Social Mobility Perceptions and Inequality Acceptance*

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Abstract

This paper examines how perceptions of social mobility affect acceptance of inequality. We conduct a randomized information intervention in a large and heterogeneous sample of Germans to manipulate beliefs about social mobility. While the information treatment renders social mobility perceptions significantly more pessimistic, we find strong evidence that these more pessimistic perceptions change neither revealed distributional preferences nor support for greater redistribution or education spending. We present suggestive evidence for the lack of a measurable treatment effect. Participants do not seem to perceive low mobility rates as unfair, as they do not link the persistence of socioeconomic status to luck. Finally, the large sample size allows us to rule out economically meaningful treatment effects.

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

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

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1 Introduction

Recent studies have documented a remarkable negative relationship between income inequality and intergenerational mobility between and within countries (Corak 2006, 2013; Chetty et al. 2014). Popularized by Alan Krueger (2012) as the "Great Gatsby Curve," this observation has fueled heated debate among scholars and policymakers, as it suggests that rising inequality may reduce upward mobility (e.g. Solon 2004). Yet why do we not see additional public calls for higher levels of redistribution if the objective likelihood of climbing the social ladder is low? One explanation is that people have miscalibrated beliefs about intergenerational mobility. If they believe in equal ex-ante prospects for moving up the social ladder, they may oppose redistributive policies and accept more inequality (e.g. Piketty 1995, Bénabou and Ok 2001).

This paper examines how perceptions of social mobility impact inequality acceptance in a large and heterogeneous sample of participants from Germany. We build on recent literature on fairness ideals that stresses meritocratic principles (e.g. Cappelen et al. 2007; Cappelen, Sørensen, and Tungodden 2010) and suggests that people are more willing to accept inequality if they associate a meritocratic society with a high degree of income mobility. To test the causal effect of mobility perceptions on inequality acceptance, we expose a randomly selected subsample of respondents to information about actual social mobility rates in Germany. Estimates of intergenerational earnings mobility typically locate Germany somewhere between the two extremes exemplified by Scandinavia and the US (Corak 2013).[1] To delineate respondents' inequality acceptance, we not only exploit answers to survey questions on two different redistributive policies, but also rely on an incentivized measure of distributional preferences.

In general, distributional preferences underlie many economic decisions and inform a wide range of social policy decisions, including tax levels, redistribution measures, and education expenditures (Luttmer and Singhal 2011, Weinzierl 2017, Saez and Stantcheva 2016, Kerschbamer and Müller 2020). Understanding how these preferences are formed and how elastic they are to information about the permeability of society thus allows us to obtain a more accurate picture of the potential consequences of changes in perceived income mobility. There is sizable heterogeneity in pro-social preferences between countries (e.g. Falk et al. 2018), which implies that the formation of distributional preferences at the individual level depends in large part on broader societal attitudes and frames of reference. As social mobility is an informative measure of the level of equality of opportunity, perceptions of social mobility may be a key determinant of the perceived level of the fairness in a given society.

This potential link raises an important empirical question: What is the relationship between low levels of social mobility and support for redistributive measures? Specifically, when presented with information about low levels of social mobility, do people become more or less pro-social in their behavior? To explore these questions, we conduct a pre-registered survey experiment, comprising about 2,500 respondents, in a high-quality online panel, the German Internet Panel (GIP). The GIP is

[1] 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).

representative along several observable covariates, offers tight control over who is participating, includes detailed background information on participants, and provides a host of information on respondents' political attitudes (Blom, Gathmann, and Krieger 2015). We randomize half of the respondents into an information treatment with the aim of providing a negative information shock to respondents' perceptions about intergenerational mobility. Subsequently, we (i) elicit respondents' distributional preferences via an incentivized distribution task, the Equality-Equivalence Test (EET, see Kerschbamer 2015) and (ii) investigate respondents' view on two important policies to curb inequality (specifically, their support for redistributional policies and education expenditures). The EET is a simple distributional task that requires a decision-maker to make a series of binary decisions between different allocations of money for herself and some other passive recipient (another respondent in the GIP). While the actual set of preferences is, of course, independent of information about the recipient, it is likely that information about recipients' background influences choices. For example, a highly inequality-averse individual may want to allocate resources unevenly between a poor and a rich recipient, which may be reinforced by learning about the persistence of social class. To explore this potential interaction effect between perceived income mobility and another person's income, we introduce a novel feature to our EET implementation and randomly inform a subset of decision-makers about the "real" relative income situation of their matched recipient. That is, decision-makers either receive no information about the recipients' income situation or are informed that the recipient is randomly drawn from the bottom or top 10% of the income distribution of the GIP.

A key feature of our study is that we integrate precise preference measures into a survey featuring a randomized information intervention on the permeability of society. Relying on an incentivized measure of distributional preferences overcomes a common critique that survey measures do not capture actual behavior and are prone to experimenter demand effects. Moreover, our incentivized measure offers tight control over the decision context. In particular, it permits us to control information about the socio-economic characteristics of recipients that decision-makers receive to investigate the sensitivity of distributional preferences to different contexts. On the other hand, survey questions offer less control and often (deliberatively) leave some room for interpretation. Thus, they cover broader aspects of certain topics than their behavioral counterpart. Together, our behavioral measure of distributional preferences and the two survey questions on participants' views on redistributive policies allow us to provide a more comprehensive picture of the potential impact of mobility perceptions on inequality acceptance.

We find that informing respondents about the prospects of upward and downward mobility in society has a large and significant effect on their mobility perceptions. As expected, treated respondents are more pessimistic about social mobility than untreated respondents. The effect size is large and comparable to other studies using information interventions including a closely related study by Alesina, Stantcheva, and Teso (2018). Yet despite the more pessimistic view on mobility on average, we find that the information treatment had no effect on policy preferences and incentivized distributional preferences. More precisely, we do not find that treated respondents become less averse to inequality than respondents in the control group. Because of the large sample size, we are able to rule out the possibility of social mobility perceptions exerting even a small effect on distributional preferences in all cases. Consistent with these findings, we also report precisely estimated null effects for respondents' policy preferences. That is, the information treatment has no effect on respondents' attitudes toward redistribution or on their support for education expenditures. As there is little reason to assign a high prior on the null hypotheses in our case, our null findings are more informative than the common practice of rejecting point nulls (see Abadie [2020] for a discussion about the informativeness of failures to reject nulls).

Taking advantage of the rich information provided by the German Internet Panel (GIP) and our own survey module, we explore possible mechanisms behind the lack of response evident in our findings. First, we find that providing information on a recipient's relative income rank in the EET does affect decision-makers' distributional choices. This finding highlights that respondents' distributional preferences are sensitive to context, as behavior responds in a predictable way. Specifically, we observe a sizeable shift toward less malevolence if decision-makers face a recipient from the bottom 10% of the income distribution as well as toward less benevolence when facing a recipient from the top 10% of the income distribution. While this finding suggests a novel perspective on distributional preferences, highlighting the potential importance of beliefs about the beneficiaries of redistributive policies, the information treatment does not amplify these effects.[2] Specifically, our respondents do not become more benevolent toward recipients from the bottom 10% after receiving information on the low prospects of upward mobility. These findings suggest that respondents do not view redistribution as a means of rectifying a lack of upward mobility. Moreover, the findings suggest that the absence of a measurable effect from the information intervention is not attributable to countervailing effects stemming from variation in beliefs about recipients' income situation.

Second, we investigate the possibility that different (pre-registered) groups of individuals may react in divergent ways to our main treatment. If this is the case, we may observe that some groups of individuals respond strongly, while others do not respond to the treatment, resulting in a zero average treatment effect. This exercise reveals no differential impact of the information treatment for all subgroups, except for low-income respondents. Overall, this evidence is in accordance with our results and additionally offers suggestive evidence for the lack of a measurable treatment effect. In particular, we observe that respondents who assign effort a greater role in determining economic success do not react to the information about actual social mobility rates, despite this information contradicting their prior beliefs on effort. That is, they essentially have the same beliefs about social mobility and share similar distributional and policy preferences as respondents who believe more in the role of luck. Moreover, we find no correlation between respondents' beliefs about the role of luck and effort for economic success (luck/effort beliefs) and social mobility perceptions. We substantiate the missing link between the persistence of socio-economic status and the determinants of economic success by looking at respondents' locus of control, which is a key personality trait that describes the extent to which people believe they can or cannot control their own life. Again, we find no evidence that locus of control is related to mobility perceptions and that respondents with a higher external locus of control (i.e. a belief that outside factors such as luck and fate, determine their life) are more affected by our treatment. Consequently, respondents may not react to our treatment because they do not seem to realize that information about low upward mobility is a signal of the extent to which someone can be held accountable for being poor or rich.

Taken together, we provide strong and consistent evidence that more pessimistic beliefs about social mobility have an effect neither on distributional preferences nor on policy preferences.

[2] This finding relates to lab evidence and nationally representative evidence that generosity toward "real-life" welfare recipients depends on recipients' worthiness (Fong 2007; Fong and Luttmer 2009).

We thereby add to and extend previous findings that have only relied on self-reported policy preferences, in particular Alesina, Teso, and Stantcheva (2018), who document perceptions of social mobility in France, Italy, Sweden, the UK, and the US. Similar to our study, they present a random subset of survey respondents with generic evidence on actual intergenerational mobility and find that this information has, on average, no meaningful and significant effect on support for redistributive policy measures. An exception are left-leaning respondents, who are more likely to respond to the treatment and thus show support for redistribution than right-leaning respondents. Our study extends their work along two dimensions. First, we replicate their main finding for Germany, lending further support to the lack of a causal relationship between perceived income mobility and demand for redistribution. Second, we investigate potential effects of income mobility perceptions on policy preferences *and* distributional preferences. An advantage of focusing on distributional preferences is that they are less dependent on trust in the government's ability to redistribute income or implement social policies effectively, which is an often-articulated reason for people's reluctance to implement redistributive policies, despite soaring concerns about inequality (e.g. Hetherington 2005, Kuziemko et al. 2015; Alesina, Stantcheva, and Teso 2018). In fact, the absence of a measureable treatment effect on distributional preferences in our experiment suggests that policy preferences may be hard to move as well, as evidenced by our findings. While this can point to the stability of these preferences, it could also be that participants fail to connect the persistence in social class to luck or inequality of opportunity, as suggested by our findings on luck/effort beliefs.

Moreover, we contribute to a broader understanding of the relationship between intergenerational mobility and inequality acceptance. In theory, beliefs about social mobility and beliefs about the importance of effort for economic success seem to be closely related. Thus, a cornerstone of our work is the theoretical literature linking people's beliefs about the latter to their support for redistribution. Piketty (1995) presents a model where individuals learn about the relative influence of effort and luck on income through their own mobility experience. Because of different experiences, individuals form different beliefs about the impact of luck on economic success, which in turn influences how much effort they exert and how much redistribution they demand. Once individuals get on different trajectories, heterogeneity in beliefs, effort, and support for redistribution may persist in the long-run. 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). The prospect of upward mobility hypothesis has received some empirical support (e.g. Alesina and La Ferrara 2005, Alesina and Giuliano 2010, Rainer and Siedler 2008).[3] For example, Rainer and Siedler (2008) present evidence, using German survey data, that people demand less redistribution if they believe in opportunities for upward mobility, and vice versa. We add to this literature by studying the causal effect of mobility perceptions on inequality acceptance. Unlike most other work on the formation of social policy preferences, we concentrate on distributional preferences, as any redistributive policy involves gains for some groups and losses for others, and thus raises concerns about fairness and efficiency. Thus, we investigate a general preference that underlies many social policy preferences.

[3] Checci and Filippin (2003) show that proposed tax rates decline with the prospect of upward mobility in a laboratory experiment.

Our paper also relates to a handful of papers documenting individual misperceptions about relative income, income, and wealth inequality (Norton and Ariely 2011, Page and Goldstein 2016, Hauser and Norton 2017), and 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. 2014, Karadja, Mollerstrom, and Seim 2017, Bastani and Waldenstrom 2019, Fehr, Mollerstrom, and Perez-Truglia 2019). All of these studies have in common that they solely focus on survey measures of preferences, whereas our study mainly focuses on an incentivized measure for distributional preferences. Other studies have focused on the external validity of incentivized measures for distributional preferences in representative surveys (Bellemare, Kroeger, and van Soest 2008, Fisman, Jakiela, Kariv, and Markovits 2015, Fisman, Jakiela, and Kariv 2017, Hedegard, Kerschbamer, Müller, and Tyran 2018, Kerschbamer and Müller 2020). These studies typically reveal considerable heterogeneity in these preferences and interesting correlations with voting behavior, political attitudes, and pro-social behavior, supporting our focus on distributional preferences.

The paper proceeds as follows. Section 2 presents detailed information about the GIP and describes the survey as well as the experimental design. At the end of Section 2, we describe our empirical strategy and hypotheses, which we laid out in a pre-analysis plan. In Section 3, we present our results. We first report the first-stage treatment effect on mobility perceptions. Next, we present the treatment effect on our main outcomes: respondents' distributional preferences and their support for redistributive policies. We continue in Section 4 with an analysis of the reaction of some sub-groups to the treatment. Section 5 concludes.

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.

[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.

2.2 The Survey Module

The survey module consists of four parts (see Figure 1 for a graphical overview). The first part contains a single question about the role of luck and effort in economic success. 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 be related to individuals' views about intergenerational mobility, too. This question is followed by another unrelated survey module eliciting attitudes toward politics in general and the EU in particular to avoid pushing respondents into a particular direction before presenting information on actual intergenerational mobility.

The second part comprises our main intervention. Half of the participants received information on intergenerational mobility in Germany (the treatment group). More precisely, the treatment group learned about the likelihood of advancing from the bottom to the top quartile of the income distribution, and vice versa. This information is based on most recent evidence for Germany (see Schnitzlein 2016, and Stockhausen 2017). Following Alesina, Stantcheva, and Teso (2018), we presented this information in an intuitive way in text form and graphically (see screenshots in the Supplementary Material) to facilitate understanding.

The information intervention aimed at shifting subjects' perception of social mobility toward greater pessimism. Immediately after the intervention, we include a manipulation check to assess the impact of the information treatment. For this purpose, we asked participants to imagine 100 households that represent Germany and asked them to answer the following question: "To what extent does economic success as 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)". We deliberately used a different wording compared to the treatment intervention (e.g. "depend" rather than "likely") to ensure that participants in the treatment group did not inattentively repeat what they had just seen, but thought about the question more carefully. Moreover, compared to quantitative measures, this qualitative measure is less likely subject to demand effects.

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 participants to make a series of incentivized binary choices between unequal monetary allocations involving themselves and another participant. 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). 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. Informing decision-makers about recipient's socio-economic background serves two purposes. First, distributional decisions not only depend on one's own relative standing in the income distribution, but potentially also on that of recipients. Thus, this variation contributes to a more comprehensive picture of distributional preferences and is an important step forward, as most of the existing work does not include such information. Second, the two conditions with recipient information (i.e. the *rich* and *poor* treatments) help us to gain further insight into a possible mechanism behind our information treatment, as this information may weaken or strengthen the impact of mobility perceptions (see hypotheses below).

Finally, in the fourth part we elicit preferences regarding one equality of outcome and one equality of opportunity policy. Specifically, we first ask participants how much economic redistribution they want in society on an 11-point scale ranging from "no redistribution" to "full redistribution." Second, we are interested in participants' 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").

2.3 The Equality-Equivalence Test

The Equality-Equivalence test (EET) is a parsimonious tool for identifying the distributional preferences of decision-makers by allowing the experimenter to infer the shape of a decision-maker's indifference curve in the self–other space.[5] 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 also pass through a specific area above and below the 45-degree line in the self–other space. Figure 2 illustrates the three areas above the 45-degree line – x_1, x_2 or x_3 – and the three areas below – y_1, y_2 or y_3 – in that space. 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.[6] 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). In general, almost all distributional types proposed in the economics literature can be represented in this way.

Empirically, the aim of the EET is to elicit the slope of the indifference curve through the 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 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 equal reference allocation to three allocations in the domain of disadvantageous inequality (areas x_1, x_2 or x_3 – *x-lists*) and to three allocations in the domain of advantageous inequality (areas y_1, y_2 or y_3 – 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.

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

The switching point from the equal reference allocation to the unequal allocation (or vice versa) 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.[7] 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*, the more benevolent she is. Similarly, in the *y-list* a more benevolent decision-maker switches later from the equal to the unequal allocation. The *x-score* and the *y-score* summarize these intensities in the *x* and the *y-list*, respectively.[8] In both 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*. 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. Inequality averse decision-makers, for example, display a positive *y-score* and a negative *x-score*. Moreover, the higher (or lower) a score, the more benevolent (or malevolent) a decision-maker is. We use the average switching row across the three lists in each domain to calculate the *x-score* and *y-score*.

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 2018 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 participants' GIP account.

2.4 Hypotheses and Empirical Strategy

Our information treatment is based on actual information about intergenerational mobility in Germany. Recent estimates show that about 15% of sons with a father from the bottom earnings quartile move up to the top-earnings quartile in Germany, while about 40% remain in the bottom quartile. Conversely, sons with a father in the top-earnings quartile only end up in 10% of cases in the bottom quartile, while 40% remain in the top quartile (Schnitzlein 2016).

We presume that most individuals have a rather optimistic view on the likelihood of upward or downward mobility, implying that the information treatment generally presents a negative shock

[7] Because we implemented the EET in an online panel with a diverse participant pool, we required respondents to make consistent choices within each list. 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.

[8] 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.

to participants' perceptions about intergenerational mobility. Accordingly, if the information intervention has an impact on distributional preferences, 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 upward mobility leads to higher support for redistribution and educational spending.

As indicated, we also provide information about the recipients' position in the income distribution to a subsample of participants. Relative to the *neutral*, "no information" treatment, we expect to see an increase in both the *x*- and *y-score* when the decision-maker is matched to a poor recipient and, conversely, a decrease in both scores if the recipient is rich. In addition, we expect that providing actual information about mobility will further amplify these effects. This echoes a common finding in the literature on fairness views: namely, that people are typically more averse to inequality if it results from factors that are known to be beyond their control (e.g. Cappelen et al., 2007). While in our setting, decision-makers have no specific information about the extent to which luck has determined a recipient's individual income, learning about low upward mobility provides a signal about how likely someone is responsible for being poor or rich.

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

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

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 intergenerational mobility. The binary variables R_i and P_i indicate whether a respondent received information on the other persons' location in the income distribution in the EET (bottom 10%/top 10%) and $\boldsymbol{X}$ 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 mobility, 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. In all of our specifications, we use 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 covariates 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 covariates come from the GIP core surveys that are conducted on a yearly basis and elicit the basic socio-demographic information of respondents, except participants' 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 covariates on a treatment indicator (i.e. whether they receive information on intergenerational 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 mobility perceptions of participants ("first stage"). Evidence on this first-stage effect is important because the exogenous manipulation of participants' 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 complement our analysis of distributional preferences with evidence from survey responses on policy preferences. 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 our information treatment. Table 2 displays the results of this exercise. Column 1 shows the correlations from bivariate regressions for each covariate, whereas column 2 presents the results from a multivariate regression including all covariates. 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.[9] 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 covariates enter simultaneously (column 2), we additionally observe a positive relationship of income and a negative relationship of age to perceived social mobility. However, we do no not find a correlation between perceived social mobility and beliefs about economic success (luck/effort beliefs). That is, respondents who believe that luck determines economic success are as optimistic or pessimistic about social mobility as respondents who believe that effort determines success. This finding 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 conscious of the fact that being rich or poor is to large extent beyond someone's control in a society with low social mobility. 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 treat-

[9] In the Supplementary Material we provide more detailed evidence on correlates for specific pre-registered subgroups that confirm the results presented here (see Section S3 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.

ment indicator (see Table 2, columns 3 and 4). The results show that the information treatment significantly affects the participants' mobility perception (column 3). That is, treated participants believe more strongly that economic success depends on parental background than non-treated participants. 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).[10] Adding covariates does not affect the coefficient estimate on perceived social mobility much (column 4) and thus confirms the associations presented in columns (1) and (2). Overall, our information treatment generated a strong "first stage," and led to a significant shift in social mobility perceptions.

3.2 Impact of Social Mobility Perceptions on Revealed Distributional Preferences

We now turn to our main contribution: specifically, whether social mobility perceptions affect individuals' 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 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 social mobility 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 3 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 less averse to disadvantageous inequality (*x-score*) or more averse to advantageous inequality (*y-score*) than participants 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 in wave 23. We find that the information on intergenerational mobility 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 response to our information treatment (see Supplemental Material, Table S2).

[10] 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.

Because of the large sample size, we are able to rule out even miniscule effects of mobility perceptions 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 results 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* $((0.18 + 0.26)/(4.5 + 5.5))$. 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, 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 survey participants.

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 3, 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. We observe the exact opposite pattern for the *y-score* (Table 3, 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-scores* are 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-randomized the recipient information in the EET with our main treatment, we can see, in a second step, whether information about social mobility magnifies the changes in distributional choices reported above. The idea is that information on social mobility is an informative signal about how likely the actual income difference between the decision-maker and the recipient is due to unequal opportunities. Thus, decision-makers who perceive real income inequality as more unfair may prefer to reverse inequality in the EET. That is, the information treatment may have a larger effect on the *x-score* when the matched partner is poor and a larger (negative) effect on the *y-score* when the matched partner is rich. Yet we do not find evidence that information on social mobility affects our estimates. Neither the interaction effect of the mobility information with the *rich* treatment, nor the interaction effect with the *poor* treatment results in significant estimates (Table 3, columns 2 and 5). The confidence intervals reported in Table 3 are reasonably small, such that we can rule out effect sizes larger than 9–11% of the total range of the scores. This suggests either that decision-makers do not consider their own relative income rank a result of the persistence of socio-economic status or that they do not view the impermeability of society as a sign of inequality of opportunity. Both interpretations mirror our earlier finding that individual perceptions of luck and effort are not related to respondents' perception about mobility.

3.3 Impact of Mobility Information on Policy Preferences

In addition to our incentivized measure of distributional preferences, we also asked respondents about their support for redistribution and educational spending. This enables us to assess the causal effect of beliefs about intergenerational mobility on policy preferences.

Table 4 presents the results. The estimates based on participants' 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 intergenerational mobility neither increases demands for redistribution nor affects attitudes toward public education spending. This observation is consistent with the insights previously gained from analyzing the EET: Information on social mobility affects neither distributional preferences nor policy preferences.

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 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 our treatment reveals no evidence that trust in government plays a role in the muted response to redistribution and support for education expenditures.[11] Second, we consider the possibility that social mobility perceptions directly shape policy preferences. In the Supplementary Material we show that mobility perceptions are significantly related to support for redistribution and education expenditures (see Section S3, Table S3). However, using our treatment as instrument for mobility perceptions reveals no evidence for a causal effect of mobility perceptions on policy preferences.

4 Heterogeneous Effects of Social Mobility Perceptions

In our pre-analysis plan, we hypothesized that our treatment will have a greater impact on subpopulations who are more optimistic. In the following, we analyze how different groups of respondents react to the treatment and we estimate a series of regressions of the following form:

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

where Y_i is one of our four main outcomes as above, $treatment_i$ is a treatment dummy for our intervention and $heterogeneous_i$ corresponds to the covariate of interest (luck vs. effort, political orien-

[11] More specifically, 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).

tation, income, and occupational status). Table 5 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. However, support for education expenditures and distributional preferences do not correlate with views about the role of effort (Panel A of Table 5). The more important question, though, is whether respondents who believe more firmly in effort and who are thus *a priori* more likely to accept inequality respond differently to information about social mobility. We have seen in our previous analysis that beliefs about determinants of economic success (luck/effort beliefs) are not related to mobility perceptions. Therefore, it is perhaps not surprising to find no differential effect of the treatment when looking at people with diverse views on the determinants of economic success. All interaction effects displayed in Panel A of Table 5 are insignificant and confidence intervals of [-0.10, 0.21] for the *x-score* and of [-0.26, 0.13] for the *y-score*, respectively, are small.

Taking advantage of the rich background information in the GIP (and deviating from our pre-analysis plan), we provide a psychological underpinning for the missing link between luck-versus-effort beliefs and mobility perceptions by looking at respondents' locus of control, which was measured in wave 15 in 2015. Locus of control is a key personal trait (Rotter, 1966), and describes the extent to which people believe they can control their own life (internal locus of control) or that outside factors such as luck and fate, determine their life (external locus of control). Respondents who believe that luck determines economic success also tend to believe that they cannot control their life. The correlation is significant and moderate in size ($\rho = 0.23, p = 0.01$). Thus, it is not surprising to see a remarkably similar picture to the analysis of luck/effort beliefs when looking at a respondent's locus of control. First, locus of control is not associated with mobility perceptions (see Table 2). Second, respondents with an external locus of control react in the same way to our treatment as respondents with an internal locus of control.[12] The importance of locus of control for the support of redistributive policies suggests that even if people would understand the link between luck/effort beliefs and social mobility, our treatment would barely influences their overall assessment of the role of luck and their support for redistribution, as these are largely driven by their individual level of perceived control their own life. Taken together, respondents' failure to see the link between social mobility and inequality of opportunity provides a reason for why our treatment does not affect distributional choices.

Political Orientation: Our previous analysis revealed that political orientation of respondents is positively related to mobility perceptions, i.e. right-leaning respondents hold more positive beliefs about social mobility (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,

[12] We interact our treatment with an indicator for above-median locus of control (external locus). The corresponding coefficient estimate is 0.063 (with a standard error of 0.078) when the dependent variable is redistribution and is 0.006 (standard error of 0.077) when the dependent variable is education expenditures.

Alesina, Stantcheva, and Teso (2018) find that left-leaning participants show more support for redistributive measures in response to receiving information about mobility.

To measure political orientation, we rely on respondents' self-assessment in the left–right spectrum and their voting intentions in the next federal election.[13] 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, and Katz 2007). In panel B of Table 5, 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.[14] 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.[15] 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. Again, 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 our 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 5). 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%, while the treatment has no effect on either score. Moreover, the poor support more redistribution, while the rich support less, although there is no effect on support for education expenditures. We observe no treatment effect, 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.[16] We categorize occupational status into six groups: semi-skilled workers (our reference group), skilled

[13] 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.

[14] The fact that distributional preferences as elicited via the EET significantly relate to ideology, voting and other political attitudes is consistent with Kerschbamer and Müller (2020).

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

[16] There is little scientifically reliable information or evidence about mobility perceptions in the German population. A 2013 study by the Allensbach Institute, an opinion and marketing research institute, indicates that the German population is split about the prospects of upward mobility. 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.

workers, employees, executives, self-employed and professionals, and others (e.g. soldiers, apprenticeship, and unpaid family workers). Panel D in Table 5 displays the results. Again, there is no evidence for a relationship between occupational status, our treatment and distributional preferences.

In contrast to distributional preferences, occupational status seems to affect policy preferences. We observe that self-employed, professional, and executive employees significantly reduce their support for redistribution relative to semiskilled workers. Yet again, the interaction effects with our information treatment turn out to be either statistically or economically insignificant. However, we do find evidence that the treatment increases support for education expenditures across all occupations, albeit only significantly for skilled workers and executive employees.

5 Conclusion

In this paper, we presented several pieces of evidence that question the importance of beliefs about social mobility as a determinant of redistributive preferences. We documented a sizable shift in perceptions regarding social mobility in response to information about the actual likelihood of up- and downward mobility, indicating that participants were generally overoptimistic about equality of opportunity, on average. Using this shift in perceptions, we presented strong evidence that more pessimistic beliefs about social mobility do not affect distributional preferences, which are fundamental inputs into a host of redistributive policies (e.g. Saez and Stantcheva 2016). In addition, we estimated the effect of mobility perceptions on preferences over equality of opportunity and redistribution policies. These measures confirm that information on mobility rates does not move preferences. In addition, exploring the heterogeneous effects of our treatment, we found consistent support for these results. This exercise also suggests that the lack of measurable effects is related to the fact that participants do not link the persistence of socio-economic status to luck. There is no evidence in our data that respondents interpret the information about low upward mobility as a signal about how likely someone is responsible for being poor or rich.

Taking our results at face value, the evidence we presented here for Germany seems to fit the empirical observations from the “Great Gatsby Curve.” Even though our information intervention induced more pessimism about social mobility, we found no measurable effect on distributional and policy preferences. But 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.

Finally, our findings speak to a debate about whether factors such as trust in government or a failure to correctly assess the purpose of certain social policies can explain low support for redistributive policies. Several recent studies suggest that missing support for redistribution is correlated with low trust in government (e.g. Hetherington 2005, Kuziemko et al 2015, Alesina, Stantcheva, and Teso 2018). However, such a concern cannot play a role in our measurement of distributional preferences. Instead, our results likely echo the notion of a missing link between concerns about a certain issue and political responses to it, as put forward by Bartels (2005). Our sample appears to be concerned about mobility, but respondents do not adjust their policy responses accordingly, perhaps because they do not view the underlying reason for their concern as unfair. While we can only speculate about these underlying reasons, we generally know very little about how people perceive certain policies, whether they are able to identify their purpose, and whether they understand their consequences. These questions seem interesting avenues for future research.

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Figures and Tables

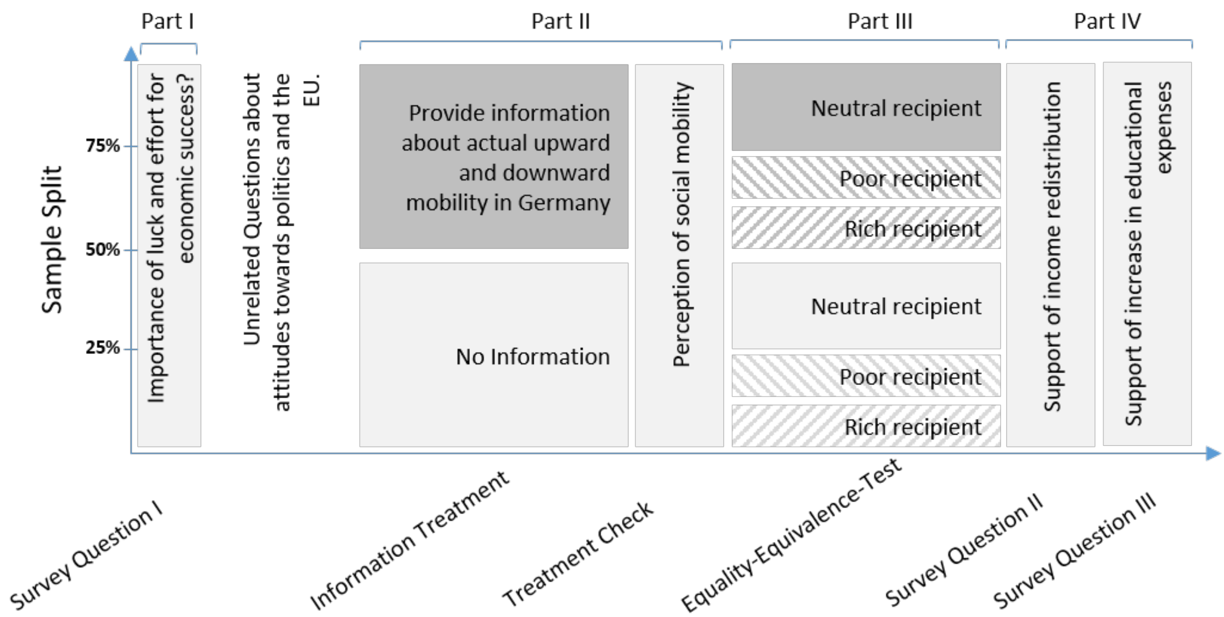

Figure 1: Experimental Setup.

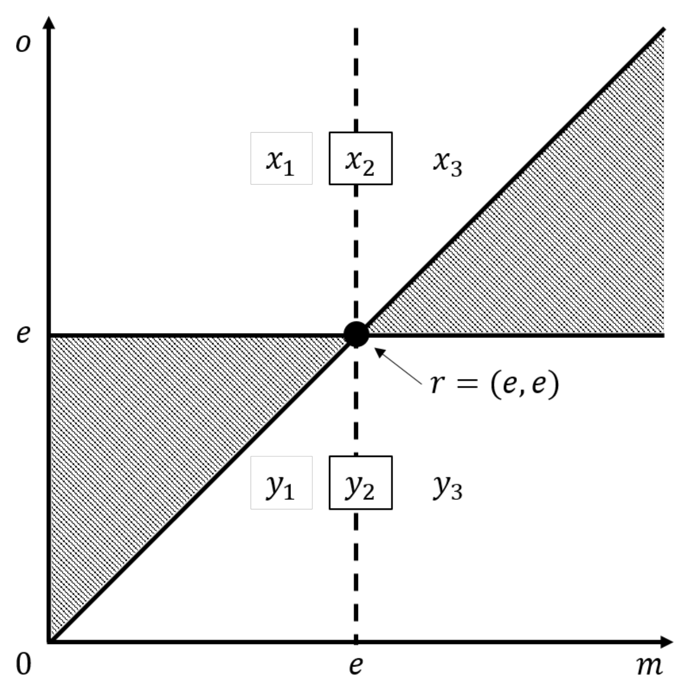

Figure 2: Domains of inequality and identification of distributional types (reproduced from Kerschbamer 2015).

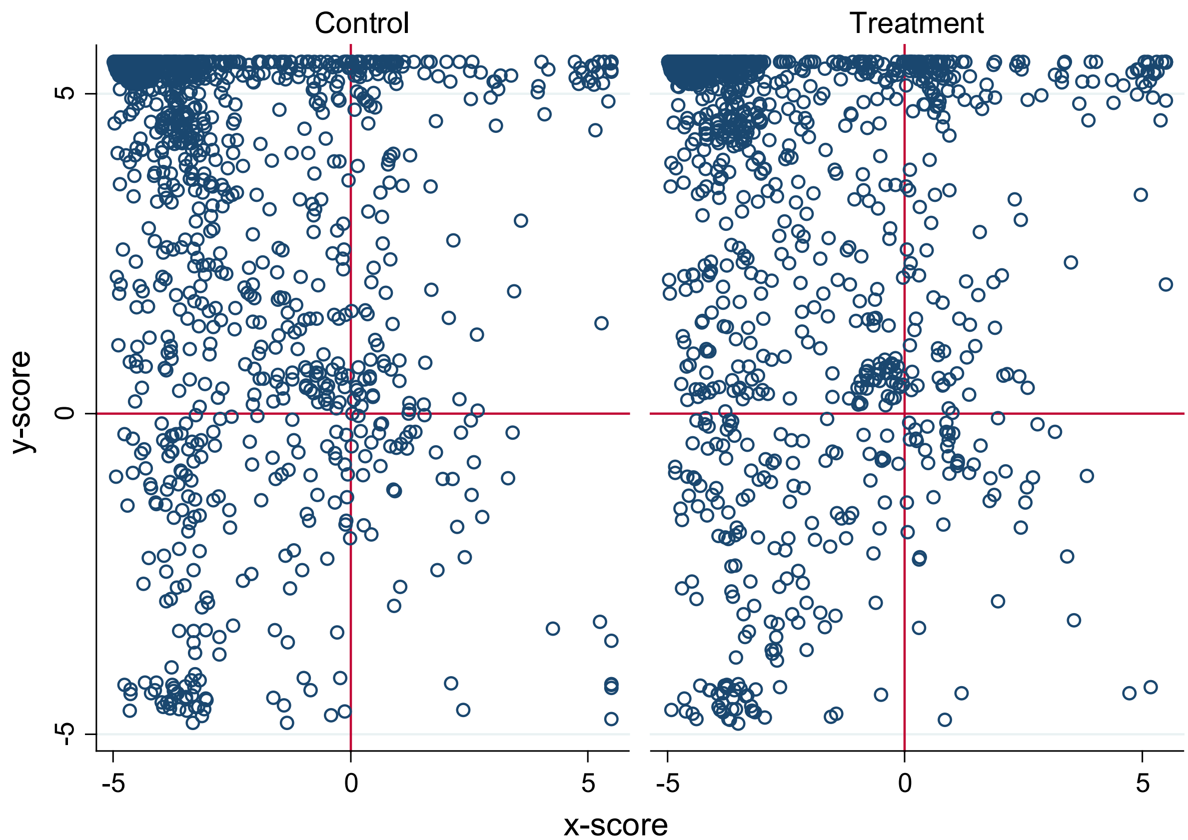

Figure 3: Jittered scatter plot of (x, y) scores separated by control and treatment.

Table 1: Randomization Check

	Control	**Treatment**	***p-value***
	(1)	(2)	(3)
Age	51.06 (15.74)	50.98 (15.02)	*0.90*
Female	0.497 (0.50)	0.489 (0.50)	*0.36*
Education	3.68 (1.17)	3.75 (1.16)	*0.10*
Married	0.55 (0.50)	0.58 (0.49)	*0.10*
Monthly Income (log)	7.32 (0.8)	7.35 (0.86)	*0.27*
Retired	0.23 (0.42)	0.21 (0.41)	*0.27*
Unemployed	0.02 (0.14)	0.02 (0.14)	*0.89*
Household Size	2.42 (1.08)	2.49 (1.09)	*0.11*
East Germany	0.21 (0.41)	0.19 (0.40)	*0.21*
Political Orientation "Left/Right"	5.56 (1.95)	5.61 (1.94)	*0.54*
Luck/Effort Beliefs	6.09 (1.94)	6.09 (1.91)	*0.99*
Locus of Control	2.18 (0.61)	2.17 (0.62)	*0.52*
Prob>F			*0.31*

Notes: Mean of covariates, standard deviations in parentheses. Columns (1) and (2) display the mean (% share) of the listed covariates in the treatment and control group. Column (3) shows the p-values of the coefficients of separate OLS regressions, in which the treatment indicator (info) was regressed on the respective covariate. Prob>F is the p-value of an F-test for joint significance of all covariates.

Table 2: Mobility Perceptions

	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)
Luck/Effort Beliefs	0.033	0.003		0.007
(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

*Note: ***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 social background, i.e. 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 social background). 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 with higher categories reflecting higher education. Political Orientation is measured on a 1–11 scale with higher values indicating more conservative political views. 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: 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)
Covariates	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 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 participant received information on actual mobility (treatment). Rich and Poor are dummies equaling 1 if a participant 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: Policy Preferences

	Redistribution		Education Expenditure	
	(1)	(2)	(3)	(4)
Treated	-0.022	-0.013	0.018	0.008
	(0.04)	(0.04)	(0.04)	(0.04)
	[-0.10,0.05]		[-0.06,0.09]	
Constant	0.011	1.272***	-0.009	-1.185***
	(0.03)	(0.21)	(0.03)	(0.21)
Covariates	No	Yes	No	Yes
R^2	0.000	0.036	0.000	0.045
N	2,641	2,491	2,649	2,498

*Notes: ***p<0.01, **p<0.05, *p<0.1*
OLS regressions with robust standard errors in parentheses and 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 participant received information on actual mobility (treatment). Controls include log income, gender, age, education level, East Germany dummy, retirement status, employment status, number of household members and marital status.

Table 5: Heterogeneous Treatment Effects

	x-score		y-score		Redistribution		Education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Beliefs about Luck/Effort								
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)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	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 Index	-0.021	-0.044	-0.181	-0.186	0.044	0.044	0.061	0.047
	(0.112)	(0.113)	(0.136)	(0.140)	(0.048)	(0.048)	(0.048)	(0.049)
Political Index	0.035	0.052	-0.187*	-0.159	-0.307***	-0.297***	-0.203***	-0.185***
	(0.082)	(0.085)	(0.097)	(0.102)	(0.035)	(0.035)	(0.034)	(0.036)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.006	0.090	0.012	0.025	0.081	0.110	0.03	0.07
N	1,744	1,658	1,744	1,658	1,777	1,687	1,778	1,689
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%								
R^2	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%								
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	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 5: 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 (0.53)	-0.373 (0.54)	0.443 (0.72)	0.458 (0.74)	0.008 (0.26)	0.066 (0.26)	0.431* (0.25)	0.485** (0.25)
Skilled Workers	0.137 (0.40)	0.128 (0.42)	-0.593 (0.48)	-0.293 (0.50)	-0.210 (0.17)	-0.232 (0.17)	-0.244 (0.16)	-0.370** (0.17)
Treated x Employee.	0.371 (0.45)	0.242 (0.46)	-0.515 (0.62)	-0.681 (0.63)	-0.128 (0.22)	-0.159 (0.22)	0.237 (0.21)	0.262 (0.21)
Employee	0.192 (0.35)	0.125 (0.37)	0.338 (0.39)	0.462 (0.41)	-0.249* (0.14)	-0.194 (0.14)	-0.056 (0.13)	-0.073 (0.13)
Treated x Exec.Employee	0.531 (0.47)	0.418 (0.47)	-0.218 (0.63)	-0.353 (0.64)	-0.066 (0.23)	-0.087 (0.22)	0.395* (0.21)	0.433** (0.21)
Exec. Employee	0.418 (0.36)	0.359 (0.38)	-0.099 (0.40)	0.160 (0.43)	-0.376*** (0.14)	-0.252* (0.15)	0.064 (0.13)	-0.022 (0.14)
Treated x Self-employed/Professional	0.741 (0.55)	0.573 (0.55)	-0.567 (0.73)	-0.826 (0.74)	-0.073 (0.26)	-0.037 (0.26)	0.313 (0.25)	0.366 (0.25)
Self-employed/Professional	0.211 (0.42)	0.266 (0.43)	0.033 (0.49)	0.363 (0.51)	-0.470*** (0.16)	-0.410** (0.17)	0.053 (0.16)	-0.044 (0.16)
Treated x Others	0.924 (0.67)	0.717 (0.68)	-0.940 (0.87)	-0.997 (0.91)	-0.016 (0.32)	-0.057 (0.33)	0.247 (0.32)	0.358 (0.32
Others	0.017 (0.47)	-0.512 (0.50)	0.271 (0.58)	0.208 (0.64)	-0.220 (0.21)	-0.251 (0.22)	-0.175 (0.19)	-0.223 (0.20)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	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****: 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 Index is a composite measure of a respondent's party preferences and 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.*

Social Mobility Perceptions and Inequality Acceptance

Supplementary Material: For Online Publication Only

Additional Figures

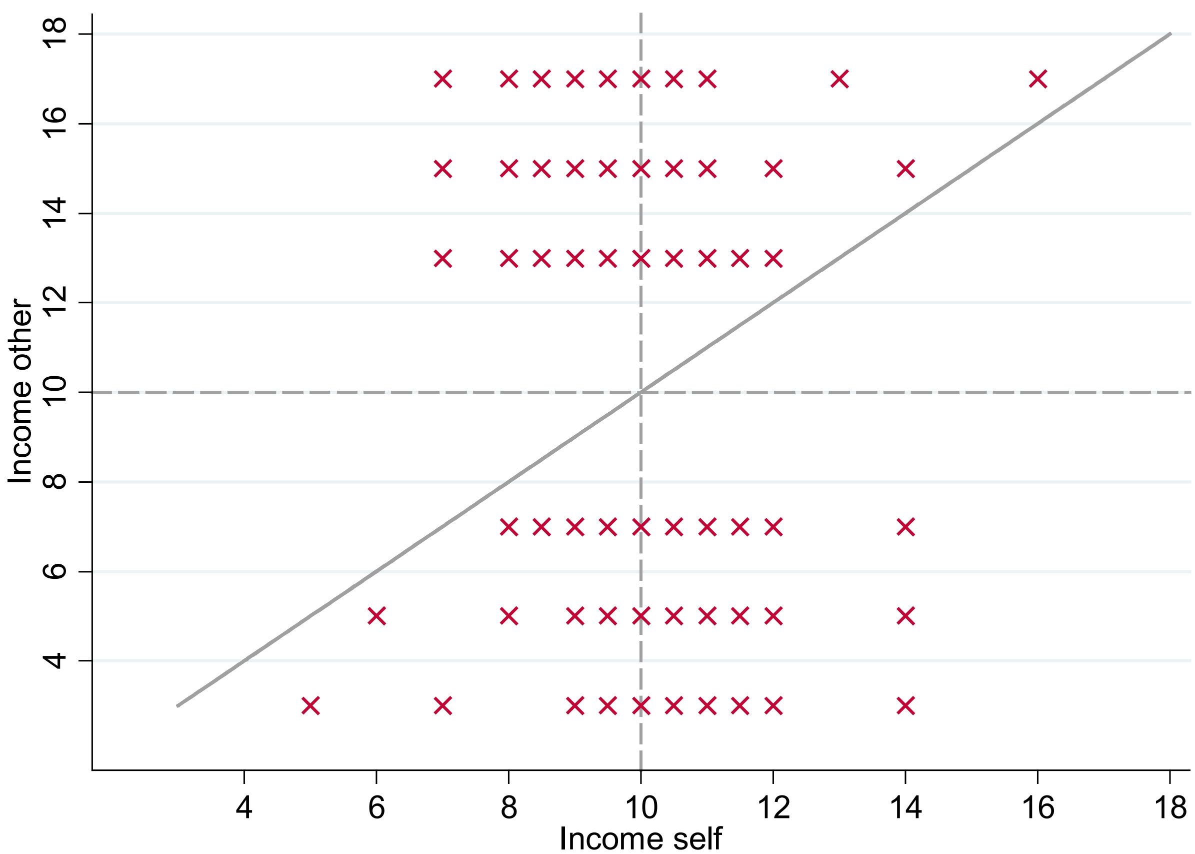

Figure S1: Parameters of the EET.

S1: Summary Statistics

Table S1: Summary Statistics

		All		Non-treated		Treated	
	Obs.	Mean	SD	Mean	SD	Mean	SD
Income (log)	2,570	7.34	0.83	7.31	0.80	7.35	0.86
Age	2,662	51.02	15.38	51.07	15.7	50.98	15.02
Female	2,662	0.49	0.50	0.50	0.50	.48	0.50
Education: University qualification	2,629	0.38	0.49	0.37	0.48	0.40	0.49
Retired	2,664	0.22	0.42	0.23	0.42	0.21	0.41
Unemployed	2,664	0.02	0.14	0.02	0.14	0.02	0.14
HH size	2,662	2.46	1.09	2.42	1.08	2.49	1.10
Married	2,659	0.56	0.5	0.55	0.50	0.58	0.49
East Germany	2,664	0.20	0.40	0.21	0.41	0.19	0.40
Luck/effort	2,660	6.09	1.92	6.09	1.94	6.09	1.91
Left/right	2,574	5.58	1.95	5.56	1.95	5.60	1.94
LoC	2,439	2.17	0.61	2.18	0.61	2.17	0.62

S2: *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
	(0.096)	(0.095)	(0.126)	(0.128)
Treated x EET wave 33	-0.113	-0.133	0.072	0.131
	(0.138)	(0.137)	(0.177)	(0.180)
Treated	0.134	0.139	-0.074	-0.137
	(0.097)	(0.097)	(0.128)	(0.130)
Constant	-2.695***	-2.444***	3.278***	3.556***
	(0.068)	(0.402)	(0.092)	(0.478)
Covariates	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 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. "EET wave 33" is an indicator variable for participating in the EET in wave 33. "Treated x EET wave 33" indicates whether a participant 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.

S3: 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 participants 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 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. Deviating from our pre-analysis plan and looking at a respondent's locus of control 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 strongly supports the impression from the correlational analysis above that luck/effort beliefs are not associated with mobility perceptions. Thus, it seems that respondents do not view the persistence of socio-economic status as a matter of luck.

Relation between Mobility Perceptions and Preferences: The previous analysis revealed that our treatment had a significant impact on mobility perceptions (see Table 2). These mobility perceptions are significantly related to support for redistribution, education expenditures as well as to the *y-score* (see Table S3). That is, more optimistic respondents 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 estimate 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).

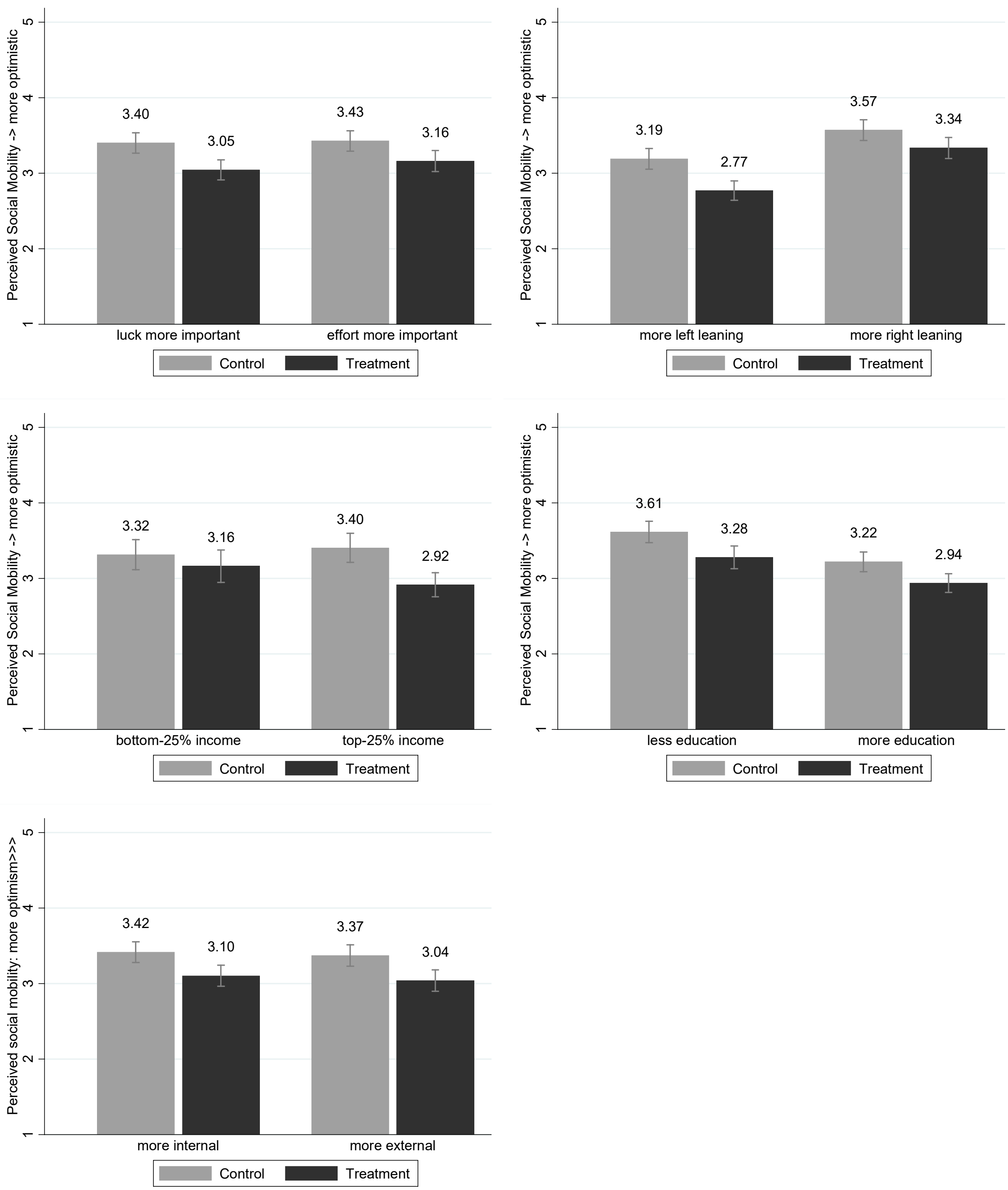

Figure S2: Mobility perception of specific subgroups across treatment status.

Notes: Groups are defined as follows: “Luck more important” (“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 left-leaning” (“more 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 25% income” (Top 25% income) indicate respondents in the bottom 25% (top 25%) of the income distribution in our sample. “Less education” (“more education”) indicates respondents with no qualification for university (with qualification for university) and “more internal” (“more external”) is the median split (2) of the locus-of-control index (index from 1–5).

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)
R^2	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: IV 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*
OLS Regressions with robust standard errors in parentheses. Panel A presents the first stage in column 1 and the relationship between mobility perceptions and our four main outcomes in columns 2–5. Panel B shows the 2SLS estimates using the random assignment to the information treatment as an instrument for mobility perception.

S4: Translation of 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.