(In-)equality of Opportunity, Fairness, and Distributional Preferences*

Dietmar Fehr
University of Heidelberg
and CESifo

Daniel Müller
University of Munich

Marcel Preuss
Cornell University

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Abstract

This paper examines how perceived importance of family background affect distributional preferences using two large-scale survey experiments. In the first experiment, we randomly inform respondents about the relationship between parental income and economic success later in life, which renders their perceptions of equality of opportunity more pessimistic. However, this changes neither revealed distributional preferences nor pro-social behavior toward the rich and poor. The second experiment shows that respondents do not account for parental influence on economic success when making (re-)distribution decisions, suggesting that people view parental influence as a legitimate reason to justify some inequality. This can explain why distributional preferences are immune to changes in perceptions of equality of opportunity.

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

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

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Contact: dietmar.fehr@awi.uni-heidelberg.de, daniel.mueller@econ.lmu.de, preuss@cornell.edu

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. Much of the discussions about rising inequality revolve around the importance of family background to economic success in later life.[2] From a prominent normative viewpoint, inequalities that are due to family background (or innate brute luck) should be offset (e.g., Dworkin 2000; Cohen 2011). On the other hand, occasional observations suggest that people do not necessarily attribute such inequalities to luck but instead consider it to some degree as deserved (e.g., Markovits 2019; Sandel 2000). This paper addresses these conflicting views and provides causal evidence on how perceived advantages and disadvantages of family background shape distributional preferences using two pre-registered survey experiments.

Information about social mobility is closely linked to what one can achieve with effort and hard work and is a signal of equality of opportunity in a society that can legitimize economic inequality. If people care about the fairness in a society, and meritocratic principles in particular (as for example suggested by Almås, Cappelen, and Tungodden 2020), learning that the future prospects of the young generation depend to a large degree on their parents' income should lead to less inequality acceptance and thus to a shift in their distributional preferences. Whether this is true may critically depend on how people legitimize parental influence as a source of inequality. We are thus particularly interested in two questions. First, what are the implications of perceived equality of opportunity on distributional preferences in general, and on pro-social behavior toward the poor and the rich, in particular? Second, do people account for parental influence and choices when deciding about redistribution?

To provide evidence on the first question, we draw on a tailor-made survey module in a high-quality online panel, the German Internet Panel (GIP). We address the second question with

[1] By *distributional preferences* we mean preferences that not only take one's own income as an 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] It is well documented that inequality is negatively related to social mobility between and within countries, i.e., more inequality today is related to a stronger impact of family background on future income of the young cohort (Corak 2006, 2013; Chetty et al. 2014), a relationship that has been popularized by Alan Krueger (2012) as the "Great Gatsby Curve."

a vignette study in a representative sample of Germans that we will discuss in more detail below. A key aspect of the survey module in the GIP is the combination of a randomized information treatment with an incentivized measure for distributional preferences.[3] To identify the causal effect of perceived societal fairness on distributional preferences, we provide a randomly selected subsample of respondents with stylized information on social mobility. 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 income, i.e. their perceived equality of opportunity. We then elicit respondents' distributional preferences via an incentivized allocation task, the Equality-Equivalence Test (EET, Kerschbamer 2015).[4] The EET features a decision maker and a passive recipient and in some conditions we also reveal information about the actual 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 novel feature allows us to assess whether perceived societal fairness affects pro-social behavior toward those who are most likely to have suffered or benefited from limited social mobility.

We present two main findings from our survey experiment in the GIP. First, informing respondents about the extremely low share of young people advancing from the bottom to the top quartile of the income distribution and moving down from the top to the bottom quartile, respectively, 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).

Second, given the strong "first-stage" effect, our results about the effect of the intervention on distributional preferences are striking. Despite the more pessimistic view on equality of opportunity, we find that the treatment has no detectable effect on distributional preferences. In particular, treated respondents are not 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. Moreover, information about social mobility makes treated 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

[3] The GIP is a probability-based sample of the general German population aged 16 to 75 years. It offers tight control over who is participating, includes detailed background information on respondents, and provides a host of information on their social and political attitudes (Blom, Gathmann, and Krieger 2015).

[4] The EET is an intuitive, theory-driven task to measure monetary trade-offs between a decision-maker and some passive recipient (another respondent in the GIP) and is predictive for a range of economic behavior, including voting, political ideology, attitudes towards redistribution and immigration, and trust (Hedegaard et al. 2019; Kerschbamer and Müller 2020).

to the control group. Specifically, while we observe that a recipient's income rank affects distributional preferences in a predictable way, i.e., it leads to more generosity if decision-makers face a recipient from the bottom 10% and to less otherwise, the information on societal fairness does not amplify this shift in distributional preferences. Consistent with these findings, 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, i.e., we are able to rule out even small effect sizes in all specifications.

We then explore this absence of a measurable effect on distributional preferences by investigating how different (pre-registered) groups of individuals react to information about social mobility. It is possible that some groups of individuals respond more strongly to the treatment, while most others do not, resulting in a zero average effect. We find little to no evidence for heterogenous effects. In particular, we do not observe that respondents who believe more in effort as a determinant of success react in a different way to the information treatment than respondents who believe that luck determines success. This lack of a relationship casts doubt on whether people view the significant but unearned economic advantages and disadvantages of birth in a society with low social mobility as pure luck.

Given the results on distributional preferences, our second study not only helps us to better understand the support for redistribution in our society, but also sheds light on one reason why information on social mobility does not affect distributional preferences. Specifically, we provide evidence on how people account for parental influence and how these views affect their distributional preferences from a spectator viewpoint. To do so, we conducted a second survey in which we exposed respondents to a vignette. The vignette describes a successful young female professional and randomly varies the extent to which parental influence contributed to her success, holding her level of income and other factors constant. More precisely, half of respondents read a description that emphasizes the positive impact of her parents, while the other half read a description that emphasizes personal responsibility.

We find that the two versions of the vignette induce views about the success of the young female professional as intended: in the condition emphasizing parental influence, respondents attribute her success less to personal responsibility than in the condition emphasizing personal responsibility. We also measure to what extent her success is considered as luck and just. While there are some differences in those perceptions between the vignettes, they are small compared to the difference in perceived responsibility. Most importantly however, we find no difference in respondents' redistributive preferences. Respondents in both conditions redistribute a similar share of income from the successful young professional to a hypothetical low-income earner. This finding illustrates, in line with our finding from the first study, that despite recognizing possible differences in the circumstances of someone's success, people do not necessarily change how much inequality they accept. In particular, it suggests that people view positive parental influence as a legitimate claim that does not justify redistribution.

In light of these results, we believe that our work leads to two main conclusions. First, the lack of a measurable effect on distributional preferences offers an explanation for the often observed disconnect between people's concerns about inequality and their demand for appropriate policy measures (McCall and Kenworthy 2009; Kuziemko et al. 2015; Ciani et al. 2021): the support for policy measures to address increasing inequality may not change that easily because of an underlying inertia in distributional preferences. In our specific context, the presented pessimistic information about social mobility has a sizable impact on perceptions of equality of opportunity. At the same time, we see, however, that respondents treat parents' influence on opportunities in a similar way as own responsibility, offering an explanation for why perceptions of equality of opportunity do not shift distributional preferences. Second, our vignette study reveals, consistent with the finding from our initial study, yet another important instance in which many people seem to disagree with luck egalitarianism (Mollerstrom, Reme, and Sorensen 2017; Weinzierl 2017). While pure luck egalitarians ought to equalize outcomes that are not under their control, respondents seem to treat parental influence differently from choices beyond control. This challenges normative fairness views by painting a nuanced picture of what people treat as controllable and not. It has also implications for optimal tax theory as a standard utilitarian tax framework would propose perfect outcome equality with higher taxes on those with more opportunities and ability, if external factors, such as parental influence (and ability), can be observed perfectly (Mankiw and Weinzierl 2010; Weinzierl 2014, 2017; Saez and Stantcheva 2016). Our findings suggest that people would object such a tax system since they give some credit to parental influence despite being beyond someone's control. Beyond these conclusions, we believe that our null result is valuable as it is based on a large sample and on 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 strands of the literature. 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 samples (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. 2021; 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 and parental influence affect 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.

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 and wealth, inequality, and inherited wealth affects inequality acceptance, tax preferences, and risk preferences (Cruces, Perez-Truglia, and Tetaz 2013; Kuziemko et al. 2015; Karadja, Mollerstrom, and Seim 2017; Bastani and Waldenstrom 2021; Fehr, Mollerstrom, and Perez-Truglia 2021, Fehr and Reichlin 2021). Unlike these papers and most other work on the formation of social policy preferences, we concentrate on distributional preferences, which are fundamental inputs 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 on Social Mobility Perceptions

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-based sample of the general German population aged 16 to 75 years.[5] At the time of our experiment, the panel included about 5,000 registered participants who are invited to take part in an online survey on a bi-monthly basis. 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 draw on the longitudinal feature and use information from previous waves of the GIP. We specified all variables and hypotheses in a pre-analysis plan

[5] 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.

(PAP) that we registered in the AEA RCT Registry (AEARCTR-0002764) 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 S10 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 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 extremely low for children born in poor households. 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 strongly (10)". There are 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 as we are not primarily interested in changes in perceptions about social mobility within individuals, but rather in changes across individuals to measure the impact of our information intervention (see also Haaland, Roth, and Wohlfahrt 2020 for a discussion of potential problems, such as priming and demand effect, when eliciting prior beliefs).

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. Embedding this incentivized measure of distributional preferences into a population survey has several advantages over survey measures of other-regarding preferences. First, the EET delivers a well-defined measure of distributional preferences. 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.

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).[6] 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. 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 were interested in respondents' views on government education expenditures, and thus asked 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 deliberatively leave some room for interpretation, and thus may cover broader aspects of inequality acceptance than our incentivized measure of distributional preferences.

[6] 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.

Follow-up waves: 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 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 (Kerschbamer 2015).[7] 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 – 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.[8] 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

[7] 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.

[8] 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).

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.[9] 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.[10] 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* (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 them 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.

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[9] 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.

[10] 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.

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; Almås, Cappelen, and Tungodden 2020). Because social mobility is closely linked to what one can achieve with effort and hard work, we expect that treated respondents are more likely to believe that inequality is due to unequal opportunities than due to different choices in life. Consequently, 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:

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

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 the persistence of socio-economic status. The binary variables P_i and R_i indicate whether a respondent received information on the recipients' 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 equality of opportunity, more benevolence, and higher support for redistribution and educational spending, respectively. As pre-specified, we use in all of our specifications OLS 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. In Table 1, we present summary statistics on pre-specified basic socio-demographic information (see also Table S1 in the Supplementary Material for more details).[11]

3 Results on Social Mobility Information

We present two sets of results from our survey experiment in the GIP. 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. Our analysis proceeds as specified in the pre-analysis plan, unless noted otherwise.

3.1 First Stage: Impact of Social Mobility Information on Perceptions

We start with investigating 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. To ease interpretation, we transform these mobility perceptions such that higher values indicate more optimism (i.e. weaker dependence on parental income status). Table 2 presents the results. Columns (1) and (2) display correlations of mobility perceptions with observables. We see, for example, that more educated people are more pessimistic and right-leaning people are more optimistic. Turning to the first-stage results, column 3 shows that the information treatment significantly shifts respondents' mobility perceptions. 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 devia-

[11] Table 1 indicates that all observables are balanced across the two groups (column 3). The coefficients are not jointly different from zero (F-test, $p = 0.56$).

tion 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), but reveals some interesting correlations with mobility perceptions. In line with previous findings in the literature (e.g. Chambers, Swan, and Heesacker 2015; Alesina, Teso, and Stantcheva 2018), we see that better educated people are significantly less optimistic than lower educated people, and that politically right-leaning people and high income people are more optimistic about mobility perceptions.[13] Importantly, these correlations are robust to focusing on the control group, which is not contaminated by the information treatment.

While this evidence points to a strong "first-stage," Table 2 also reveals that mobility perceptions are not related to the beliefs about the determinants of economic success (luck/effort beliefs) as one would expect. 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 bad luck or luck, 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.[14] This finding also squares with results showing that many people reject the levelling of income differences due to brute luck (Mollerstrom, Reme, and Sorensen 2017; Weinzierl 2014, 2017). We return to this finding when discussing possible explanations for the treatment effect in Section 4.

3.2 Impact of Social 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 correlations of respondents' perceptions about equality of opportunity – our "first stage" – and distributional preferences. First, we note that our measure of distributional

[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.

[13] 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.

[14] Other evidence suggests that people consciously misperceive the role of luck and effort in determining success. For example, Fehr and Vollmann (2020) show that success causes people to downplay the role of luck and that these meritocratic beliefs lead to more inequality acceptance.

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 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 on social mobility and distributional preferences. We focus first on distributional preferences measured 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).[15]

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 allow us to get upper

[15] This result also points 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).

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* $((0.18 + 0.26)/(4.5 + 5.5))$. 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 average *x-score* is 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 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 unlikely view the income gap as less fair.

3.3 Impact of Social 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. Naturally, these two measures cover a broader aspect of inequality acceptance and thus complement our measure of distributional preferences.

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 arguably lower stakes in the EET in relation to redistributive policy measures.

We further explore the robustness of this finding on policy preferences 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.[16] 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 wave: Using data from a follow-up wave of the GIP, we check the persistence of our results (not pre-registered). About 4 months after our intervention, respondents again answered questions about redistribution and education expenditures. For both measures we observe a strong correlation between the measures in the main and follow-up wave (redistribution, $\rho = 0.43$

[16] 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).

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.

3.4 Heterogeneous Effects of Social Mobility Information

We now explore how different (pre-registered) groups of individuals react to information about social mobility. One possible reason for the lack of a measurable effect may be heterogenous responses to the treatment, resulting in small or zero average effects. That is, some groups of individuals may respond (more strongly) to the treatment, for example, because they are more optimistic about equality of opportunity *a priori*, while others do not react to the treatment. In the following, we analyze how different groups of respondents react to the treatment and estimate a series of regressions of the following form:

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

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 socio-economic characteristic 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 relevant characteristic (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 S2). Indeed, political ideology plays a key role for attitudes toward social policies (Karadja, Mollerstrom, and Seim 2017; Alesina, Stantcheva, and Teso 2018; Fehr, Mollerstrom, and Perez-Truglia 2021). 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.[17] 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 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.[18] 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.[19] 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.

[17] 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.

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

[19] According to a 2013 public opinion poll by the Allensbach Institute, an opinion and marketing research institute, about 50% of respondents think that the likelihood of a working-class child moving upward in the social hierarchy is "very good." Contrary to our sample, respondents with professional and university degrees display a more optimistic view than unskilled and skilled workers (see Table 2).

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.

4 Vignette Study on Parental Influence

4.1 Data Collection, Survey Module and Hypotheses

The previous analysis has shown that distributional preferences are immune to information on the persistence of socio-economic status (i.e., low down- and upward mobility). We have also presented some suggestive evidence that this finding is due to the lack of a relationship between beliefs about the determinants of success and perceived equality of opportunity. We now explore one reason why information on social mobility does not affect distributional preferences with a new survey for which we collaborated with an online panel provider and recruited 1,055 respondents who are representative for the German population in terms of age, gender and education (see Section S11 in the Supplementary Material for a full transcript of the survey module).[20]

We are here particularly interested in the question on whether people consider parental influence on their children's success as an unfair advantage that warrants (more) ex-post redistribution or not. For this purpose, we designed a vignette that describes a successful young female professional and varies the extent to which parental influence had contributed to her success, holding success and the level of income constant (see Section S7 in the Supplementary Material for details on the vignette). We randomize half of the respondents into a condition that emphasizes the positive impact of parents (*high parental influence*), and the other half into a condition that emphasizes the positive impact of personal choices and effort (*low parental influence*). In addition, we cross-randomize whether respondents are asked about their perception of equality of opportunity in Germany before or after the vignettes. The purpose of randomizing this question is to see whether inducing respondents to think about the dependence of economic success on family background affects how they respond to the vignette. After the vignette, we elicit respondents' views about the success of the vignette's main character. More precisely, we ask (i) whether her success is "just", (ii) to what extent her success is due to luck, and, as a manipulation check, (iii) to what degree she is personally responsible for her success (all on a scale from 1 to 5). In addition, we measure respondents' redistributive preferences by asking them how much of a fictious income of 100,000 Euro they would redistribute from the financially successful main character of the vignette to another fictitious low-income earner.

[20] We worked with *respondi*, an internationally well-known panel provider, since we had no longer access to the GIP (the GIP is only accessible for researchers affiliated with the Collaborative Research Center 884 "Political Economy of Reforms" at the University of Mannheim). We pre-registered the study at AsPredicted (#87835) before the start of the data collection and obtained IRB approval (Cornell IRB0010830).

The results are summarized in Figure 4.[21] Panels (a) – (c) show that respondents consider the main characters' success as less just (a), view luck as more important for her success (b), and hold her less responsible for her success (c) when parental influence is higher. The differences in *justice* and *luck* are small, in particular in comparison with how respondents judge *responsibility* for success. Importantly, despite the large difference in the perceived responsibility, we see no difference in the amount of redistribution from the successful vignette character to a low-income earner (d). For more rigorous statistical support of these observations, we estimate the following regression specification:

$$Y_i = \alpha + \beta_1\, influence_i + \beta_2\, mobility_i + \beta_3(influence_i \times mobility_i) + \gamma \boldsymbol{X} + \varepsilon_i \quad (3)$$

where Y_i is one of the four outcomes outlined above, $influence$ is a binary variable indicating high parental influence, $mobility$ is a binary variable indicating that the mobility perception question is asked before the vignette, and $\boldsymbol{X}$ is a set of controls (age, gender, education, income, employment status, household size, marital status, region).

4.2 Results on Parental Influence

The results, presented in Table 7, support the observations from Figure 4. First of all, we consistently observe that inducing respondents to think about the dependence of economic success on family background before the vignettes has no impact on the outcome variables. Second, the result from the manipulation check confirms that respondents do understand the differences between the vignettes (column 1, Table 7). Those exposed to the *high parental influence* vignette believe that the main character is less responsible for her success than those exposed to the *low parental influence* vignette. The magnitude of the estimate (0.39 of a standard deviation) is sizable and twice as large as the effect of the mobility information treatment in the main experiment. Third, looking at respondents' views about justice and luck (columns 3-6) reveals that both coefficient estimates are substantially smaller than the estimates on *responsibility*. While respondents feel that success in the *low parental influence* vignette is somewhat more just than in the *high parental influence* vignette (0.28 of a standard deviation), there is no significant difference in the perceived importance of luck (0.14 of a standard deviation). That is, we find no indication that respondents assign more weight on luck in the *high parental influence* than in the *low parental influence* vignette, even though success (career and income) is constant across conditions and even though respondents differ substantially in their views about responsibility for success. Fourth, the vignette does not lead to differences in redistributive preferences (column 7, Table 7). The effect size is 0.09 of a standard deviation, insignificant, and economically small (90% confidence interval: $[-0.07, 0.26]$).

[21] As pre-registered, we exclude all respondents from our analysis who did not pass a simple attention check (25 percent or 248 respondents of the sample). The restricted sample is balanced along a set of observables (see Table S5) and our results do not change when analyzing the full sample (see Table S6).

Overall, our results show that people recognize differences in the circumstances for success, but that this does not lead to a difference in redistributive preferences. This suggests that people do not equate different circumstances with differences in luck. Instead, they seem to view parental influence to some degree as an entitlement that is not particularly unmerited. If this is the case, then there is little reason to expect people to redistribute more or less in response to changing mobility and also no reason to expect that distributional preferences depend on mobility perceptions. Together, this provides a possible explanation for our findings from the first survey experiment and offers a novel and more nuanced picture of the role of uncontrollable and controllable factors in (normative) fairness considerations.

5 Conclusion

In this paper, we presented evidence that questions the importance of equality of opportunity, an important measure of the fairness of a society, as a determinant of distributional preferences. We documented a sizable shift in perceptions about parental influence on economic success in response to information about social mobility, indicating that respondents were generally overoptimistic about equality of opportunity, on average. Using this shift in perceptions, we presented robust 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. Similarly, there is no measurable effect of more pessimistic beliefs about equality of opportunity on policy preferences, such as redistribution and education expenses.

We presented additional evidence from another experiment suggesting that people view parental influence as a sufficiently legitimate reason to justify some inequality. That is, even if people recognize differences in the circumstances of growing up that result in unequal economic success, we do not see a change in how much inequality they are willing to accept. This result is in line with some previous findings showing that people often do not adhere to a norm of luck egalitarianism (the idea that all income differences due to brute luck should be equalized), but instead hold different views on controllable and uncontrollable factors.

Overall, our findings thus imply that people view parental influence as a legitimate reason to justify some inequality. Taking these results at face value, the evidence we presented here for Germany fits the empirical observations from the "Great Gatsby Curve." If people do not support more redistribution in response to a negative information shock to social mobility, it is less surprising that inequality and social 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 may stem from the underlying fundamentals and how people view the accountability for different sources of inequality.

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Figures and Tables

Figure 1: Experimental Setup

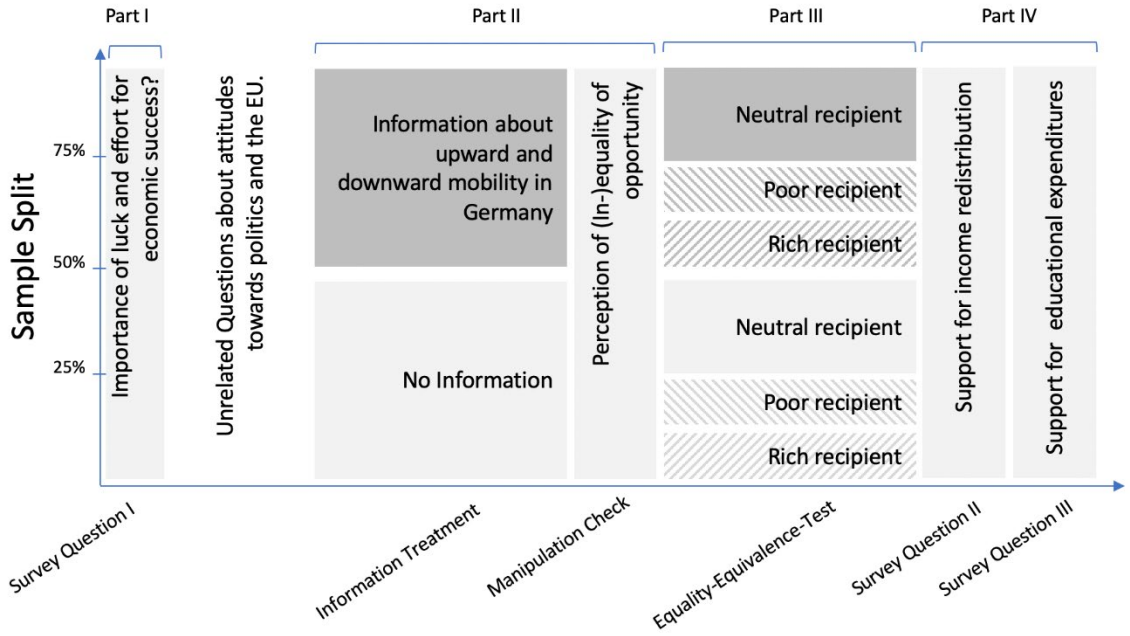
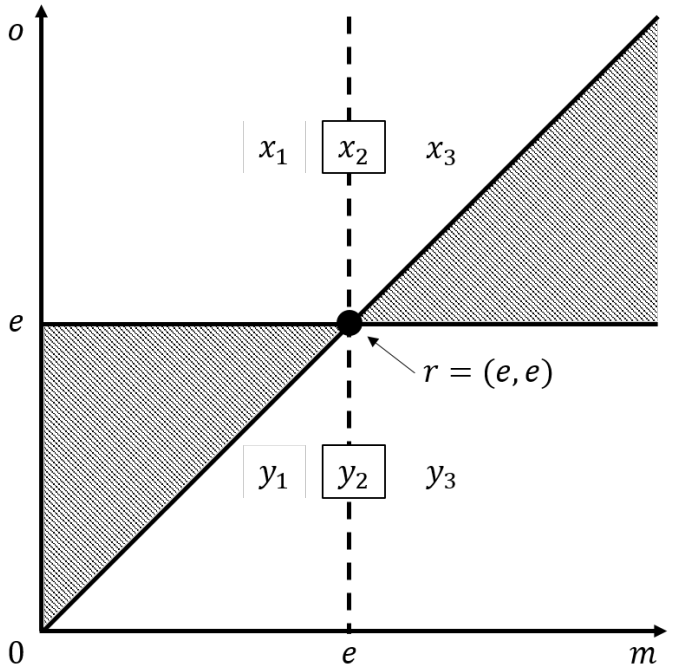
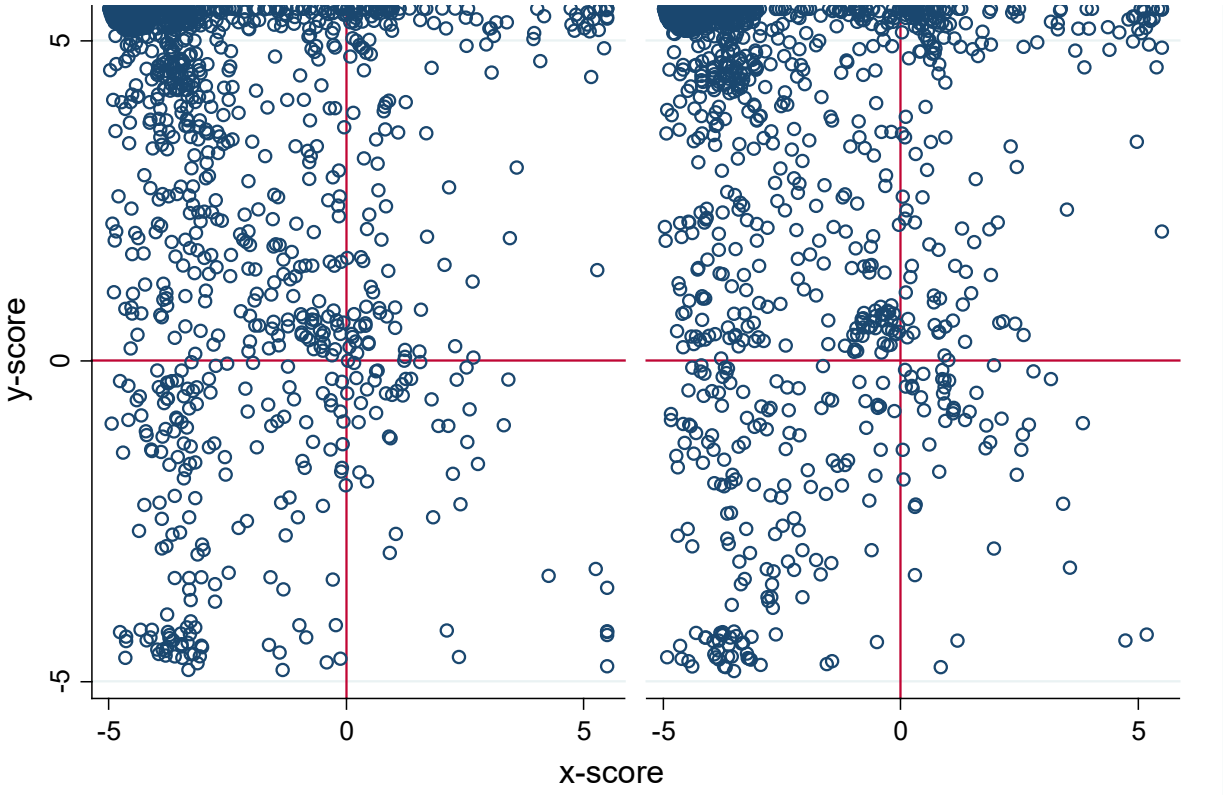


Figure 2: Domains of Inequality and Identification of Distributional Types



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: GIP: Scatter Plot of (x, y) Scores Seperated by Control and Treatment



Notes: Jittered scatterplot of *(x, y) Scores* using data from the GIP survey. Control condition (left panel) and treatment condition (right panel). The y-score measures behavior in the domain of advantageous 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 in the neighborhood of zero (+/- 0.5) 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.

Figure 4: Vignette Study: Main Outcomes

Notes: Raw means of outcomes variables in the vignette study. Sample restricted to respondents who passed an attention check. "Justice" is the perceived justice of success in the vignette, "Luck" measures to what extent success in the vignette is due to luck, and "Responsibility" measures the extent of responsibility for success in the vignette (all variables are measured on a scale from 1 – agree – to 5 – disagree). "Redistribution" is the amount of redistribution between the financially successful main character of the vignette and another fictitious low-income earner (between 0 and 100,000 Euro, scale in thousands). "Parental Influence Low" is the condition that emphasized personal responsibility and "Parental Influence High" is the condition that emphasized the positive impact of parents.

Table 1: GIP: Balance of Observables across Treatment

	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: Data from GIP study. Mean of observables and standard deviations in parentheses. Columns (1) and (2) display the mean (% share) of the listed observables 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 observables.

Table 2: GIP: 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 with robust standard errors in parentheses using data from the GIP. 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 optimism (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 and reports the "first-stage" effects. 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: GIP: 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 using data from the GIP. Redistribution and Education Expenditure are normalized to zero mean and unit variance and higher values indicate more redistribution and more spending on education, respectively. 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 gender, age, number of household members, log income and education, as well as indicators for East Germany, retirement status, employment status, and marital status.

Table 4: GIP: 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 using data from the GIP. 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 gender, age, number of household members, log income and education, as well as indicators for East Germany, retirement status, employment status, and marital status.

Table 5: GIP: Treatment Effect on Policy Preferences

	Redistribution			**Education Expenditure**		
	Main survey		Follow-up wave	Main survey		Follow-up wave
	(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 using data from the GIP. Redistribution and Education Expenditure are normalized to zero mean and unit variance and higher values indicate more redistribution and more spending on education, respectively. Treated indicates whether a respondent received information on the persistence of socio-economic status (i.e., downward and upward mobility in society). Controls include gender, age, number of household members, log income and education, as well as indicators for East Germany, retirement status, employment status, and marital status. The follow-up wave was conducted about 4 months after the main survey.

Table 6: GIP: 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
R^2	0.008	0.086	0.005	0.016	0.033	0.063	0.000	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
R^2	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%								
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%								
Controls	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 6: GIP: 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
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 using data from the GIP. The x-score (y-score) measures benevolence in the disadvantageous (advantageous) domain of inequality, where higher values mean more benevolence. Redistribution and Education Expenditure are normalized to zero mean and unit variance and higher values indicate more redistribution and more spending on education, respectively. 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 standardized 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.

Table 7: Vignette: Treatment Effects

	Responsibility		**Justice**		**Luck**		**Redistribution**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mobility (pre-vignette=1)	-0.033	-0.053	-0.093	-0.072	0.002	-0.020	0.037	0.025
	(0.090)	(0.089)	(0.098)	(0.098)	(0.098)	(0.099)	(0.101)	(0.093)
Parental Influence (high=1)	-0.392***	-0.377***	-0.276***	-0.249***	0.142	0.107	0.095	0.058
	(0.099)	(0.098)	(0.094)	(0.094)	(0.102)	(0.102)	(0.101)	(0.097)
*Mobility*Influence*	-0.049	-0.053	-0.098	-0.133	0.149	0.194	-0.010	0.038
	(0.138)	(0.136)	(0.138)	(0.137)	(0.140)	(0.139)	(0.141)	(0.134)
Constant	0.229***	0.035	0.213***	-0.093	-0.112	0.582**	-0.064**	0.622**
	(0.064)	(0.251)	(0.066)	(0.249)	(0.072)	(0.252)	(0.074)	(0.254)
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
R^2	*0.045*	*0.100*	*0.015*	*0.068*	*0.032*	*0.086*	*0.002*	*0.141*
N	*807*	*807*	*807*	*807*	*807*	*807*	*807*	*807*

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

OLS regressions with robust standard errors in parentheses. Sample restricted to respondents who passed an attention check (pre-registered). "Responsibility" measures the extent of responsibility for success, "Justice" is the perceived justice of success, and "Luck" measures to what extent success in the vignette is due to luck (all variables are measured on a scale from 1 (agree) to 5 (disagree) and standardized to zero mean and unit variance). "Redistribution" is the amount of redistribution between the financially successful main character of the vignette and another fictitious low-income earner (between 0 and 100,000 Euro, standardized to zero mean and unit variance). "Mobility (pre-vignette=1)" is a binary variable indicating that the mobility perceptions are asked before the vignette, and "Parental Influence (high=1)" is a binary variable indicating high parental influence in the vignette. Controls include log income, gender, age, education level, employment status, marital status, household size, and region dummies.

Supplementary Material:
For Online Publication Only

(In-)equality of Opportunity, Fairness, and Distributional Preferences

Dietmar Fehr
University of Heidelberg
and CESifo

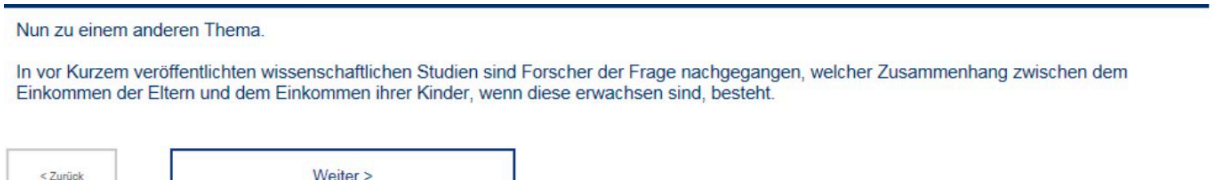
Daniel Müller
University of Munich

Marcel Preuss
Cornell University

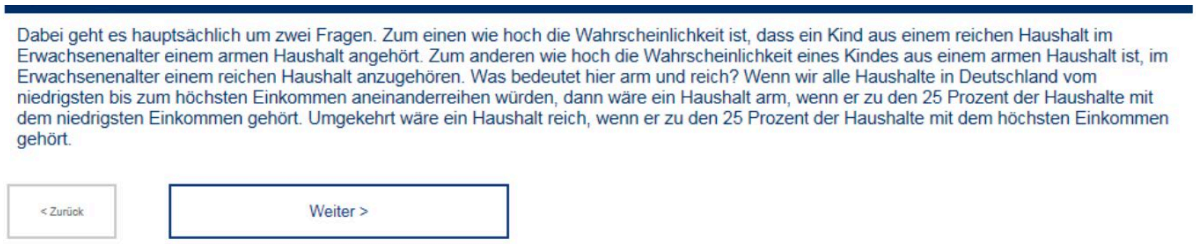
Main Study – GIP

S1: Treatment Intervention

Intro Treatment



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.



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

Für die Studien wurden beispielsweise die aktuellsten Daten einer unabhängigen, wissenschaftlichen Umfrage von über 12.000 Privathaushalten in Deutschland, die seit 1984 jährlich mit den gleichen Personen und Familien durchgeführt wird, herangezogen.

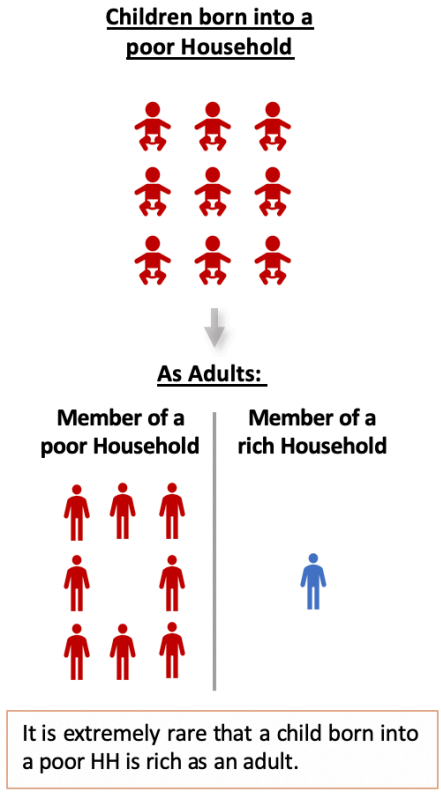
Die Daten zeigen, dass die erwartete Wahrscheinlichkeit, mit der ein Kind aus einem armen Haushalt als Erwachsene/-r reich ist, sehr gering ist. Hingegen ist die Wahrscheinlichkeit für ein Kind aus einem armen Haushalt, auch als Erwachsene/-r arm zu sein, sehr groß. Die folgende Grafik veranschaulicht diese Zusammenhänge.

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:



Umgekehrt ist die erwartete Wahrscheinlichkeit, mit der ein Kind aus einem reichen Haushalt als Erwachsene/-r arm ist, sehr gering. Hingegen ist die Wahrscheinlichkeit für ein Kind aus einem reichen Haushalt, auch als Erwachsene/-r reich zu sein, sehr groß. Die folgende Grafik veranschaulicht diese Zusammenhänge.

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:

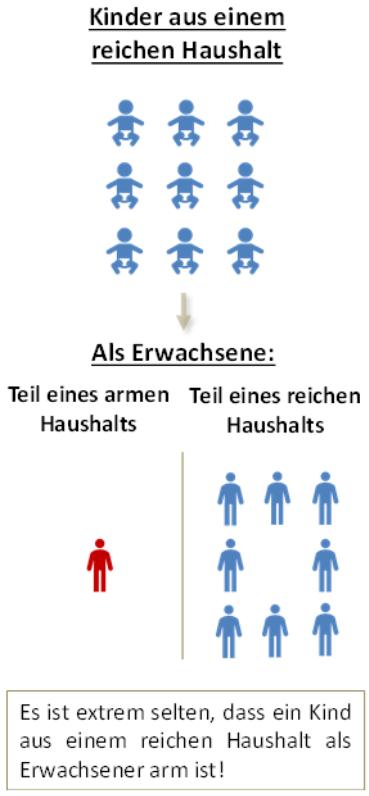
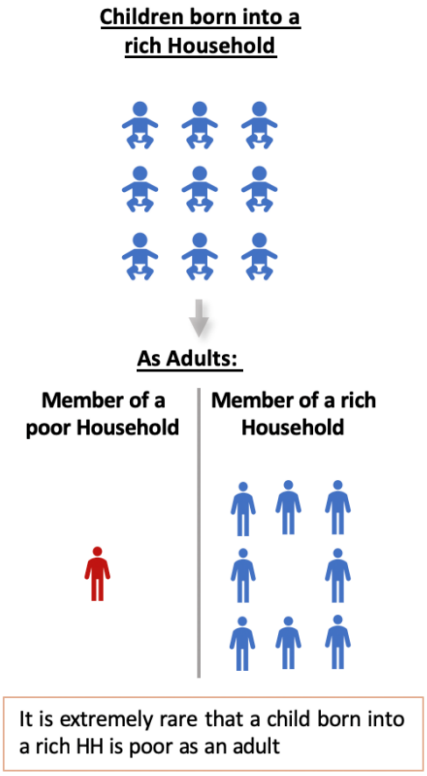
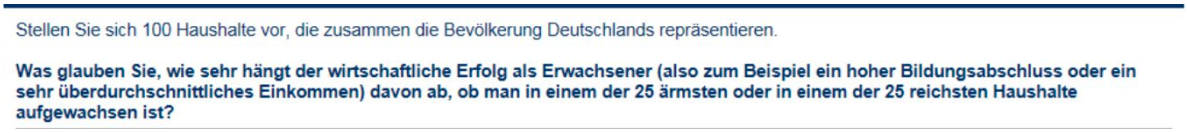


Illustration translated into English



Manipulation Check

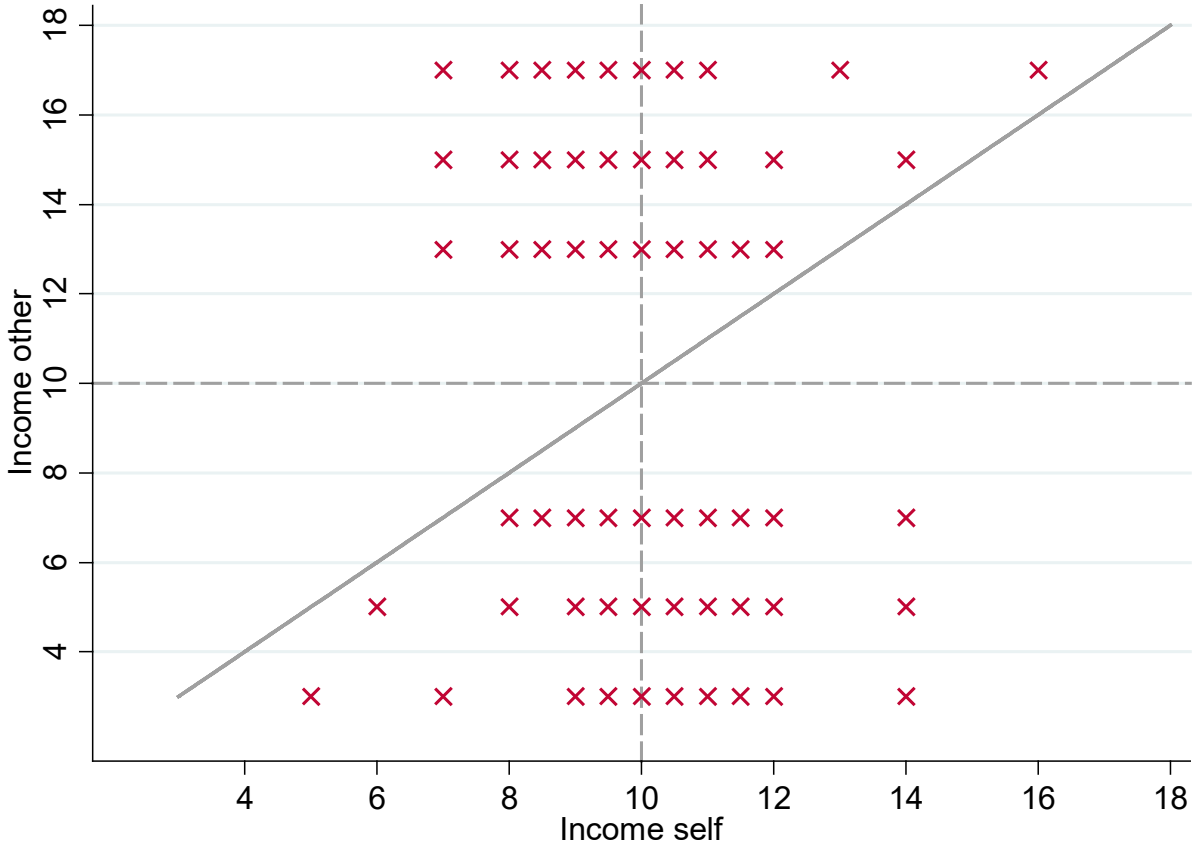


Imagine 100 households that together represent the population of Germany.

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: GIP: 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: Data from the GIP. Mean of observables and standard deviations in parentheses. Column (1) displays the mean (% share) of the listed observables for the pooled sample, while columns (2) and (3) display the mean (% share) separately for the treatment and control group. 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).

S4: Difference-in-Difference Estimates – Distributional Preferences

Table S2: GIP: 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

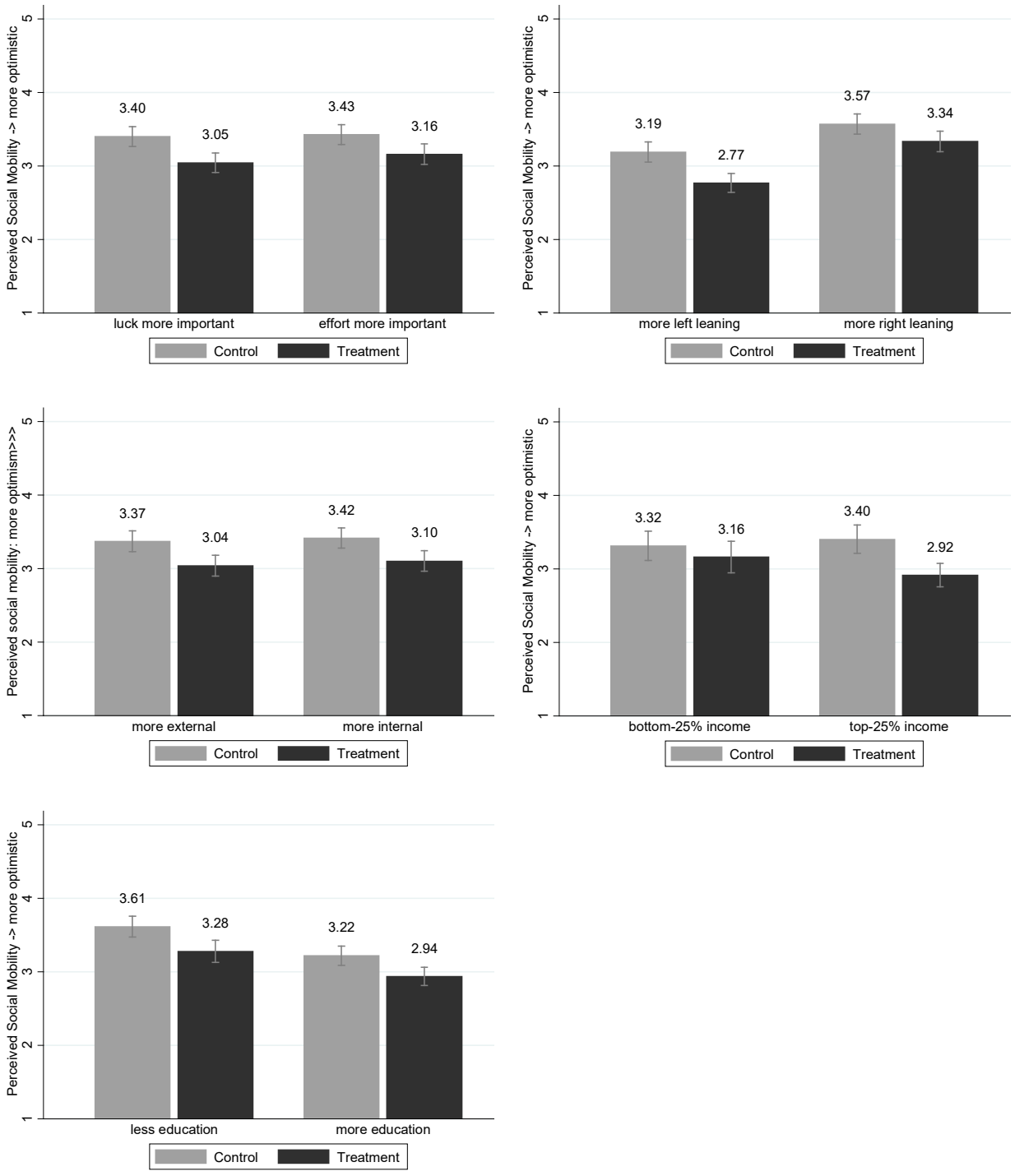
Notes: ***p<0.01, **p<0.05, *p<0.1

OLS regressions with standard errors clustered at the individual level in parentheses using data from the GIP. 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 gender, age, number of household members, log income and education, as well as indicators for East Germany, retirement status, employment status, 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: GIP: Mobility Perception of Specific Subgroups across Treatment Status.



Notes: Data from the GIP. Groups are defined as follows: Left-column: "Luck (Effort) more important" indicates respondents below (at or above) the median response (6) to the 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 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).

Table S3: GIP: 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: 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

2SLS estimates on mobility perceptions using data from the GIP. 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: GIP: Correlates of Distributional Preferences: Political Orientation, Redistribution and Equality of Opportunity

	Mobility Perceptions	**x-score**	**y-score**	**Mobility Perceptions**	**x-score**	**y-score**	**Mobility Percep-tions**	**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 using data from the GIP. "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 gender, age, number of household members, log income and education, as well as indicators for East Germany, retirement status, employment status, and marital status.

Vignette Study

S7: Vignette

High parental influence

We now present you with a description of a fictitious person. Please read the text carefully and then answer the questions below.

-- Page Break --

Sabine was born in Hannover in 1985. After very good grades at elementary school, she then attended high school.

Sabine's parents always made a point of traveling with her, going to museums and to the theater. They also encouraged many extracurricular activities that Sabine pursued in her free time. Thus, Sabine got to know different countries and cultures at an early age and developed distinctive social skills.

After graduating from high school and a voluntary social year, she began to study. At the university, besides her studies, she was active in a student organization, where she was elected president thanks to her open personality. She successfully completed her studies and started working for an international company.

With her above-average skills and ambition, Sabine quickly impressed her superiors and climbed the career ladder. She has recently become one of the top earners in Germany.

Low parental influence

We now present you with a description of a fictitious person. Please read the text carefully and then answer the questions below.

-- Page Break --

Sabine was born in Hannover in 1985. After very good grades at elementary school, she then attended high school.

Sabine's parents always made a point of traveling with her, going to museums and to the theater. They also encouraged many extracurricular activities that Sabine pursued in her free time. Thus, Sabine got to know different countries and cultures at an early age and developed distinctive social skills.

After graduating from high school and a voluntary social year, she began to study. At the university, besides her studies, she was active in a student organization, where she was elected president thanks to her open personality. She successfully completed her studies and started working for an international company.

With her above-average skills and ambition, Sabine quickly impressed her superiors and climbed the career ladder. She has recently become one of the top earners in Germany.

S8: Balance of Observables – Vignette Study

Table S5: Vignette: Balance of Observables

	Influence (Low=1)	**Influence (High=1)**	***p-value***
	(1)	(2)	(3)
Age	46.74 (14.89)	45.46 (15.44)	*0.23*
Female	0.49 (0.50)	0.50 (0.50)	*0.86*
Education *No degree=1/Highest degree=5*	3.77 (1.27)	3.68 (1.27)	*0.28*
Married=1	0.44 (0.50)	0.38 (0.49)	*0.07*
Monthly Income	29.97 (15.87)	29.90 (16.00)	*0.95*
Employed=1	0.45 (0.50)	0.47 (0.50)	*0.57*
Household Size	2.20 (1.15)	2.19 (1.10)	*0.89*
East Germany=1	0.21 (0.41)	0.23 (0.42)	*0.39*
Prob>F			*0.93*

Notes: Data from the vignette study. Mean of controls and standard deviations in parentheses. The sample is restricted to respondents who passed the attention check. Columns (1) and (2) display the mean (% share) of the listed observables in the low and high parental influence vignette, respectively. Column (3) shows the p-values of the coefficients of separate OLS regressions, in which the treatment indicator (vignette) is regressed on the observable in that row. Education is a categorical variable, where 1 indicates no degree and 5 indicates highest degree (i.e., university qualification).
Prob>F is the p-value of an F-test for joint significance of all observables.

S9:Robustness Check – Vignette Study

Table S6: Vignette: Robustness – Treatment Effects

	Justice		**Luck**		**Responsibility**		**Redistribution**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mobility (pre-vign.=1)	-0.081	-0.069	0.031	0.002	-0.018	-0.040	0.108	0.082
	(0.086)	(0.086)	(0.087)	(0.086)	(0.078)	(0.076)	(0.089)	(0.082)
Influence (high=1)	-0.280***	-0.257***	0.171*	0.142	-0.415***	-0.392***	0.097	0.089
	(0.084)	(0.084)	(0.088)	(0.088)	(0.085)	(0.084)	(0.085)	(0.082)
*Mobility*Influence*	-0.013	-0.036	0.063	0.097	-0.041	-0.056	-0.087	-0.065
	(0.122)	(0.120)	(0.123)	(0.120)	(0.120)	(0.118)	(0.123)	(0.119)
Constant	0.183***	-0.388*	-0.117*	0.871***	0.227***	-0.071	-0.080	0.639***
	(0.058)	(0.223)	(0.062)	(0.224)	(0.054)	(0.215)	(0.060)	(0.219)
Controls	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
R^2	*0.023*	*0.082*	*0.012*	*0.078*	*0.048*	*0.106*	*0.002*	*0.131*
N	*1055*	*1055*	*1055*	*1055*	*1055*	*1055*	*1055*	*1055*

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

OLS regressions with robust standard errors in parentheses using data from the vignette. Full sample, including respondents who "failed" the attention checked. "Justice" is the perceived justice of success in the vignette, "Luck" measures to what extent success in the vignette is due to luck, and "Responsibility" measures the extent of responsibility for success in the vignette (all variables are measured on a scale from 1 (agree) to 5 (disagree) and standardized to zero mean and unit variance). "Redistribution" is the amount of redistribution between the financially successful main character of the vignette and another fictitious low-income earner (between 0 and 100,000 Euro, standardized to zero mean and unit variance). "Mobility (pre-vign.=1)" is a binary variable indicating that the mobility perceptions are asked before the vignette, and "Influence (high=1)" is a binary variable indicating high parental influence in the vignette. Controls include log income, gender, age, education level, employment status, marital status, household size, and region dummies.

S10: Survey Module – GIP

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

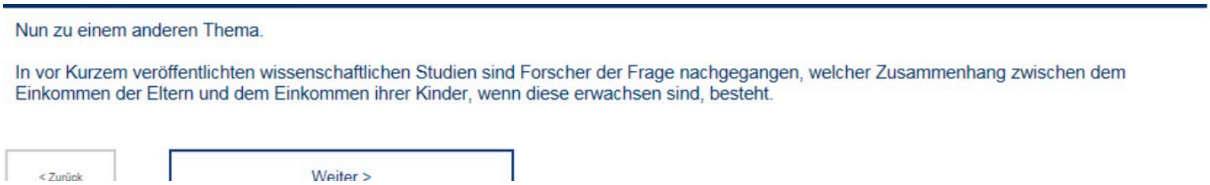
In the following we present screenshots (and a translation) of the questions in our survey module, the treatment intervention and the EET.

Luck – Effort

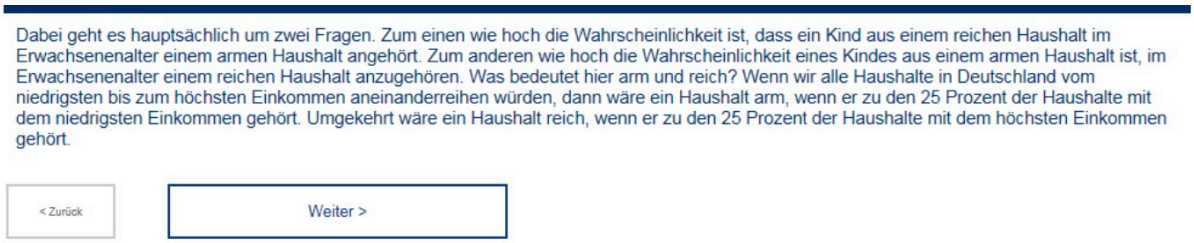
How financially successful a person is in life can depend on circumstances beyond one's control, such as luck. Or, on the other hand, it can depend on circumstances that one can influence oneself, such as personal effort.

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



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.



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

Für die Studien wurden beispielsweise die aktuellsten Daten einer unabhängigen, wissenschaftlichen Umfrage von über 12.000 Privathaushalten in Deutschland, die seit 1984 jährlich mit den gleichen Personen und Familien durchgeführt wird, herangezogen.

Die Daten zeigen, dass die erwartete Wahrscheinlichkeit, mit der ein Kind aus einem armen Haushalt als Erwachsene/-r reich ist, sehr gering ist. Hingegen ist die Wahrscheinlichkeit für ein Kind aus einem armen Haushalt, auch als Erwachsene/-r arm zu sein, sehr groß. Die folgende Grafik veranschaulicht diese Zusammenhänge.

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:

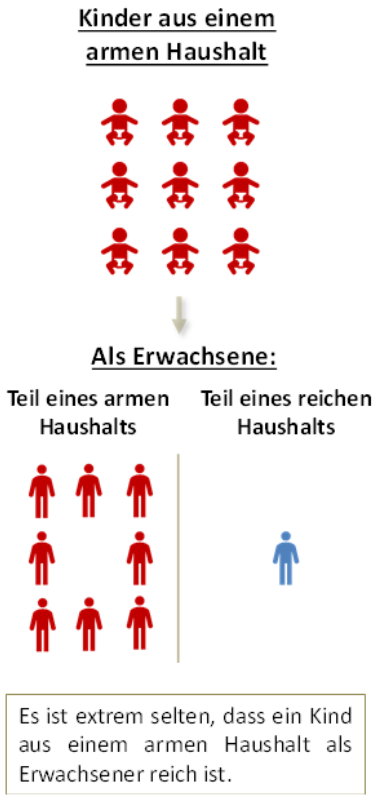
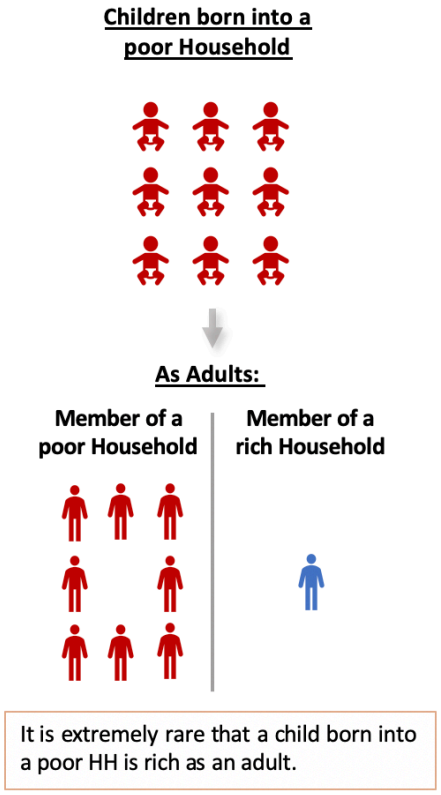


Illustration translated into English:

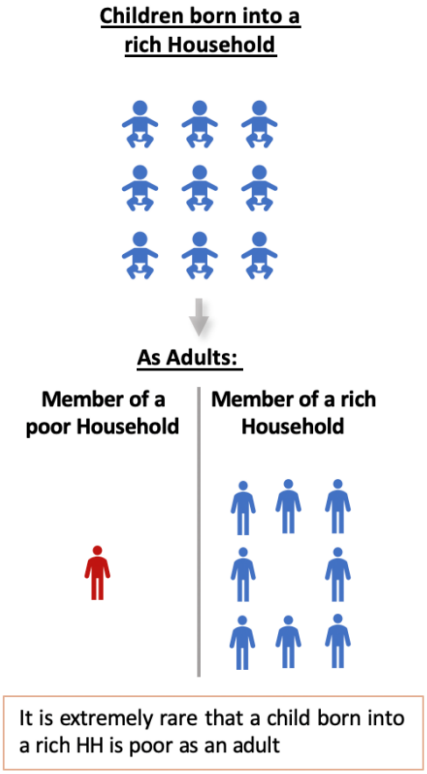


Umgekehrt ist die erwartete Wahrscheinlichkeit, mit der ein Kind aus einem reichen Haushalt als Erwachsene/-r arm ist, sehr gering. Hingegen ist die Wahrscheinlichkeit für ein Kind aus einem reichen Haushalt, auch als Erwachsene/-r reich zu sein, sehr groß. Die folgende Grafik veranschaulicht diese Zusammenhänge.

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



Manipulation Check

Stellen Sie sich 100 Haushalte vor, die zusammen die Bevölkerung Deutschlands repräsentieren.

Was glauben Sie, wie sehr hängt der wirtschaftliche Erfolg als Erwachsener (also zum Beispiel ein hoher Bildungsabschluss oder ein sehr überdurchschnittliches Einkommen) davon ab, ob man in einem der 25 ärmsten oder in einem der 25 reichsten Haushalte aufgewachsen ist?

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.

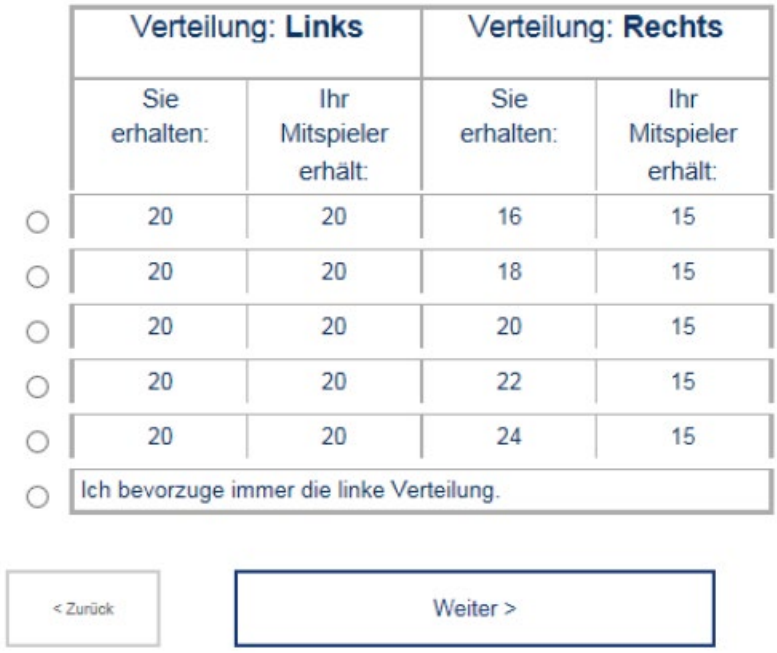
<u>Example:</u>

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.



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”).

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	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.			

< Zurück Weiter >

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:
○	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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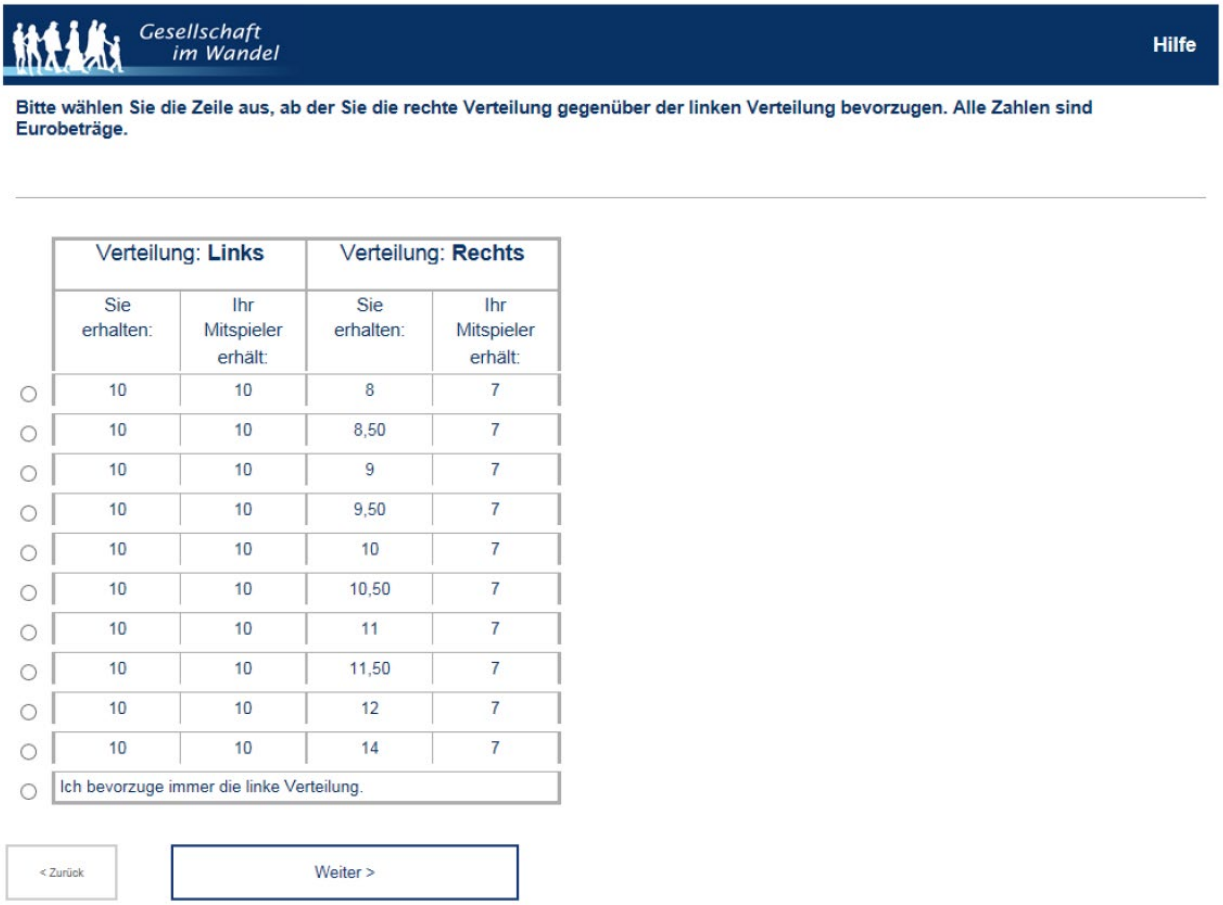

Choice lists for disadvantageous domain (*x-score*)

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:
○	10	10	7	13
○	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

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:
○	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.			

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Weiter >

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:
○	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.			

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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)]

S11: Survey – Vignette Study

Welcome to our survey. We are researchers from Heidelberg University, University of Munich and Cornell University (USA) and our goal is to learn more about people's attitudes towards certain political and economic issues. Please read the following information carefully before agreeing to participate in the survey.

- Your participation in this study is completely voluntary and anonymous. You can stop participating at any time. However, for the success of our research project, it is important that you complete the entire survey once you have started. Every vote, assessment and opinion is important for us to draw the right conclusions. Completing this survey should take (on average) about 7 minutes.
- Your name will not be stored at any time. Your answers are included in a scientific study and are processed in aggregated form. No one can draw conclusions about your identity from your answers.
- Please note that it is very important for the validity of our study that you **answer honestly** and **read the questions very carefully** before answering. If at any time you do not know an answer, please provide your best guess **without consulting external sources**. However, please make sure you take enough time to read and understand the questions.
- To participate in this study, you must be **at least 18 years old** and live in Germany. If you do not meet these requirements, please do not continue.

Yes, I would like to participate in this study and confirm that I live in Germany and am at least 18 years old. (1) / No, I do not wish to participate. (2)

We want to start the survey with a few questions about you first.

Q1 Were you born in Germany?

Yes (1) / No (2)

Q2 You are…

male (1) / female (2) / other (3)

Q3 Please state your age:

Q4 What is your marital status?

Married (1) / Registered civil partnership (2) / Widowed (3) / Divorced (4) / Single (5)

Q5 What is your highest level of education?

No degree (yet) (1) / Secondary school diploma (successfully completion of 9th or 10th grade) (2) / High school diploma (Mittlere Reife, Fachoberschulreife, or similar) (3) / University entrance qualification (general university entrance qualification, subject-specific university entrance qualification or university of applied sciences entrance

qualification) (4) / University degree (Bachelor, Master, Diploma, State Examination, Doctorate) (5)

Q6 Are you currently …

Full-time employed (1) / Part-time employed (2) / Intern (3) / Vocational training (4) / Self-employed (5) / Unemployed (6) / Retired (7) / Housewife/husband (8)

Q7 How many people live permanently in your household, including yourself?

1 (1) / 2 (2) / 3 (3) / 4 (4) / 5 (5) / more than 5 (6)

Q8 How many children do you have?

0 (1) / 1 (2) / 2 (3) / 3 (4) / 4 (5) / more than 4 (6)

The next questions are about your economic situation and a few estimates about it.

Q9 How financially successful a person is in life may depend partly on circumstances beyond one's control, for example chance, or partly on circumstances that one can influence oneself, for example 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 (1) / 1 (2) / … / 9 (10) / only effort 10 (11)

Q10 What is your **household's monthly income** after all taxes and duties are deducted? *(Income includes wages or salaries, income from self-employment, investment income, pension payments, and other private payments such as alimony or child support.)*

€0 - €500 (1) / €501 - €1000 (2) / €1001 - €1500 (3) / €1501 - €2000 (4) / €2001 - €2500 (5) / €2501 - €3000 (6) / €3001 - €3500 (7) / €3501 - €4000 (8) / €4001 - €5000 (9) / €5001 - €6000 (10) / €6001 - €7000 (11) / more than €7000 (12)

Q11 Which of the following investments of value did you or others in your household own in 2021? *Multiple answers possible.*

Stocks (1) / Equity Bonds (2) / Equity funds (3) / Index funds (4) / ETFs (Exchange Traded Funds) (5) / Certificates (6) / Savings account (7) / No value investments (8)

Q12 Do you live for rent or do you own the apartment or house?

Apartment/house for rent (1) / Own apartment (2) / Own house (3)

Q13 What is the living area of your house or apartment (in m2)? *If you are not sure, please provide as accurate an estimate as possible.*

up to 40m2 (1) / 41-60m2 (2) / 61-80m2 (3) / 81-100m2 (4) / 101-120m2 (5) / 121-140m2 (6) / 141-160m2 (7) / 161-180m2 (8) / more than 180m2 (9)

Q14 Did you or anyone else in your household have income from renting or leasing real or personal property in 2021?

Yes (1) / No (2)

Q15 Approximately what was the total income from rentals and leases in 2021? *Indicate your estimate in euros.*

Q16 In which state do you have your main residence?

Q17 What is your zip code?

Q18 [*Randomized: asked after Q17 or after Q24*] Imagine 100 households that together represent the population of Germany. How much do you think economic success as an adult depends on whether you grew up in one of the 25 poorest households or one of the 25 richest?

Very little 1 (1) / 2 (2) / … / 9 (9) / Very strongly 10 (10)

Q19 In questionnaires like ours, there are sometimes participants who don't read the questions carefully and just click through the survey quickly. This means that there are a lot of random answers that can affect the results of research studies. To show that you read through our questions carefully, please indicate "Brown" as the answer to the next question. Which of the following colors do you like best?

Blue (1) / Red (2) / Green (3) / Yellow (4) / Brown (5)

Vignette [*Conditions randomized*]

We now present you with a description of a **fictitious person**. Please read the text carefully and then answer the questions below.

Condition 1: Sabine was born in Hannover in 1985. After very good grades at elementary school, she then attended high school.

Sabine's parents always made a point of traveling with her, going to museums and to the theater. They also encouraged many extracurricular activities that Sabine pursued in her free time. Thus, Sabine got to know different countries and cultures at an early age and developed distinctive social skills.

After graduating from high school and a voluntary social year, she began to study. At the university, besides her studies, she was active in a student organization, where she was elected president thanks to her open personality. She successfully completed her studies and started working for an international company.

With her above-average skills and ambition, Sabine quickly impressed her superiors and climbed the career ladder. She has recently become one of the top earners in Germany.

Condition 2: Sabine was born in Hannover in 1985. After very good grades at elementary school, she then attended high school.

Sabine's parents always made a point of traveling with her, going to museums and to the theater. They also encouraged many extracurricular activities that Sabine pursued in her free time. Thus, Sabine got to know different countries and cultures at an early age and developed distinctive social skills.

After graduating from high school and a voluntary social year, she began to study. At the university, besides her studies, she was active in a student organization, where she was elected

president thanks to her open personality. She successfully completed her studies and started working for an international company.

With her above-average skills and ambition, Sabine quickly impressed her superiors and climbed the career ladder. She has recently become one of the top earners in Germany.

Q21 How much do you agree with the following statements?

That Sabine is so successful is only just. Strongly disagree (1) / Strongly agree (5)

Sabine's success is mainly due to luck. Strongly disagree (1) / Strongly agree (5)

Q22 Sabine currently has an annual income of 100,000 euros (before deduction of all taxes and duties). Someone else, let's call the person Anja, has an annual income of 20,000 Euro (before deduction of all taxes and duties). Like Sabine, Anja works in a full-time job throughout the year. Nothing else is known. If it were practically feasible, how much of Sabine's income would you redistribute to Anja?

Please use the slider to determine the amount. Below you will see the amount that will be re-distributed and the final income of Sabine and Anja. You can repeat this procedure as often as you like and try different values. To finally confirm your input, simply exit the page.

Slider from 0 Euro to 100,000 Euro. Screen automatically calculates and displays (1) the amount to be redistributed (in Euro), (2) Sabine's income after redistribution (in Euro), and (3) Anja's income after redistribution (in Euro).

See screenshot below

Q23 How much do you agree with the following statements? Sabine is herself responsible for her success

Strongly disagree (1) / Strongly agree (5)

We will now move on to other topics. Here, we are primarily concerned with your political and social assessments.

Q24 In politics, people often talk about "left" and "right" when it comes to labeling different political attitudes. When you think of your own political views: where would you rank those views?

Far left 0 (1) / 1 (2) / … / 9 (10) / Far right 10 (11)

Q25 And on economic and social policy issues? Where would you rank your views on these issues?

Far left 0 (1) / 1 (2) / … / 9 (10) / Far right 10 (11)

Q26 What has more to do with why a person is rich?

Because she or he has worked harder than others (1) / Because she or he had more advantages than others (2)

Q27 What has more to do with why a person is poor?

Lack of own effort (1) / Circumstances beyond one's control (2)

Q28 How often do you think you can trust the government to do the right thing?

Never (1) / Only sometimes (2) / Most times (3) / Always (4)

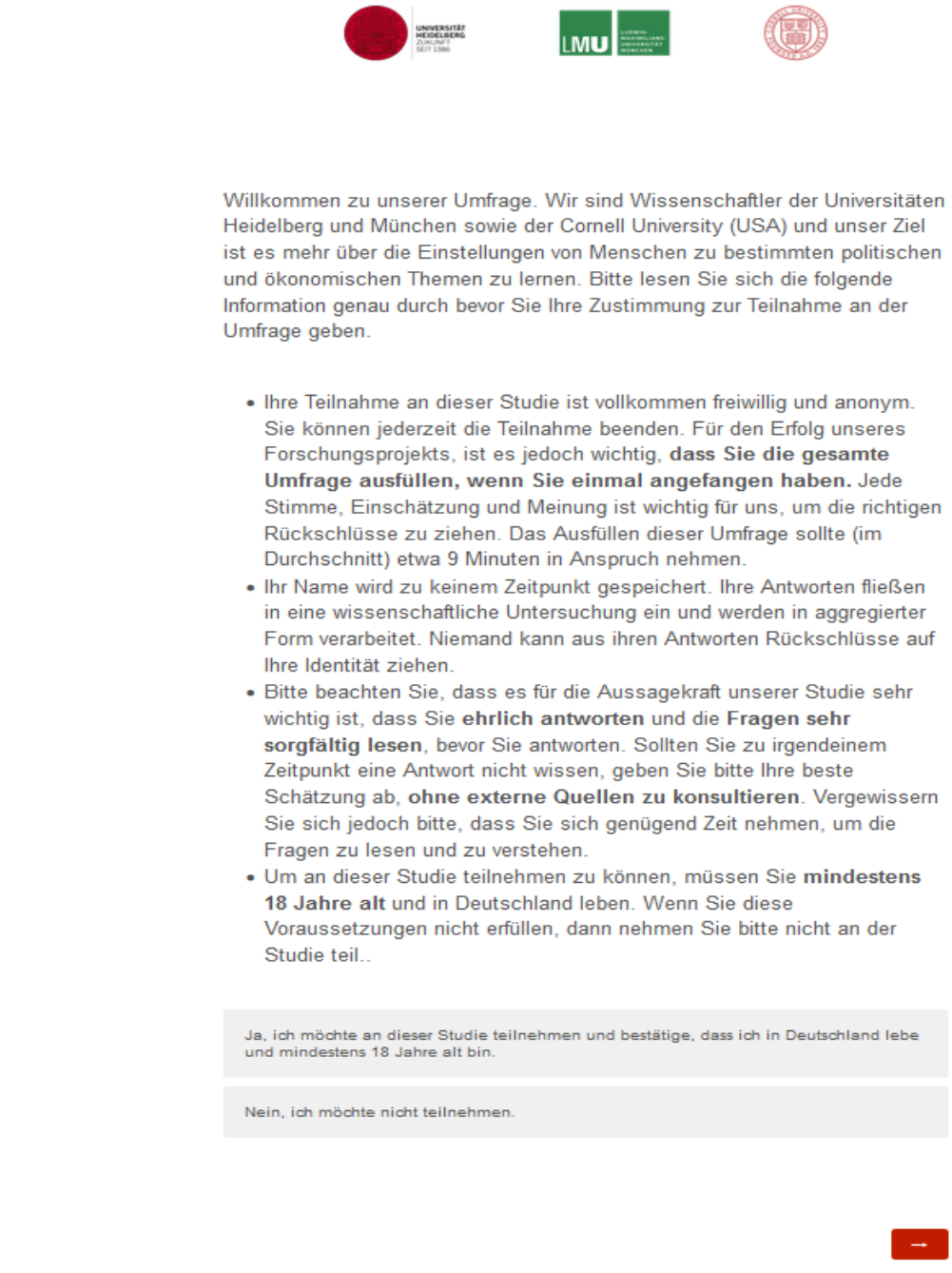
Q29 In Germany, everyone has a chance to make it and be economically successful.

Fully agree (1) / Disagree at all (5)

Q30 There are probably many distractions (other people, TV, music, etc.) while you are answering the questions in this survey. Answer with very little attention. Please indicate how much attention you have paid to this study.

Very little attention (1) / Very much attention (4)

Screenshot Consent Page



Willkommen zu unserer Umfrage. Wir sind Wissenschaftler der Universitäten Heidelberg und München sowie der Cornell University (USA) und unser Ziel ist es mehr über die Einstellungen von Menschen zu bestimmten politischen und ökonomischen Themen zu lernen. Bitte lesen Sie sich die folgende Information genau durch bevor Sie Ihre Zustimmung zur Teilnahme an der Umfrage geben.

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- Ihr Name wird zu keinem Zeitpunkt gespeichert. Ihre Antworten fließen in eine wissenschaftliche Untersuchung ein und werden in aggregierter Form verarbeitet. Niemand kann aus ihren Antworten Rückschlüsse auf Ihre Identität ziehen.
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- Um an dieser Studie teilnehmen zu können, müssen Sie **mindestens 18 Jahre alt** und in Deutschland leben. Wenn Sie diese Voraussetzungen nicht erfüllen, dann nehmen Sie bitte nicht an der Studie teil..

Ja, ich möchte an dieser Studie teilnehmen und bestätige, dass ich in Deutschland lebe und mindestens 18 Jahre alt bin.

Nein, ich möchte nicht teilnehmen.

→

Screenshot Redistribution

Sabine erzielt aktuell ein Jahreseinkommen von 100.000 Euro (vor Abzug aller Steuern und Abgaben). Jemand anderes, nennen wir die Person *Anja*, erzielt ein Jahreseinkommen von 20.000 Euro (vor Abzug aller Steuern und Abgaben). Wie *Sabine* arbeitet *Anja* das ganze Jahr über in einer Vollzeitbeschäftigung. Sonst ist nichts weiter bekannt.

Wenn es praktisch machbar wäre, wieviel von *Sabine's* Einkommen würden Sie an *Anja* umverteilen?

Bitte benutzen Sie den Schieberegler um den Betrag zu bestimmen, erst danach können Sie die Seite verlassen. Unter dem Schieberegler sehen Sie den Betrag, den Sie umverteilen wollen und das endgültige Einkommen von Sabine und Anja. Sie können beliebig viele Werte ausprobieren. Um ihre Eingabe endgültig zu bestätigen, stellen Sie den Regler auf die gewünschte Position und klicken auf den "Weiter" Pfeil.

Betrag, der umverteilt werden soll: 34000

Einkommen von Sabine nach Umverteilung: 66000

Einkommen von Anja nach Umverteilung: 54000

Pre-analysis Plan:

Dietmar Fehr, Daniel Müller, and Marcel Preuss

February 2018

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.

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 Müller (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:

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

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%), $\boldsymbol{X}$ 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):

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

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:

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

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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Pre-registration Vignette Study

Available at https://aspredicted.org/2RL_FKX

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Social mobility perceptions and inequality acceptance - Additional Survey (#87835)

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1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
The current survey serves as an extension of Fehr, Mueller and Preuss (2020). We will conduct a survey experiment among a large and heterogeneous sample of the German population. In the core of the survey, participants are randomized to two versions of a vignette. The vignette tells the story of a person's early education and her career. The two versions of this vignette experimentally manipulate the strength of her parent's influence on her early education and career. The main research question is whether participants recognize differences in accountability and if so, whether it affects their views concerning the person's success.
We hypothesize that participants' redistributive preferences do not differ across vignettes, even though one person benefited more from parental influence than the other. We think this will be the case because participants consider the role of luck to be similar across both vignettes or because they consider the described person's success to be equally fair across vignettes, or both.

3) Describe the key dependent variable(s) specifying how they will be measured.
The key dependent variables are two survey questions and a "redistribution" question, in which participants are asked to re-allocate money between two fictional persons. The answers to these three questions are elicited after the vignette treatment. The two survey questions each consist of a statement that refers to the success the fictional person in the vignette:
1.That Sabine is so successful is only fair.
2.Sabine's success is mainly due to luck.
Participants are asked to indicate their support of these statements.

Lastly, we check whether participants recognize the difference in parental influence that is illustrated by the two vignette studies. To this end, we ask:
1.Sabine has only herself to thank for her success.

The answers to all three questions will be measured on a five-point Likert scale and coded from 1="fully disagree" to 5="fully agree".

4) How many and which conditions will participants be assigned to?
The main treatment is the random assignment to one of the two vignettes. We also cross-randomize subjects into a condition in which they are asked about their perceptions of parental influence on economic success before the vignette and a condition in which this question is asked after the vignette (and after the main outcomes). Thus, there will be four treatment conditions in total:
1.Perception question first, Vignette with parental influence second
2.Perception first, Vignette without parental influence second
3.Vignette with parental influence first, Perception question second
4.Vignette without parental influence first, Perception question second

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
We will run OLS regressions (including some controls gathered in the survey) to test the effect of the vignette (and the order of the questions asked) on the main outcomes.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
We will include a simple attention check in the survey and exclude those participants who fail this check.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
We will collect about 250 observations per treatment cell (that is, about 1,000 observations in total).

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
We will investigate whether there is an interaction effect between the order and the vignettes. Specifically, we are interested in whether the vignette with parental influence has a stronger effect on the specified outcome measures in the treatment arms where the perception question is asked first.