Perceptions of Equality of Opportunity and Inequality Acceptance[*]

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Abstract

This paper examines the relationship between perceptions of equality of opportunity and distributional preferences. We run a survey experiment in a large sample of Germans and randomly inform respondents about social mobility in Germany. While this information treatment renders perceptions of equality of opportunity significantly more pessimistic, it changes neither revealed distributional preferences nor pro-social behavior toward the poor and the rich. The large sample size allows us to rule out economically meaningful treatment effects. We provide some suggestive evidence that respondents do not link the lack of equality of opportunity to the role of luck.

Keywords: social mobility, distributional preferences, inequality, survey experiment

JEL Classification: C93, D31, H23, H24, H41

1 Introduction

Distributional preferences underlie many economic decisions, but they are particularly relevant for social policy preferences, including redistribution, taxation, or transfers, as any of these policies involve gains for some groups and losses for others.[1] Understanding the origin of the preferences underlying these policies becomes increasingly important as inequality is on the rise in many countries around the world. This rise in inequality has fueled a debate about the importance of family background to economic success. The observation that inequality is negatively related to social mobility between and within countries (Corak 2006, 2013; Chetty et al. 2014), popularized by Alan Krueger (2012) as the "Great Gatsby Curve," suggests that low social mobility may even contribute to rising inequality. In addition, information about social mobility can be a signal of the equality of opportunity in a society and is linked to what one can achieve with effort and hard work. If people care about the fairness of a society, and meritocratic principles in particular (as for example suggested by Almås, Cappelen, and Tungodden 2020), learning that social mobility is lower than expected should affect their acceptance of inequality.

This paper investigates the suggested link between social mobility and distributional preferences. We are particularly interested in two questions. First, do people's perceptions of equality of opportunity affect their distributional preferences? Second, do these perceptions specifically affect pro-social behavior toward the poor and the rich, respectively? We address these questions using a pre-registered survey experiment, comprising about 2,500 respondents in a high-quality online survey, the German Internet Panel (GIP).[2] To identify the causal effect of equality of opportunity perceptions on inequality acceptance, we expose a randomly selected subsample of respondents to information about social mobility in Germany. Estimates of intergenerational earnings mobility typically locate Germany somewhere between the two extremes, the Scandinavian countries and the US (Corak 2013).[3] In our study, half of the respondents receive stylized infor-

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[1] By *distributional preferences* we mean preferences that not only take one's own income as argument but also other people's income. Although related, these preferences differ from *preferences for redistribution*, which express preferences over the degree of redistribution in a society. Mounting evidence indicates that distributional preferences influence economic and political behavior in a variety of domains, ranging from donations to charities (Derin-Güre and Uler, 2010, Kamas and Preston, 2015) and public goods provision (Hedegaard et al., 2019, Offerman et al., 1996) to expert behavior in credence goods markets (Kerschbamer et al., 2017), competitive behavior (Balafoutas et al., 2012), and voting preferences (Fisman et al 2017; Kerschbamer and Müller 2020, Epper, Fehr, and Senn 2020).

[2] The GIP is representative along several observables, offers tight control over who is participating, includes detailed socio-economic background information on respondents, and provides a host of information on respondents' political attitudes (Blom, Gathmann, and Krieger 2015).

[3] Recent research shows that intergenerational elasticity, a common measure of intergenerational mobility, is similar in Germany and in the US, yet emphasizes at the same time that comparability across countries is sensitive to the choice of income measures (Schnitzlein 2016).

mation on the share of people advancing from the bottom to the top quartile of the income distribution, and vice versa. This information reveals significant intergenerational persistence of socio-economic status in Germany. Subsequently, we measure a respondent's belief about the dependence of economic success on parental socio-economic status, i.e. the perceived inequality of opportunity. We then elicit respondents' distributional preferences via an incentivized allocation task, the Equality-Equivalence Test (EET, Kerschbamer 2015). We complement this outcome measure with respondents' views on two important policies to curb inequality (specifically, their support for redistribution and education expenditures).

The EET is an intuitive, theory-driven task to measure distributional preferences that has been shown to predict voting behavior, political ideology, political attitudes towards redistribution and immigration, trust and cooperative preferences (Hedegaard et al. 2019; Kerschbamer and Müller 2020). The EET asks a decision-maker a series of binary decisions between different allocations of money for herself and some other passive recipient (another respondent in the GIP). These choices then allow us to classify the decision-maker into a well-defined set of mutually exclusive distributional types. In addition, the test delivers two measures of preference intensity (the *scores*) that we use in our analysis. These scores capture benevolence and malevolence – defined as the willingness-to-pay to increase (decrease) another person's income – in the domain of advantageous and disadvantageous inequality. We also introduce a novel feature into the EET and inform a randomly chosen subset of decision-makers about the actual relative income situation of the recipient, i.e., whether the recipient will be drawn from the bottom or top 10% of the income distribution of the GIP. This feature allows us to assess whether presenting pessimistic information on social mobility affects pro-social behavior toward those who are most likely to have suffered or benefited from limited social mobility.

Embedding this incentivized measure of distributional preferences into a population survey has several advantages over survey measures of other-regarding preferences. First, the EET gives us a more precise measure than hypothetical choices. Second, it offers tight control over the decision context allowing us to present the exact same decision situation to each respondent avoiding concerns about misconceptions of survey questions. Finally, it overcomes a common critique that survey measures of other-regarding preferences and preferences for redistribution do not capture actual behavior and are prone to experimenter demand effects.

We present three main findings. First, informing respondents about the persistence of socio-economic status in their society has a large and significant effect on their perceptions of equality of opportunity. As expected, treated respondents are more likely than untreated respondents to assert that economic success in adulthood depends on growing up in a poor or rich household. That is, they are more pessimistic about equality of opportunity in society. The effect size is large and comparable to other studies using information interventions (see Haaland, Roth, and Wohlfahrt 2020 for a comprehensive review of information provision experiments). Moreover, when we inform respondents that the recipient in the EET belongs to the top or the bottom 10%

of the income distribution, we find that this information affects distributional preferences. Specifically, we observe a sizeable shift towards less malevolence if decision-makers face a recipient from the bottom 10% of the income distribution, i.e., a lower willingness-to-pay to decrease the recipients' income. Similarly, we observe a shift towards less benevolence when facing a recipient from the top 10% of the income distribution, i.e., a lower willingness-to-pay to increase the recipients' income. This finding highlights that respondents' distributional preferences are not generally invariant to information, as behavior responds to information on socio-economic status of recipients in a predictable way.

Given the strong first-stage effect and respondents' reaction to the socio-economic status of recipients, our second set of results about the effect of information about social mobility on distributional preferences is striking. Despite the more pessimistic view on equality of opportunity, we find that the treatment has no detectable effect on distributional preferences. For instance, we do not find that treated respondents become more averse to inequality than respondents in the control group. This result also holds when we control for respondents' distributional preferences measured prior to our intervention. Consistent with these findings, but in contrast to the literature, we also find no effect on preferences for redistribution and on preferences for education expenditures. The null effects for both distributional preferences and policy preferences are precisely estimated and we are able to rule out even small effect sizes in all specifications.

Third, the information on social mobility does not amplify the shift in distributional preferences that we observe when we inform respondents that the recipient in the EET belongs to the top or the bottom 10% of the income distribution. More specifically, information about social mobility makes respondents neither more benevolent toward recipients from the bottom 10% nor less benevolent toward the top 10%, despite leading to greater pessimism about equality of opportunity compared to the control group. That is, we find no interaction effect between the two treatment variations.

To shed more light on the lack of a measurable effect on distributional preferences, we investigate how different (pre-registered) groups of individuals react to information on social mobility. It is possible that some groups of individuals respond more strongly, while most others do not respond to the treatment, resulting in a zero average treatment effect. In fact, we find that our treatment has a stronger effect on participants in the bottom 25% of the income distribution who become less inequality averse in response to pessimistic information on social mobility. However, we find no evidence that political ideology, occupation and beliefs about the determinants of economic success play a role. In particular, we observe that respondents believing more in effort as a determinant of success react in the same way to the presented information about social mobility as respondents believing more in luck. Additionally, we find that beliefs about the role of luck do not correlate with perceived inequality of opportunity in our control group. Both observations cast doubt on whether people are conscious of the fact that being rich or poor is to a large extent beyond someone's control in a society with low social mobility. If respondents do not view the

provided information as evidence that luck predominantly determines economic success, then there may be no reason to reduce inequality.

Together, the lack of a measurable effect on distributional preferences suggests an explanation for the often observed disagreement between people's increasing concerns about inequality and appropriate policy measures: Support for policy measures may not change that easily because the underlying preferences are slow-moving. The presented pessimistic information on social mobility is not sufficient to instate an immediate response of preferences despite its sizable impact on perceptions of equality of opportunity. Our null finding is valuable beyond this possible explanation as it is based on a large sample and a precisely measured outcome in a well-identified setting with an arguably low prior on the null hypothesis (see Abadie 2020).

We contribute to several strand of literatures. First, our work contributes to the literature that studies the foundations and the consequences of distributional preferences. Several papers have provided evidence for a strong heterogeneity of these preferences in lab populations (e.g. Andreoni and Miller 2002; Charness and Rabin 2002; Engelmann and Strobel 2004; Fisman, Kariv, and Markovits 2007; Cappelen et al. 2007; Bruhin et al. 2019) and in the general population (Bellemare, Kröger, and van Soest 2008; Fisman, Jakiela, Kariv, and Markovits 2015; Hedegaard et al. 2019; Kerschbamer and Müller 2020). More recently, a few papers have shown that broader societal attitudes, macroeconomic conditions and culture can affect distributional preferences (see e.g., Fisman, Jakiela, and Kariv 2015; Falk et al. 2018; Almås, Cappelen, and Tungodden 2020). We add to this emerging literature by exploring how the fairness of society in terms of perceived equality of opportunity influences distributional preferences.

Second, our study contributes to the literature on the relationship between social mobility and redistribution. Thus, a cornerstone of our work is the theoretical literature linking social mobility to people's support for redistribution (e.g. Piketty 1995; Alesina and Angeletos 2005). For instance, Bénabou and Ok (2001) show that poorer people do not necessarily support redistribution efforts because they expect to be richer in the future (and thus lend credence to the prospect of upward mobility). Other recent work by Alesina, Stantcheva and Teso (2018) documents perceptions of social mobility in France, Italy, Sweden, the UK, and the US and show that left-leaning (but not right-leaning) respondents demand more redistribution if they become more pessimistic about social mobility. In contrast to this work, we focus on how equality of opportunity perceptions affect distributional preferences and pro-social behavior toward the rich and poor. This outcome measure shapes individual opinions on social policies and offer, in contrast to survey measures of policy preferences, a clear interpretation and are less prone to a mismatch of stated attitudes and actual behavior, and context effects, such as attitudes toward the government.

Finally, the study also relates to a handful of papers documenting individual misperceptions about relative income and inequality (Norton and Ariely 2011; Page and Goldstein 2016; Hauser and Norton 2017), and to recent studies using randomized information treatments to estimate how information about relative income, inequality, and inherited wealth affects inequality

acceptance (Cruces, Perez-Truglia, and Tetaz 2013; Kuziemko et al. 2015; Karadja, Mollerstrom, and Seim 2017; Bastani and Waldenstrom 2019; Fehr, Mollerstrom, and Perez-Truglia 2021). Unlike these papers and most other work on the formation of social policy preferences, we concentrate on how perceived equality of opportunity shapes distributional preferences, which are fundamental input of policy preferences. Our results show that distributional preferences are more or less stable in response to changes in the perceptions of social mobility, which may explain why policy preferences do not change that easily in response to "informational" shocks.

2 Survey Experiment

2.1 Data Collection

We designed a survey module for the German Internet Panel (GIP). The GIP is an online panel survey maintained by the University of Mannheim and is based on a probability sample of the general German population aged 16 to 75 years.[4] The panel includes about 5,000 registered participants who are invited to take part in a bi-monthly online survey. The surveys typically include questions regarding attitudes toward political reforms, social policies, education and politics in general, and it collects and updates socio-demographic information of participants once a year.

We implemented our module in wave 33 of the GIP, which was fielded in January 2018 (Blom et al. 2018). In total, 2,684 participants took part in this wave and 2,656 participants completed our module. In addition, we also rely on information from previous waves of the GIP. In particular, we draw on socio-economic details provided by participants in wave 31, and on occupational status from wave 19. We specified all variables and hypotheses in a pre-analysis plan (PAP) that we registered in the AEA RCT Registry (AEARCTR-0002764) in March 2018 before we had access to the data.

2.2 The Survey Module

Main Survey: The survey module consists of four parts (see Figure 1 for a graphical overview and Section S7 in the Supplementary Material for a transcript of the survey module). The first part contains a single question about the role of luck and effort in economic success. After briefly explaining how life outcomes can depend on luck and effort, we asked respondents to what extent these two factors determine economics success on a scale from 0 (only luck) to 10 (only effort). Beliefs about the importance of luck for economic success are tightly linked to inequality acceptance (Alesina et al. 2001; Fong 2001; Corneo and Grüner 2002; Alesina and La Ferrara 2005; Alesina and Angeletos 2005) and thus may also be related to individuals' views about intergenerational mobility. This question is followed by another unrelated survey module eliciting attitudes

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[4] The recruitment of survey participants was done in face-to-face interviews and thus includes people without internet access at the time of recruitment (these people received tablets with internet access to participate in the survey). See Blom, Gathmann, and Krieger (2015) for more details on the GIP.

toward politics in general and the EU in particular to avoid pushing respondents into a particular direction before our treatment intervention.

The second part comprises our main intervention. Half of the respondents received information on the persistence of socio-economic status in Germany (the treatment group). As our intervention aimed at shifting subjects' perception of equality of opportunity toward greater pessimism, we focused on upward mobility from the bottom to the top quartile *and* on downward mobility from the top to the bottom quartile. These two statistics are a commonly used measure of social mobility in the literature. Although this information is based on most recent evidence for Germany (see Schnitzlein 2016, and Stockhausen 2017), we presented and visualized the key information in an easy-to-digest way to facilitate understanding. That is, we first introduced the topic and explained the specific concepts of upward and downward mobility. We then stated, without referring to numbers, that the probability of being rich in adulthood is very low for children born in poor households and the probability of staying poor high. We also illustrated these relationships graphically. We present the information on downward mobility in the same way (see Section S1 in the Supplementary Material for details and screenshots).

Immediately after the intervention, we assessed the impact of the information treatment. We asked respondents to imagine 100 households that represent Germany and asked them to answer the following question: "To what extent does economic success as an adult depend on whether one has grown up in the poorest 25 households or in the richest 25 households?" on a 10-point scale ranging from "very little (1)" to "very strong (10)". There a few things to note here. First, the question deliberately used a different wording compared to the treatment information and elicited respondents' perceived equality of opportunity in society. Second, we can directly observe how respondents process and interpret the provided information on downward and upward mobility. Third, compared to quantitative measures, this qualitative measure is less likely subject to demand effects. Fourth, we did not elicit beliefs about social mobility prior to the treatment (see also Haaland, Roth, and Wohlfahrt 2020 for a discussion of potential problems when eliciting prior beliefs) as we are not primarily interested in changes in perceptions about social mobility.

In the third part, we elicited the distributional preferences of all respondents using a version of the Equality Equivalence Test (Kerschbamer 2015), which we explain in more detail below. This test requires respondents to make a series of incentivized binary choices between unequal monetary allocations involving themselves and another respondent. A novel feature of our implementation of the Equality Equivalence Test (EET) is that we inform half of the respondents about the relative position of their matched recipient in the income distribution. We randomly assign 25% of decision-makers to a recipient from the top 10% of the income distribution in the GIP (*rich* treatment) and 25% of decision-makers to a recipient from the bottom 10% of the income

distribution (*poor* treatment).[5] The remaining 50% of decision-makers received no information about their recipient (*neutral* treatment), except that he or she is another respondent taking part in the GIP. In addition, we take advantage of the longitudinal character of the GIP and that distributional preferences were already measured in a previous wave using the same instrument (see below for a more detailed explanation). This allows us to investigate intra-personal changes of distributional preferences in response to the treatment and avoids issues such as consistency bias in responses and demand effects.

Finally, in the fourth part we elicit preferences regarding two policy measures: redistribution and educational spending. First, we explained that economic redistribution reduces income disparities between citizens through taxes and transfer payments and asked respondents how much redistribution they want in society on an 11-point scale ranging from "no redistribution" to "full redistribution." Second, we are interested in respondents' views on government education expenditures, and thus ask whether the government should spend more or less on education (on a five-point scale ranging from "spend much more than now" to "much less than now"). These questions deliberately leave some room for interpretation, and thus may cover broader aspects of inequality acceptance than our incentivized measure of distributional preferences.

Follow-up survey: To assess the persistence of the treatment on policy preferences, and to differentiate possible priming effects from genuine learning from the information, we draw on a follow-up wave of the GIP, which took place about 4 months after our intervention. This wave included the exact same measure for education expenditures and a slightly different question on respondents' preferences for redistribution ("The government should take measures to reduce income disparities." using a five-point scale from "fully agree" to "fully disagree"). The attrition rate between the two waves is comparatively low (11 percent) and there is no differential attrition between treatments (11 percent in the control and 12 percent in the treatment group, t-test, $p = 0.48$).

2.3 The Equality-Equivalence Test

The EET (Kerschbamer 2015) is a parsimonious tool for identifying the distributional preferences of decision-makers by allowing the experimenter to infer the slope of a decision-maker's indifference curve in the self–other space.[6] The test relies on four basic assumptions on a decision-maker's preferences that ensure well-behaved indifference curves that run through an equal reference allocation r and also pass through a specific area above and below the 45-degree line. Figure 2 illustrates the three areas above the 45-degree line – x_1, x_2 or x_3 – and the three areas below –

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[5] Note that we did not disclose any information to decision makers other than saying that we will match them with a recipient from the bottom or top 10% of the income distribution, which was done after the survey ended.

[6] The self–other space is an Euclidean plane with income to *self, m,* on the x-axis and income to *other, o,* on the y-axis.

y_1, y_2 *or* y_3. The combination of these areas above and below the 45-degree line identifies the distributional type of a decision-maker. The standard selfish type, for example, has vertical indifference curves that run through x_2 and y_2.[7] An inequality-averse decision-maker (Fehr and Schmidt, 1999) exhibits indifference curves that run through x_3 and y_3. That is, they are characterized by a positive slope (malevolence) in the domain of disadvantageous inequality (areas above the 45-degree line) and a negative slope (benevolence) in the domain of advantageous inequality (areas below the 45-degree line). Virtually all distributional types proposed in the economics literature can be represented in this way.

Empirically, the EET elicits the slope of the indifference curve, that runs through an arbitrarily chosen equal reference point, in both the domain of disadvantageous and advantageous inequality (i.e. the slope above and below the 45-degree line). The core of the experimental procedure thus consists of a series of binary decisions between two allocations of money for the decision-maker, the *self*, and a passive anonymous recipient, the *other*. In each allocation decision, one unequal allocation is compared to the same fixed equal reference allocation. In our implementation of the EET, we use 10 euro to *self* and *other* (10, 10) as an equal reference allocation. We compare this allocation to three sets of allocations in the domain of disadvantageous inequality (*x-lists*) and to three sets of allocations in the domain of advantageous inequality (y-*lists*). In the three *x-lists,* payoffs to *other* are either 13, 15, or 17 euros, while the payoff to *self* was incrementally increased from 7 to 16 euros. In the *y-lists*, we fix payoffs to the *other* at 3, 5, and 7 euros and incrementally increase the payoff to *self* from 5 to 14 euros (see Figure S1 in the Supplementary Material). The order of the lists was randomized at the individual level. In the empirical analysis below, we use the average x- and the average y-score as dependent variable (as specified in the pre-analysis plan).

The switching point from the equal reference allocation to the unequal allocation indicates the interval (of income to *other*) through which the indifference curve must run. Multiple switching points are ruled out by monotonicity, i.e. a decision-maker strictly prefers more material payoffs to less material payoffs, while holding *other* material payoffs constant.[8] In addition, the switching point yields a measure of preference intensity in the sense that the earlier a decision-maker switches from equal to unequal in the *x-list (y-list)*, the more (less) benevolent she is. The *x-score* and the *y-score* summarize these intensities in the *x* and the *y-list*, respectively.[9] In both

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[7] Note that the test cannot exactly identify vertical indifference curves, but only with "arbitrary precision". Thus, selfishness constitutes a free test parameter. We define an individual as selfish if her indifference curves are within a 50 euro cent range of the vertical line through the equal reference allocation of (10,10).

[8] Consequently, we rule out inconsistencies in the experiment by design. More precisely, respondents indicate the row in which they prefer to switch for the first time. The interface then automatically highlighted all preferred allocations within that list and respondents could revise their choice and go back and forth between the different lists.

[9] In our case, the x-score (y-score) is calculated as 6.5 – row (row - 5.5) where *row* indicates the row number in which the respondent switched from the equal to the unequal allocation.

domains, a positive score implies benevolence towards the passive recipient where benevolence is defined as a willingness-to-pay to increase the payoff of *other* (i.e., a negatively-sloped indifference curve). Conversely, a negative score implies malevolence toward the recipient, i.e. the decision-maker displays a willingness-to-pay to decrease the payoff to the recipient (i.e., a positively-sloped indifference curve). Inequality averse decision-makers, for example, display a positive *y-score* and a negative *x-score*. Moreover, the higher (lower) a score, the more benevolent (malevolent) a decision-maker is.

The overwhelming majority of respondents (89%) previously completed the EET (using the same parameterization) in wave 23 in spring 2016 and were thus familiar with the test and procedures (see Kerschbamer and Müller 2020 for more details). Payments to respondents were determined after the field time of wave 33 in spring 2018. We randomly selected 250 respondents for payment of their decisions in the EET. For each of these decision-makers, we first randomly drew one list and then one row in this list. We paid out the decision in this row to both the decision-maker and a recipient. Accordingly, we also randomly selected 250 respondents as recipients and matched each of them to one decision-maker. In the *rich* treatment, we drew recipients from the top 10% of the income distribution; in the poor treatment, we drew from the bottom 10%; and in the *neutral* treatment we drew recipients from all participating respondents. Selected respondents (both in the role as decision-maker or recipient) received an e-mail notification about the payment, which was directly transferred to the respondents' GIP account.

2.4 Hypotheses and Empirical Strategy

We build on a recent literature on fairness views indicating that many people share meritocratic fairness principles, i.e., they are willing to accept more inequality if it results from factors that are not beyond control (e.g. Konow 2000; Fong 2001; Cappelen et al. 2007; Almas, Cappelen, and Tungodden 2020). This suggests that beliefs about the sources of inequality in society play a crucial role for individuals' attitudes toward inequality and social policy responses. We expect that treated respondents are more likely to believe that inequality is due to unequal opportunities than due to different choices in life. As a consequence, we expect people in the treatment group to accept less inequality. More precisely, we expect to observe a shift to more malevolence in the domain of disadvantageous inequality (i.e. a decrease in the *x-score*) and to more benevolence in the advantageous domain (i.e. an increase in the *y-score*). Further, we expect that greater pessimism about equality of opportunity leads to greater support for redistribution and educational spending.

Since the treatment information specifically mentions the likelihood of advancing from the bottom to the top quartile of the income distribution and vice versa, it may have effects on how respondents view recipients who belong to these two groups. The idea is that if decision-makers become more pessimistic about equality of opportunity, they are more likely to perceive the recipient's income situation as the result of unequal opportunities. Consequently, if a decision-

maker thinks that the recipient is rich, then believing less in equality of opportunity should decrease benevolence toward the recipient. If a decision-maker thinks that the recipient is poorer, however, then we should see that believing less in equality of opportunity increases benevolence toward the recipient. To test this hypothesis, we provide information about the recipients' position in the income distribution to a subsample of respondents in the EET. If the decision-maker is matched with a poor recipient (*poor* treatment), we expect to see an increase in both the *x-* and *y-score* when providing information about the persistence of socio-economic status. Conversely, we expect a decrease in both scores if the recipient is rich (*rich* treatment). Independently of the social mobility information treatment, we also expect an increase in both the *x-score* and *y-score* in the *poor* treatment and a decrease of both scores in the *rich* treatment relative to the *neutral*, "no information" treatment

The general empirical framework in which we study the effects of information about social mobility on our outcomes of interest – the *x-score*, the *y-score*, redistributive preferences, and education expenditures – takes the following form:

$$Y_i = \alpha + \beta_1 treated_i + \beta_2 R_i + \beta_3 P_i + \beta_4 (treated_i \times R_i) + \beta_5 (treated_i \times P_i) + X + \varepsilon_i \qquad (1)$$

where Y_i is one of our four main outcomes (x-score, y-score, redistribution, and education expenditures) and $treated_i$ is a binary variable indicating whether respondent i received information on social mobility. The binary variables P_i and R_i indicate whether a respondent received information on the other persons' location in the income distribution in the EET (bottom 10%/top 10%) and X is a set of standard controls (including age, gender, log income, marital status, size of household, employment status, retirement status, education, and a region indicator). We code all variables such that higher values refer to more optimistic perceptions about equality of opportunity, more benevolence, and higher support for redistribution and educational spending, respectively. To account for differential responses to our treatment, we also consider how the information treatment interacts with a set of pre-registered socio-economic characteristics and attitudes. We will discuss this in more detail in Section 4 below. As pre-specified, we use in all of our specifications OLS regressions and robust standard errors. In addition to the standard discussion of statistical significance of our results, we will present the 90% confidence intervals of our estimates, which enables us to say more about the economic effect sizes.

2.5 Summary Statistics and Randomization Check

In Table 1, we present the means of the controls specified in the pre-analysis plan (PAP) for the control and treatment groups (for more detailed summary statistics, see Table S1 in the Supplementary Material). All controls come from the GIP core surveys that are conducted on a yearly basis and elicit the basic socio-demographic information of respondents, except respondents' assessment of the role of luck and effort in economic success, which we elicit as part of our module

(wave 33). The table indicates that almost all means are balanced across the two groups. To provide a more formal verification of this observation, we run a randomization check. In column 3, we present the p-values from regressing the controls on a treatment indicator (i.e. whether they receive information on social mobility or not). None of the p-values is statistically significant at the five-percent level. Performing an omnibus F-test to see if the coefficients are jointly different from zero ($p = 0.56$) confirms that our sample is balanced.

3 Results: From Perceptions to Preferences

We present three sets of results. First, we provide evidence that our treatment intervention has an effect on respondents' perceptions of equality of opportunity (our "first stage"). For the ease of presentation, we will use perceptions of equality of opportunity and mobility perceptions interchangeably. Evidence on this first-stage effect is important because the exogenous manipulation of respondents' mobility perceptions is a prerequisite to causally answer our main research question. Second, and most importantly, we assess the effect of these perceptions on respondents' distributional preferences and study how these effects interact with information about the relative-income rank of their interaction partners. Third, we explore the differential effect of our treatment on distributional preferences for specific pre-registered subgroups. Our analysis proceeds as specified in the pre-analysis plan, unless noted otherwise.

3.1 First Stage: Impact of Mobility Information on Mobility Perceptions

Before we present evidence on a first-stage effect, we look at the correlates of mobility perceptions focusing on the control group, as they are not contaminated by the information treatment. To ease interpretation, we transform the variable such that higher values indicate more optimism (i.e. weaker dependence on parental income status). Table 2 displays the results of this exercise. Column 1 shows the correlations from bivariate regressions for each control, whereas column 2 presents the results from a multivariate regression including all controls jointly. We observe that better educated people are much less optimistic than lower educated people and that politically right-leaning people are more optimistic about social mobility.[10] These associations hold in both bivariate and multivariate regressions and are in line with previous findings in the literature (e.g. Chambers, Swan, and Heesacker 2015; Alesina, Teso, and Stantcheva 2018). If all controls enter simultaneously (column 2), we additionally observe a positive relationship of income and a neg-

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[10] Relatedly, right-leaning people more likely believe that effort is important for economic success. In the Supplementary Material we provide more detailed evidence on correlates for specific pre-registered subgroups that confirm the results presented here (see Section S5 and Figure S2). Weber (2020) presents cross-country evidence showing that perceptions of social mobility are associated with a self-serving bias about personal mobility experiences. In contrast, we find no evidence that intra- or intergenerational mobility is related to social mobility perceptions.

ative relationship of age to perceived equality of opportunity. However, we do no not find a correlation between mobility perceptions and beliefs about the role of luck and effort in economic success (luck/effort beliefs). That is, respondents who believe that luck determines economic success are as optimistic or pessimistic about equality of opportunity as respondents who believe that effort determines success. Since we measure mobility perceptions as beliefs about the dependence of success on parental socio-economic status, this finding is surprising and suggests that people do not consider being born into a poor or rich household as unlucky or lucky, respectively. In other words, it casts doubt on whether people are fully conscious of the fact that being born rich or poor and the many of the consequences thereof are beyond someone's control.[11] We return to this finding when discussing the treatment effects.

Next, we test whether the treatment manipulation was successful. For this purpose, we regress the answers to the question on how strongly one believes that economic success depends on being born into a household in the top or the bottom quartile of the income distribution on a treatment indicator (see Table 2, columns 3 and 4). The results show that the information treatment significantly affects the respondents' mobility perception (column 3). Treated respondents believe more strongly that economic success depends on parental background than non-treated respondents, i.e. they become more pessimistic about equality of opportunity. The magnitude of the shift in beliefs is sizable. Receiving information on mobility translates into a 0.18 standard deviation increase in pessimism, which is comparable in size to the "first stage" effect in Alesina, Stantcheva, and Teso (2018).[12] Adding controls does not affect the coefficient estimate on mobility perceptions much (column 4) and thus confirms the associations presented in columns (1) and (2).

3.2 Impact of Mobility Information on Distributional Preferences

We now turn to our main contribution – the question whether and how information on social mobility affects individuals' distributional preferences. Before looking at our experimental evidence, we explore the relationship between respondents' perceptions about equality of opportunity -- our "first stage" – and our preference outcomes. First, we note that our measure of distributional preferences is associated with social policy preferences such as respondents' demand for redistribution, with attitudes toward equality of opportunity and with respondents' political orientation (see Table S4 in the Supplementary Material). Second, in Table 3 we show that mobility perceptions are negatively related to the support for redistribution, education expenditures as

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[11] Evidence suggests that people misperceive the importance of luck for success and thus equate success with merit (Fehr and Vollmann, 2020).

[12] The reported coefficient estimates of the two qualitative measures on mobility perceptions in Alesina, Stantcheva, and Teso (2018) correspond to a shift in perceptions of about 0.22 standard deviations (Table 4, columns 6 and 7). Examining 750 RCTs on education policies, Kraft (2019) proposes that 0.2 standard deviations and higher can be considered a large effect. See also Haaland, Roth, and Wohlfahrt (2020) who report effect sizes between 0.2 and 0.5 standard deviations for a selected sample of information provision experiments.

well as to the *y-score*. That is, more optimistic respondents show less support for policies aimed at reducing inequality and are less benevolent in the advantageous domain suggesting more tolerance toward inequality, in general.

Next, we use our experiment to examine the causal relation between information of social mobility and distributional preferences. We focus first on the EET without information on a recipient's income situation (*neutral* treatment). In a second step, we analyze how information about a recipient's relative income rank affects distributive choices and, in particular, how this information interacts with mobility perceptions. In this way, we are able to paint a more comprehensive picture of how perceptions of equality of opportunity relate to distributional preferences.

No information about recipient's income rank: Figure 3 presents a scatter plot of *x-scores* and *y-scores* differentiated by treatment and control. The figure shows no apparent differences between conditions. A majority of respondents displays a negative *x-score* and a positive *y-score* in both conditions, i.e., they can be classified as inequality averse. The remaining observations are dispersed over the whole range of parameter values with small clusters around altruistic (top-right corner), spiteful (bottom-left corner) and selfish types (center).

To provide rigorous support for this observation, we follow our main specification (1) and regress the individual average *x-scores* and *y-scores* on a treatment indicator (columns 1 and 3). In addition, we include a set of dummy variables indicating the different information conditions in the EET with and without a full set of individual controls. Table 4 displays the results. For both scores, the estimated coefficient of the treatment variable ("*Treated*") is not statistically different from zero at conventional significance levels. In other words, we do not find evidence that treated respondents become more averse to disadvantageous (*x-score*) or advantageous inequality (*y-score*) than respondents in the control group. Adding individual controls does nothing to change this conclusion.

Moreover, taking advantage of the longitudinal character of the survey, we can corroborate this finding by controlling for the *(x,y)-scores* elicited prior to the information treatment in wave 23. We find that the information treatment does not affect the *(x,y)-scores* over time. That is, we do not find systematic within-subject changes of peoples' scores from the previous wave 23 and the current wave 33 in the treatment group (see Supplementary Material, Table S2).[13]

Because of the large sample size, we are able to rule out even small treatment effects on distributional preferences. We present 90% confidence intervals, which allows us to get upper bounds of effects sizes. For example, the 90% confidence interval when regressing the *y-score* on a treatment indicator without controls is $[-0.18, 0.26]$. Given that the *y-score* can take on values in the interval $[-4.5, 5.5]$, we can rule out effect sizes larger than 4.4% of the total range of the *y-score*

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[13] The results also point to the intertemporal stability of distributional preferences (see, for example, Chuang and Schechter (2015), Bruhin, Fehr, and Schunk (2019), and Fisman et al. (2020) for related evidence).

((0.18 + 0.26)/(4.5 + 5.5)). The same number is just 3.6% for the *x-score*. Thus, we can rule out significant effect sizes for distributional preferences.

Information on recipient's income rank: To shed more light on the formation of distributional preferences and the determinants of pro-social behavior toward poor and rich respondents, we randomly informed a subset of respondents about whether the recipient in the EET belongs to the top or bottom 10% of the income distribution of participants in the GIP.

As expected, providing this additional information has an effect on respondents' distributional choices. Knowing that the recipient is from the bottom 10% of the income distribution leads to a sizable and significant shift of the *x-score* (Table 4, column 2). Given that the *x-score* is, on average, negative (–2.6), the observed positive estimate implies that respondents are less malevolent in the *poor* treatment compared to the *neutral* treatment. There is no evidence that a recipient from the top 10% of the income distribution (*rich* treatment) alters distributional choices in the domain of disadvantageous inequality, i.e., there is no effect on the *x-score*. We observe the opposite pattern for the *y-score* (Table 4, column 5). While there is a significant and negative shift of the *y-score* when the recipient is from the top 10%, we find no evidence that a recipient from the bottom 10% affects the decision-maker's choices. Because the *y-score* is positive on average (3.5), this finding indicates that respondents' distributional choices are less benevolent in the former case. In other words, respondents are less willing to forgo their own payoffs to increase the payoff of a "rich" recipient, which is why they switch earlier from the equal to unequal distribution. These findings illustrate the sensitivity of the distributional preference measure to the decision context, as behavior responds to the presented information in a predictable way.

Because we cross-randomize the recipient information in the EET with our main treatment, we can examine whether the main treatment intervention magnifies the changes in distributional choices reported above. We expected decision-makers who become more pessimistic about equality of opportunity to be more benevolent to poor recipients and less to the rich. Yet, we do not find evidence that our treatment affects the estimated coefficients for the *x-score* and *y-score*, despite shifting mobility perceptions. Neither the interaction effect of the mobility information with the *rich* treatment, nor the interaction effect with the *poor* treatment results in estimates significantly different from zero (Table 4, columns 2 and 5). The confidence intervals reported in Table 4 are again small, such that we can rule out effect sizes larger than 9–11% of the total range of the scores. This means that decision-makers do not want to treat rich and poor recipients differently as perceived inequality of opportunity increases, suggesting that they do not more likely view the income gap as unfair.

3.3 Impact of Mobility Information on Policy Preferences

In addition to the incentivized measures of distributional preferences, we also ask respondents about their support for redistribution and educational spending in the main survey and draw on

a follow-up survey. The answers to these questions enable us to assess the causal effect of beliefs about equality of opportunity on policy preferences and its persistence.

Main Survey: Table 5 presents the results. The estimates based on responses to those survey questions are precisely estimated null effects. The 90% confidence intervals for redistribution and education expenditures are $[-0.10, 0.05]$ and $[-0.06, 0.09]$, respecively. Since the former variable is coded on a 1 to 10 scale and the latter on a 0 to 4 scale, the tight confidence intervals allow us to rule out effect sizes larger than 1% and 3%, respectively, in the total range of possible answers. Thus, more pessimism about equality of opportunity neither increases demand for redistribution nor affects attitudes toward public education spending. This observation is consistent with the insights gained from analyzing the EET and also provides reassurance that our results on distributional preferences are unlikely driven by the lower stakes of our EET in relation to redistributive policy measures.

We further explore the robustness of these findings along two margins. First, previous research has pointed to the possibility that low trust in the government explains the missing response of policy preferences to inequality concerns (e.g. Hetherington 2005; Kuziemko et al. 2015; Alesina, Stantcheva, and Teso 2018). While we did not pre-specify this possibility, we can use information on respondents' trust in various legal and political institutions from the GIP to examine this possibility. Specifically, we use the question about how much trust they place in the federal government. Interacting this information with the information treatment reveals no evidence that trust in government plays a role in the muted response to redistribution and support for education expenditures.[14] Second, we consider the possibility that social mobility perceptions directly shape policy preferences. In the Supplementary material we use the treatment as an instrument for mobility perceptions, and show that there is no evidence for a causal effect of mobility perceptions on policy preferences (see Section S6, Table S3).

Follow-up survey: Using data from a follow-up survey, we check the persistence of our results. About 4 months after our intervention, respondents again answered questions about redistribution and education expenditures. For both measures we observe a strong correlation between main and follow-up survey (redistribution, $\rho = 0.43$ and education expenditures, $\rho = 0.54$). Columns 3 and 6 in Table 5 present the results on the impact of the treatment on the follow-up measures. Again, we see precisely estimated null effects for both preferences for redistribution and education expenditures.

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[14] More precisely, we interact our treatment with an indicator for above-median trust. The corresponding coefficient estimate is 0.055 (with a standard error of 0.083) when the dependent variable is redistribution and is 0.014 (standard error of 0.081) when the dependent variable is education expenditures. We obtain similar results if we consider information on respondents' trust in parliament (Bundestag) and political parties as major actors in the passage of legislation. These findings are consistent with recent findings that political trust unlikely affects support for redistribution (Peyton, 2020).

4 Heterogeneous Effects

In the pre-analysis plan, we hypothesized that the treatment will have a greater impact on sub-populations who are likely more optimistic about equality of opportunity *a priori*. In the following, we analyze how these different groups of respondents react to the treatment and estimate a series of regressions of the following form:

$$Y_i = \alpha + \beta_1\, treated_i + \beta_2\, heterogeneous_i + \beta_3(treated_i \times heterogeneous_i) + \gamma X + \varepsilon_i \ , \quad (2)$$

where Y_i is one of our four main outcomes as above, $treated_i$ is a treatment dummy for our intervention and $heterogeneous_i$ corresponds to the covariate of interest (luck vs. effort, political orientation, income, and occupational status). Table 6 presents the results. For the sake of clarity, we present only the coefficient estimates for the covariate (β_2) and its interaction (β_3).

Luck versus Effort: People who believe more firmly in the importance of effort for economic success may oppose redistribution or higher spending on education. Indeed, in line with previous findings (Alesina et al. 2001; Fong 2001; Corneo and Grüner 2002; Alesina and La Ferrara 2005; Alesina and Angeletos 2005; Gaertner, Mollerstrom, and Seim 2017, 2019), support for redistribution in our sample is related to the view that effort determines economic success (Panel A of Table 6). The more important question, though, is whether respondents who believe more firmly in effort respond differently to information about social mobility. We find that all interaction effects displayed in Panel A of Table 6 are insignificant and confidence intervals are small ([-0.10, 0.21] for the *x-score* and [-0.26, 0.13] for the *y-score*, respectively).

Therefore, learning that the prospects of getting ahead are low has no differential effect on those who believe in luck and those who believe in effort. In fact, we observe that perceptions of equality of opportunity are independent of these beliefs in the control group. Moreover, the treatment reduces these perceptions to the same extent in both groups (see Figure S2).

Political Orientation: Our previous analysis revealed that political orientation of respondents is positively related to perceptions of equality of opportunity, i.e. right-leaning respondents hold more optimistic beliefs (see Figure 4). Indeed, political ideology plays a key role for attitudes toward social policies (Karadja, Mollerstrom, and Seim 2017; Alesina, Stantcheva, and Teso, 2018). For example, Alesina, Stantcheva, and Teso (2018) find that left-leaning respondents show more support for redistributive measures in response to receiving information about social mobility.

To measure political orientation, we use respondents' self-assessment in the left–right spectrum and their voting intentions in the next federal election.[15] To estimate the impact of respondents' political orientation, we construct an index using the equally-weighted average of the standardized answers to each of the two questions (following the methodology in Kling, Liebman,

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[15] Respondents indicate their political orientation on 11-point Likert scale and state which party they would vote for in the next national election, which took place two month later.

and Katz 2007). In panel B of Table 6, we present the results for the standardized index (using the two measures separately yields similar results). Right-leaning respondents display a smaller *y-score* than left-leaning respondents. However, the treatment has no effect. The confidence intervals are tight, such that we can dismiss effect sizes larger than 4.4% (*x-score*) and 5.3% (*y-score*) of the parameter range, respectively.[16] Second, there is a strong and persistent effect of political orientation on support for redistribution and expenditures on education. Right-leaning respondents are significantly less likely to support these two policies than left-leaning respondents. However, there is no additional effect of the treatment, and confidence intervals are small: $[-0.05, 0.14]$ for redistribution and $[-0.03, 0.16]$ for education expenditures, thus allowing us to rule out effect sizes larger than 1.9% and 3.8%.

Income: To see whether poor and rich respondents react differently to our treatment, we interact the treatment with a dummy variable for the bottom 25% and the top 25% of the income distribution in the sample (see Panel C in Table 6). Poor respondents (bottom 25%) display a significantly lower *x-score* and a significantly higher *y-score* compared to the top 75% respondents. The treatment increases the *x-score* (i.e. it induces less malevolence in the domain of disadvantageous inequality) and decreases the *y-score* (i.e. it induces less benevolence in the domain of advantageous inequality). On the other hand, rich respondents (top 25%) are less malevolent in the domain of disadvantageous inequality than the bottom 75%. Yet, the treatment has no effect on either score, even though the shift in rich respondents' mobility perceptions is among the largest we observe in our sample (see Figure S2). Moreover, the poor support more redistribution, while the rich support less, although there is no correlation with the support for education expenditures. We observe no treatment effect here, neither for redistribution, nor for education spending.

Occupational status: We hypothesized that occupational groups who have received more education are, on average, more optimistic about mobility and thus react more strongly to our treatment.[17] We categorize occupational status into six groups: semi-skilled workers (the reference group), skilled workers, employees, executives, self-employed and professionals, and others (e.g. soldiers, apprenticeship, and unpaid family workers). Panel D in Table 6 displays the results. There is no evidence for a relationship between occupational status, our treatment and distributional preferences.

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[16] The confidence interval for the *x-score* is $[-0.24, 0.20]$ and for the y-*score* is $[-0.45, 0.09]$.

[17] There is little scientifically reliable evidence about mobility perceptions in the German population. A 2013 public opinion poll by the Allensbach Institute, an opinion and marketing research institute, indicates that about 50% of respondents think that the likelihood of a working-class child moving upward in the social hierarchy is "very good." Respondents with professional and university degrees display a more optimistic view than unskilled and skilled workers. In our sample however, education and mobility perceptions are negatively correlated (see Table 2).

5 Conclusion

In this paper, we presented evidence that questions the importance of beliefs about equality of opportunity, an important measure of the fairness of a society, as a determinant of distributional preferences. We documented a sizable shift in mobility perceptions in response to information about the persistence of socio-economic status, indicating that respondents were generally over-optimistic about equality of opportunity, on average. Using this shift in perceptions, we presented strong evidence that more pessimistic beliefs about equality of opportunity neither affect distributional preferences in general nor pro-social attitudes toward respondents at the tails of the income distribution in particular; both of which are fundamental inputs for redistributive policies.

We complemented this result with evidence on the effect of mobility perceptions on policy preferences, such as redistribution and education expenses. Again, these measures confirm that mobility perceptions do not move preferences: There is no measurable effect of more pessimistic beliefs about equality of opportunity on policy preferences. Exploring the heterogeneous effects of the treatment, we found consistent support for these results. Moreover, we provide suggestive evidence that the lack of an effect is related to a disconnect between people's mobility perceptions and their beliefs about the role of luck.

Taking these results at face value, the evidence we presented here for Germany seems to fit the empirical observations from the "Great Gatsby Curve." If people do not support more redistribution in response to a negative shock to social mobility, it is less surprising that inequality and mobility are negatively correlated. More generally, our results suggest that the often observed disconnect between people's increasing concerns about inequality and appropriate policy measures stems from the underlying fundamentals. In fact, our results confirm the common view that preferences are slow-moving component of decision-making. Negative information on the fairness of a society is not sufficient to instate an immediate response of preferences, or even of charitable behavior toward the poor despite its sizable impact on mobility perceptions.

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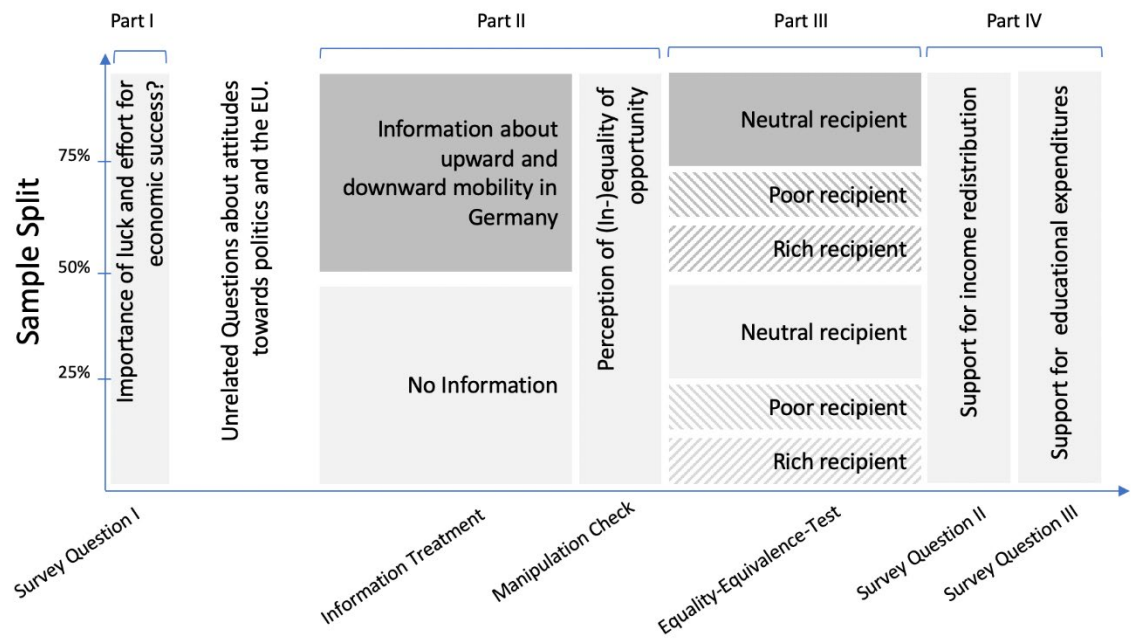## Figures and Tables
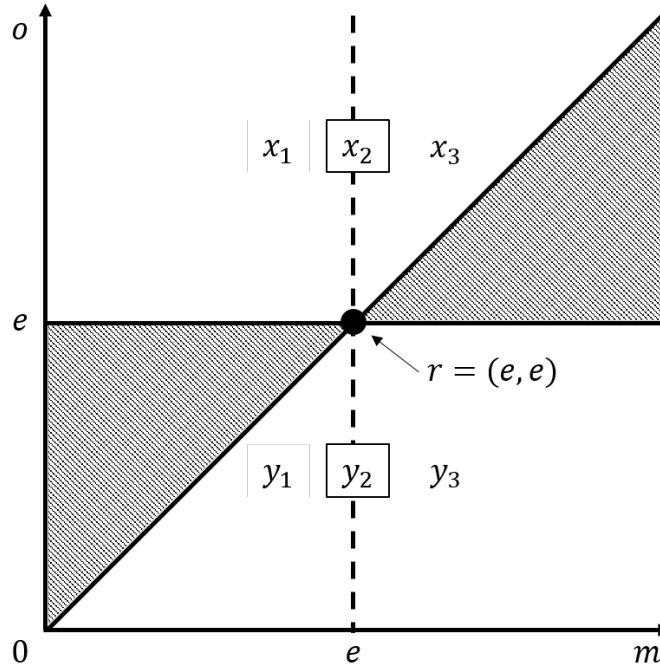
Figure 1: Experimental Setup

Figure 2: Domains of Inequality and Identification of Distributional Types

Notes: The figure is reproduced from Kerschbamer (2015). "m" denotes the income to self, "o" denotes the income to other, and r=(e,e) denotes the reference allocation. The domain of disadvantageous (advantageous) inequality is above (below) the 45-degree and the 3 times 3 combinations of (x1,x2,x3) – possible behavioral motives in the domain of disadvantageous inequality - and (y1,y2,y3) – behavioral motives in the domain of advantageous inequality – result in the 9 different distributional types. For example, the standard selfish type has vertical indifferences curves that run through area x_2 and y_2, wheras an inequality-averse decision-maker exhibits indifference curves that run through area x_3 and y_3.

Figure 3: Jittered Scatter Plot of (x, y) Scores Separated by Control and Treatment
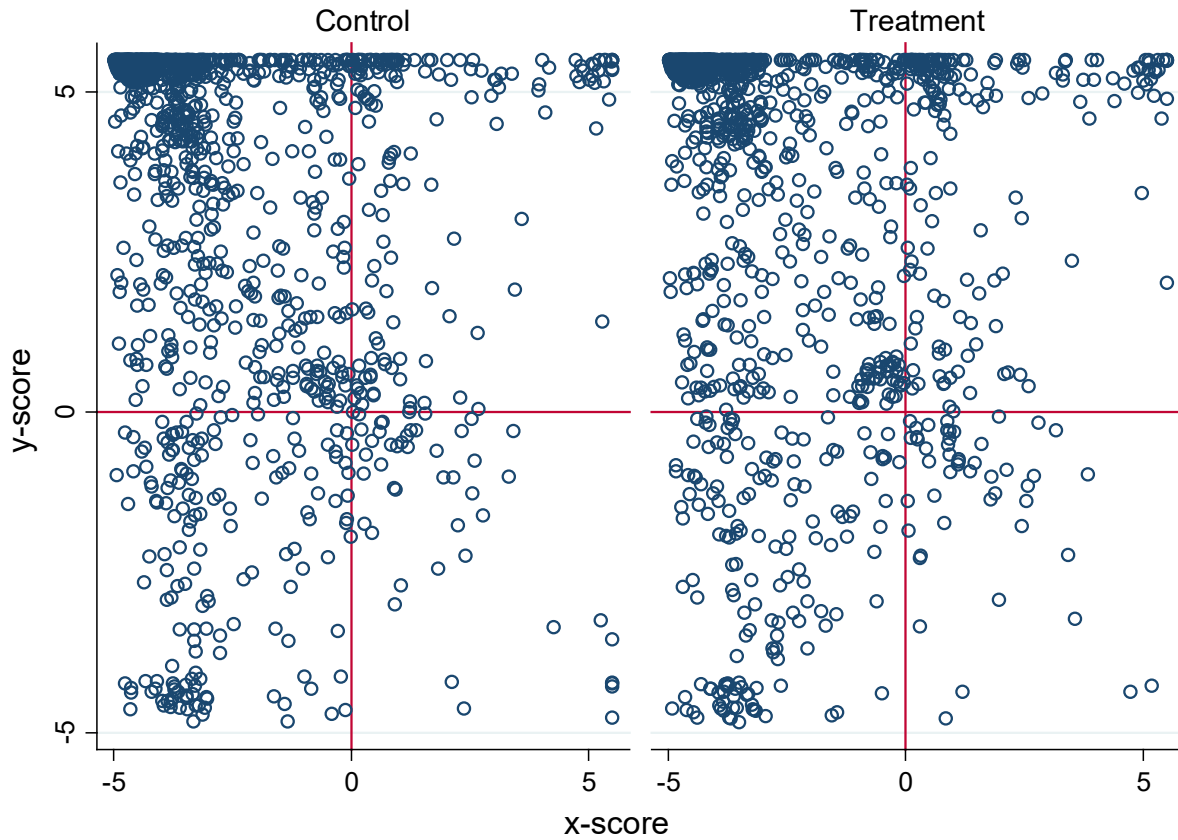
Notes: The y-score measures behavior in the domain of advantegous inequality, the x-score measures behavior in the domain of disadvantageous inequality. Positive scores imply benevolence, which is greater the higher the score. Negative scores imply malevolence, which is greater the lower the score. A score of zero implies selfishness. Inequality averse decision-makers are, for example, characterized by a positive y-score and a negative x-score (i.e., they cluster in top-left quadrant). Equality averse types cluster in the bottom-right quadrant, altruistic types in the top-right quadrant, and spiteful types in the bottom-left quadrant. Maximin preferences are characterized by a positive y-score and a zero x-score.

Table 1: Randomization Check

	Control	Treatment	*p-value*
	(1)	(2)	(3)
Age	51.06 (15.74)	50.98 (15.02)	0.90
Female=1	0.50 (0.50)	0.48 (0.50)	0.36
Education No degree=1/Highest degree=5	3.68 (1.17)	3.75 (1.16)	0.10
Married=1	0.55 (0.50)	0.58 (0.49)	0.10
Monthly Income (log)	7.32 (0.8)	7.35 (0.86)	0.27
Retired=1	0.23 (0.42)	0.21 (0.41)	0.27
Unemployed=1	0.02 (0.14)	0.02 (0.14)	0.89
Household Size	2.42 (1.08)	2.49 (1.09)	0.11
East Germany=1	0.21 (0.41)	0.19 (0.40)	0.21
Political Orientation: "Left=1/Right=11"	5.56 (1.95)	5.60 (1.94)	0.54
Economic Success "Luck=1/Effort=11" Beliefs	6.09 (1.94)	6.09 (1.91)	0.99
Locus of Control (LoC) "Internal LoC=1/External LoC=5"	2.18 (0.61)	2.17 (0.62)	0.52
Prob>F			0.31

Notes: Mean of controls and standard deviations in parentheses. Columns (1) and (2) display the mean (% share) of the listed controls in the treatment and control group. Column (3) shows the p-values of the coefficients of separate OLS regressions, in which the treatment indicator (information on social mobility) is regressed on the respective control. Education is a categorical variable, where 1 indicates no degree and 5 indicates highest degree (i.e., university qualification). Political Orientation is measured on a 1–11 scale with higher values indicating more conservative political views. Economic Success is measured on a 1–11 scale with higher values indicating a stronger belief that effort is important for economic success. Locus of Control is an equally-weighted index of four questions on a 1–5 scale where higher values indicate a more external locus of control (i.e. a belief that life is determined by outside factors such as luck and fate).
Prob>F is the p-value of an F-test for joint significance of all controls.

Table 2: Correlates of Mobility Perceptions and First-Stage Effects

	Mobility Perceptions			
	(1)	(2)	(3)	(4)
Treated			-0.177***	-0.164***
			(0.039)	(0.041)
Age	-0.001	-0.006*		-0.006***
	(0.002)	(0.003)		(0.002)
Female	-0.007	0.066		0.082*
	(0.055)	(0.063)		(0.045)
Education	-0.108***	-0.010***		-0.086***
	(0.024)	(0.029)		(0.020)
Married	0.029	0.145		0.127***
	(0.055)	(0.069)		(0.049)
Monthly Income (log)	0.043	0.097**		0.051*
	(0.035)	(0.044)		(0.028)
Retired	0.005	0.040		0.100
	(0.064)	(0.092)		(0.067)
Unemployed	0.157	0.323		0.337*
	(0.186)	(0.269)		(0.177)
Household Size	-0.003	-0.027		-0.025
	(0.027)	(0.033)		(0.022)
East Germany	-0.046	0.027		0.056
	(0.064)	(0.070)		(0.052)
Political Orientation	0.085***	0.070**		0.102***
"Left/Right" (z-score)	(0.028)	(0.030)		(0.022)
Economic Success:	0.033	0.003		0.007
Luck/Effort Beliefs (z-score)	(0.029)	(0.033)		(0.025)
Locus of Control	-0.022	-0.007		-0.049
(z-score)	(0.030)	(0.033)		(0.036)
R^2		0.03	0.01	0.04
N		1,111	2,661	2,241

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$

OLS regressions. Robust standard errors in parentheses. The dependent variable is Mobility Perceptions ("How does economic success depend on being born into poor or rich household?") measured on a 1–10 scale. The variable is normalized to zero mean and unit variance and higher values indicate more opti-mism (i.e. weaker dependence on parental income). The first two columns report correlates from bivariate regressions (column 1) and a multivariate regression (column 2) using data from the control group only. Column 3 and 4 includes all data. Education is a categorical variable, where 1 indicates no degree and 5 indicates highest degree (i.e., university qualification). Political Orientation is measured on a 1–11 scale with higher values indicating more conservative political views. Economic Success: Luck/Effort Beliefs are measured on a 1–11 scale with higher values indicating a stronger belief that effort is important for economic success. Locus of Control is an equally-weighted index of four questions on a 1–5 scale where higher values indicate a more external locus of control (i.e. a belief that life is determined by outside factors such as luck and fate).

Table 3: Correlation Mobility Perceptions and Outcomes

	Redistribution	Education Expenditure.	x-score	y-score
Mobility Perception	-0.090***	-0.139***	-0.009	-0.124**
	(0.021)	(0.022)	(0.047)	(0.063)
Rich			0.120	-0.464***
			(0.110)	(0.145)
Poor			0.468***	-0.152
			(0.121)	(0.148)
Constant	1.475***	-0.998***	-2.148***	3.827***
	(0.229)	(0.226)	(0.554)	(0.630)
Controls	Yes	Yes	Yes	Yes
R^2	0.008	0.025	0.009	0.006
N	2,641	2,648	2,583	2,583

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$

OLS regressions with robust standard errors in parentheses. Redistribution (Education Expenditure) is the z-score of the stated demand for redistribution of income (demand for more spending on education), where higher values imply a higher demand (higher spending). The x-score (y-score) measures benevolence in the disadvantageous (advantageous) domain of inequality, where higher values mean more benevolence. Mobility Perceptions ("How does economic success depend on being born into poor or rich household?") measured on a 1–10 scale. The variable is normalized to zero mean and unit variance and higher values indicate more optimism (i.e. weaker dependence on parental income). Rich and Poor are dummies equaling 1 if a respondent received information about the relative income of the other person in the EET (i.e. that the person is among the richest 10% or poorest 10% poorest in the sample, respectively). Controls include log income, gender, age, education level, East Germany dummy, retirement status, employment status, number of household members, and marital status.

Table 4: Treatment Effect on Distributional Preferences

	x-score			y-score		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.02	0.013	0.02	0.041	0.015	-0.036
	(0.09)	(0.12)	(0.12)	(0.11)	(0.15)	(0.16)
	[-0.13,0.17]			[-0.18, 0.26]		
Rich		0.164	0.179		-0.489**	-0.605***
		(0.15)	(0.15)		(0.20)	(0.2)
Poor		0.459***	0.464***		-0.016	-0.187
		(0.17)	(0.17)		(0.20)	(0.21)
Treated x Rich		-0.049	-0.12		0.182	0.272
		(0.22)	(0.22)		(0.28)	(0.29)
		[-0.48,0.38]	[-0.37,0.73]			
Treated x Poor		0.070	0.008		-0.079	0.063
		(0.25)	(0.24)		(0.29)	(0.3)
		[-0.41,0.55]	[-0.64,0.48]			
Constant	-2.583***	-2.737***	-2.504***	3.476***	3.602***	4.117***
	(0.07)	(0.09)	(0.59)	(0.08)	(0.11)	(0.6)
Controls	No	No	Yes	No	No	Yes
R^2	0.000	0.007	0.086	0.000	0.004	0.014
N	2,583	2,583	2,443	2,583	2,583	2,443

*Notes: ***p<0.01, **p<0.05, *p<0.1*

OLS regressions with robust standard errors in parentheses and 90% confidence intervals in brackets. The x-score (y-score) measures benevolence in the disadvantageous (advantageous) domain of inequality, where higher values mean more benevolence. Treated indicates whether a respondent received information on the persistence of socio-economic status (i.e., downward and upward mobility in society). Rich and Poor are dummies equaling 1 if a respondent received information about the relative income of the other person in the EET (i.e. that the person is among the richest 10% or poorest 10% poorest in the sample, respectively). Controls include log income, gender, age, education level, East Germany dummy, retirement status, employment status, number of household members, and marital status.

Table 5: Treatment Effect on Policy Preferences

	Redistribution			Education Expenditure		
	Main		Follow-up	Main survey		Follow-up
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.022	-0.013	-0.008	0.018	0.008	-0.013
	(0.04)	(0.04)	(0.041)	(0.04)	(0.04)	(0.041)
	[-0.10,0.05]		[-0.09, 0.07]	[-0.06,0.09]		[-0.09, 0.07]
Constant	0.011	1.272***	0.003	-0.009	-1.185***	0.003
	(0.03)	(0.21)	(0.029)	(0.03)	(0.21)	(0.029)
Controls	No	Yes	No	No	Yes	No
R^2	0.000	0.036	0.000	0.000	0.045	0.000
N	2,641	2,491	2,362	2,649	2,498	2,362

*Notes: ***p<0.01, **p<0.05, *p<0.1*

OLS regressions with robust standard errors in parentheses and 90% confidence intervals in brackets. Redistribution (Education Expenditure) is the z-score of the stated demand for redistribution of income (demand for more spending on education), where higher values imply a higher demand (higher spending). Treated indicates whether a respondent received information on the persistence of socio-economic status (i.e., downward and upward mobility in society). Controls include log income, gender, age, education level, East Germany dummy, retirement status, employment status, number of household members and marital status. The follow-up survey was conducted about 4 months after the main survey.

Table 6: Heterogeneous Treatment Effects

	x-score		y-score		Redistribution		Education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Economic Success: Luck/Effort Beliefs								
Treated x Luck/Effort	0.131	0.053	-0.099	-0.061	-0.043	-0.035	-0.004	-0.014
	(0.09)	(0.09)	(0.12)	(0.12)	(0.04)	(0.04)	(0.04)	(0.04)
Luck/Effort	-0.109	-0.232	-0.061	-0.093	-0.159***	-0.15***	-0.008	-0.009
	(0.07)	(0.07)	(0.08)	(0.08)	(0.03)	(0.03)	(0.03)	(0.03)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R²	0.008	0.086	0.005	0.016	0.033	0.063	0	0.045
N	2,581	2,441	2,581	2,441	2,639	2,489	2,645	2,495
B: Political Ideology								
Treated x Political Orient. (Left/Right)x	0.010	-0.018	-0.099	-0.113	0.002	0.008	0. 022	0.016
	(0.097)	(0.095)	(0.116)	(0.118)	(0.043)	(0.039)	(0.042)	(0.043)
Political Orient. (Left/Right)	0.056	0.054	-0.181**	-0.144*	-0.214***	-0.217***	-0.165***	-0.154***
	(0.070)	(0.067)	(0.079)	(0.081)	(0.031)	(0.031)	(0.029)	(0.030)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R²	0.01	0.07	0.01	0.02	0.05	0.08	0.02	0.06
N	2,502	2,419	2,502	2,419	2,555	2,416	2,560	2,421
C: Income								
Treated x Low income	0.403*	0.381*	-0.461*	-0.406	-0.116	-0.086	-0.039	-0.001
	(0.222)	(0.214)	(0.263)	(0.264)	(0.095)	(0.096)	(0.096)	(0.095)
Low income	-0.307**	-0.273*	0.526***	0.410**	0.229***	0.193***	-0.111*	-0.024
	(0.150)	(0.153)	(0.179)	(0.190)	(0.067)	(0.071)	(0.067)	(0.071)
Ref. group: top-75%								
R²	0.010	0.087	0.007	0.014	0.006	0.027	0.003	0.044
N	2,497	2,443	2,497	2,443	2549	2491	2555	2498
Treated x High income	-0.048	-0.068	0.183	0.186	-0.009	-0.009	0.111	0.113
	(0.225)	(0.219)	(0.263)	(0.265)	(0.086)	(0.087)	(0.089)	(0.088)
High income	0.447***	0.313*	-0.131	-0.064	-0.342***	-0.365***	0.102	-0.001
	(0.165)	(0.173)	(0.195)	(0.207)	(0.062)	(0.068)	(0.066)	(0.070)
Ref. group: bottom-75%								
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R²	0.014	0.088	0.004	0.013	0.025	0.045	0.007	0.046
N	2,497	2,443	2,497	2,443	2549	2491	2555	2498

Continued

Table 6: Heterogeneous Treatment Effects (*continued*)

	x-score		y-score		Redistribution		Education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D: Occupational Status								
Treated x Skilled workers	-0.142	-0.373	0.443	0.458	0.008	0.066	0.431*	0.485**
	(0.53)	(0.54)	(0.72)	(0.74)	(0.26)	(0.26)	(0.25)	(0.25)
Skilled Workers	0.137	0.128	-0.593	-0.293	-0.210	-0.232	-0.244	-0.370**
	(0.40)	(0.42)	(0.48)	(0.50)	(0.17)	(0.17)	(0.16)	(0.17)
Treated x Employee.	0.371	0.242	-0.515	-0.681	-0.128	-0.159	0.237	0.262
	(0.45)	(0.46)	(0.62)	(0.63)	(0.22)	(0.22)	(0.21)	(0.21)
Employee	0.192	0.125	0.338	0.462	-0.249*	-0.194	-0.056	-0.073
	(0.35)	(0.37)	(0.39)	(0.41)	(0.14)	(0.14)	(0.13)	(0.13)
Treated x Executive Employee	0.531	0.418	-0.218	-0.353	-0.066	-0.087	0.395*	0.433**
	(0.47)	(0.47)	(0.63)	(0.64)	(0.23)	(0.22)	(0.21)	(0.21)
Executive Employee	0.418	0.359	-0.099	0.160	-0.376***	-0.252*	0.064	-0.022
	(0.36)	(0.38)	(0.40)	(0.43)	(0.14)	(0.15)	(0.13)	(0.14)
Treated x Self-employed/Professional	0.741	0.573	-0.567	-0.826	-0.073	-0.037	0.313	0.366
	(0.55)	(0.55)	(0.73)	(0.74)	(0.26)	(0.26)	(0.25)	(0.25)
Self-employed/Professional	0.211	0.266	0.033	0.363	-0.470***	-0.410**	0.053	-0.044
	(0.42)	(0.43)	(0.49)	(0.51)	(0.16)	(0.17)	(0.16)	(0.16)
Treated x Others	0.924	0.717	-0.940	-0.997	-0.016	-0.057	0.247	0.358
	(0.67)	(0.68)	(0.87)	(0.91)	(0.32)	(0.33)	(0.32)	(0.32
Others	0.017	-0.512	0.271	0.208	-0.220	-0.251	-0.175	-0.223
	(0.47)	(0.50)	(0.58)	(0.64)	(0.21)	(0.22)	(0.19)	(0.20)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
R²	0.015	0.072	0.008	0.015	0.012	0.042	0.015	0.033
N	2,256	2,185	2,256	2,185	2,304	2,228	2,310	2,233

Notes: ****p<0.01; **p<0.05; *p<0.1*

OLS regressions with robust standard errors in parentheses. The x-score (y-score) measures benevolence in the disadvantageous (advantageous) domain of inequality, where higher values mean more benevolence. Redistribution (Education Expenditure) is the z-score of the stated demand for redistribution of income (demand for more spending on education), where higher values imply a higher demand (higher spending). Panels A to D show the coefficient estimates for the covariate of interest and its interaction with the information treatment. **Panel A***: Luck/Effort is the z-score of answer to the question about the role of luck and effort in determining economic success. Higher values imply a higher role of effort.* **Panel B:** *Political Orient. (Left/Right) is the self-reported location in the political left–right spectrum. Higher values indicate more right-leaning political values.* **Panel C:** *Low (High) income is an indicator for respondents in bottom (top) quartile of the income distribution of the sample.* **Panel D***: Skilled workers, Employees, Executive Employees, Self-employed and Professionals, Others are indicators for a respondent's occupation. Omitted category: semiskilled workers. Regressions on the (x,y)-score include indicators for the treatment variation in the EET (i.e. the information about the relative position in the income distribution of the other person). Controls include gender, age, number of household members, log income (except panel C) and education (except panel D), as well as indicators for East Germany, retirement status, employment status, and marital status.*

Supplementary Material:
For Online Publication Only

Perceptions of Equality of Opportunity and Inequality Acceptance

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S1: Treatment Intervention

Intro Treatment

Nun zu einem anderen Thema.

In vor Kurzem veröffentlichten wissenschaftlichen Studien sind Forscher der Frage nachgegangen, welcher Zusammenhang zwischen dem Einkommen der Eltern und dem Einkommen ihrer Kinder, wenn diese erwachsen sind, besteht.

< Zurück   Weiter >

In recently published scientific studies, researchers have explored the question of the relationship between parents' income and their children's income when they are adults.

Dabei geht es hauptsächlich um zwei Fragen. Zum einen wie hoch die Wahrscheinlichkeit ist, dass ein Kind aus einem reichen Haushalt im Erwachsenenalter einem armen Haushalt angehört. Zum anderen wie hoch die Wahrscheinlichkeit eines Kindes aus einem armen Haushalt ist, im Erwachsenenalter einem reichen Haushalt anzugehören. Was bedeutet hier arm und reich? Wenn wir alle Haushalte in Deutschland vom niedrigsten bis zum höchsten Einkommen aneinanderreihen würden, dann wäre ein Haushalt arm, wenn er zu den 25 Prozent der Haushalte mit dem niedrigsten Einkommen gehört. Umgekehrt wäre ein Haushalt reich, wenn er zu den 25 Prozent der Haushalte mit dem höchsten Einkommen gehört.

< Zurück   Weiter >

There are two main questions at stake here. The first is the probability that a child from a rich household will belong to a poor household in adulthood. The second is the probability that a child from a poor household will belong to a rich household in adulthood. What do poor and rich mean here? If we were to line up all households in Germany from the lowest to the highest income, a household would be poor if it belonged to the 25 percent of households with the lowest income. Conversely, a household would be rich if it belonged to the 25 percent of households with the highest income.

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Treatment Information

The studies used the most recent data from an independent, scientific survey of more than 12,000 private households in Germany, which has been conducted annually with the same individuals and families since 1984.

The data show that the expected probability of a child from a poor household being rich as an adult is very low. By contrast, the probability of a child from a poor household also being poor as an adult is very high. The following graph illustrates these relationships.

Illustration from Original Survey:

Kinder aus einem armen Haushalt

Als Erwachsene:

Teil eines armen Haushalts   Teil eines reichen Haushalts

Es ist extrem selten, dass ein Kind aus einem armen Haushalt als Erwachsener reich ist.

Illustration translated into English:

Children born into a poor Household

As Adults:

Member of a poor Household   Member of a rich Household

It is extremely rare that a child born into a poor HH is rich as an adult.

Conversely, the expected probability of a child from a rich household being poor as an adult is very low. By contrast, the probability that a child from a rich household will also be rich as an adult is very high. The following chart illustrates these relationships.
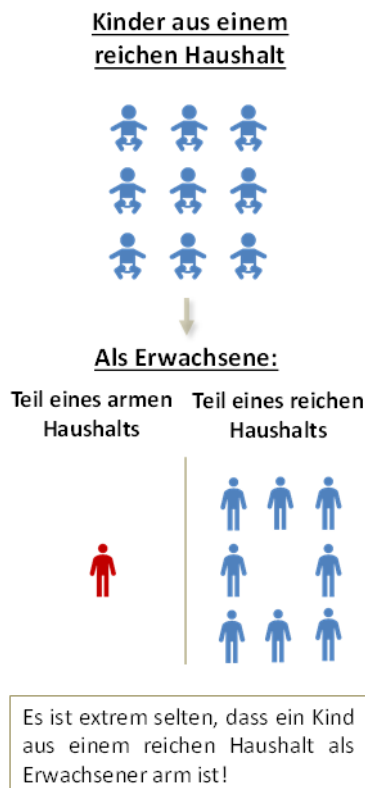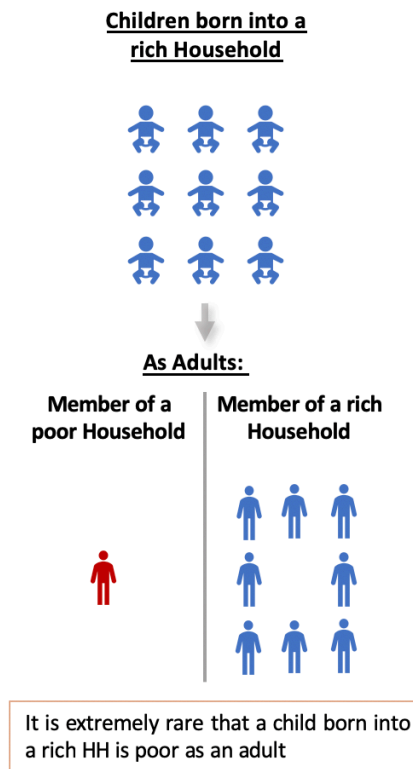
Illustration from Original Survey: Illustration translated into English

Kinder aus einem reichen Haushalt

Als Erwachsene:

Teil eines armen Haushalts   Teil eines reichen Haushalts

Es ist extrem selten, dass ein Kind aus einem reichen Haushalt als Erwachsener arm ist!

Children born into a rich Household

As Adults:

Member of a poor Household   Member of a rich Household

It is extremely rare that a child born into a rich HH is poor as an adult

Manipulation Check

Imagine 100 households that together represent the population of Germany.

What do you think? To what extent does economic success as an adult (e.g., attaining a high level of education or an above-average income) depend on whether you grew up in one of the 25 poorest or one of the 25 richest households? [very little (0) – very much (10)]

S2: Parameters of the EET

Figure S1: Parameterization of the EET.
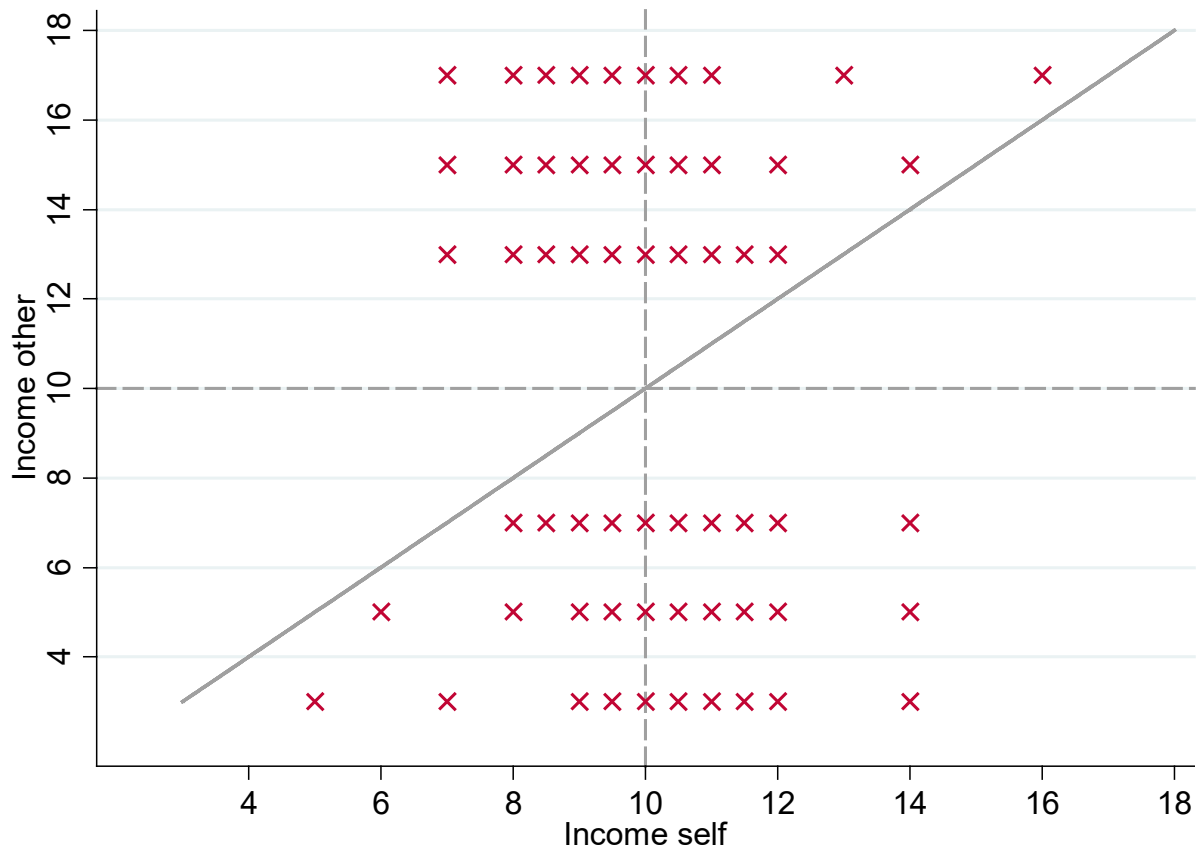
Notes: The reference allocation is 10,10. Points below (above) the 45-degree line are allocations in the advantageous (disadvantageous) domain. Each list keeps the income of Other fixed at x Euro, with $x \in \{3, 5, 7, 13, 15, 17\}$ and varies the income of Self.

S3: Summary Statistics

Table S1: Summary Statistics

	All	Control	Treatment
		(1)	(2)
Age	51.02	51.06	50.98
	(15.38)	(15.74)	(15.02)
Female=1	0.49	0.50	0.48
	(0.50)	(0.50)	(0.50)
Education	3.72	3.68	3.75
No degree=1/Highest degree=5	(1.18)	(1.17)	(1.16)
Married=1	0.56	0.55	0.58
	(0.50)	(0.50)	(0.49)
Monthly Income (log)	7.34	7.32	7.35
	(0.83)	(0.8)	(0.86)
Retired=1	0.22	0.23	0.21
	(0.42)	(0.42)	(0.41)
Unemployed=1	0.02	0.02	0.02
	(0.14)	(0.14)	(0.14)
Household Size	2.46	2.42	2.49
	(1.09)	(1.08)	(1.09)
East Germany=1	0.20	0.21	0.19
	(0.40)	(0.41)	(0.40)
Political Orientation:	5.58	5.56	5.60
"Left=1/Right=11"	(1.95)	(1.95)	(1.94)
Economic Success	6.09	6.09	6.09
"Luck=1/Effort=11" Beliefs	(1.92)	(1.94)	(1.91)
Locus of Control (LoC)	2.17	2.18	2.17
"Internal LoC=1/External LoC=5"	(0.62)	(0.61)	(0.62)

Notes: *Mean of controls and standard deviations in parentheses. Columns (1) and (2) display the mean (% share) of the listed controls in the treatment and control group. Column (3) shows the p-values of the coefficients of separate OLS regressions, in which the treatment indicator (information on social mobility) is regressed on the respective control. Education is a categorical variable, where 1 indicates no degree and 5 indicates highest degree (i.e., university qualification). Political Orientation is measured on a 1–11 scale with higher values indicating more conservative political views. Economic Success is measured on a 1–11 scale with higher values indicating a stronger belief that effort is important for economic success. Locus of Control is an equally-weighted index of four questions on a 1–5 scale where higher values indicate a more external locus of control (i.e. a belief that life is determined by outside factors such as luck and fate).*

Prob>F is the p-value of an F-test for joint significance of all controls.

S4: *Difference-in-Difference Estimates – Distributional Preferences*

Table S2: Difference-in-Difference Estimates for Distributional Preferences

	x-score		y-score	
EET wave 33	0.114	0.138	0.191*	0.138
	(0.077)	(0.078)	(0.109)	(0.111)
Treated x EET wave 33	-0.113	-0.133	0.072	0.131
	(0.109)	(0.111)	(0.153)	(0.156)
Treated	0.134	0.139	-0.074	-0.137
	(0.097)	(0.097)	(0.128)	(0.130)
Constant	-2.695***	-2.046***	3.278***	3.186***
	(0.068)	(0.507)	(0.092)	(0.552)
Controls	No	Yes	No	Yes
R^2	0.01	0.07	0.01	0.01
N	4,584	4,354	4,584	4,354

Notes: ***p<0.01, **p<0.05, *p<0.1

OLS regressions with standard errors clustered at the individual level in parentheses. The x-score (y-score) measures benevolence in the disadvantageous (advantageous) domain of inequality, where higher values mean more benevolence. "EET wave 33" is an indicator variable for participating in the EET in wave 33. "Treated x EET wave 33" indicates whether a respondent received information in wave 33 and "Treated" is an indicator for participation in the EET in wave 23 (and being in the treatment group in wave 33). Controls include log income, gender, age, education level, East Germany dummy, retirement status, employment status, number of household members and marital status.

S5:   Heterogeneity in Mobility Perceptions

In Section 3.1, we presented the correlates of mobility perceptions. Here, we provide additional evidence on specific subgroups. We hypothesized in our pre-analysis plan that our treatment will have a greater impact on subgroups who are more optimistic. Figure S2 displays the mobility perceptions for the different groups by treatment status. We first consider only the control group and note that right leaning and less educated respondents are the most optimistic. Accordingly, we observe the strongest disparities in perceptions in the control group along political orientation (left- and right-leaning) and education (successful qualification to attend university versus no qualification to attend university). Comparing perceptions across control and treatment group reveals that treated respondents have in all cases more pessimistic perceptions than non-treated respondents. Again, we observe the largest gap in perceptions along political orientation and education. Interestingly, perceptions do not differ much for beliefs about the role of luck and effort in economic success ("luck/effort beliefs") in both control and treatment group. Moreover, the gap between treated and non-treated respondents who believe to a greater extent in luck and who largely believe in effort is very similar. This is confirmed by looking at a respondent's locus of control, which reveals a remarkably similar picture to luck/effort beliefs. Locus of control describes the extent to which people believe they can control their own life or that outside factors such as luck and fate, determine their life (Rotter 1966). It is considered a key personal trait and thus provides a psychological underpinning to the missing link between luck/effort beliefs and mobility perceptions. Together, this suggests that respondents do not view the persistence of socio-economic status as a matter of luck.

Figure S2: Mobility Perception of Specific Subgroups across Treatment Status.
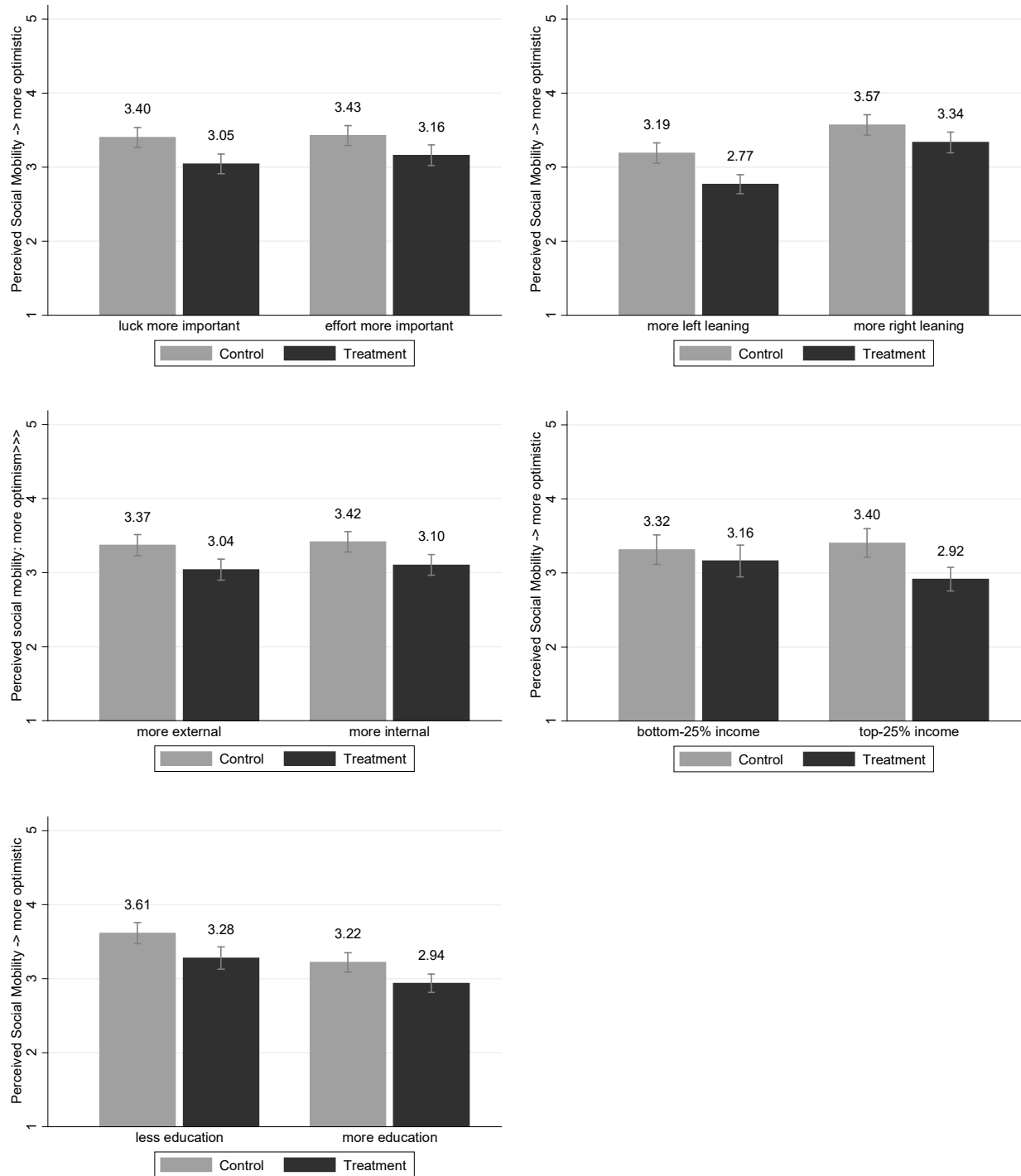
Notes: Groups are defined as follows: Left-column: "Luck (Effort) more important" indicates respondents below (at or above) the median response (6) to question about the importance of luck and effort for economic success (scale 1–10),)."more internal (external)" is the median split (2) of the locus-of-control index (index from 1–5). Right-column: "More left-leaning (right-leaning)" indicates respondents below (at or above) the median response (6) on the self-assessment in the left-right political spectrum (scale 1–10), "bottom (top) 25% income" indicate respondents in the bottom 25% (top 25%) of the income distribution in our sample, and "less (more) education" indicates respondents with no qualification for university (with qualification for university).

S6: Relation between Mobility Perceptions and Preferences

The previous analysis revealed that our treatment had a significant impact on mobility percep-
tions (see Table 2). These mobility perceptions are significantly related to support for redistribu-
tion, education expenditures as well as to the *y-score* (see Table S3). That is, more optimistic re-
spondents show less support for policies aimed at reducing inequality and are less benevolent in
the advantageous domain (and more malevolent in the disadvantageous domain) suggesting
more tolerance toward inequality, in general.

Using the information treatment as an instrument for mobility perceptions, we can esti-
mate the causal effect of mobility perceptions on outcomes. Note that we have to assume that the
treatment is uncorrelated with the error term, i.e. that the only effect of the treatment on outcomes
is through perceptions, as we have hypothesized. Our results indicate that there is no causal effect
of mobility perceptions on distributional and policy preferences. All estimates are insignificant
(see Panel B in Table S3).

Table S3: Mobility Perceptions

Panel A: OLS Estimates					
	Mobility Perception	**Redistribution**	**Education Exp.**	**x-score**	**y-score**
Treated	-0.177***				
	(0.039)				
Mobility Perception		-0.054***	-0.088***	-0.019	-0.085*
		(0.01)	(0.01)	(0.04)	(0.05)
Rich				-0.031	-0.372
				(0.17)	(0.23)
Poor				0.665***	-0.098
				(0.19)	(0.23)
Mobility*Rich				0.074	-0.008
				(0.06)	(0.08)
Mobility*Poor				-0.077	0.018
				(0.06)	(0.08)
Constant	0.088***	0.122***	0.198***	-2.689***	3.798***
	(0.027)	(0.03)	(0.03)	(0.10)	(0.13)
R²	0.008	0.008	0.025	0.009	0.006
F-statistic	21.0	--	--	--	--
N	2,661	2,641	2,648	2,583	2,583
Panel B: 2SLS Estimates					
Mobility Perception		0.068	-0.058	-0.042	-0.047
		(0.12)	(0.12)	(0.39)	(0.48)
Rich				-0.209	0.897
				(1.56)	(2.00)
Poor				0.994	-0.623
				(1.76)	(2.05)
Mobility*Rich				0.154	-0.573
				(0.69)	(0.89)
Mobility*Poor				-0.222	0.251
				(0.78)	(0.90)
Constant		-0.156	0.130	-2.635***	3.715***
		(0.28)	(0.28)	(0.89)	(1.09)
N		2,641	2,648	2,583	2,583

Notes: ***p<0.01, **p<0.05, *p<0.1*

Panel A presents the first stage in column 1 and the OLS estimates of the relationship between outcomes and mobility perceptions in columns 2–5. Panel B shows the 2SLS estimates using the random assignment to the information treatment as an instrument for mobility perceptions. Specifications do not include controls.

Table S4: Correlates of Distributional Preferences: Political Orientation, Redistribution and Equality of Opportunity

	Mobility Perceptions	x-score	y-score	Mobility Perceptions	x-score	y-score	Mobility Perceptions	x-score	y-score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Political Orientation	0.052***	0.043*	-0.100***						
(Left/Right)	(0.011)	(0.025)	(0.031)						
Redistribution				-0.191***	-0.318***	0.441***			
				(0.046)	(0.106)	(0.133)			
Equality of							0.129***	0.335***	-0.341***
Opportunity							(0.044)	(0.099)	(0.130)
Rich		0.091	-0.489***		0.047	-0.437***		0.034	-0.414***
		(0.110)	(0.147)		(0.115)	(0.154)		(0.115)	(0.154)
Poor		0.437***	-0.127		0.460***	-0.135		0.461***	-0.139
		(0.122)	(0.150)		(0.128)	(0.157)		(0.128)	(0.157)
Constant	0.008	-2.422***	4.191***	0.656***	-1.924***	3.749***	0.502**	-2.140***	4.087***
	(0.238)	(0.586)	(0.670)	(0.241)	(0.575)	(0.671)	(0.238)	(0.572)	(0.666)
N	2,433	2,372	2,372	2,262	2,205	2,205	2,262	2,205	2,205
R^2	0.03	0.09	0.02	0.03	0.09	0.02	0.03	0.09	0.02

*Notes: ***p<0.01, **p<0.05, *p<0.1*

OLS regressions with robust standard errors in parentheses. "Mobility Perceptions" ("How does economic success depend on being born into poor or rich household?") is measured on a 1–10 scale. The variable is normalized to zero mean and unit variance and higher values indicate more optimism (i.e. weaker dependence on parental income). The x-score (y-score) measures benevolence in the disadvantageous (advantageous) domain of inequality, where higher values mean more benevolence. "Political Orientation (Left/Right)" is the self-reported location in the political left–right spectrum. Higher values indicate more right-leaning political values. "Redistribution" is an indicator for respondents saying income inequality should be reduced and "Equality of Opportunity" is an indicator for respondents saying that everyone should have equal chances to achieve a good income (both questions from wave 24). Rich and Poor are dummies equaling 1 if a respondent received information about the relative income of the other person in the EET (i.e. that the person is among the richest 10% or poorest 10% poorest in the sample, respectively). Controls include log income, gender, age, education level, East Germany dummy, retirement status, employment status, number of household members, and marital status.

S7: Survey Instrument

The codebook for wave 33 of the GIP is available here:

https://doi.org/10.4232/1.13155 and https://search.gesis.org/research_data/ZA6953

In the following we present screenshots (and a translation) of the questions in our survey module, the treatment intervention and the EET.

Luck – Effort

Wie finanziell erfolgreich eine Person im Leben ist, kann zum einen von Umständen abhängen, auf die man selbst keinen Einfluss hat, wie zum Beispiel Glück. Oder zum anderen von Umständen abhängen, die man selbst beeinflussen kann, wie zum Beispiel persönliche Anstrengung.

Inwiefern ist, Ihrer Meinung nach, Glück oder Anstrengung dafür ausschlaggebend, ob jemand in Deutschland viel Geld verdient?

How financially successful a person is in life can depend on circumstances beyond one's control, such as luck. Or, on the other hand, it can depend on circumstances that one can influence oneself, such as personal effort.

In your opinion, to what extent does luck or effort determine whether someone earns a lot of money in Germany? [only luck (0) – only effort (10)]

Intro Treatment

Nun zu einem anderen Thema.

In vor Kurzem veröffentlichten wissenschaftlichen Studien sind Forscher der Frage nachgegangen, welcher Zusammenhang zwischen dem Einkommen der Eltern und dem Einkommen ihrer Kinder, wenn diese erwachsen sind, besteht.

< Zurück   Weiter >

In recently published scientific studies, researchers have explored the question of the relationship between parents' income and their children's income when they are adults.

Dabei geht es hauptsächlich um zwei Fragen. Zum einen wie hoch die Wahrscheinlichkeit ist, dass ein Kind aus einem reichen Haushalt im Erwachsenenalter einem armen Haushalt angehört. Zum anderen wie hoch die Wahrscheinlichkeit eines Kindes aus einem armen Haushalt ist, im Erwachsenenalter einem reichen Haushalt anzugehören. Was bedeutet hier arm und reich? Wenn wir alle Haushalte in Deutschland vom niedrigsten bis zum höchsten Einkommen aneinanderreihen würden, dann wäre ein Haushalt arm, wenn er zu den 25 Prozent der Haushalte mit dem niedrigsten Einkommen gehört. Umgekehrt wäre ein Haushalt reich, wenn er zu den 25 Prozent der Haushalte mit dem höchsten Einkommen gehört.

< Zurück   Weiter >

There are two main questions at stake here. The first is the probability that a child from a rich household will belong to a poor household in adulthood. The second is the probability that a child from a poor household will belong to a rich household in adulthood. What do poor and rich mean here? If we were to line up all households in Germany from the lowest to the highest income, a household would be poor if it belonged to the 25 percent of households with the lowest income.

Conversely, a household would be rich if it belonged to the 25 percent of households with the highest income.

Treatment Information

The studies used the most recent data from an independent, scientific survey of more than 12,000 private households in Germany, which has been conducted annually with the same individuals and families since 1984.

The data show that the expected probability of a child from a poor household being rich as an adult is very low. By contrast, the probability of a child from a poor household also being poor as an adult is very high. The following graph illustrates these relationships.

Illustration from Original Survey: *Illustration translated into English:*

Conversely, the expected probability of a child from a rich household being poor as an adult is very low. By contrast, the probability that a child from a rich household will also be rich as an adult is very high. The following chart illustrates these relationships.
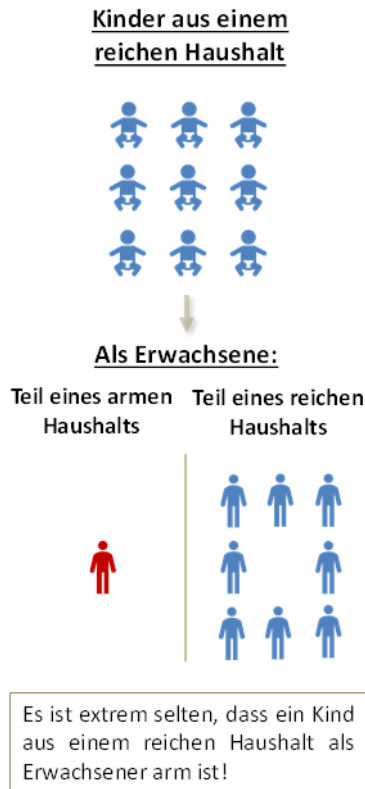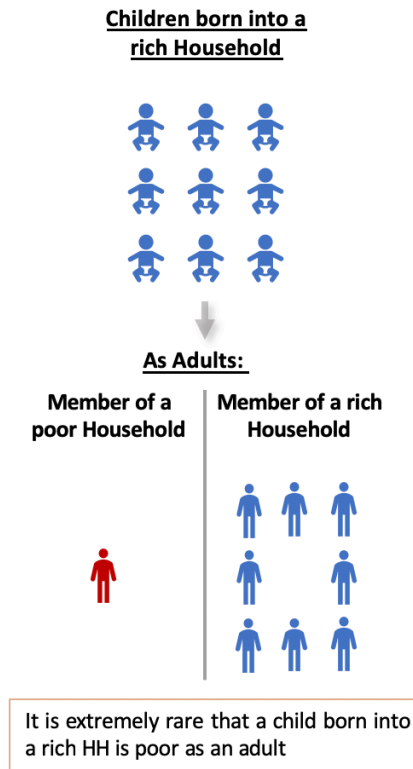
Illustration from Original Survey: Illustration translated into English

Kinder aus einem
reichen Haushalt

Als Erwachsene:

Teil eines armen   Teil eines reichen
Haushalts   Haushalts

Es ist extrem selten, dass ein Kind aus einem reichen Haushalt als Erwachsener arm ist!

Children born into a
rich Household

As Adults:

Member of a   Member of a rich
poor Household   Household

It is extremely rare that a child born into a rich HH is poor as an adult

Manipulation Check

Imagine 100 households that together represent the population of Germany.

What do you think? To what extent does economic success as an adult (e.g., attaining a high level of education or an above-average income) depend on whether you grew up in one of the 25 poorest or one of the 25 richest households? [very little (0) – very much (10)]

Instructions for Equality-Equivalence Test

Dear participant of "Gesellschaft im Wandel",

In the following, we would like to ask you to distribute money between you and another anonymous participant of "Gesellschaft im Wandel". [if expAE33040 = 1: The other participant is selected from the group of participants whose income is among the 10 percent of the highest incomes of all participants.] [if expAE33040 = 2: The other participant is selected from the group of participants whose income is among the 10 percent of the lowest incomes of all participants.] We will call the other randomly chosen participant your recipient. The distributional decisions concern real money; some randomly chosen decisions will actually be paid to the participants.

You will now successively see six tables. The two left columns in the table always show a distribution where you and your recipient are getting the same amount of money. The two right columns in the table always show a distribution where your recipient always receives the same amount of money, while your amount of money increases from one row to the next. All in all, this implies that the distribution on the left hand side always stays the same, whereas the one on the right hand side becomes more favorable for you, because you receive more money the further you go down in the table.

We would thus expect that participants prefer the left distribution at the beginning and then want to switch to the right distribution at some point. However, there might be participants who always prefer one distribution over the other. We want you to indicate in which row you would like to switch from the left distribution to the right distribution, i.e. from which row onwards you prefer the right distribution. On the following page, we will explain these tables with an example.

Later, the computer will randomly select exactly 250 participants from among all participants who have filled out all 6 tables, and will in turn randomly pay out one row from each table. The participant's decision in this row then determines whether the left or right distribution is paid out with real money. In addition, this decision is assigned to another participant in this survey and this participant receives the amount of the other player. The money will be credited to the

participants' study accounts. No participant can be selected more than once. We are expecting around 3000 participants in this survey.

To sum up: In this part of the survey, you are taking decisions in tables in which you are asked to indicate the row in which you for the first time prefer the right over the left distribution. [if expAE33040 = 1: You know about your recipient that their income is among the 10 percent of the highest incomes of all participants.] [if expAE33040 = 2: You know about your recipient that their income is among the 10 percent of the lowest incomes of all participants.] In addition to a chance to earn money in the role of an active participant, you also have a chance to earn money as a passive recipient.

Example:

You can see in this table that you and the recipient both receive 20 euros in each row in the left distribution. In the right distribution, your amount of money increases from row to row while the passive recipient always receives 15 euros.

You are now supposed to choose the row in which you for the first time prefer the right over the left distribution. For example, if you for the first time prefer the right over the left distribution in the penultimate row, meaning you would rather receive 22 euro and the recipient 15 euros (right distribution) than both of you receiving 20 euros (left distribution) and you preferred the left distribution in all prior rows, then you should indicate the penultimate row as the one where you first preferred the right distribution over the left one.

We would now like to ask you to choose the row in which you would like to change from the left to the right distribution. In order to do so, please click on the row that you choose. After you have marked the row, the rest of the table will be completed automatically. For example, if you mark the first row, this implies that you always prefer the right distribution over the left one. Please control your decision one more time before you click on Continue.

Please select the row from which you prefer the right distribution over the left distribution. All numbers are in euro.

	Verteilung: **Links**		Verteilung: **Rechts**	
	Sie erhalten:	Ihr Mitspieler erhält:	Sie erhalten:	Ihr Mitspieler erhält:
○	20	20	16	15
○	20	20	18	15
○	20	20	20	15
○	20	20	22	15
○	20	20	24	15
○	Ich bevorzuge immer die linke Verteilung.			

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Choice lists for advantageous domain (*y-score*)

The two left columns ("Verteilung Links") show the equal reference allocation (10 Euro for self, 10 Euro for the other participant). The two right columns ("Verteilung Rechts") show the unequal allocations. In the advantageous domain the payoff of the other participant is fixed (e.g., 3 Euro as in the screenshot below) and the payoff for self increases (from 5 Euro to 14 Euro in the screenshot below). Respondents had to indicate in which row the want to switch from the equal allocation ("Verteilung Links") to the unequal allocation ("Verteilung Rechts").

	Verteilung: **Links**		Verteilung: **Rechts**	
	Sie erhalten:	Ihr Mitspieler erhält:	Sie erhalten:	Ihr Mitspieler erhält:
○	10	10	5	3
○	10	10	7	3
○	10	10	9	3
○	10	10	9,50	3
○	10	10	10	3
○	10	10	10,50	3
○	10	10	11	3
○	10	10	11,50	3
○	10	10	12	3
○	10	10	14	3
○	Ich bevorzuge immer die linke Verteilung.			

Bitte wählen Sie die Zeile aus, ab der Sie die rechte Verteilung gegenüber der linken Verteilung bevorzugen. Alle Zahlen sind Eurobeträge.

< Zurück   Weiter >

Gesellschaft
im Wandel

Bitte wählen Sie die Zeile aus, ab der Sie die rechte Verteilung gegenüber der linken Verteilung bevorzugen. Alle Zahlen sind Eurobeträge.

	Verteilung: **Links**		Verteilung: **Rechts**	
	Sie erhalten:	Ihr Mitspieler erhält:	Sie erhalten:	Ihr Mitspieler erhält:
○	10	10	6	5
○	10	10	8	5
○	10	10	9	5
○	10	10	9,50	5
○	10	10	10	5
○	10	10	10,50	5
○	10	10	11	5
○	10	10	11,50	5
○	10	10	12	5
○	10	10	14	5
○	Ich bevorzuge immer die linke Verteilung.			

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Gesellschaft im Wandel

Bitte wählen Sie die Zeile aus, ab der Sie die rechte Verteilung gegenüber der linken Verteilung bevorzugen. Alle Zahlen sind Eurobeträge.

	Verteilung: **Links**		Verteilung: **Rechts**	
	Sie erhalten:	Ihr Mitspieler erhält:	Sie erhalten:	Ihr Mitspieler erhält:
○	10	10	8	7
○	10	10	8,50	7
○	10	10	9	7
○	10	10	9,50	7
○	10	10	10	7
○	10	10	10,50	7
○	10	10	11	7
○	10	10	11,50	7
○	10	10	12	7
○	10	10	14	7
○	Ich bevorzuge immer die linke Verteilung.			

< Zurück   Weiter >

Choice lists for disadvantageous domain (*x-score*)

The two left columns ("Verteilung Links") show the equal reference allocation (10 Euro for self, 10 Euro for the other participant). The two right columns ("Verteilung Rechts") show the unequal allocations. Again, in the disadvantageous domain the payoff of the other participant is fixed (e.g., 13 Euro as in the screenshot below) and the payoff for self increases (from 7 Euro to 12 Euro in the screenshot below). Respondents had to indicate in which row the want to switch from the equal allocation ("Verteilung Links") to the unequal allocation ("Verteilung Rechts").

Gesellschaft im Wandel   **Hilfe**

Bitte wählen Sie die Zeile aus, ab der Sie die rechte Verteilung gegenüber der linken Verteilung bevorzugen. Alle Zahlen sind Eurobeträge.

	Verteilung: **Links**		Verteilung: **Rechts**	
	Sie erhalten:	Ihr Mitspieler erhält:	Sie erhalten:	Ihr Mitspieler erhält:
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○	10	10	8	13
○	10	10	8,50	13
○	10	10	9	13
○	10	10	9,50	13
○	10	10	10	13
○	10	10	10,50	13
○	10	10	11	13
○	10	10	11,50	13
○	10	10	12	13
○	Ich bevorzuge immer die linke Verteilung.			

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Gesellschaft im Wandel

Bitte wählen Sie die Zeile aus, ab der Sie die rechte Verteilung gegenüber der linken Verteilung bevorzugen. Alle Zahlen sind Eurobeträge.

	Verteilung: **Links**		Verteilung: **Rechts**	
	Sie erhalten:	Ihr Mitspieler erhält:	Sie erhalten:	Ihr Mitspieler erhält:
○	10	10	7	15
○	10	10	8	15
○	10	10	8,50	15
○	10	10	9	15
○	10	10	9,50	15
○	10	10	10	15
○	10	10	10,50	15
○	10	10	11	15
○	10	10	12	15
○	10	10	14	15
○	Ich bevorzuge immer die linke Verteilung.			

< Zurück   Weiter >

Gesellschaft im Wandel

Bitte wählen Sie die Zeile aus, ab der Sie die rechte Verteilung gegenüber der linken Verteilung bevorzugen. Alle Zahlen sind Eurobeträge.

	Verteilung: **Links**		Verteilung: **Rechts**	
	Sie erhalten:	Ihr Mitspieler erhält:	Sie erhalten:	Ihr Mitspieler erhält:
○	10	10	7	17
○	10	10	8	17
○	10	10	8,50	17
○	10	10	9	17
○	10	10	9,50	17
○	10	10	10	17
○	10	10	10,50	17
○	10	10	11	17
○	10	10	13	17
○	10	10	16	17
○	Ich bevorzuge immer die linke Verteilung.			

< Zurück

Weiter >

Redistribution

Redistribution of income means that the state reduces income differences between citizens through taxes and transfer payments.

How much redistribution of income do you support between citizens in Germany?

No redistribution (1) means that the state does not intervene in the distribution of income. Full redistribution (10) means that everyone earns the same income after redistribution. [scale 1-10]

Education Policy

Should the federal government spend more or less money on the education system than it currently does?

Keep in mind that higher spending must be financed, among other things, through taxes, i.e. ultimately through deductions from wages.

[Spend much more than at present (1), spend a little more than at present (2), spend the same as at the moment (3), spend a little less than at present (4). spend much less than at present (5)]

<center>Pre-analysis Plan:</center>

Social Mobility and Distributional Preferences: Representative Evidence from Germany

<center>Dietmar Fehr, Daniel Müller, and Marcel Preuss</center>

<center>February 2018</center>

1. Introduction

The recent surge in income and wealth inequality has rekindled the public and economic debates about the causes and consequences of inequality. While inequality is to some extent inevitable, its acceptance critically hinges on individuals' views about the fairness of inequality. An important aspect of individuals' tolerance for inequality is their perception of social mobility. If people believe that they face ex-ante equal prospects and that they can move up the social ladder, they may be willing to accept more inequality.

In this project, we investigate the relationship between intergenerational mobility and distributional preferences, which are key inputs into social policy measures and individual decision-making. To this end, we implemented a survey module in a representative panel study of Germans. This survey module allows us to present a randomly selected subgroup information about actual mobility rates in Germany and subsequently measure participants' distributional preferences in an incentivized way. This pre-analysis plan presents the data sources, the structure of the experiment, the empirical strategy, and hypotheses.

2. Research strategy

We implement a survey module in the German Internet Panel (GIP). The GIP is an online panel survey maintained by the University of Mannheim and based on a probability sampling method of the general German population aged 16 to 75 years. The recruitment of survey participants was done in face-to-face interviews and thus includes people without internet access at the time of recruitment (these people received tablets with internet access to participate in the survey). The panel includes about 5,000 registered participants, who are invited to take part in an online interview every other month. The interviews typically include questions regarding attitudes towards reform policies, the welfare state, German and international politics, health, inequality, education and employment. Once a year, the GIP collects and updates key socio-demographic information of participants. The data is publicly available at the GESIS Institute for Social Sciences.

<center>60</center>

We implemented our module in Wave 33 of the GIP, which was fielded in January 2018. We completed the pre-analysis plan in February 2018 and we had no access to the data set before the plan was registered at the AEA RCT trial (the preliminary data will be available on March 7, 2018).

3. Design of the survey module

Our survey module consists of four parts (for more details about the module, see appendix). The first part contains a single question about the role of luck and effort for economic success (answers on 11-point scale, with 0 = only luck and 10 = only effort). After the first part, participants complete another unrelated survey module. We introduced this time lag to avoid priming respondents into a particular direction before presenting information on actual intergenerational mobility.

The second part is our main intervention. We will provide half of the respondents with information on actual intergenerational mobility (treatment group). More precisely, the treatment group will learn the likelihood of advancing to the top quartile of the income distribution when born into a household in the bottom-income quartile in Germany, as well as the likelihood of moving from the top-income quartile to the bottom-income quartile when born into a top-income quartile household. This information is based on evidence for Germany (Schnitzlein, 2016; Stockhausen, 2017) and is presented in a generic way. Immediately after the intervention, we elicit respondents' beliefs about social mobility. This serves as manipulation check of our information intervention.

In the third part, we measure the distributional preferences of all survey respondents using a version of the Equality Equivalence Test (Kerschbamer, 2015). The Equality Equivalence Test (EET) is an incentivized measure for distributional preferences and consists of a series of binary allocation decisions in which the decision maker is asked to distribute money between herself and some other, unknown person. The test elicits behavior in the domain of advantageous as well as disadvantageous inequality and classifies people into mutually exclusive and well-delineated sets of distributional preference types *at the individual-level*.

In the EET, subjects make a series of binary choices each of which involves two allocations corresponding to a point in the self-other space. Each choice consists of a symmetric and an asymmetric allocation. In the symmetric allocation, the decision maker and the recipient receive the same material outcome, whereas the asymmetric allocation entails unequal material consequences. In half of the choices, the asymmetric allocation covers the domain of disadvantageous inequality, while in the other half it covers the domain of advantageous inequality. In both domains, the EET systematically varies the price of giving (or taking) by increasing the material payoff of the decision maker in the asymmetric allocation while keeping recipients' payoffs constant.

We will use the same version of the EET as in wave 23 of the GIP, which was implemented by one of the authors (Kerschbamer and Muller, 2017). This allows us (i) to draw on a comprehensive distribution of distributional preference types elicited before the intervention and (ii) to compare distributional preferences over two different waves (this will be part of a second project that will

not be described here). More specifically, this version of the EET consists of six choice lists. In each choice list respondents will see ten binary decisions between a symmetric and an asymmetric allocation. The payoffs for the decision maker and the recipient in the symmetric allocation will be 10 Euro in each choice and each choice list. The asymmetric allocation in three choice lists will be in the domain of advantageous inequality. There, the recipients' payoffs in a choice list will be either 3, 5, or 7 Euro, while the payoffs of the decision makers vary from 5 to 14 Euro. In the remaining three choice lists the asymmetric allocation is in the domain of disadvantageous inequality. That is, the recipient's payoffs will be either 13, 15, or 17 Euro while the decision makers' payoffs vary from 7 to 16 Euro. (The six choice list are displayed in Tables 1-6 in the appendix.) Due to constraints of the survey, respondents will not indicate their choice for each allocation in a table, but instead will indicate their switching point from the symmetric to the asymmetric allocation. As a consequence, we will get consistent choices for all decision makers within (but not necessarily across) lists. Choice lists will be displayed in a randomly determined order and each list will be shown on a separate page (though respondents can go back and forth between lists and revise their decisions).

A novel feature of our EET implementation is that a random subsample of respondents receives information about the recipient's income position. In particular, about a quarter of participants will be informed that the recipient belongs to the top-10 percent of the income distribution of all survey participants. Another quarter will be informed that the recipient belongs to the bottom-10 percent of the income distribution of all survey participants. The remaining half of participants will receive no information about the income position of the recipient. Note that this group will have the same information about the recipient as participants in wave 23.

The EET is incentivized. We will randomly select 250 participants for payment. This subset of participants will be paid according to their decision in a randomly selected row of one randomly selected choice list. In addition, we will randomly match a recipient (from the pool of eligible survey participants) to each selected decision maker. This recipient will be paid according to the decision maker's choice in the selected row of the selected choice list.

In the fourth part, we ask respondents about their attitudes toward redistribution and public spending on education. Answers to the redistribution questions are on an 11-point scale (0 = no redistribution, 10 = full redistribution) and answers to the education spending question are on a 5-point scale (1 = spend more than currently, 5= spend significantly less than currently).

4. Hypotheses

We presume that most individuals overestimate the likelihood of social mobility (i.e., they are too optimistic about social mobility). Thus, our treatment is designed to induce more pessimistic beliefs about social mobility. Accordingly, we hypothesize that our treatment provides on average a negative information shock for individuals' perceptions about social mobility. Therefore, we

expect that treated individuals demand more redistribution and more expenditures on education. Moreover, we expect that if the information intervention has an impact on distributional preferences, then we will observe a shift towards more inequality aversion (see also Section 7.3).

5. Definition of Outcomes

Our main variables of interest are: (i) respondents' distributional preferences, which we elicited using the EET, (ii) a question on respondents' demand for redistribution, and (iii) a question capturing attitudes toward expenditures on education.

For the EET we will use the (x,y) score of each individual. We briefly sketch how this score is computed (see Kerschbamer, 2015 for details). For each choice in the domain of disadvantageous inequality we will attribute an x-score using the following rule:

$$xscore_i = 6.5 - ROW_i$$

where ROW_i refers to the switching point of the decision maker. An earlier switching point (lower row number) implies a higher willingness–to-pay to increase the payoff of the recipient. Therefore, a higher x-score corresponds to a more benevolent behavior. Similarly, we will compute a y-score for the domain of advantageous inequality:

$$yscore_i = ROW_i - 5.5$$

Again, a higher score implies more benevolence.

In the regression analysis, we follow Kerschbamer and Muller (2017) and will use the average value of the three x- and y-scores for every subject as the dependent variable. We are then able to study the treatment effect for two distinct categories: How does being ahead of the other person influence the revealed attitudes towards inequality (y score) and how are these attitudes influenced when being behind of the other person (x-score)?

The attitudinal question on subjects' demand on redistribution (Red_i) is on a scale from 1 (no redistribution) to 10 (full redistribution). The question on education expenditures (Edu_i) has five answer categories from 1 (spend much more than the status quo) to 5 (spend much less than the status quo). We will recode the answer categories of this question, such that a higher number corresponds to more expenditure, i.e., 1 (spend much less than the status quo) to 5 (spend much more than the status quo). Accordingly, a positive regression coefficient can be interpreted as an increase in both redistribution and education spending.

6. Empirical Strategy

The general framework in which we will study the effects of information about intergenerational mobility on preferences for redistribution will take the following form:

$$Y_i = \alpha + \beta T_i + \delta R_i + \vartheta P_i + \beta_{Rich}(T_i \times R_i) + \beta_{Poor}(T_i \times P_i) + \gamma X + \varepsilon_i \quad (1)$$

where T_i is a Dummy-Variable equaling 1 in the case a subject received information on social mobility, R_i and P_i are dummies equaling 1 if a subject in the EET received information on the other persons location in the income distribution (bottom 10%/top 10%), X is a set of standard controls (including Age, Gender, Income, Marital Status, Size of household, Employment status, Retirement status, Education, East Germany; see also Section 7.1) and Y_i is one of our four main outcomes defined above (x,y score, Red_i, Edu_i). We will use OLS regressions and robust standard errors.

7. Main Analysis

7.1. Baseline Balance

We will test for baseline balance for the following variables:

- Age (in years)
- Gender (female/male)
- Income (log of midpoint of interval)
- Marital status (married/not married)
- Size of household
- Employment status (unemployed/employed)
- Retirement status (retired/not retired)
- Education (no degree/high school without university qualification/high school with university qualification or apprenticeship combined with high school without university qualification/ apprenticeship and high school degree with university qualification/University degree or more)
- East Germany (yes/no)
- Luck/effort (11-point scale)
- Ideology (11-point scale)

All these variables come from the German Internet Panel (GIP) wave 31, except the variable luck/effort, which is part of the most recent wave.

We will regress these variables on a treatment indicator to see whether the covariates are correlated with the treatment. We will also conduct a joint F-test to see if the coefficients are jointly different from zero.

7.2. Manipulation check

To check whether our information treatment has an effect on individuals' perception of social mobility ("first stage"), we compare the responses to the question "What do you think, how does economic success as adult depend on whether one has grown up in one of the 25 poorest households or in one of 25 richest households?" in the control group and treatment group. We will regress the answers to this question on a treatment indicator (with and without covariates).

7.3. Information about Intergenerational Mobility

To study the main treatment effect, we estimate regression (1). Our treatment is designed to shift subjects' perception of social mobility towards more pessimism. Given that that we find a "first-stage" result (manipulation check), we hypothesize that demand for redistribution and education expenditures increase. Moreover, a higher demand for redistribution in the EET would correspond to a lower x-score, as it measures the willingness of the decision maker to accept a higher income of her partner. The corresponding effect in the domain of advantageous inequality is a higher y-score. Observing this combination of effects would imply a shift in the social preferences in the direction of higher *inequality aversion* through the treatment.

Thus,

i. $H_0: \beta = 0 \ vs. \ H_1: \beta > 0 \ for \ Y_i = \{Red, Edu, yscore\}$

ii. $H_0: \beta = 0 \ vs. \ H_1: \beta < 0 \ for \ Y_i = \{xscore\}$

7.4. Information about Partner in EET

Besides studying the effect of the information treatment on its own, we are also interested in understanding the interaction of our treatment with the allocated partner during the EET. That is, how does the information provided alter the social behavior of people when they interact with relatively poor (relatively rich) people? In terms of regression (1) these effects are captured by β_{poor} and β_{rich}.

As before, we hypothesize that the treatment leads to a shift in the beliefs about the determinants of economic success in the direction of a higher influence of the social background. In combination with the information about the economic status of the partner, this would imply that the level of empathy (benevolence) increases against those who are relatively poor – as their position is now less judged to be their own fault. We expect the opposite effect for the subgroup that is matched with a rich person (top 10%). We thus test:

iii. $H_0: \beta_{poor} = 0 \ vs. \ H_1: \beta_{poor} > 0$

iv. $H_0: \beta_{rich} = 0 \ vs. \ H_1: \beta_{rich} < 0$

7.5. Control Group: Information about Partner in EET

We analyze the effect of information about the partner's economic status, when allocating money during the EET. To abstract from the information treatment on social mobility, we only consider the control group. That is, we focus on the coefficients δ and ϑ of regression (1), which single out the effect of having a relatively rich (δ) or a relatively poor partner (ϑ). The reference group during the analysis are therefore those participants who did not receive any information about their partners.

We hypothesize that people vary their behavior, if the other person is known to be relatively poor (relatively rich). Hence, we test:

    v. $H_0: \delta = 0 \ vs. H_1: \delta < 0$

    vi. $H_0: \vartheta = 0 \ vs. H_1: \vartheta > 0$

8. Heterogeneous Treatment Effects

Our treatment should induce more pessimism in beliefs about social mobility. It is likely that the impact of the treatment depends on prior held beliefs and characteristics of subjects. We proceed in two steps.

First, we use information on respondents' distributional preference types from wave 23 and investigate whether our information intervention has an effect on the x,y scores.

We will estimate the following regression specification (difference-in-difference strategy):

$$Y_{it} = \alpha + \delta_0 T_i + \delta_1 EET_t + \beta_{DiD}(T_i \times EET_t) + \gamma X + \varepsilon_{it} \tag{2}$$

Where Y_{it} is either the *xscore* or *yscore* measured by the EET, T_i is a treatment dummy for our intervention in wave 33, EET_{23} is dummy for the EET in wave 23, and β_{DiD} is the coefficient of interest, i.e., the Differences-in-Differences estimate. (Alternatively, we will also look at a specification using fixed effects.)

Second, we concentrate on the data of wave 33 and estimate the following regression:

$$Y_i = \alpha + \beta T_i + \varphi heterogeneous_i + \sigma(T_i \times heterogeneous_i) + \gamma X + \varepsilon_i \tag{3}$$

Where $Y_i = \{xscore, xscore, Red, Edu\}$, T_i is a treatment dummy for our intervention and $heterogeneous_i$ corresponds to the variable(s) of interest (specified below). For the two outcomes Red_i and Edu_i we use the whole sample. When looking at the x,y scores we restrict the analysis to the subgroup that did not receive any information about their partner during the EET (alternatively we run the regression on the all subgroups of the EET and additionally include dummies for the subgroups in the EET that received information on the other persons location in the income distribution (bottom 10%/top 10%)).

8.1. Luck/Effort

Before randomly assigning the information treatment, we ask participants to state their belief about the role of effort and luck in determining economic success – see experimental design section. It seems plausible that those who think effort rather than luck is crucial for success are comparatively more optimistic about social mobility than those who believe otherwise.

We hypothesize, that the treatment effect is amplified for those, who believe more firmly in effort (are more optimistic) and that the beliefs also have a level effect. That is,

    vii. $H_0: \varphi = 0 \ vs. H_1: \varphi \neq 0$

    viii. $H_0: \sigma = 0 \ vs. H_1: \sigma \neq 0$

8.2. Status Groups

How optimistic people are about social mobility may be related to their occupational status. A report of the Allensbach Institute (2013), based on a representative survey of Germans, suggests that the perceptions about the likelihood of social mobility differ significantly across occupational status. We will use information about the job prescription of the participants provided in wave 19 of the GIP to distinguish the following groups:

> (1) Semiskilled Worker (*German: Angelernte Arbeiter*)
>
> (2) Skilled Worker (*Facharbeiter*)
>
> (3) Employees / officials (*Einfache Angestellte / Beamte*)
>
> (4) Executive Employees / Officials (*Leitende Angestellte / Beamte*)
>
> (5) Self-employed / Liberal Profession (*Selbstständige / Freie Berufe*)
>
> (6) Others

According to Allensbach (2013), the degree of optimism when asked to evaluate the likelihood of social mobility increases monotonically from group (1) to group (5). We therefore expect that the information treatment is more effective for those the more optimistic groups.

We will estimate a specification similar to regression (3), where we include both dummies for every group (2) to (6) and the corresponding interaction terms with the treatment variable. We expect that our treatment has more impact on people who hold ex-ante more positive beliefs about social mobility.

Since the information to construct the different status groups is from 2015 (Wave 19), we will consider an alternative measure as a proxy for status. Specifically, we will rely on information on educational attainment from September 2017 (Wave 31). We will use the same categories for this variable as specified in Section 7.1. Similarly, we expect that subjects with more education hold more positive views about mobility and that they are more affected by our treatment.

8.3. Income

We will consider three income groups and test whether our treatment had a differential impact on these groups. More precisely, we will use a specification similar to regression (3) and include dummies (and interactions) for the bottom 25% income group and top 75% income group in regression (3). We expect that the x,y score decreases (increases) for low-income (high-income) subjects (two-sided tests).

8.4. Political Preferences

The last part of our heterogeneity analysis will look at political preferences. Alesina et al. (2018) find strong effects of partisanship on the effectiveness of their information treatment on intergenerational mobility. In fact, those who categorize themselves as left-wing show a strong reaction in the direction of more redistribution as a response to the treatment, while no such effects can be shown for those on the political right. In wave 30 of the GIP participants are asked to place their own political world-view on a scale from 1 (far left) to 11 (far right) and to give an answer to the question which party they intend to vote for during the next national election.

We will use both questions to proxy for political orientation and add interaction terms with the treatment dummy as well as level dummies in a regression similar to regression (3). Additionally, we will look at an index of both questions following the approach of Kling et al. (2007). We expect that our treatment has a larger effect on left-leaning subjects.

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