GLOBAL INEQUALITY

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ABSTRACT

Although there is abundant evidence on individual preferences for policies that reduce national inequality, there is very little evidence on preferences for policies addressing global inequality. To investigate the latter, we conduct a two-year, face-to-face survey experiment on a representative sample of Germans. We measure how individuals form perceptions of their ranks in the national and global income distributions, and how those perceptions relate to their national and global policy preferences. We find that Germans systematically underestimate their true place in the world's income distribution, but that correcting those misperceptions does not affect their support for policies related to global inequality.

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> Let us suppose that the great empire of China, with all its myriads of inhabitants, was suddenly swallowed up by an earthquake, and let us consider how a man of humanity in Europe [...] would be affected upon receiving intelligence of this dreadful calamity. He would, I imagine, first of all, express very strongly his sorrow for the misfortune of that unhappy people [...]. And when all this fine philosophy was over [...] he would pursue his business or his pleasure, take his repose or his diversion, with the same ease and tranquillity, as if no such accident had happened. If he was to lose his little finger tomorrow, he would not sleep tonight; but, provided he never saw them [...] the destruction of that immense multitude seems plainly an object less interesting to him, than this paltry misfortune of his own.
>
> Adam Smith, The Theory of Moral Sentiments

1 Introduction

As inequality in many Western democracies has become more pronounced (Piketty, 2014; OECD, 2015; Alvaredo et al., 2018b), the debate around income redistribution has intensified. In the academic literature, this debate has focused largely 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 – such as taxes and welfare programs – that serve to redistribute resources domestically. By contrast, comparable institutions and policies are scarce at the global level. Nevertheless, 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). As a result, institutions and tools for promoting global redistribution may become more important.[1] Moreover, there are many pressing policy issues that, even if not discussed expressly as tools for income redistribution, involve significant components of redistribution of resources across countries. Examples of such policy issues include pandemic response, trade wars, climate change abatement, and migration. For example, Weyl (2018) shows that migration from poor to rich countries has contributed to a large reduction in global inequality, while Milanovic (2016) points to a large reduction in global inequality due to globalization. In this paper, we take a first step toward studying individual preferences about policies that could help reduce global inequality.

To understand why some individuals support policies aimed at reducing global inequality

[1] There are also programs that redistribute across countries at the regional level, for example in the European Union (e.g., Becker et al., 2013), and we see an increasing focus on and demand for foreign aid programs in rich countries. A recent example is a referendum in Zurich, Switzerland, in which 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 (for more details see https://ea-foundation.org/files/prospectus-1-percent-initiative.pdf and https://tinyurl.com/yckz56v4).

and others do not, we conducted a two-year incentivized survey experiment in a representative sample of the German population. Following three different trains of thought in the economics literature, we focus on perceived relative income, i.e., the individual's perceived rank in the national *and* global income distributions. To the extent that individuals may misperceive their income ranks, those systematic misperceptions may translate into systematic biases in the support for policies addressing global inequality.

The first line of reasoning originates in the canonical models of income redistribution from political economy, such as Meltzer and Richard (1981) and Romer (1975). This class of models, when applied to the global arena, predict that an individual's attitudes towards policies like global redistribution should depend on their perceived rank in the global income distribution. Intuitively, these models assume that individuals are *purely selfish*; thus, people deciding whether to support redistribution primarily care about the effects of the policy on their own material well-being. As a result, we would expect individuals with a higher global income rank to be less supportive of such policies, at least to the extent that they would likely be net losers of global redistribution.[2]

A second perspective, originating in the behavioral economics literature, departs from the assumption that individuals are solely self-interested. A vast theoretical and experimental literature shows that people care not only about their own monetary outcomes, but also about the outcome of others and about fairness (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002). One robust finding from this literature is that individuals are often willing to sacrifice some of their own material well-being to help those who are less fortunate than them. In our context, these models suggest that individuals with higher global income ranks may feel more pressure to donate to the global poor. To the extent that other-regarding concerns motivate redistribution, these individuals should also favor global redistribution.

The third perspective is inspired by a literature on international trade. For example, following the logic of Stolper and Samuelson (1941), we would expect that globalization and immigration may affect individuals differently, depending on their position in the national income distribution. Due to the global abundance of low skill workers, low skill (and low income) workers from rich countries can be negatively affected by openness to trade.[3] To the extent that

[2]For instance, in Meltzer and Richard (1981), individuals with different market skills have to vote for an income tax rate. In equilibrium, individuals rationally anticipate the disincentive effects of taxation on the labor-leisure choices of their fellow citizens and take the effect into account when voting. When applied to the national arena (i.e., individuals from a given country voting for a domestic income tax), the model predicts that preferences for redistribution will be a decreasing function of an individual's relative skill (and thus relative income). We can easily transfer this model to the global arena by assuming that the individuals are voting for a global income tax: the corresponding prediction predicts that individuals who are higher up in the global income distribution should be less supportive of global income redistribution.

[3]In its original form, the Stolper-Samuelson effect provides insights on the distributional effects of international trade within a given country and predicts that in a two goods and two production factor world the one factor that faces more competitive pressure from trade liberalization and globalization must end up worse off compared to others in the same country. Despite the rather restrictive assumptions of the original theorem and the scant em-

globalization entails more openness to trade, individuals at the bottom of the German income distribution may be less supportive of globalization.[4] Similarly, these individuals should be less supportive of immigration, given that immigrants are disproportionately low skilled.

We designed our survey experiment with three main goals. First, we aim to measure attitudes towards policies related to global inequality. Second, we aim to measure individuals' perceptions of their relative positions in the national and global income distributions, respectively. Third, we aim to study the correlational and causal effects of these relative income perceptions on policy preferences. We embedded our survey in the German Socio-Economic Panel (SOEP), a representative longitudinal study of German households. The SOEP contains an innovation sample (SOEP-IS) allowing researchers to implement tailor-made survey experiments. The surveys are administered by trained interviewers who visit respondents in their homes each year. This offers unique advantages over other survey modes (e.g., phone and online surveys), such as the ability to interview multiple household members in private and follow-up a year later with little attrition. The design of our survey takes advantage of this structure to investigate the extent to which misperceptions of relative income are robust and meaningful, or whether they primarily reflect disinterest from participants and other forms of measurement error.

Our survey elicited preferences over a range of policies related to national and global inequality. We elicited the demand for both national and global redistribution and respondents' support for creating of an international institution with a mandate to implement redistribution at the global scale. Given that immigration and globalization can have significant redistributive implications at the global scale, we asked two questions that elicited support for immigration and globalization, respectively. As some of these questions involve abstract concepts that can be difficult for respondents to think about, we took care to clearly define and explain all concepts involved such as "economic redistribution." Lastly, we measured willingness to donate money to the national poor and the global poor by asking respondents to: (i) split €50 between them and a German household at the bottom ten percent of the national income distribution; and (ii) split another €50 between them and a poor household, from Kenya or Uganda, at the bottom ten percent of the global income distribution.

Our survey also elicited respondents' perceptions about their household's position in the national and global income distributions. We used a number of measures to minimize the usual concerns with the measurement of misperceptions. For example, we offered significant rewards for accurate responses to encourage participant attention and honesty. Likewise, interviewers were present in person and could provide help in real time, minimizing the risk of

pirical support, the model has significantly contributed to the debate on the distributional effects of globalization (Goldberg and Pavcnik, 2007).

[4]This resonates well with the stagnating income growth of the lower middle class in rich countries (i.e., around the 80th percentile in the global income distribution), popularized in the "elephant graph" (Lakner and Milanovic, 2016; Milanovic, 2016, but see also Alvaredo et al., 2018a for a more nuanced picture using newer data). Similarly, evidence suggests that local US and German labor markets suffered the most the more they were exposed to trade from China (Autor et al., 2013, 2016; Dauth et al., 2014).

non-response to specific survey items or misunderstandings. We also took care to minimize any social desirability bias by requiring respondents to provide responses in private, without the interviewer being able to see the tablet screen. The survey mode also guaranteed that respondents could not use the Internet to look up information or speak to other household members while completing the survey (Grewenig et al., 2020).

To study how perceptions of relative income affect policy preferences causally, we created exogenous variation by implementing an information-provision experiment (Cruces et al., 2013; Karadja et al., 2017). After eliciting prior beliefs on relative income, but before eliciting policy preferences, we randomly assigned participants to either a control group receiving no information, or to a treatment group in which they received easy-to-digest information about their true position in both the national and global income distributions. The provision of information creates exogenous variation in perceptions that we can leverage to measure the causal effect of perceived income ranks. For example, take a group of individuals who underestimate their global relative incomes by ten percentage points. We would expect the individuals who are not assigned to information to continue underestimating their global relative income by ten percentage points, while individuals who are assigned to the information should adjust their perceptions upwards. The information provision thus creates a positive shock to the individual's perceived global relative income. We can then test, for example, whether respondents, in the spirit of Meltzer and Richard (1981), become less supportive of global redistribution upon learning that they are higher up in the global income distribution.

One year after the baseline survey, we conducted a follow-up survey that re-elicited respondents' perceptions about their relative incomes, again incentivized for accuracy, as well as their policy preferences. This approach allowed us to assess whether the information provided in the baseline survey had persistent effects a full year later. Moreover, the follow-up survey provides additional measurements. In particular, we conducted an information-acquisition task to measure 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 results documents preferences over policies related to global inequality. We find substantial variation across individuals in their preference for global redistribution, in their giving behavior and in their opinions on globalization and immigration. Preferences for global redistribution are significantly correlated to preferences for national redistribution. They also share many of the same correlates such as political orientation, and beliefs about the roles of effort and luck in economic success. Preferences for global redistribution are significantly and positively correlated to preferences for immigration and globalization, suggesting that support for those policies may respond to redistributive motives. In addition, preferences for global redistribution are significantly, albeit far from perfectly, correlated to behavior in the global giving task. This suggests that other-regarding preferences play an important role.

The second set of results measures misperceptions about relative positions in the national

and global income distributions and documents their meaningfulness. The absolute size of misperceptions about national and global relative positions are similar, with a mean absolute error of 23 percentage points for both. Both types of misperceptions are also similar in that they display a middle-class bias: German households who are rich by national standards tend to think that they are middle-class, while households who are rich by global standards tend to think that they are the global middle class. Nevertheless, there are some notable differences in the distribution of global and national misperceptions. On the one hand, respondents are, on average, correct about their national relative positions, with approximately an equal number of respondents overestimating and underestimating their positions. On the other hand, households are much more likely to underestimate their positions in the global income distribution than to overestimate it: Germans underestimate their place in the global income distribution by an average of 15 percentage points. This could be consequential: if all Germans were informed about their true place in the world's income distribution, that could increase, or decrease, their average support for global redistribution and related policies.

Some researchers have raised questions about the interpretation of the evidence on misperceptions. For example, a significant fraction of survey respondents' misperceptions may be due to their lack of attention to the survey, lack of interest in the topic, confusion about what the survey question is trying to elicit (Enke and Graeber, 2020), or experimenter-demand effects (Zizzo, 2010; de Quidt et al., 2018; Mummolo and Peterson, 2019). We take advantage of the unique features of SOEP and some methodological innovations to provide novel evidence that misperceptions are indeed meaningful. The evidence indicates that misperceptions are persistent, as individuals who overestimate their rank in one year are likely to overestimate it a year later as well. We show that misperceptions are also robust within households: if one person overestimates their rank, other members of their household are likely to do the same. We also provide evidence that households are genuinely interested in learning about their relative income. Providing information to individuals affects their perceptions a year later, implying that individuals truly incorporate the information. Moreover, we find that providing one member of a household with information not only affects the perceptions of the same household member a year later, but of other household members as well. This evidence suggests that individuals care enough about the information on relative income to share it voluntarily with family members in the 12 months that separated the two survey waves. Finally, using the information-acquisition experiment, we document that individuals are willing to pay non-trivial amounts for information about their global and national income ranks.

The third set of results looks at the relationship between policy preferences and perceptions of relative income. As a benchmark, we start with the relationship between national income rank and preferences for national redistribution, which has been studied before in other countries using experimental (Cruces et al., 2013; Karadja et al., 2017) and non-experimental methods (see e.g., Fong, 2001; Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Mollerstrom and

Seim, 2014). As has been documented previously, we find that the demand for national redistribution is negatively correlated to the perceived national income rank. Moreover, and also consistent with previous work (Cruces et al., 2013; Kuziemko et al., 2015; Karadja et al., 2017; Fernandez-Albertos and Kuo, 2018; Alesina et al., 2018b; Fenton, 2020), we find a large heterogeneity by ideological orientation, with the correlation being driven almost entirely by left-of-center individuals (about a third of the sample). The results from the information-provision experiment further corroborate these findings: information about national relative income affects demand for national redistribution in the predicted direction, and only for left-of-center respondents. This evidence is consistent with selfish motives a-la Meltzer-Richard in the national arena.

On the contrary, we do not find evidence that correcting misperceptions on global relative income affects support for policies related to global inequality. If anything, we find that individuals care about their national income rank: among the left-leaning respondents, individuals who find out that they are higher in the national income distribution tend to decrease their support for global redistribution, while right leaning respondents who learn they are higher in the national income distribution tend to increase global giving. This suggests that the relevant reference group is people nationally, not globally.

This study ties into several strands of literature. First, it is related to a literature measuring preferences for redistribution. In addition to selfish motives (Fong, 2001; Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Mollerstrom and Seim, 2014), this literature highlights other relevant factors, such as beliefs about the relative importance of effort versus luck in generating individual economic success, and other-regarding preferences (e.g., Alesina and Giuliano, 2011; Mollerstrom and Seim, 2014; Alesina et al., 2018b; Gärtner et al., 2019). We contribute to this literature by providing, to the best of our knowledge, first evidence on the formation of preferences for global redistribution.

We also add to a growing literature on the role of misperceptions as a determinant of political opinions and policy preferences. For example, a number of studies have documented the role of misperceptions about relative income (Cruces et al., 2013; Karadja et al., 2017; Engelhardt and Wagener, 2017; Fernandez-Albertos and Kuo, 2018; Hvidberg et al., 2020), wealth inequality (Norton and Ariely, 2011; Kuziemko et al., 2015; Fehr and Reichlin, 2021), income mobility (Alesina et al., 2018b; Fehr et al., 2019; Gärtner et al., 2019), and immigration (Alesina et al., 2018a; Haaland and Roth, 2019). One common concern raised in regard to this literature is that misperceptions mostly reflect measurement error, inattention, or disinterest from the survey respondent. We contribute to this literature by leveraging the setting provided by the SOEP and methodological innovations to provide unique evidence that misperceptions are meaningful.[5]

[5]Our methodological innovations could be used also in other research areas, including (but not limited to) misperceptions about the inflation rate (Cavallo et al., 2017), housing prices (Fuster et al., 2019), and cost of living (Bottan and Perez-Truglia, 2017).

Our study also relates to research on international aid and migration in political science, as well as in sociology and economics. Some literature on international aid argues that such giving 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; Bechtel et al., 2014; Nair, 2018; Eichenauer et al., 2018). Nair (2018) is the most closely related, as it explores the link between global relative income, support for foreign aid and other policy variables. There are several conceptual and methodological differences between our study and Nair (2018), however. For instance, while Nair (2018) focuses on information about global relative income, we provide information and elicit beliefs about both the global and national income ranks.[6] This was ex-ante important for the reason that some economic theories suggest that national income rank, instead of global income rank, should matter for policy preferences. This feature of the design also turned out to be important ex-post, as we find that national relative income, rather than global relative income, affects the demand for global redistribution.

Finally, our findings are also related to recent work on group identity and altruism. For instance, Enke et al. (2019) define moral universalism as the extent to which people exhibit the same level of altruism and trust towards strangers as towards in-group members. They provide evidence of significant heterogeneity in moral universalism across individuals. While our finding that preferences for national redistribution are correlated to preferences for global redistribution could be interpreted as evidence that moral universalism is significant for some individuals, the fact that some individuals want to redistribute domestically but not globally tells us that moral universalism does not apply to all. Other work, such as Cappelen et al. (2013) has focused on giving of students from two rich countries (Germany and Norway) to students in two of the world's poorest countries (Uganda and Tanzania). This type of international altruism has also been studied in other fields beyond economics such as political science (Nair, 2018) and sociology (Bader and Keuschnigg, 2020). In contrast to this work, we take a broader approach and focus not only on giving, but also on other aspects, such as redistribution, globalization and immigration, which are guided by economic frameworks such as Meltzer-Richard and Stolper-Samuelson.

The paper continues as follows. Section 2 outlines our research design and describes our data. Section 3 documents our first set of results related to the preferences for global redistribution and other policies. Section 4 documents the second set of results, on the misperceptions of relative income. Section 5 presents the third set of results, about the effects of perceived relative income on policy preferences. Section 6 concludes.

[6] 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.

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.[7] The surveys are conducted computer-assisted in face-to-face interviews by trained professional interviewers. We designed two tailor-made survey modules, including a randomized information treatment, and incentivized belief and outcome measures, and implemented them in two consecutive waves of the SOEP-IS. The baseline survey took place in the Fall of 2017 and a follow-up survey in the Fall of 2018.[8] In Appendices B and C, we provide the English translations of the two original survey instruments (which were in German).

2.1 Survey Design: Baseline

The baseline survey had 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 the position in the income distribution; and iv) outcome measures on preferences for redistribution, support for globalization and immigration, and behavior in an incentivized giving task (we will refer to these measures jointly as "policy preferences").[9]

We asked all questions (except the questions on support for a global redistributive institution, globalization, and immigration) both in the national (i.e., German) context, and in the global context. In particular, we asked respondents in (ii) to state their perceived position in both the national and global income distribution. Third, we randomized whether respondents saw the national or the global question first at the individual level to ease presentation and comprehension. That means 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, and about giving in the national context, first in part (iv).

The pre-treatment part (i) included two questions about how respondents perceive the role of effort and luck in economic success in the national and global context (Effort vs. Luck Belief). These beliefs in the national context are typically strong predictors of various political opinions, such as 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 evi-

[7] The 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 on the SOEP, and Richter and Schupp (2015) for the SOEP-IS).

[8] Data sources: SOEP Innovation Sample (2021)

[9] Each survey item in our module briefly explained the subject of the question, stated the question and explained the response scale, for better comprehension.

dence). 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.

Given that there is growing evidence that information effects on individual views about redistribution and policies are subject to strong heterogeneity in political orientation (e.g., Karadja et al., 2017; Alesina et al., 2018b,a; Fenton, 2020), we purposefully placed our module after the questions about political attitudes that are routinely included in the SOEP-IS. This allowed us to estimate the heterogeneity of the experimental results by political orientation without having to worry about imbalanced sub-samples and the possibility that the information treatment influenced responses on political orientation. Specifically, we use respondents' self-placement in the political left-right spectrum on a scale from far left (0) to far right (10). A sizable share of respondents (about 41 percent) chose the mid-point, while a slight majority of the remaining respondents lean left.[10] To simplify the exposition of our results, the baseline specification splits the sample between left-of-center (0-4) respondents and center/right-of-center respondents (5-10).

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 and global income distributions on a scale from 0 (poorest person) to 100 (richest person). To minimize social desirability bias, we required respondents to answer these questions in private without the interviewer seeing the tablet screen. Both relative income questions were incentivized for accuracy, and respondents were informed that they would receive €20 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 rank 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 with part (iii), in which we randomized half of the respondents into a treatment providing them information about their true rank in the national and global income distributions. The information revealed how many people are poorer at the national *and* global level, 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). The other half of respondents received no information.

Then, in part (iv), we measured our outcomes of interest. We first asked how much economic redistribution respondents demanded at the national and global level with answers ranging from 1 (indicating no demand for redistribution) to 10 (indicating a desire for com-

[10] For the full distribution of responses, see Appendix A.1.

plete redistribution that equalizes post-redistribution income between citizens or people in the world). Similarly, we asked to what extent respondents would support the creation of an international institution with a mandate to implement global redistribution, about their preferred level of globalization, and about their view on immigration policies that would allow more people from poor countries to live and work in Germany. Again, answers to these questions ranged from 1 indicating no support, less immigration, and no globalization, respectively, to 10 indicating full support, more immigration, and complete globalization.

Importantly, most of these questions involve abstract concepts, such as "economic redistribution," that can be difficult for respondents to think about, not least at the global level. Therefore, we took great care to define and explain all involved concepts and answer scales in a simple and comprehensible way. For example, we explained that redistribution of income at the national level means that the state reduces the income gap between citizens through taxes and transfers, and subsequently introduced the question about global redistribution by asking them to imagine that it would be possible to redistribute income around the world in a similar fashion to how a state can redistribute income within a country. The trained interviewers also received information on how to respond to potential questions that the respondents had while taking the survey.

Among our outcome variables in part (iv), we also have two incentivized questions that cover the altruistic aspect of redistribution. To this end, we used two simple giving tasks with real stakes in a national and a global context, respectively. More precisely, respondents were asked to: a) distribute €50 between them and a poor German household; and b) distribute another €50 between them and a poor global household. Respondents made their decisions in private: interviewers were not able to see the tablet screen. German households were drawn from the bottom ten percent of the income distribution of SOEP-IS households that are not in our sample.[11] 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.

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

2.2 Survey Design: Follow-Up

We designed a follow-up survey that we implemented in the same sample of respondents a year later. One of the purposes of this survey was to test whether the information provided to the survey participants had persistent effects a year later. As in the baseline survey, we began by collecting information on income and the number of household members. We then asked respondents to guess their rank in the national and global income distributions, rewarding accurate predictions with €10 each. 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, all participants answered the same questions about demand for redistribution, globalization, and immigration as in the baseline survey. In the follow-up survey, however, we did not include the incentivized distribution task.

The follow-up survey included some additional questions designed to complement the results from the baseline survey. Most importantly, 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 presented, separately for the national and the global income distribution, five scenarios in which respondents were required to 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 in Scenario 1 to €10 in Scenario 5, in increasing increments (€0.1, €1, €2.5, €5, and €10). We informed respondents that one of the overall five scenarios would be randomly selected and implemented.[12] Respondents made their decisions in private. To avoid having 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 income rank, either later in the survey, or in later waves of the survey. The survey included a few additional questions. After the elicitation of each belief on relative income, we elicited how certain respondents were about their answers on a 0-10 scale. We also asked respondents to what extent they believe that the rich and poor benefit from globalization and immigration. Finally, we included a battery of four questions eliciting whether the respondent trust the government, the media, official statistics and research.

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 year. A total of 1,392 respondents took part in the baseline survey, while 1,144 participated in the follow-up survey (82 percent of the 1,392 respondents in the baseline survey). Interviews with a single household member typically

[12]The 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.

lasted for about 60 minutes, out of which our modules comprised on average 8-10 minutes.

There are some advantages of working with the SOEP that are worth emphasizing. The SOEP team undertakes various efforts to optimize data quality. For example, new survey items are pre-tested before the data collection. During the data collection, there are a number of institutional safeguards that have been developed by SOEP in over 35 years of history.[13] After the data collection, there are several routines to check data plausibility and consistency. In addition to the data quality, there are some unique features of SOEP that we take advantage of for our research design. All household members over age of 16 are interviewed in computer-assisted, face-to-face interviews performed by trained professionals. 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 within a wave. For this reason, we can study the diffusion of information within the household across waves. While we only designed a module of the survey, we have access to responses to questions in all modules, including a rich set of measures of socio-economic indicators. Moreover, due to the longitudinal character of the SOEP, we can track outcomes in years before and after the baseline survey.

Appendix A.1 provides descriptive statistics for the baseline and follow-up survey. We show that, consistent with successful random assignment, the observable pre-treatment characteristics are balanced across all treatment groups. 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 here for two reasons. First, attrition is low: 18 percent of the respondents in the baseline survey did not participate in our follow-up survey one year later. Second, and most importantly, there is no significant difference in the attrition rates between individuals who were in the control group (17 percent attrition), and individuals who were in the treatment group in the baseline survey (19 percent attrition, p-value=0.432 for t-test of proportions).[14] In addition to the low attrition rate, our study stands out relative to other information-provision studies in terms of the length of time between our baseline and follow-up surveys. For examples, Kuziemko et al. (2015) conducted their follow-up survey one month later (with a response rate of 14 percent), Cavallo et al. (2017) conducted it two months later (response rate of 36.1 percent), and Karadja et al. (2017) conducted it three months later (response rate of 80 percent), and Haaland and Roth (2019) conducted it one week later (with a response rate of 66.3 percent).

3 Policy Preferences

We start with a descriptive analysis of policy preferences from the baseline control group (i.e., individuals who did not receive any feedback from us regarding their true income rank).

[13] For more details, see Goebel et al., 2018.

[14] In Appendix A.2, we provide further evidence that attrition was random.

Looking at preferences for redistribution, Figure 2.a reveals significant variation as to how much redistribution individuals want at both the national and the global level, and even though the two preferences are correlated (correlation coefficient 0.70, p-value < 0.001 as illustrated in Figure 2.b), the correlation is not perfect: there are some respondents who want extensive national redistribution but very little global redistribution, and vice versa.[15] There is also significant variation in the extent to which respondents support the idea of a global institution with a redistributive mandate (Figure 2.c). Likewise, there is significant heterogeneity in how much respondents support globalization (Figure 2.d) and immigration (Figure 2.e).

These preference measures are unincentivized self-reports, but our survey also contained two incentivized giving tasks. In each of these two tasks, respondents could split €50 between themselves and a poor household in the national context and in the global context. As opposed to the demand for redistribution measures, which captures both selfish and altruistic preferences, the giving tasks are only reflecting altruism. Figure 2.f shows that there is substantial giving among the households: the average share of giving to a poor German household is 56 percent (M=€28.1, SD=14.8) while the average share of giving to a Kenyan household is 64 percent (M=€31.8, SD=15.9). The two measures are correlated (correlation coefficient 0.74, p-value < 0.01), but again, there are some respondents who give a high share to a national poor, but a low share to a global poor and vice versa (Figure 2.g).

Table 1 documents the correlations between the different policy preferences. In general we see that they are all correlated. More specifically, we note that there is a significant positive correlation between the real-stakes donations with preferences for redistribution. That is, demanding more national redistribution is related to higher donation to the national poor and demanding more global redistribution is associated with higher giving to the global poor.[16] This indicates that demand for redistribution likely has altruistic as well as selfish components, both at the national and at the global level. The magnitude of those correlations are, however, not as large as, i.e., the positive correlation between national and global demand for redistribution, or the correlation between national and global giving.

Next, we investigate the correlates of the policy preferences. In Table 2, we report the results of bivariate regressions for each variable listed in the table (i.e., each cell in the table corresponds to an independent regression of the corresponding dependent variable on the row variable). In general, we see that the policy preferences share many correlates, which should not be surprising given that they are correlated to each other (as documented in Table 1). We first look at the extent to which our measure of demand for national redistribution is correlated with personal characteristics. We can compare this to previous evidence (Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Mollerstrom and Seim, 2014; Karadja et al., 2017; Gärtner et al.,

[15] About 42 percent of respondents in the control group state exactly the same level of redistribution in the national and global arena and for 28 percent of respondents the response differs in one level.

[16] See Appendix A.3 for a less parametric approach.

2017, 2019). We mostly confirm these previous findings. For example, the demand for national redistribution is greater for individuals with lower income, for individuals who believe that effort drives economic success, for left-leaning individuals, and for respondents living in East Germany, while it is lower for older individuals. However, we fail to find support for some previously documented findings. 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).

Column (2) displays the results of the corresponding correlational analysis for demand for global redistribution. While the correlates are mostly the same, there are three notable differences. There is no relation between demand for global redistribution and a respondent's income, current residence (East Germany), and national Effort vs. Luck Beliefs.[17] The correlates of supporting a global institution with a redistributive mandate are largely the same as for demand for global redistribution (Column 3). We note, however, that respondents located in East Germany are less supportive of such an organization than those in the West, and that German citizens are less supportive than respondents without the German citizenship.

There are some robust correlational patterns for the giving decision (Columns 4 and 5 in Table 2). In particular, we note that giving at both the national and the global level is related to higher enducation and income, whereas older respondents and East German respondents give less. Respondents who believe that individual economic success globally depends on luck also give more in both contexts, and there is a tendency for left-leaning respondents to give more in both contexts as well. Finally, we also see some consistent patterns for support of globalization and immigration (Columns 6 and 7 in Table 2). Again, older respondents, respondents in East Germany, and German citizens are less in favor of globalization and of generous immigration policies. We also note that higher income is associated with more positive views on globalization and immigration, respectively. Similarly, left-leaning respondents and people who believe that it is luck rather than effort that determines an individual's economic success in the global arena more likely support globalization and favor generous immigration policies. At the national level, however, Effort vs. Luck Beliefs are uncorrelated with these preferences.

4 Perceptions of Relative Income

4.1 Misperceptions

What do respondents know about their national and global relative income? On the one hand, there are reasons to expect that misperceptions for global relative income will be more substan-

[17] It is also interesting to note that respondents are in general 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 Effort vs. Luck Belief scale is 4.58 (SD=1.68) for the national and 5.18 (SD=1.94) for the global context (p-value < 0.001).

tial than those of national relative income. For example, the information about the national income distribution may be more accessible than information about the global income distribution. National newspapers may more often provide information related to the national income distribution, but rarely provide information related to the global income distribution. The same case can be made about indirect sources of information about the income distribution, such as salary discussions with social contacts, or casual observation of other people's consumption: the majority of these conversations and observations may be about a national rather than a global context. On the other hand, there are also reasons to expect lower misperceptions for global relative income than for national relative income, at least in a rich country like Germany. Even if a household has no idea whether it is poor or rich within Germany, just knowing that Germany is a rich country may be enough to infer that one is very likely at the top of the global income distribution.

Figure 3 shows the perceptions for national income rank (Figure 3.a) and global income rank (Figure 3.b). The results indicate that substantial misperceptions exist for both the global and national beliefs. Figure 4.a shows the histograms of misperceptions: i.e., the difference between prior beliefs and reality.[18] Here, a positive (negative) number indicates that the respondent overestimates (underestimates) 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 higher than she believes.[19] A visual inspection of Figure 4.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 overestimating their national rank as there are people underestimating it. This is not true for global rank: respondents underestimate their relative position in the global income distribution by an average of 15 percentage points (SD=0.26, p-value < 0.001 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, and find that these are very similar for national and global beliefs (23 percentage points in both cases). In other words, at the

[18] In Appendix A.4, we also show the distribution of the gap between the information provided to the individuals and the prior beliefs.

[19] One potential concern is that misperceptions may be partly due to the fact that individuals do not know their absolute, rather than relative, income. There are two pieces of evidence indicating that this is not a significant source for concern. First, Karadja et al. (2017) can match self-reported absolute income to the actual absolute income from the tax records. They show that self-reported absolute income is highly correlated to the actual absolute income. Second, in our own data we find that household members are highly consistent with each other in their perceptions of absolute income. More precisely, we find that just 11.4 percent of the overall variation in perceived absolute income corresponds to the within-household variation (these results exclude 3 outliers in perceived absolute income). In comparison, 10.8 percent of the overall variation in the perceived number of household members corresponds to the within-household variation.

individual level, Germans are as (in)accurate about their national income rank as they are about their global income rank.

Figure 4.b shows the relationship between the national and the global biases. They are significantly (albeit not perfectly) correlated: the correlation coefficient is 0.61 (p-value < 0.001), implying that if a respondent overestimates her position relative to other Germans, chances are that she will also overestimate her income globally. This, in turn, may imply that respondents are, to some extent, extrapolating their beliefs about their national relative position to the global arena.[20]

We assess whether the misperceptions are consistent with the middle-class bias that would be expected under assortativity neglect. That is, the poor interact disproportionally with poor people and thus end up overestimating their relative income; in constrast the rich interact disproportionally with rich people and thus end up underestimating their relative income.[21] The results are presented in Figure 5.a for national relative income and Figure 5.b for global relative income. Figure 5.a shows that, consistent with prior evidence (Cruces et al., 2013), there is a middle-class bias in the perceptions about national relative income. Households below the median income overestimate their relative income, while households above the median income underestimate their relative income. Figure 5.b shows that a middle-class bias may also exist for beliefs about global relative income. However, since the vast majority of German households are in the top two deciles of the global income distribution, there is not sufficient data to provide a sharp test of the middle-class bias at the global level.

The results presented so far indicate substantial misperceptions about national and global relative income. However, this kind of data on misperceptions come with certain challenges due to their self-reported nature. For instance, some respondents may not be paying attention to the question, or may be uninformed simply because they do not care about the topic. In the next sections, we take advantage of our unique data and specific features of SOEP to address these concerns.

4.2 Consistency Across Household Members and Over Time

We start by noting that misperceptions exist in our data even though we provided significant rewards for the respondents to correctly state their national and global position in the relative income distribution. The incentives should, 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.

Next, we show that the misperceptions are robust across household members and over time.

[20] Moreover, the two types of biases have similar correlates (results presented in Appendix A.6).

[21] Frick 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).

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. Focusing on the control group, Figure 6 shows that the persistence is significant: for national ranks, for each one percentage-point bias in the baseline survey, a respondent is biased in the same direction by 0.4 percentage points in the follow-up survey (p-value < 0.001). Results are similar in magnitude for global rank (correlation is 0.27, p-value < 0.001). 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.[22]

We further document that misperceptions are quite consistent between household members. If misperceptions reflect real, meaningful biases, we should expect them to be correlated across members of the same household. Indeed, we find that misperceptions are highly correlated between household members: a minority (41.8 percent) of the overall variance in misperceptions of national rank corresponds to the within-household variance.[23] As a benchmark, we can reproduce this exercise for a factual question for which we would expect household members to be almost perfectly consistent with each other: the number of household members. We find that perceptions about the household size are highly correlated between household members: just 10.8 percent of the overall variance corresponds to the within-household variance.[24] In sum, members of the same household are largely consistent with each other regarding their misperceptions of income rank, although not as consistent as they are regarding the perceived household size.

4.3 Persistence of Learning

Providing information on the respondent's income rank could have spurious effects. A first concern has to do with experimenter-demand effect: subjects may react to the information due to the fact that they feel social pressure from the experimenter (Zizzo, 2010). While this is a valid concern, recent evidence suggests that the magnitude of experimenter demand effects is small (de Quidt et al., 2018; Mummolo and Peterson, 2019). Moreover, we took some precautions to try to minimize the scope of experimenter-demand effect. Most importantly, despite the

[22] For details, see Appendix A.7.

[23] We follow the strategy from Chetty et al. (2011), by estimating a regression of the variable of interest (in this case, the misperception of national income rank) on a constant and household-level random-effects. With the regression estimates we can compute the parameter $1 - \rho$, which corresponds to the within-household variance as a share of the overall variance. The results are roughly similar for the global misperceptions: 58.1 percent of the overall variance corresponds to within-household variance.

[24] There are some small inconsistencies between household members in their perceptions of household size. These inconsistencies may be due to lack of attention, typos, or due to gray areas: e.g., one spouse includes a child currently at college as a member of the household while the other spouse does not.

survey being conducted face-to-face with the interviewer visiting people in their homes, the subjects received the information and answered questions related to relative income in private: the surveyor handed them a tablet and then turned around to give privacy to the respondent. A second concern has to do with anchoring. For example, Cavallo et al. (2017) shows that providing individuals with fictitious information on prices had an effect on their subsequent inflation expectations even though the individuals were explicitly told that the information was fictitious and thus were expected to ignore it.

If the reaction to the information was due to spurious reasons such as experimenter demand or anchoring, we would not expect the effects of providing information to be long-lasting. Thus, as in other studies, we measure the long-term effects of the information (see e.g., Kuziemko et al., 2015; Cavallo et al., 2017; Karadja et al., 2017; Haaland and Roth, 2019; Haaland et al., 2020). Let $r_{i,nat}^{prior}$ denote the perceived national rank in the baseline survey (i.e., the prior belief, before receiving information) and $r_{i,nat}^{signal}$ denote the signal that was given as feedback if the individual was in the treatment group. Consequently, $r_{i,nat}^{signal} - r_{i,nat}^{prior}$ is the misperception about the national rank. Let T_i be an indicator variable indicating whether the individual received relative-income information in the baseline survey. The regression specification is the following:

$$r_{i,nat}^{t+1} = \alpha_{nat} \cdot \left(r_{i,nat}^{signal} - r_{i,nat}^{prior} \right) \cdot T_i + \beta_1 \cdot \left(r_{i,nat}^{signal} - r_{i,nat}^{prior} \right) + X_i \beta_2 + \varepsilon_i. \quad (1)$$

The dependent variable, $r_{i,nat}^{t+1}$, is the perceived national rank in the follow-up survey, and X_i is a set of control variables such as the respondent's demographic characteristics.[25] The coefficient α_{nat} tells us the rate of pass-through between the information given, and subsequent beliefs (and we use an analogous specification for global relative income). 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., $\alpha_{nat} = 1$). In theory, Bayesian individuals would form posterior beliefs by taking a weighted average between the signal provided to them 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 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 on the pass-through rate are presented in Table 3. Column (1) suggests a pass-through coefficient of 0.153 at the national level: i.e., for each percentage point that the treatment corrected a respondent's misperception about national relative income, a year later she

[25] See the table notes for a list of the full set of control variables.

reports beliefs that have moved 0.153 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 the information provided to them a year later. In Column (3) we reproduce the analysis, but focusing on perceptions of global income rank instead of national income rank. The pass-through estimate for global relative income (0.122, from Column 3) is similar as that of the national relative income (0.153, from Column 1).

Columns (5) and (7) of Table 3 present the results from a falsification test, in which 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 "placebo" rate of pass-through is in both cases close to zero, statistically insignificant and precisely estimated.[26]

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 7.a shows that, on average, individuals are aware that they do not know their position in the income distributions well: only about six percent of respondents report to be 90-100 percent certain about their national relative position assessment; and only eleven percent of respondents report this level of certainty about their global income rank assessment. Moreover, Figure 7.b shows the relationship between respondents' confidence in their answer and their accuracy. We see evidence of self-awareness, particularly in the case of global rank: e.g., the misperception is around 32 percentage points for those who are completely uncertain or only 10 percent sure, whereas it is around 12 percentiles for those who report to be 90-100 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 4, 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 0.433 (p-value = 0.002) and in global rank by 0.622 (p-value < 0.001) in the follow-up one year later.

4.4 Information Diffusion within the Household

Due to the fact that we randomized the information treatment at the individual level, sometimes an individual received information about their household's true relative rank in the national and the global income distributions, while other members of the same household did not. We exploit this feature to measure intra-household information diffusion. If individu-

[26]The 90%-CI for national ranks is [-0.022, 0.060] and for global ranks it is [-0.075, 0.013]. Moreover, Appendix A.2 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.

als take the time to discuss the information they receive with other household members, they presumably find it interesting and/or useful.

Let T_i^{peer} take the value 1 if the individual did not receive the information but another member of her household did, and 0 otherwise (i.e., if the individual received the information or if none of the household members received the information).[27] We can extend the specification from Equation (1) to accommodate for information spillovers within the household:

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

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 – 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 permitted.[28] The results for perceptions of national income rank are presented in Column (2) of Table 3, and suggest that there is significant diffusion of information within households. The coefficient of 0.152 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 0.152 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 0.153 in Column (1) to 0.196 in Column (2). The comparisons between the pass-through for own information versus peer information suggests that 78 percent ($= \frac{0.152}{0.196}$) of the information travels to other people in the household. This is a high degree of information diffusion. We reproduce the analysis for the global rank in Column (4). The rate of pass-through is somewhat lower (0.109) but still marginally statistically significant (p-value = 0.089). The comparisons between the pass-through for own information versus peer information suggest that 68 percent ($= \frac{0.109}{0.160}$) of the information about global income rank makes its way to other members of the household. We can conduct the same falsification test as discussed above, where the dependent variable is the belief in the baseline survey (i.e., before anyone received the information). These results are presented in Columns (6) and (8) of Table 3. As expected, all the coefficients are close to zero, statistically insignificant and precisely

[27] This is a common definition in the study of spillovers, based on the assumption that if the individual receives the treatment directly then it should not matter whether his or her peers received the treatment or not. We provide direct evidence in support of this specification in Appendix A.8.

[28] See the table notes for a list of the full set of control variables. One important control is the number of household respondents: 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 the number of respondents who could have been assigned to the information.

estimated.[29]

Columns (2) and (4) of Table 4 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 (2) and (4), respectively. The evidence is mixed: the household peer treatment increased belief certainty in national rank by just 0.076 and this effect is statistically insignificant. However, given that this point estimate is not precisely estimated (90%-CI: -0.282, 0.435), we cannot rule out large positive effects. For global rank, the evidence is clearer: the household peer treatment increased own belief certainty by 0.506, which is not only statistically significant (p-value = 0.031) but also almost as large in magnitude as the effect of own treatment (with a corresponding coefficient of 0.807, reported in Column 4 too).

4.5 Demand for Information

If individuals care about their relative income, they should be willing to pay to receive this information. To test this hypothesis, we exploit the information-acquisition experiment included in the follow-up survey. We start by looking at whether the responses people gave are consistent across scenarios: i.e., whether their demand curves are downward-sloping. Around five percent of respondents provided inconsistent responses in at least one of the two WTP questions.[30] This level of consistency is at the lower end of the range of other studies using similar methods to elicit the WTP for information.[31]

The distribution of WTP is shown in Figure 8.a. This figure uses data from respondents in the control group only: since they did not receive information in the baseline survey, the interpretation of the findings is more straightforward for this group.[32] We find significant demand for information on relative income: we estimate the mean WTP in the control group using an interval regression model and find that this is €5.71 (SE 0.33) for national rank and €5.71 (SE

[29]The 90%-CI for information on national relative income provided to another household member is [-0.095, 0.016] and for global relative income [-0.051, 0.073].

[30]For example, they chose €5 instead of information, but then chose information instead of €10. 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.

[31]For instance, the share of inconsistent respondents was about 2 percent in Allcott and Kessler (2019), 5 percent in Fuster et al. (2019), and 15 percent in Cullen and Perez-Truglia (2018).

[32]Note that individuals may still be willing to acquire information even if they received feedback in the baseline survey. Even if the income distribution is stable over time, a household's per capita income can change from year to year. As a result, whatever information on relative income a household received a year before may no longer be relevant if the household has a different income. Likewise, even if the household's income was the same as in the previous year, households may have forgotten the information given to them a year prior, in which case they would be willing to pay to see it again. Indeed, the evidence on the persistence of learning presented in Section 4.3 suggests that, one year later, most households in the treatment group may have forgotten a lot of the information given to them.

0.34) for global rank.[33] Figure 8.b shows the relationship between the WTP for national vs. global rank. The two are highly correlated, but not perfectly so: 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 predicted mean 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 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 and Kessler, 2019) and future national home prices ($4.16, Fuster et al., 2019).[34]

5 The Effects of Perceived Relative Income on Policy Preferences

We now turn to the question of how perceived relative income affects policy preferences. Previous work has shown a significant polarization along political orientation with respect to information on relative income, income inequality, and social mobility (e.g., Karadja et al., 2017; Kuziemko et al., 2015; Fenton, 2020; Alesina et al., 2018b). 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. To account for this heterogeneity in political orientation, we split the sample into left-of-center respondents and center/right-of-center respondents.[35] To ease the comparison of results across outcomes, we standardize the dependent variables throughout this section by subtracting the control group mean from each observation and then dividing by the control group standard deviation.

Before presenting the experimental results, we explore the raw correlations between respondents' relative income perceptions on the one hand, and their policy preferences on the other hand. The results are presented in Table 5, and are based only on individuals in the baseline

[33] This 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.

[34] In 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.

[35] The results are similar if we analyze center (5 in the 0-10 scale) separately from right-of-center (6-10). Results reported in Appendix A.9.

survey control group. Panel (a) of Table 5 displays the results for all control group respondents. It is apparent that perceived global rank is not related to demand for redistribution, neither at the national nor at the global level, nor to giving and to the support for more globalization and generous immigration policies. Perceived national rank is, however, related to demand for national, but not global redistribution. Similarly, behavior in the two giving tasks is significantly associated with perceived relative income in the German income distribution, with those who perceive themselves to be higher up in the income distribution giving more to the national and global poor. Support for globalization and for generous immigration policies are also positively related to relative income perceptions at the national level, although the relationship is weaker and only marginally significant for the support for globalization.

Panels (b) and (c) of Table 5 explore heterogeneity by political orientation. In line with the previous literature, we 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-of-center respondents. Left-of-center respondents also display a positive association between perceived relative global income and support for a redistributive global institution (also with national relative income) , and they are more willing to give to the poorest 10 percent both nationally and globally, if they are higher up in the global income distribution. For center/right-of-center respondents correlation coefficients are generally smaller in magnitude, except that higher perceived national relative income is significantly related to national and global giving, and support for globalization and immigration.

Next, we use our information experiment to investigate the causal relation between relative income and policy preferences. We use the following specification, which is based on the same intuition from Equation (1):

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

where $r_{i,nat}^{signal} - r_{i,nat}^{prior}$ is the misperception about the national rank as before and 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_{glob}, where $\frac{\alpha_{nat}}{100}$ shows the causal effect of a respondent receiving information implying that her national rank is 1 percentage point higher than she previously thought.[36] Correspondingly, $\frac{\alpha_{glob}}{100}$ shows the causal effect of a respondent being told that her global rank is 1 percentage point higher than

[36]This baseline specification assumes that there is a linear relationship between policy preferences and the income ranks. In Appendix A.10 we use binned scatterplots to show that this linear approximation is reasonable, and also that the results are not driven by outliers. Moreover, we use histograms to provide an even less parametric look at the data.

she believed it to be. The variables $r_{i,nat}^{signal} - r_{i,nat}^{prior}$ and $r_{i,glob}^{signal} - r_{i,glob}^{prior}$ control for the non-random variation in prior misperceptions: i.e., they guarantee that α_{nat} and α_{glob} are identified by random variation in information provision.[37] X_i is a set of demographic controls, as indicated in the table notes. Note that the estimates from this regression correspond to intention-to-treat effects, because of potential non-compliance: when individuals are provided with information, they may not incorporate that information fully into their beliefs, for example because they do not trust it or because they are not paying attention to the survey. Even when beliefs are re-elicited immediately after the information provision, which is not the case here but has been done in other work, the pass-through from information to posterior beliefs tends to be closer to 0.5.[38] If this is the case here, then the treatment-on-the-treated effects could be twice as large as the intention-to-treat estimates that we report below.

The experimental results are presented in Table 6. The results roughly line up with the raw correlations for left-of-center and center/right-of-center respondents shown in Table 5.[39] Panel (a) of Table 6 presents the average treatment effects and indicate that preferences for redistribution (national and global), and support for a global redistributive organization, decrease with perceived national relative income, but the magnitude is small and statistically insignificant. The effects of perceived global relative income are even smaller. The relation between national relative income and behavior in the respective giving tasks are positive, but statistically insignificant. The effect of global relative income in both giving tasks is close to zero. Similarly, the signs of the estimates for support for globalization and immigration are generally the same as for the raw correlations, but again the estimates are statistically insignificant.

Panel (b) of Table 6 shows that the effects on demand for redistribution are large and significant for the left-leaning respondents: informing left-of-center respondents that their national income rank is ten percentage points higher than they previously believed decreases their support for national redistribution by around 0.077 standard deviations, while the effects of national rank on global redistribution are slightly higher in magnitude (0.093 standard deviations). Similarly, receiving information that one has a higher relative income in Germany than

[37] In the baseline specification, the perceptions of national *and* global ranks are included simultaneously in the regression. In Appendix A.10 we show that the results are robust under an alternative specification that includes of national *or* global ranks separately.

[38] For instance, Bottan and Perez-Truglia (2020) estimates that the average subject forms home price expectations by assigning a weight of 0.445 to the signal and the remaining weight of 0.555 to their prior beliefs (the difference in slopes from Figure A.5). Cavallo et al. (2017) shows that, when forming inflation expectations, the average Argentine respondent assigns a weight of 0.432 to the signal provided to them (coefficient α-statistics reported in Panel B, Column 1 of Table 1). And Nathan et al. (2020) shows that, when forming beliefs about the average tax rate, the average subject a weight of 0.459 to the signal (the difference in slopes from Figure A.5).

[39] In Appendix A.10 we provide a falsification test of the information intervention, by showing that there are no "effects" on the two survey outcomes measured pre-treatment (the belief in the importance of effort versus luck for individual economic success both at the national the global level). In Appendix A.11, we present results for the average effects of receiving information (i.e., regardless of whether the feedback was above or below the prior belief), and in Appendix A.12, we present the effects on the redistributive preferences and support for globalization and immigration elicited in the follow-up survey.

previously believed, causally decreases support for a redistributive global institution among the left-of-center. The coefficient for this outcome (-1.047, p-value = 0.023) is similar in magnitude and statistical significance as the coefficient on the main outcome on global redistribution (-0.932, p-value = 0.018). The point estimates for the support for globalization and immigration outcomes (-0.487 and -0.430) are also negative although somewhat smaller in magnitude than the other coefficients and statistically insignificant. In contrast, we find no evidence that information about global rank has an effect on any of the outcomes for people to the left on the political spectrum.

For the center/right-of-center sample (panel (c) of Table 6), we find that most effects are close to zero and statistically insignificant: this is true for the demand for national redistribution (90%-CI: -0.400, 0.574) and global redistribution (90%-CI: -0.379, 0.614), and for the support for a global, redistributive organization (90%-CI: -0.374, 0.576). The confidence intervals suggest that we can rule out effects that are less than half of the effect sizes for left-of-center respondents. There are, however, larger effect sizes for national giving (0.501) and global giving (0.459). Specifically, when we only look at right-of-center respondents, we see that those who learned that they are 10 percentage points higher in the national income distribution than they previously thought increase their giving to a poor household in Germany by 0.081 standard deviations (p-value = 0.063) and to a poor household in Kenya by 0.105 standard deviations (p.value = 0.027).[40] The effect on support for generous immigration policies is close to zero and statistically insignificant (90%-CI: -0.484, 0.474), whereas the point estimate of the effect on support for globalization is positive, but not statistically significant (90%-CI: -0.243, 0.793). Again, we see no evidence that information about global rank has an effect on any of the outcomes: the point estimates and standard errors are smaller than the corresponding values for information on national rank.

It could be tempting to ascribe the negative relation between national relative income and demand for global redistribution to a Stolper-Samuelson effect, as this framework would predict that national, rather than global, relative income is what matters for opinions on global policies, such as trade, globalization and immigration. However, as we see no evidence of an effect of information on relative national income on support for globalization (or for more generous immigration policies) in the hypothesized positive direction, it seems unlikely that a Stolper-Samuelson inspired framework holds much explanatory power.[41] We thus rather see the negative link between national relative income and the demand for global redistribution as suggestive of a reference group effect. When thinking about policies to reduce global inequality,

[40] Results reported in Appendix A.9.

[41] While the Stolper-Samuelson framework does not seem to explain the effects of relative income, we find that it can explain other features of policy preferences. Appendix A.13 present results from four questions included in the follow-up survey on how globalization and immigration affect the poor and the rich. Consistent with this framework, most people believe that the poor are typically worse off as a result of globalization and immigration while the rich are better off.

it seems to matter more where one stands in the national income distribution than in the global income distribution.

The observation of a Meltzer-Richard style effect for demand for redistribution that is driven by the left-of-center respondents may be explained by the fact that demand for redistribution captures both selfish and altruistic preferences, and the different role these play across the political spectrum. For right-of-center respondents, there are indications that higher national relative income is related both correlationally and causally to more giving to poor Germans and Kenyans, which could counter-act the effect of relative income on the part of redistributive preferences which reflects selfish rather than altruistic concerns. For respondents to the left there is scant evidence of such an altruism component, and instead we see them reducing their demand for redistribution, both at the national and the global level, in reaction to learning that they are richer than they thought at the national level. Therefore, we not only document significant heterogeneity (based on political leanings) of an information treatment effect on policy preferences, but are also able to partly explain why this heterogeneity arises.

6 Conclusions

Economic inequality is extremely prevalent in the world, on both national and global scales. National inequality has received abundant attention from researchers. As a result, significant knowledge has accumulated on patterns of national inequality and on individual preferences for national redistribution. However, this is not the case for global inequality. In this paper, we take first steps toward filling this gap in the literature. Using a two-year survey in a representative sample of German households, we begin by investigating the correlates of: (i) preferences for global (in addition to national) redistribution; (ii) attitudes toward globalization and immigration (that can arguably contribute to a reduction of global inequality); and (iii) willingness to give to the global poor.

While the aforementioned preferences conceivably depend on many factors, we investigate the importance of perceived relative income. We document substantial misperceptions about national and global relative income, which are similar in absolute magnitude. However, while the share of people over- and underestimating national relative income averages out in the population, a vast majority of Germans underestimate their global relative income. Taking advantage of some methodological innovations, we provide unique evidence that these misperceptions are meaningful and robust and do not reflect mere disinterest on the part of respondents. For example, we 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 further show that individuals are willing to pay non-trivial amounts for information about their global and national income ranks.

Our survey incorporated an incentivized experiment in which treated respondents received information about their true income ranks, both nationally and globally. This enabled us to study the causal effect of perceived national and global relative income on policy preferences. Consistent with previous work, we find that perceived rank in the national income distribution is a significant negative determinant of demand for national redistribution, at least among left-wing respondents. On the contrary, we find no evidence that perceived rank in the global income distribution affects support for global redistribution, donations to the global poor, globalization or immigration. If anything, when thinking about these policy preferences, it matters more how one compares to other people nationally than to others around the globe.

We studied preferences for policies addressing global inequality among people in one of the richest countries in the world. The vast majority of citizens would thus be net contributors to global redistribution. Our results indicate that poorer Germans may fail to realize that more extensive global redistribution would redistribute their income to other parts of the world where people are even poorer. Similarly, if migration from poor to rich countries continues to increase and issues about globalization and disintegration of markets (e.g., Brexit) intensify, we will likely see more economic pressure on the lower part of the income distribution in rich countries. Indeed, evidence suggests that globalization and immigration has contributed to the income growth of a "global middle class" (e.g., Milanovic, 2016; Weyl, 2018), but this has possibly come at the expense of the lower middle class in rich countries (Autor et al., 2013; Dauth et al., 2014; Autor et al., 2016; Lakner and Milanovic, 2016).

The findings of our study also highlight the complexity of the question of how to best address issues of global inequality. This is important, as none of the frameworks that guided our analysis cover the whole picture. Clearly, the present study is an early step in the process of better understanding the drivers of demand for global redistribution and further research is therefore needed in both developed and developing countries to fully understand how individuals form opinions on policies that address global inequality.

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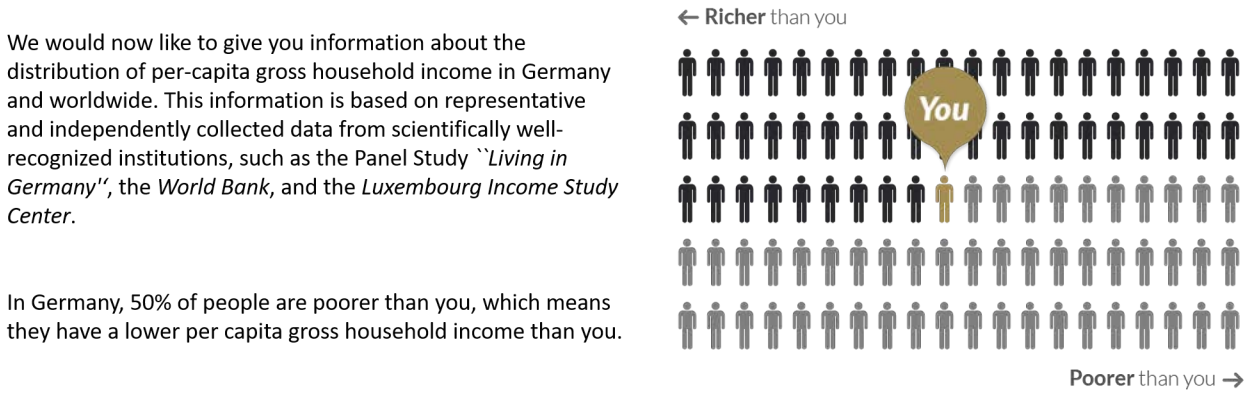
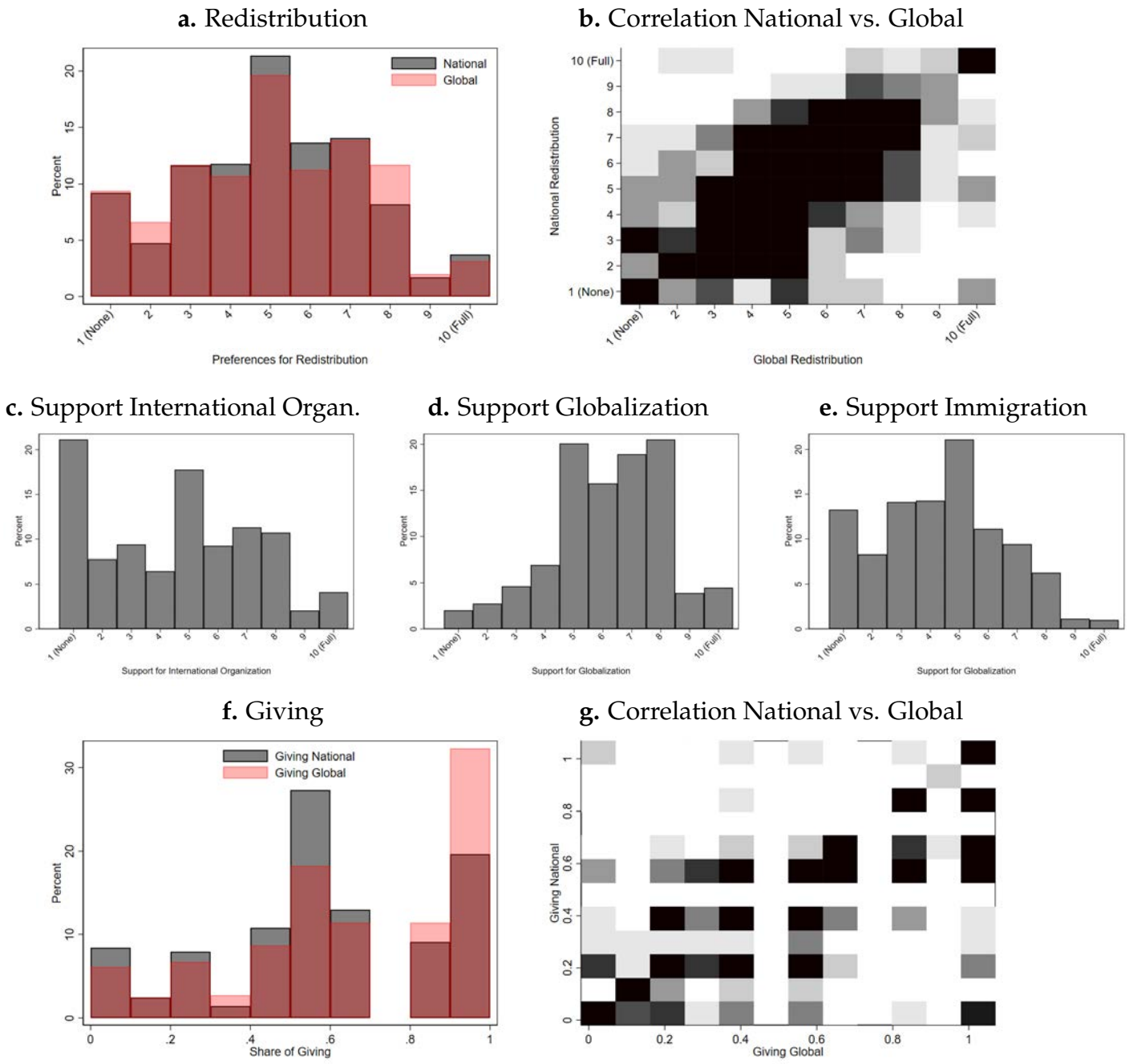
Figure 1: Screenshot of a Sample of the Information Treatment



Notes: Visualization of the information treatment providing information about actual relative income at the national level (information about actual global relative income was presented analogously). Respondents received first some general information on the data sources and then learned the share of people in Germany with less per-capita gross household income. The information was illustrated using customized graphs that indicated the relative position to make it easier to understand and digest.

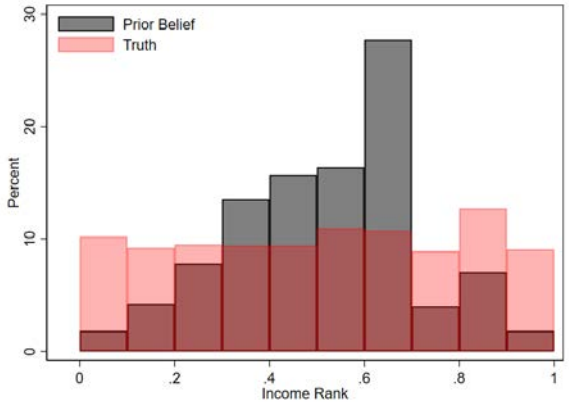
Figure 2: Distribution of Outcomes



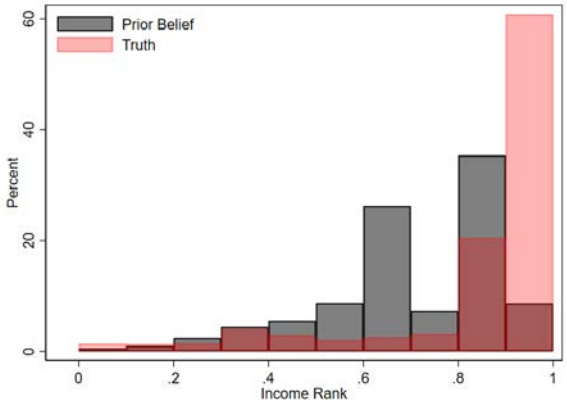
Notes: Distribution of preferences for national redistribution (gray) and global redistribution (red) in (a.) and their correlation in (b.) with darker areas indicating more responses in this area. Distribution of support for an international organization with a mandate to redistribute in (c.), support for globalization in (d.), and support for immigration in (e.), respectively. Distribution of the share of national giving (gray) and global giving (red) in (f.) and their correlation in (g.) with darker areas indicating more responses in this area. All panels use data from baseline survey control group.

Figure 3: Prior Beliefs about Income Rank vs. True Income Rank

a. National Income Rank



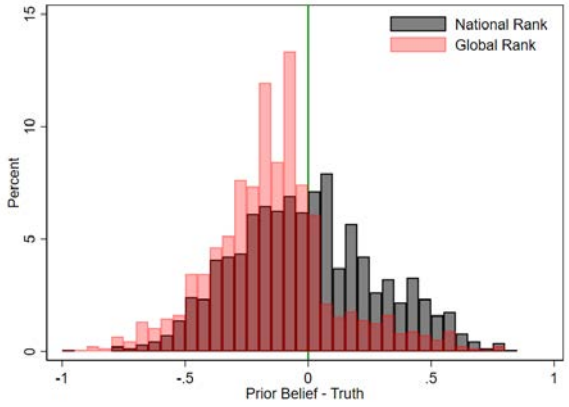
b. Global Income Rank



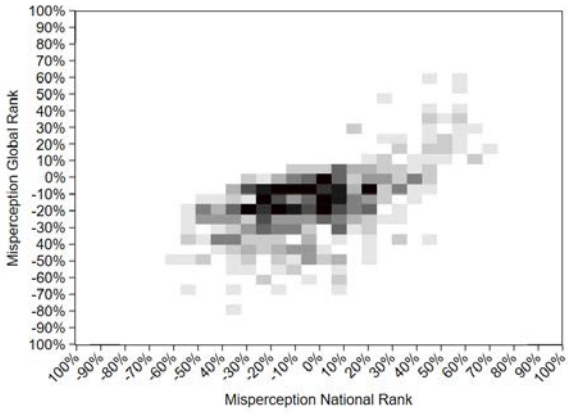
Notes: Distribution of prior beliefs about own income rank (gray) and the true income rank (red) at the national level (a.) and global level (b.). Data from baseline survey.

Figure 4: Misperceptions

a. Distribution of Misperceptions



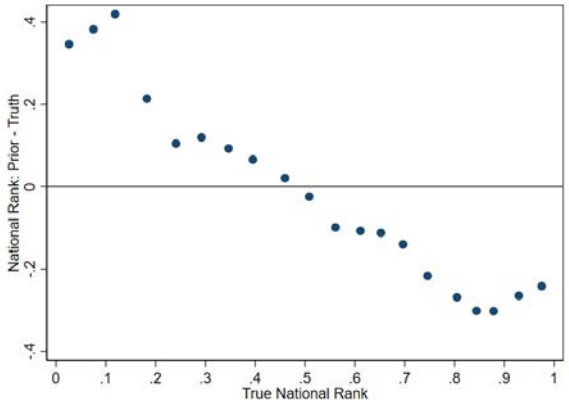
b. Correlation of Misperceptions



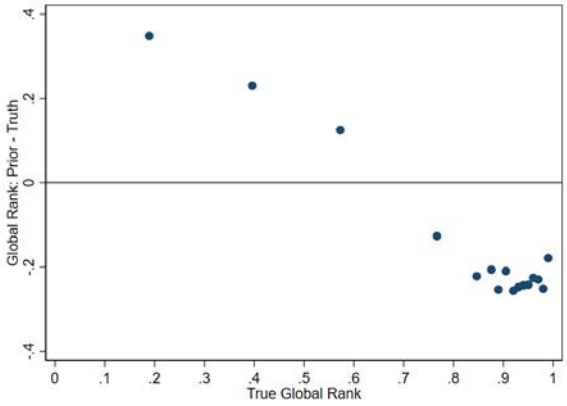
Notes: Distribution of misperceptions about income rank at the national level (gray) and global level (red) in (a.) and their correlation in (b.) with darker areas indicating more responses in this area. Misperceptions are calculated as difference between prior beliefs about income rank and true income rank. Positive (negative) differences correspond to overestimation (underestimation) of own income rank. Data from baseline survey.

Figure 5: Middle-Class Bias

a. National Income Rank



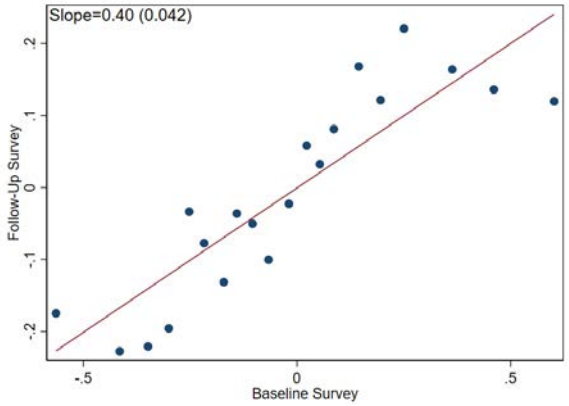
b. Global Income Rank



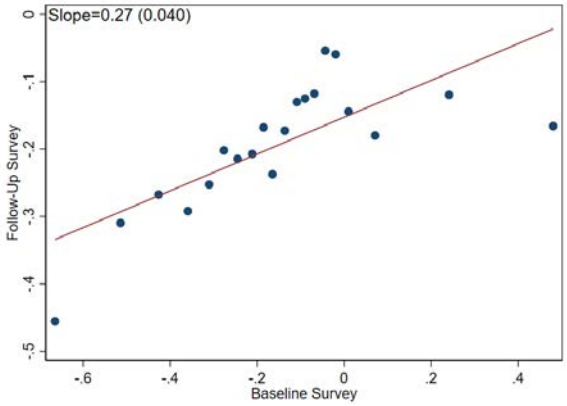
Notes: Binned scatterplots with 20 equally-sized bins showing the relationship between true income rank (x-axis) and misperceptions at the national level (y-axis) in (a.) and at the global level (y-axis) in (b.). Data from the baseline survey.

Figure 6: Year-over-year Persistence of Misperceptions

a. Prior Belief National Income Rank



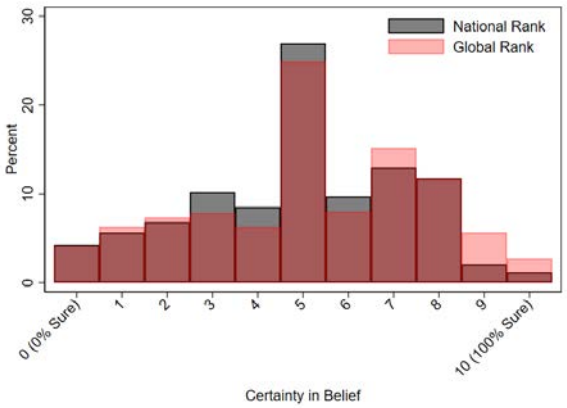
b. Prior Belief Global Income Rank



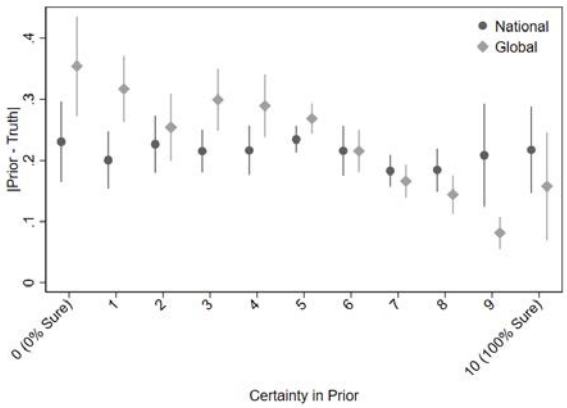
Notes: Binned scatterplots with 20 equally-sized bins showing the persistence of misperceptions between the baseline and the follow-up survey (one year later) for prior belief national income rank in (a.) and prior belief global income rank in (b.). Misperceptions are calculated as difference between prior beliefs about income rank and true income rank. Data from baseline and follow-up survey (control group only).

Figure 7: Confidence in Beliefs about Income Rank

a. Distribution of Confidence



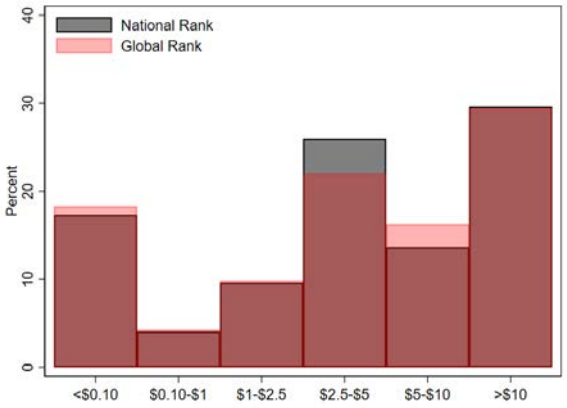
b. Confidence vs. Misperceptions



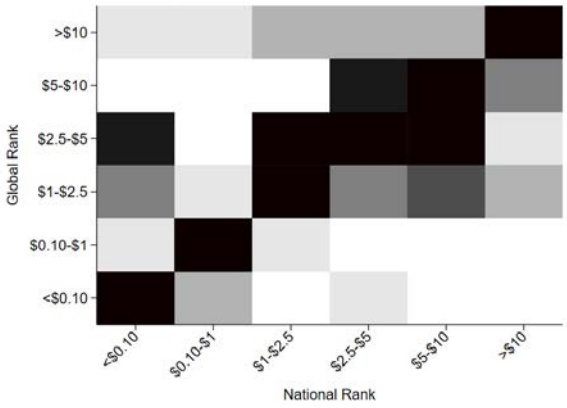
Notes: Distribution of reported confidence in beliefs about national income rank (gray) and global income rank (red) in follow-up survey in (a.) and coefficient plots of relationship between confidence and misperceptions for both national and global income rank in (b.). Misperceptions are calculated as difference between prior beliefs about income rank and true income rank. Data from follow-up survey (control group only).

Figure 8: WTP for Information on True Income Rank

a. Distribution



b. Correlation National vs. Global



Notes: Distribution of willingness to pay (WTP) for information on true national income rank (gray) and global income rank (red) in (a.) and their correlation in (b.) with darker areas indicating more responses in this area. Data from follow-up survey (control group only), excluding the 5 percent of the respondents who provided inconsistent answers.

Table 1: Pairwise Correlation between Redistribution Preferences, Giving, and Support for Immigration and Globalization

	Nat. Redist.	Glob. Redist.	Sup. Int. Org.	Giving Nat.	Giving Glob.	Sup. Global.	Sup. Immig.
National Redistribution	1.000						
Global Redistribution	0.695***	1.000					
Sup. Int. Org.	0.540***	0.586***	1.000				
Share Giving National	0.079***	0.111***	0.145***	1.000			
Share Giving Global	0.056**	0.118***	0.153***	0.749***	1.000		
Sup. Global.	0.122***	0.166***	0.205***	0.201***	0.187***	1.000	
Sup. Immig.	0.158***	0.263***	0.297***	0.283***	0.279***	0.323***	1.000

Notes: ***p-value < 0.01, **p-value < 0.05, *p-value < 0.1. Pairwise correlations between the various policy preferences. All variables are measured on a 1–10 scale with 1 indicating "no redistribution/no support" and 10 indicating "full redistribution/support," except "Giving National/Global," which is the share of giving and thus measured on a scale from 0 to 1. Data from the baseline survey.

Table 2: Correlates of Policy Preferences

	Nat. Redist. (1)	Global Redist. (2)	Sup. Int. Org. (3)	Giving Nat. (4)	Giving Glob. (5)	Sup. Global. (6)	Sup. Immig. (7)
Age	-0.008*	-0.016***	-0.014**	-0.001*	-0.002***	-0.013***	-0.018***
	(0.005)	(0.005)	(0.005)	(0.001)	(0.001)	(0.004)	(0.004)
Female (=1)	-0.047	-0.065	-0.090	0.028	0.072***	-0.383**	-0.036
	(0.173)	(0.179)	(0.207)	(0.023)	(0.024)	(0.151)	(0.165)
Education: Upper Secondary (=1)	-0.085	-0.239	-0.370*	-0.057**	-0.076***	-0.125	-0.580***
	(0.177)	(0.185)	(0.216)	(0.023)	(0.024)	(0.153)	(0.169)
Education: College & More (=1)	-0.076	-0.075	0.325	0.129***	0.138***	0.196	0.704***
	(0.198)	(0.213)	(0.262)	(0.027)	(0.028)	(0.169)	(0.194)
Equivalized Monthly Net HH Income (log)	-0.374*	0.046	0.451**	0.198***	0.213***	0.709***	0.787***
	(0.198)	(0.200)	(0.227)	(0.025)	(0.026)	(0.156)	(0.185)
Unemployed (=1)	0.456	0.522	-0.099	-0.114*	-0.142**	-0.401	-0.078
	(0.453)	(0.423)	(0.559)	(0.065)	(0.065)	(0.322)	(0.432)
East Germany (=1)	0.527***	-0.028	-0.843***	-0.114***	-0.119***	-0.465**	-1.184***
	(0.203)	(0.208)	(0.241)	(0.026)	(0.030)	(0.184)	(0.185)
German Citizenship (=1)	-0.574	-0.600	-1.145**	0.001	0.027	-0.547	-0.930***
	(0.456)	(0.469)	(0.487)	(0.055)	(0.051)	(0.350)	(0.342)
Political Orientation: Left-of-Center (=1)	1.014***	1.117***	1.260***	0.059**	0.070***	0.295*	1.039***
	(0.170)	(0.181)	(0.215)	(0.024)	(0.026)	(0.152)	(0.170)
Effort vs. Luck Belief (National)	0.174***	0.086	0.100	0.012*	0.011	-0.030	0.031
	(0.054)	(0.056)	(0.065)	(0.007)	(0.007)	(0.048)	(0.053)
Effort vs. Luck Belief (Global)	0.174***	0.121**	0.128**	0.026***	0.025***	0.075*	0.138***
	(0.047)	(0.050)	(0.055)	(0.006)	(0.006)	(0.040)	(0.044)
Risk Aversion	-0.041	-0.079*	-0.050	-0.004	-0.006	0.011	-0.037
	(0.038)	(0.041)	(0.049)	(0.005)	(0.005)	(0.034)	(0.038)

Notes: ***p-value < 0.01, **p-value < 0.05, *p-value < 0.1. OLS regressions with robust standard errors in parentheses using data from the baseline control group. Columns display coefficients from separate regressions for each control variable. All dependent variables are measured on a 1–10 scale with 1 indicating "no redistribution/no support" and 10 indicating "full redistribution/support," except "Giving National/Global," which is the share of giving and thus measured on a scale from 0 to 1. All controls are defined as binary variables (except Age, Monthly Net HH Income, Effort vs. Luck Belief (National/Global), and Risk Aversion). "Effort vs. Luck Belief (National/Global)" is measured on a 1–10 scale with higher numbers indicating a stronger belief that luck determines economic success and "Risk Aversion" is measured on a 0-10 scale with higher numbers indicating less risk aversion.

Table 3: Effects of Information Provision on Beliefs about Income Rank One Year Later

	Prior Belief in Follow-Up Survey				Prior Belief in Baseline Survey			
	(1) National	(2) National	(3) Global	(4) Global	(5) National	(6) National	(7) Global	(8) Global
National Rank: Treat*(Feedback - Prior)	0.153***	0.196***			0.024	0.008		
	(0.040)	(0.044)			(0.023)	(0.029)		
National Rank: Peer Treatment*(Feedback - Prior)		0.152***				-0.040		
		(0.058)				(0.034)		
Global Rank: Treat*(Feedback - Prior)			0.122***	0.160***			-0.018	-0.014
			(0.043)	(0.046)			(0.024)	(0.027)
Global Rank: Peer Treatment*(Feedback - Prior)				0.109*				0.011
				(0.064)				(0.038)
Observations	1,137	1,137	1,122	1,122	1,222	1,137	1,122	1,122

Notes: ***p-value < 0.01, **p-value <0.1, *p-value<0.1. OLS regressions estimating the effect of information provision on beliefs about income rank one year later using data from the follow-up survey (Columns 1-4) and from the baseline survey (Columns 5-8). Standard errors clustered at the household level in parentheses. The dependent variables are the perceived income rank in the national and global income distribution (prior beliefs). "Peer Treatment" takes the value 1 if the respondent did not receive the information but another member of her household and 0 otherwise (i.e., if the respondent received the information or if none of the household members received the information). The control variables used in the analysis are HH gross income, the number of household members, the prior belief about the income rank, the change in the true income rank in between the two surveys, a set of dummies for the number of survey respondents in the household, and the following demographic characteristics: age and dummies for gender, education, disability, unemployment, retirement, self-employment, political party and East Germany.

Table 4: Effects of Information Provision on Belief Certainty One Year Later

	Certainty in Follow-Up Survey			
	(1) National	(2) National	(3) Global	(4) Global
Treatment	0.434***	0.462***	0.622***	0.807***
	(0.137)	(0.168)	(0.145)	(0.178)
Peer Treatment		0.076		0.506**
		(0.218)		(0.234)
Observations	1,139	1,139	1,125	1,125

Notes: ***p-value < 0.01, **p-value <0.1, *p-value<0.1. OLS regressions estimating the effect of information provision on confidence about prior beliefs on income rank one year later using data from the follow-up survey. Standard errors clustered at the household level in parentheses. The dependent variable is the confidence in stated prior beliefs on income rank (at the national and global level) measured on a 1–10 scale emulating steps of 10 percent. "Treatment" is an indicator for treatment information on relative income and "Peer Treatment" takes the value 1 if the respondent did not receive the information but another member of her household and 0 otherwise (i.e., if the respondent received the information or if none of the household members received the information). The control variables used in the analysis are HH gross income, the number of household members, the prior belief about the income rank, the change in the true income rank in between the two surveys, a set of dummies for the number of survey respondents in the household, and the following demographic characteristics: age and dummies for gender, education, disability, unemployment, retirement, self-employment, political party and East Germany.

Table 5: Correlation between Prior Beliefs about Income Rank and Policy Preferences

(a)	All						
	(1) Nat. Redist.	(2) Glob. Redist.	(3) Sup. Int. Org	(4) Giving Nat.	(5) Giving Glob.	(6) Sup. Global.	(7) Sup. Immig.
Prior Belief National Rank	-0.520**	-0.142	-0.012	0.942***	0.780***	0.417*	0.462**
	(0.240)	(0.232)	(0.225)	(0.222)	(0.235)	(0.246)	(0.232)
Prior Belief Global Rank	0.117	0.124	0.257	0.161	0.172	-0.110	-0.128
	(0.232)	(0.230)	(0.231)	(0.211)	(0.227)	(0.256)	(0.247)
Observations	683	679	667	689	688	680	687
(b)	Left-of-center						
	(1) Nat. Redist.	(2) Glob. Redist.	(3) Sup. Int. Org	(4) Giving Nat.	(5) Giving Glob.	(6) Sup. Global.	(7) Sup. Immig.
Prior Belief National Rank	-0.935***	-0.972***	-0.814**	0.532	0.233	-0.248	-0.007
	(0.354)	(0.352)	(0.394)	(0.394)	(0.399)	(0.403)	(0.371)
Prior Belief Global Rank	0.591	0.615	1.171***	1.179***	1.526***	0.729*	0.445
	(0.367)	(0.385)	(0.401)	(0.348)	(0.408)	(0.419)	(0.460)
Observations	235	235	232	238	238	235	234
(c)	Center/Right-of-center						
	(1) Nat. Redist.	(2) Glob. Redist.	(3) Sup. Int. Org	(4) Giving Nat.	(5) Giving Glob.	(6) Sup. Global.	(7) Sup. Immig.
Prior Belief National Rank	-0.497	0.129	0.204	1.033***	0.916***	0.683**	0.505*
	(0.309)	(0.299)	(0.265)	(0.265)	(0.279)	(0.303)	(0.293)
Prior Belief Global Rank	-0.131	-0.111	-0.189	-0.331	-0.480*	-0.505*	-0.419
	(0.276)	(0.274)	(0.261)	(0.252)	(0.257)	(0.303)	(0.285)
Observations	448	444	435	451	450	445	453

Notes: ***p-value < 0.01, **p-value < 0.05, *p-value < 0.1. OLS regressions with standard errors clustered at the household level in parentheses using data from the baseline survey control group. All dependent variables are standardized by subtracting the control group mean from each observation and then dividing by the control group standard deviation. Prior Belief National (Global) Rank is the perceived relative rank in the national (global) income distribution. Panel (a) uses data for all respondents in the baseline survey control group, panel (b) displays results for left-of-center respondents and panel (c) displays results for center/right-of-center respondents. Left-of-center is defined as *below the median response of 5* on the self-assessment scale (0-10) for political orientation from left to right, whereas center/right-of-center subsumes respondents *at or above the median (5)* on this scale. Analysis does not include control variables.

Table 6: Experimental Results: Effects of Information Provision on Policy Preferences

(a)	All						
	(1) Nat. Redist.	(2) Glob. Redist.	(3) Sup. Int. Org.	(4) Giving Nat.	(5) Giving Glob.	(6) Sup. Global.	(7) Sup. Immig.
National Rank: Treat*(Feedback - Prior)	-0.189	-0.220	-0.294	0.323	0.214	0.022	-0.121
	(0.251)	(0.245)	(0.245)	(0.234)	(0.246)	(0.266)	(0.243)
Global Rank: Treat*(Feedback - Prior)	0.016	0.117	0.171	0.038	-0.011	-0.246	0.092
	(0.262)	(0.246)	(0.240)	(0.240)	(0.247)	(0.258)	(0.241)
Observations	1350	1341	1325	1357	1357	1345	1358
(b)	Left-of-center						
	(1) Nat. Redist.	(2) Glob. Redist.	(3) Sup. Int. Org.	(4) Giving Nat.	(5) Giving Glob.	(6) Sup. Global.	(7) Sup. Immig.
National Rank: Treat*(Feedback - Prior)	-0.774*	-0.932**	-1.047**	-0.065	-0.268	-0.487	-0.430
	(0.457)	(0.392)	(0.459)	(0.451)	(0.457)	(0.470)	(0.411)
Global Rank: Treat*(Feedback - Prior)	0.152	0.125	0.552	0.541	0.411	-0.071	0.669
	(0.492)	(0.443)	(0.468)	(0.450)	(0.467)	(0.512)	(0.459)
Observations	454	452	447	458	457	454	454
(c)	Center/Right-of-center						
	(1) Nat. Redist.	(2) Glob. Redist.	(3) Sup. Int. Org.	(4) Giving Nat.	(5) Giving Glob.	(6) Sup. Global.	(7) Sup. Immig.
National Rank: Treat*(Feedback - Prior)	0.088	0.117	0.101	0.501*	0.459	0.274	-0.007
	(0.295)	(0.302)	(0.288)	(0.279)	(0.299)	(0.314)	(0.291)
Global Rank: Treat*(Feedback - Prior)	0.013	0.172	0.057	-0.139	-0.170	-0.330	-0.026
	(0.299)	(0.287)	(0.277)	(0.278)	(0.287)	(0.281)	(0.267)
Observations	896	889	878	899	900	891	904

Notes: ***p-value < 0.01, **p-value < 0.05, *p-value < 0.1. OLS regressions estimating the effect of information provision on policy preferences using data from the baseline survey. Standard errors clustered at the household level in parentheses. All dependent variables are standardized by subtracting the control group mean from each observation and then dividing by the control group standard deviation. Panel (a) uses data for all respondents, panel (b) displays results for left-of-center respondents, and panel (c) displays results for center/right-of-center respondents. Left-of-center is defined as *below the median response of 5* on the self-assessment scale (0-10) for political orientation from left to right, whereas center/right-of-center subsumes respondents *at or above the median (5)* on this scale. The control variables used in the analysis are the prior misperceptions about the national and global income rank, and the following demographic characteristics: age and dummies for gender, education, disability, unemployment, retirement, self-employment, political party and East Germany.