KIEL WORKING PAPER

Does ethnic heterogeneity decrease workers' effort in the presence of income redistribution? An experimental analysis

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ABSTRACT

DOES ETHNIC HETEROGENEITY DECREASE WORKERS' EFFORT IN THE PRESENCE OF INCOME REDISTRIBUTION? AN EXPERIMENTAL ANALYSIS

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Ethnic discrimination is ubiquitous, and it has been shown to exert adverse effects on income redistribution. The reason is that a country's ethnic majority, if richer than the average, may be unwilling to transfer resources to the country's ethnic minorities if poorer than the average. A yet untested mechanism is that a country's ethnic majority may reduce their work effort knowing that their income will finance redistribution to ethnic minorities. We test for this mechanism experimentally in triadic interactions. A German citizen acting as a worker is randomly matched with a recipient who can be another German, an economic migrant, or an asylum seeker in Germany. Workers know that another German citizen may transfer part of their earnings to the recipient. The recipient does not exert any work effort. Even if the recipient's identity does not affect effort in the aggregate, social identity strongly moderates this relationship. Participants with a strong German identity, i.e., who report feeling close to other Germans, exert significantly less effort than other participants if the recipient is an asylum seeker. They also exert more effort when matched with a German recipient than an asylum seeker, while participants with a less strong German identity do the opposite. Moreover, participants with a strong German identity exert slightly more effort when matched with economic migrants than with asylum seekers, while others tend to do the opposite, albeit statistically insignificantly. Workers' beliefs over the third party's redistribution rate do not mediate such results and are generally inaccurate.

Keywords: Redistribution, Discrimination, Taxes, Beliefs, Real effort, Experiment

JEL classification: C91, H23, I31, J15, J30

1 Introduction

Since the influential work by Alesina and Glaeser (2004), it has been argued that increased ethnic heterogeneity may lead to less income redistribution and shrinking welfare states. The reason is that ethnic minorities, such as Blacks or Hispanics in the US, typically occupy the lower tiers of the income distribution. Ethnic majorities, such as Whites in the US, who are motivated by aversion toward ethnic minorities, will thus demand low redistribution to provide little benefit to ethnic minorities. The greater ethnic homogeneity in Europe compared to the US thus partly explains the higher redistribution rates in the former than the latter. This thesis has received extensive empirical support (Luttmer, 2001; Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Alesina, Murard and Rapoport, 2021), by carrying out online-survey experiments (Alesina, Miano and Stantcheva, 2018; Alesina, Ferroni and Stantcheva, 2021) or by exploiting exogenous migrant placement policies (Dahlberg, Edmark and Lundqvist, 2012).[1] Besides, another strand of the literature finds ethnic diversity associated with lower quantity and quality of public goods provision (Alesina, Baqir and Easterly, 1999; Algan, Hémet and Laitin, 2016; Tabellini, 2020). One of the leading explanations proposed for these findings is ingroup favoritism or group loyalty effects (Luttmer, 2001). The rationale behind this concept is that people may attach a higher value to the well-being of their "ingroup," the group to which they feel connected, as compared to others (the "outgroup") (Tajfel et al., 1971; Brewer, 1999). Group loyalty effects have been extensively studied in the (socio-)psychological and recent economic literature (Balliet, Wu and de Dreu, 2014; Romano et al., 2017, 2021).[2] However, the literature remains largely silent on whether outgroup members among potential welfare state beneficiaries may even lead to a withdrawal of working effort by the native population.

Our paper aims to fill this gap in the literature. We report evidence from an experiment testing whether people's work commitment in a real-effort task is affected by the migration status of potential beneficiaries of earning redistribution. Participants from a University student pool (the "workers") of German citizenship could earn money depending on their performance in a real-effort task (Gill and Prowse, 2012). Participants were informed that a third-party allocator (the "allocator" in the following) would be able to transfer part of their earnings to another person (the "recipient"). The allocator could choose any tax rate from 0% (in which case the initial earnings would be earned in full by the worker) to 100% (in which case all of the worker's earnings would be transferred to the recipient). This setting mimics a vastly simplified - and rather extreme at the high end of redistribution - version of a welfare state. The experimental design is similar to the first phases in Cappelen et al. (2013) and Almås, Cappelen and Tungodden (2020). Each participant performed a

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[1]See the survey of the literature by Stichnoth and van der Straeten (2013).

[2]See Anderson, Fryer and Holt (2006) and Cooper and Kagel (2016) for reviews of the literature.

slider task (Gill and Prowse, 2012) for three rounds. In a between-subject design, we used three treatments varying whether the recipient was (i) a German citizen, (ii) an asylum seeker, or (iii) an economic migrant. In this non-strategic interaction, the recipient could not influence the worker's payoffs. Hence, the design allows studying preference-based group effects independently of beliefs about whether actions could be reciprocated in the future.[3]

The optimal strategy for a self-interested worker is always to perform the highest possible effort compatible with the marginal disutility of effort - which should not vary over treatments. We hypothesized, nevertheless, that workers would be more inclined to exert higher effort when the recipient is a fellow country person rather than an immigrant (Hypothesis 1). This would be the case if ingroup favoritism (based on nationality) applied to effort levels similar to what has been observed in prior research (see literature cited above). Our second hypothesis was that the higher the expected tax rate, the lower workers' effort (Hypothesis 2).

While our analyses do not reveal any group effects on task performance in the aggregate, we find results consistent with our hypotheses after splitting the sample according to a simple measure of workers' identification with Germans, i.e., their *national identification*. This measure is based on a simple question where we asked participants to state how close they feel to Germans. We divide the sample into those who report feeling "very close" or "close" to Germans[4] ("*Close*" henceforth) and those who do not (*"Non-Close"* henceforth). As noted by Fong and Luttmer (2009), a question on subjective closeness is likely to be less prone to social desirability bias than other commonly used questions on racial or ethnic identification, where subjects might feel reluctant to reveal an aversion against a specific group of people. We find that *Close* participants exert significantly less effort than *Non-Close* participants if the recipient is an asylum seeker. Moreover, *Close* participants exert more effort when matched with a German recipient than an asylum seeker, while *Non-Close* participants do the opposite. Although the latter two results are either weakly significant or at the margins of significance, the difference of the difference is statistically significant. Moreover, *Close* and *Non-Close* participants also seem to differ in the way they treat economic migrants and asylum seekers. *Close* participants exert slightly more effort when matched with economic migrants than with asylum seekers, while *Non-Close* participants tend to do the opposite, albeit at statistically insignificant levels. The difference of the difference is, in this case, weakly significant. Contrary to Hypothesis 2, workers' beliefs over the degree of redistribution are not related to effort. However, it is noticeable that *Close* participants expect relatively larger shares of their earnings to be redistributed toward an asylum seeker

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[3]See Everett, Faber and Crockett (2015) for an extensive review on the role of beliefs and preferences in explaining prosocial behavior and Durrheim et al. (2016) for the role of expectations of ingroup reciprocity.

[4]We use the answer score to the question "How close are the following groups to you?" which could range from 1 "very close" to 5 "very distant". Groups were "People in your town", "Germans", "Europeans", and "Peope all over the world". See Appendix.

than *Non-Close* participants. This expectation is wrong as allocators transfer significantly more to German recipients than asylum seekers (Grimalda et al., 2022). While workers from the *Non-Close* group correctly anticipate that German recipients will benefit most from redistribution, they grossly under-estimate the actual size of the transfers accruing to asylum seekers. Overall, these results entail that the discrimination effect we find in the *Close* group primarily by a decrease in pure altruism toward asylum seekers, rather than statistical discrimination (Becker, 1971), where statistical discrimination in this setting entails a belief that the allocator will mostly favor redistribution toward asylum seekers.

From a broad perspective, our paper contributes to the vast literature shedding light on the relevance of ethnic diversity for preferences for redistribution (Alesina and Glaeser, 2004; Fong and Luttmer, 2009; Alesina and Giuliano, 2011; Alesina, Miano and Stantcheva, 2018; Alesina, Murard and Rapoport, 2019) and public goods provision (Alesina, Baqir and Easterly, 1999; Algan, Hémet and Laitin, 2016; Tabellini, 2020). We add to this literature finding that people with a strong ingroup identification even act against their monetary self-interest by exerting less effort if potential beneficiaries of the welfare system are from an outgroup. Our results extend those from Hedegaard and Tyran (2018) to a different context, who find that entrepreneurs prefer selecting workers from their nationality rather than from a different nationality even when the former has lower productivity than the latter, thus reducing their expected profits. Our study also contributes to the experimental literature on labor market relationships, which shows that productivity may be affected by transient mood and states of happiness (Oswald, Proto and Sgroi, 2015). Our paper is also closely related to the literature on self-image derived from membership in a social group (Tajfel et al., 1971; Turner, Brown and Tajfel, 1979; Tajfel, 1982), which has been extensively studied in social psychology and sociology (Tajfel, 1982; Brewer, 1999) before it was introduced to the economics literature by Akerlof and Kranton (2000). Various studies in the experimental-economic literature have documented group effects in dictator and two-person response games (Chen and Li, 2009; Ockenfels and Werner, 2014; Tanaka and Camerer, 2016; Abbink and Harris, 2019), coordination games (Goette, Huffman and Meier, 2006; Charness, Rigotti and Rustichini, 2007; Charness and Rustichini, 2011; Guala, Mittone and Ploner, 2013), trust games (Hargreaves Heap and Zizzo, 2009; Slonim and Guillen, 2010; Falk and Zehnder, 2013), (third-party) punishment games (Bernhard, Fehr and Fischbacher, 2006; Goette, Huffman and Meier, 2006; Abbink et al., 2010), contests (Abbink et al., 2010; Chakravarty et al., 2016) and variants of public goods games (Sell, Griffith and Wilson, 1993; Solow and Kirkwood, 2002; Eckel and Grossman, 2005; Croson, Marks and Snyder, 2008; Charness, Cobo-Reyes and Jiménez, 2014) in which members of the ingroup are typically treated preferentially compared to outgroup members. These effects are generally found to be present in minimal groups that are assigned completely randomly (Tajfel et al., 1971; Chen and Li, 2009; Sutter, 2009) or artificially enhanced (e.g., by performing a common task

such as puzzle-solving or identifying paintings) (Eckel and Grossman, 2005; Chen and Li, 2009; Hargreaves Heap and Zizzo, 2009; Rong, Houser and Dai, 2016) and as in naturally occurring groups that may be based on gender, ethnicity, religious affiliation, or membership in universities, organizations, or political parties (Fershtman and Gneezy, 2001; Bernhard, Fehr and Fischbacher, 2006; Goette, Huffman and Meier, 2006; Croson, Marks and Snyder, 2008; Charness and Rustichini, 2011; Falk and Zehnder, 2013; Ockenfels and Werner, 2014; Kranton and Sanders, 2017; Abbink and Harris, 2019). Our contribution to this literature is twofold, as our study involves naturally occurring groups, namely German citizens, Asylum seekers, and Economic migrants, in a situation akin to a welfare state. In addition, we methodologically extend the research on group effects, contributing to the growing literature that utilizes real-effort tasks in the lab (for a comparison of stated effort and real effort methods see Charness, Gneezy and Henderson, 2018), instead of the formerly dominating approach using stated costly effort (Fehr, Kirchsteiger and Riedl, 1993; Fehr et al., 1998). To the best of our knowledge, this is the first study on whether real effort in the laboratory depends on the characteristics of potential beneficiaries knowing that part of one's earnings is subject to redistribution.

The paper is structured as follows. Section 2 outlines the experimental design and the theoretical background. Section 3 presents the results. Section 4 discusses the findings and concludes.

2 Experimental design and theoretical background

The experiment took place during ten sessions in the laboratory for experimental economics at the University of Kiel. Four sessions in the same laboratory took place in September and October 2019 and two sessions in January 2020, thus ensuring participants used the same technical devices in all sessions. All participants attended only one session.

The experiment discussed in this paper is part of a research project on preferences for redistribution for which hypotheses and analysis plans were pre-registered in the OSF Registries (available at https://osf.io/xj7tf). Even if the hypotheses for this experiment were not pre-registered, we would view them as straightforward extensions of existing theories and evidence. They are ultimately in line with the project's overall hypotheses.

The sample comprises 172 students from the University of Kiel acting as workers. 86 participants identified as females, 85 as males, and one as non-binary. The mean age was 25.7 years.[5] The vast majority, 163 participants, was born in Germany, as were most of their parents (162 and 155 of their mothers and fathers, respectively). Nine participants

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[5]We excluded one participant's observation from the analysis who did not center any slider during the three rounds, although she touched 31, 30 and 30 sliders, respectively. Our results are robust to including this observation and using the number of touched sliders as dependent variable.

Table 1: Balance Table

	Asylum seeker	German	Economic migrant	Total	F-test
Female	0.564	0.410	0.534	0.500	0.207
	(0.067)	(0.063)	(0.066)	(0.038)	
Age in years	26.164	25.508	25.483	25.707	0.684
	(0.663)	(0.600)	(0.511)	(0.341)	
Dual citizenship	0.073	0.033	0.052	0.052	0.626
	(0.035)	(0.023)	(0.029)	(0.017)	
Born in Germany	0.909	0.951	0.983	0.948	0.190
	(0.039)	(0.028)	(0.017)	(0.017)	
Political orientation	2.527	2.508	2.569	2.534	0.854
	(0.068)	(0.086)	(0.074)	(0.044)	
Closeness	2.309	2.339	2.362	2.337	0.947
	(0.103)	(0.110)	(0.127)	(0.066)	

Notes: The table shows background characteristics for the participants in our experiment. Means and standard errors (in parentheses) reported. "Female" is the average share of females. "Age in years" is the average age in years. "Dual citizenship" is the share of participants holding a dual citizenship. "Born in Germany" is the share of participants born in Germany. "Political orientation" is ranging from 1 (very left) to 5 (very right). "Closeness" is a measure of closeness to Germans, ranging from 1 (very close) to 5 (very distant). The last column reports p-values from an F-test of joint significance in a regression of background characteristics on treatment indicators.

reported having dual citizenship besides their German nationality. Their political orientation, measured on an interval ranging from 1 (extremely left-wing) to 5 (extremely right-wing), has a distribution slightly skewed to the left from the center (mean = 2.5, SE = 0.04), as typical for a university student pool. Table 1 shows that the treatments were balanced concerning observable characteristics.

Task. — We used a variant of the widely used slider task, first introduced by Gill and Prowse (2012), which has recently been used in laboratory labor market experiments (Araujo et al., 2016; Chen and Schildberg-Hörisch, 2019; Gill and Prowse, 2019). After a general explanation, participants performed three rounds of the slider task. Participants were shown a screen with 50 sliders in a randomly determined initial position in each round. Each slider could be positioned between 0 and 100 (see Appendix for a screenshot). Sliders should be moved to their midpoint with the computer's mouse at 50. Participants could earn 5 Euros if they completed at least 25 out of 50 sliders, whereas earnings would be zero below this threshold, as described by 1. For each centered slider above the threshold, they could receive additional 20 Cents such that earnings m were capped at 10 Euros. Participants were told that they would be paid according to their performance in a randomly chosen round, determining their payoff. Hence, the earnings maximizing strategy was exerting the highest possible effort in each round.

$$m = \begin{cases} 0 & e < 25 \\ (e - 25) \cdot 0.2 + 5 & e \geqq 25 \end{cases} \tag{1}$$

Treatments. — Before performing the task, we informed participants (the "workers") that their final payoffs would, in addition to their performance, also depend on the choice made by a third person (the "allocator"). The allocator could redistribute earnings (shares between 0 and 100 percent in steps of 20 percent) from them to another person (the "recipient"). The experimental design is similar to the situation faced by "stakeholders" and "workers" during the first phases of the experiments by Cappelen et al. (2013) and Almås, Cappelen and Tungodden (2020). Utilizing a non-strategic interaction in which the recipient has no possibility to react to the worker's behavior allows studying preferences independently from belief-based group effects, which could originate from repeated game strategies (Everett, Faber and Crockett, 2015).

Subjects were randomly assigned to one out of three possible treatment conditions in a between-subject design. In each of the three conditions, we varied the recipient's background. The recipient was either (i) a German citizen, (ii) an asylum seeker, or (iii) an economic migrant (the exact wording was "migrant for economic reasons"). The allocator was always described as a German citizen. Thus, the allocator belonged to what may be presumed to represent the subject's "ingroup" in the current context. After each round, we elicited workers' beliefs about the tax chosen by the third-party allocator. The tax determines the share of earnings transferred to the recipient. We incentivized the elicitation of beliefs by an additional payment (worth 50 Euro cents) for correct beliefs about the tax rate.

In addition, in line with what was done for the allocator's decisions, we manipulated the efficiency of the redistribution mechanism. During the first round, the efficiency factor was always equal to one, i.e., subjects knew that the allocator could transfer earnings one-to-one from them to the recipient. The order of the efficiency factor in the second and third round, where each Euro from the worker transferred to the recipient would be either doubled (factor 2) or halved (factor 0.5), was randomized. This efficiency manipulation enables us to see how individuals weigh fairness and efficiency motives in their preferences (Cappelen et al., 2013; Durante, Putterman and van der Weele, 2014).

Theoretical background. — To guide our analysis of workers' behavior, we assume a simple utility function (equation 2) of the following type.

$$U = m(e) \cdot (1 - t^e) - c(e) + \theta_i \cdot m(e) \cdot t^e \tag{2}$$

Agents are assumed to derive utility from their expected earnings $m(e) \cdot (1 - t^e)$, wherein t^e is the expected share to be redistributed (the "tax rate"), minus their costs of providing effort $c(e)$. Furthermore, they are assumed to have social preferences weighted by $\theta_i \leqq 1$ towards

the recipient, such that exerting effort to benefit the recipient may generate additional utility (Ariely, Kamenica and Prelec, 2008). The index i identifies the three possible identities of recipients: $i = \{G, E, A\}$, where G identifies German recipients, E economic migrants, and A asylum seekers. Since preferences for ingroup favoritism appears to be widespread (Luttmer, 2001; Chen and Li, 2009; Fong and Luttmer, 2011; Romano et al., 2017), it is natural to assume that : $\theta_G > \theta_E$ and $\theta_G > \theta_A$. It is more challenging to hypothesize regarding the relative value of θ_E and θ_A. On the one hand, it is plausible that asylum seekers suffer less discrimination than economic migrants because they are needier and deserve compensation for their past traumatic experiences. On the other hand, economic immigrants may be seen more favorably than asylum seekers for their availability to work. In the lack of any solid theoretical argument going in one direction or the other, we posit the following order in equation 3.

$$\theta_G > \theta_E = \theta_A \tag{3}$$

We assume an invertible cost function $c(e)$ fulfilling the regularity conditions $c'(e) > 0$, $c''(e) > 0$ and $\lim_{e \to \infty} c(e) = \infty$.

$$e^* = \begin{cases} c'^{-1}\left[0.2 \cdot (1 - t^e + \theta \cdot t^e)\right] & e^* > 25 \\ 0 & e^* \leq 25 \end{cases} \tag{4}$$

The optimal effort level e^* depends negatively on the tax rate and positively on θ_i in case of an interior solution, considering the payoff determining mechanism. Moreover, suppose the social preferences parameter θ_i towards potential beneficiaries varies with the recipient's identity (German, asylum seeker, or economic migrant) due to group identity effects. In that case, we should observe differences in exerted effort across treatments. Hence, the above simple model leads to two main hypotheses.

- *Hypothesis 1:* Due to ingroup favoritism, we expected effort levels to be higher if the recipient was a member of participants' ingroup (German citizen) than when the recipient was an asylum seeker or an economic migrant.

- *Hypothesis 2:* We expected that the larger the beliefs about the rate to be imposed by the allocator, the lower the exerted effort.

In the following section, we analyze the experimental data.

Figure 1: Effort by Recipient for each Round
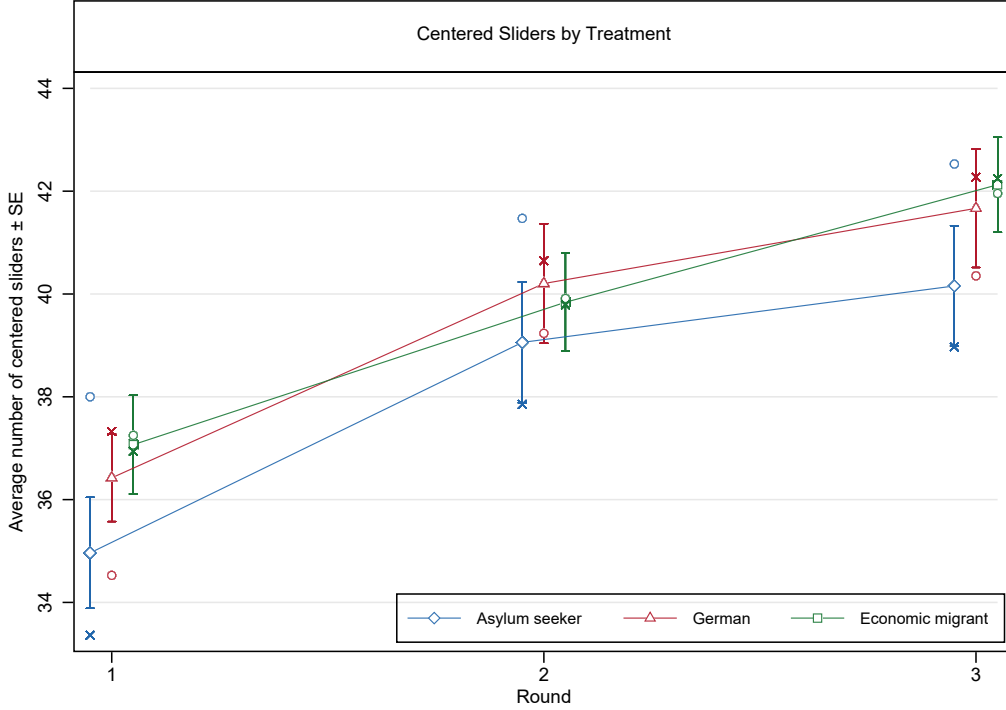
Note: This figure shows mean number of centered sliders $\pm 1*$SE by round of experiment. Crosses (circles) show the mean for the subgroup of people reporting to be close (neutral of distant) to Germans.

3 Results

3.1 Descriptive results

Table 2 provides summary statistics[6] of the number of centered sliders as well as of the beliefs about tax rates to be imposed by the third-person allocator. Figure 1 depicts means and their standard errors for the number of centered sliders by communicated type of the recipient and self-reported identification with the objective ingroup of Germans.

On average, our subjects complete 39 sliders in each 2-minutes period across the three treatments and three rounds. In all treatments, we observe significant learning effects as the number of centered sliders increases from an average of 36.2 sliders in the first round to 41.4 in the third round ($p < 0.001$, two-sided t-test). The number of observations is 172 in the first and 161 in the second and third rounds. Unfortunately, an omission in the programming prevents us from determining the order of rounds for observations from the last two sessions conducted in the laboratory. Consequently, we only used data from the first round for these two sessions. A first glimpse at the mean number of completed sliders in Table 2 (also

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[6]More detailed summary statistics are provided in the Appendix Table B.6.

Table 2: Centered Sliders and Tax Beliefs by Round and Efficiency

	By round				By efficiency factor	
Centered sliders	Round 1	Round 2	Round 3	All rounds	2x	0.5x
Asylum seeker	34.96	39.06	40.16	37.98	39.67	39.55
	(1.08)	(1.18)	(1.17)	(0.68)	(1.20)	(1.15)
Obs.	55	51	51	157	51	51
German	36.42	40.20	41.67	39.34	40.37	41.50
	(0.85)	(1.16)	(1.15)	(0.63)	(1.19)	(1.13)
Obs.	59	54	54	167	54	54
Economic migrant	36.44	39.84	42.13	39.65	40.73	41.23
	(1.13)	(0.95)	(0.93)	(0.57)	(0.93)	(0.97)
Obs.	58	56	56	170	56	56
All recipients	36.17	39.71	41.35	39.01	40.27	40.79
	(0.56)	(0.63)	(0.62)	(0.36)	(0.64)	(0.62)
Obs.	172	161	161	494	161	161
Beliefs about tax rate	Round 1	Round 2	Round 3	All rounds	2x	0.5x
Asylum seeker	35.60	35.27	33.55	34.83	28.94	39.88
	(3.83)	(4.42)	(4.47)	(2.43)	(4.12)	(4.62)
Obs.	55	51	51	157	51	51
German	32.73	33.57	35.00	33.74	29.06	39.52
	(2.90)	(3.74)	(3.68)	(1.97)	(3.51)	(3.76)
Obs.	59	54	54	167	54	54
Economic migrant	34.34	34.95	34.59	34.62	28.13	41.41
	(3.41)	(3.53)	(3.69)	(2.03)	(2.91)	(4.00)
Obs.	58	56	56	170	56	56
All recipients	34.19	34.59	34.40	34.39	28.70	40.29
	(1.94)	(2.23)	(2.26)	(1.23)	(2.02)	(2.37)
Obs.	172	161	161	494	161	161

Notes: The table shows means and standard errors (in parentheses) for centered sliders and beliefs about taxes by treatment, round and efficiency factor. The efficiency factor always equals one in the first round.

depicted in Figure 1) reveals only a slight variation between the three treatments, indicating weak effects from group identity on average. The average number of centered sliders over all rounds is 37.98 for the recipient being an Asylum seeker, 39.34 for the recipient being a German citizen, and 40.73 when the recipient is an Economic migrant.

Similarly, there is only a slight variation between treatments in the beliefs about the tax rate. Participants expected an average tax rate of 34.83 when the recipient is an Asylum seeker, 33.74 when the recipient is a German citizen, and 34.62 for Economic migrants. Instead, expected tax rates are considerably higher when the efficiency factor of the underlying redistribution mechanism is lower. Averaging over all treatment conditions, participants expected a tax rate of 28.7 percent for the doubling factor and 40.29 for the transfer-halving factor.

3.2 Regression results

To provide a quantitative assessment of the participants' behavior, we fit a random-effects Tobit model for panel data. Equation 5 describes the regression model in its base form. The Tobit model accounts for censoring in the latent dependent variable y_{it}^*. In this context, using the number of completed sliders as dependent variable, the latent variable may be interpreted as capturing the propensity to exert effort, or the desired level of effort. The effort variable is censored from below at 0 and above at 50. Beliefs about taxes are censored from below at 0 and above at 100. α is the intercept, c' is a vector of controls, and u_{it} is the error term.

$$y_{it}^* = \alpha + \beta_{GER} \cdot GER + \beta_{ECON} \cdot ECON + \sum_{t=1}^{2} \delta_t \cdot r_t + \gamma \cdot DOUBLE + c'\eta + u_{it} \quad (5)$$

The regression model allows quantifying the treatment effects, i.e., the effect of varying recipient identity (asylum seeker, German citizen, economic migrant), as well as to control for learning and individual-level variation. β_{GER} and β_{ECON} are regression coefficients for the treatment indicators, with the recipient being an Asylum seeker serving as the base category. Because the treatment variables are time-invariant, we cannot use a fixed-effects model. The regressions include indicators for the second and third round ($\sum_{t=1}^{2} \delta_t \cdot r_t$) to account for learning effects. $DOUBLE$ is an indicator variable for the transfer-doubling efficiency factor.

Without violating the rank condition, we can either include dummies for the second and the third round and one of the efficiency factors (either doubling or one half) or for both efficiency factors but only for one of the rounds. With the number of centered sliders as the outcome variable, the round dummies are highly significant and statistically different from each other according to a Wald test (p < 0.001). In contrast, coefficients for efficiency factors

in unreported regressions do not reach statistical significance. The opposite holds for beliefs about the tax rates imposed by the allocator as the dependent variable. We thus included round indicators in the effort regressions and efficiency factor indicators in the case of the beliefs regressions. Estimating a pooled OLS regression with standard errors clustered at the individual level leads qualitatively to the same results as the Tobit model showing only minor differences in standard errors. The following subsections discuss the results concerning exerted effort and elicited beliefs about tax rates based on the Tobit model for panel data.

3.2.1 Effort

The first three columns of Table 3 show regression results in which the number of centered sliders serves as the dependent variable. Regressions in columns (2) and (3) show results from a regression where we added interactions between the treatment indicators and the variable *Close*, which is a simple measure of subjective identification with the (in-)group of (other) Germans. Concretely, the variable *Close* is equal to 1 if a subject stated to feel close or very close to (other) Germans (N = 110), and it is 0 if a subject placed themselves as neutral, distant, or very distant (N = 62). Figure 2 shows the main results concerning the between- and within-group comparisons, where we contrast participants based on their reported closeness to other Germans.

Aggregate results. —Indicator variables for the second and third rounds turn out to be positive, with point estimates of about 3.9 and 5.7 relative to the first period. These indicate the presence of learning effects that are statistically highly significant ($p < 0.001$). Gender has a statistically significant effect, as females completed roughly six sliders ($p < 0.001$, Wald test) less than male participants. This result may be due to men's higher familiarity with video games. The efficiency of the redistribution mechanism shows no significant effect on effort. The Tobit regression reveals no treatment effects from the recipient's identity in the aggregate, as coefficients on the recipient's characteristics are not significantly different from zero (first column of Table 3). Participants complete roughly one slider less when Person 2 is German than when she is an economic migrant ($p = 0.446$), and 0.3 sliders more when Person 2 is German than when Person 2 is an asylum seeker ($p = 0.830$). We thus do not observe any bias based on objective affiliation to their ingroup, given that all our subjects were students of German citizenship.

Heterogeneity within groups defined by their closeness. — The picture changes if we consider the degree of participant's closeness to German identity. Participants who reported a strong identification with Germans are by 4.1 completed sliders less successful in the slider task when the recipient is an Asylum seeker than when the recipient is German. This result borders the 10% level of statistical significance ($p = 0.117$, Wald test) in the whole sample. If we remove extreme outlier observations according to the Tukey's fences method, however,

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Table 3: Main Results: Tobit Regressions

	(1) Effort	(2) Effort	(3) Effort	(4) Beliefs	(5) Beliefs
German	0.287	-4.143*	-4.152*	-0.51	17.16*
	(1.33)	(2.28)	(2.27)	(5.85)	(10.01)
Economic migrant	1.29	-1.772	-1.781	1.251	15.96*
	(1.33)	(2.14)	(2.14)	(5.83)	(9.47)
Round 2	3.921****	3.900****	3.895****		
	(0.47)	(0.47)	(0.47)		
Round 3	5.708****	5.691****	5.686****	0.009	0.001
	(0.47)	(0.47)	(0.46)	(2.22)	(2.22)
Female	-6.002****	-5.968****	-5.965****	-5.095	-5.225
	(1.11)	(1.09)	(1.09)	(4.83)	(4.76)
2x Efficiency	-0.452	-0.45	-0.45	-5.519**	-5.514**
	(0.42)	(0.42)	(0.42)	(2.49)	(2.49)
0.5x Efficiency				7.328***	7.330***
				(2.47)	(2.47)
Belief about tax	-0.003	-0.001			
	(0.01)	(0.01)			
Close		-4.065**	-4.076**		18.90**
		(1.98)	(1.97)		(8.76)
German × Close		6.663**	6.678**		-26.49**
		(2.78)	(2.78)		(12.19)
Economic migrant × Close		4.736*	4.750*		-22.57*
		(2.71)	(2.71)		(11.91)

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Table 3: Main Results: Tobit Regressions

	(1) Effort	(2) Effort	(3) Effort	(4) Beliefs	(5) Beliefs
Constant	42.60****	45.55****	45.52****	35.29***	22.03*
	(2.74)	(2.99)	(2.97)	(11.82)	(13.03)
Obs.	494	494	494	494	494
Right-censored	46	46	46	20	20
Left-censored	0	0	0	75	75
No. of panels	172	172	172	172	172
Log-likelihood	-1438.8	-1435.8	-1435.8	-1955.6	-1952.8

Hypothesis tests (p-values)

	(1) Effort	(2) Effort	(3) Effort	(4) Beliefs	(5) Beliefs
Round 2 = Round 3	p = 0.000	p = 0.000	p = 0.000		
2x Eff. = 0.5x Eff.				p = 0.000	p = 0.000
German × Close = 0		p = 0.017	p = 0.016		p = 0.029
Economic migrant × Close = 0		p = 0.081	p = 0.079		p = 0.058
German = 0	p = 0.830	p = 0.069	p = 0.068	p = 0.930	p = 0.086
Economic migrant = 0	p = 0.332	p = 0.408	p = 0.406	p = 0.830	p = 0.092
German = Economic migrant	p = 0.446	p = 0.271	p = 0.271	p = 0.759	p = 0.898
Close = 0		p = 0.040	p = 0.039		p = 0.031
Close + German × Close = 0		p = 0.186	p = 0.185		p = 0.374
Close + Economic migrant × Close = 0		p = 0.718	p = 0.717		p = 0.650
German + German × Close = 0		p = 0.117	p = 0.115		p = 0.183
Economic migrant + Economic migrant × Close = 0		p = 0.074	p = 0.073		p = 0.360
Economic migrant (1 + Close) = German (1 + Close)		p = 0.785	p = 0.786		p = 0.702

Table 3: Main Results: Tobit Regressions

	(1)	(2)	(3)	(4)	(5)
	Effort	Effort	Effort	Beliefs	Beliefs

Notes: The table shows panel data regression results from a Tobit random-effects model accounting for left-censoring at 0 and right-censoring at 50 for the first three columns, and at 100 for the fourth and the fifth column. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects with age larger or equal 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task.

the difference reaches marginal statistical significance (p = 0.099) (see Appendix Table B.1). Participants with higher closeness to German identity complete also roughly 3 sliders more when the recipient is an Economic migrant than when the recipient is an Asylum seeker (p = 0.074). There are no significant differences in effort when the recipient is a German or an Economic migrant in the group having a close identification with Germans (insignificant difference = 0.4 completed sliders more when the recipient is an Economic migrant instead of a German, p = 0.786).

Conversely, participants without strong identification with Germans exert *lower* effort if the recipient is German compared to when she is an Asylum seeker (difference = -4.1 sliders, p = 0.069). In addition, there is a tendency in this group to exert lower effort in the treatment where the recipient is an Economic migrant relative to when the recipient is an Asylum seeker, albeit not reaching statistical significance at conventional levels (difference = -1.8 sliders, p = 0.406). The difference between the recipient being an Economic migrant or a German does not reach statistical significance in the participants with a low identification (difference = 2.4 sliders, p = 0.271).

Heterogeneity between groups defined by their closeness. — Comparing the effort between groups defined by the strength of their identification with Germans, we can report the following results. Participants reporting a strong identification with other Germans exert significantly[7] less effort when the recipient is an Asylum seeker than participants without a strong identification with Germans (difference = -4.1 sliders, p = 0.040). In addition, on average, effort is higher in the group of participants who identify with their ingroup when the recipient is a German citizen, as compared to those who reported no identification, albeit not statistical significance at the 10 percent level (difference = 2.6 sliders, p = 0.186, Wald test). There are no significant differences between groups defined by their reported closeness when the recipient is an Economic migrant (difference = 0.7 sliders more among the *Close* participants, p = 0.718).

Finally, we can compare the differences in differences when the recipient is an Asylum seeker versus when the recipient is either a German or an Economic migrant between the groups defined by their identification with (other) Germans. Participants with a strong identification with Germans exert significantly more effort if the recipient is another German than when the recipient is an Asylum seeker relative to the same difference among participants without a strong identification with Germans (difference = 6.7 sliders, p = 0.016). When the recipient is an Asylum seeker or an Economic migrant, the difference in difference only reaches statistical significance at the 10 percent level (difference = 4.7 sliders, p = 0.081). Hence, consistent with previous results, the treatment effect of the recipient being a German or an Economic migrant instead of an Asylum seeker is positive among those with a solid self-

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[7]The p-value from a one-sided t-test accounting for unequal variances using the mean number of sliders over the three periods between those with and without a strong ingroup identification is p = 0.029.

reported identification with Germans relative to those without a strong level of identification.

Views on outgroups. — We cannot replicate the heterogeneity in treatment effects we find concerning participants' self-reported identification with Germans using PCA indices based on questions on views about immigrants in general, Asylum seekers, and Economic migrants (see Table B.3 in the Appendix). We find no statistically significant heterogeneity at all interacting treatment indicators with dummy variables equal to one if views on these groups as captured by the PCA indices are less favorable than the median value in the sample. This finding supports the interpretation that a survey question concerning closeness to a respondent's ingroup is less affected by social-desirability biases. Instead, respondents may be more reluctant to report negative attitudes or prejudices in questions about outgroups.

Taste-based discrimination and gender. — In our framework, there are two competing explanations for ingroup biases, which can arise either due to the presence of taste-based discrimination or due to expectations about the share of earnings that the third-party allocator would transfer to the recipient. Regression results from column (3) of 3 show that beliefs about the share to be redistributed do not show statistically significant effects on exerted effort. Furthermore, when we contrast regression results from column (2) and column (3), we observe that controlling for beliefs about the share to be transferred does not affect treatment effects.

Overall, these results favor an explanation of effort differences among those reporting a strong ingroup identification based on taste-based discrimination instead of being caused by an expectation to be taxed more strongly in case the potential recipient is from the Asylum seeker outgroup. In contrast to part of the previous literature (Fershtman and Gneezy, 2001; Solow and Kirkwood, 2002; Croson, Marks and Snyder, 2008), we do not find a differential strength of group loyalty effects along the gender dimension. There is no significant difference between females and males in the reaction to the treatments while female participants generally performed slightly worse on the slider task, as can be seen in Appendix Table B.1.

3.2.2 Beliefs about tax rates

In columns (4) and (5) in Table 3, we show regression results with the expected share of participants' earnings that the allocator would redistribute as the dependent variable. Akin to the analysis of exerted effort, we interact treatment indicators with the variable Close, i.e., the dummy for self-reported identification with the ingroup (other Germans) in the fourth column of 3. As noted above, the regressions with beliefs as dependent variables do not contain an indicator for the second round, allowing us to control the transfer-halving efficiency factor. Apart from this adjustment, we use the equivalent right-hand-side variables as in the third column.

Figure 2: Main Results: Between-group analysis and Within-group analysis

Effect ± SE based on Tobit Coefficient
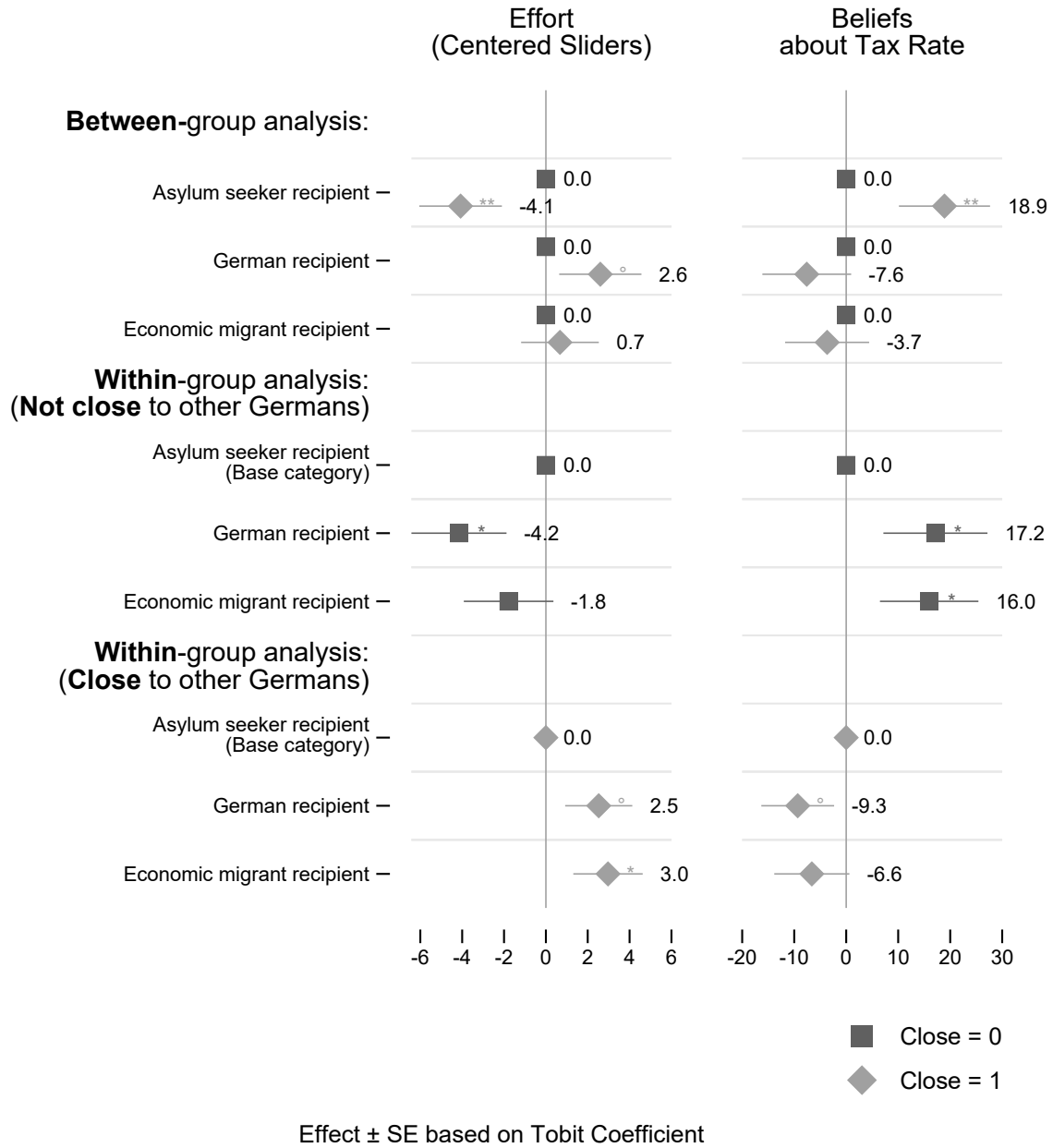
Note: This figure shows the main results based on the Tobit regressions. The dependent variable in the first (second) column is the number of centered sliders (beliefs about tax rate). In the first three rows, the results of the between-group comparison, i.e., between those who report feeling close and those who do not, are depicted. Those who do not feel close to other Germans serve as the base category. The remaining six rows show the differences within the groups based on their reported closeness to other Germans, whereby the recipient being an Asylum seeker is the base category. (**, *, °) indicate two-sided p-values below 0.05, 0.1, and 0.2, respectively.

Aggregate results. — In contrast to findings concerning exerted effort, the efficiency factor plays a vital role in the tax rate beliefs. Participants, on average, expect a 5.5 percentage point smaller tax rate when the redistributed share of earnings would be doubled (p = 0.027). On the contrary, for an efficiency factor equal to 0.5, participants' tax beliefs are, on average, 7.3 percentage points higher (p = 0.003). Both effects are not only statistically significantly different relative to the base category of a one-to-one transfer but also significantly different from each other (p < 0.001, Wald test). These findings are consistent with the idea that participants expected the allocator to be willing to transfer a minimum amount of money to the recipient. As a result, participants expected allocators to completely disregard efficiency concerns and transfer more when it was less efficient. Hence, the participants' average belief contrasts the prediction of economic theory and recent experimental findings (Krawczyk, 2010; Almås, Cappelen and Tungodden, 2020) that allocators may choose to redistribute less if redistribution involves a cost due to efficiency losses. In another experiment related to this project, Grimalda et al. (2022) analyze allocators' choices about the share to be redistributed from the workers to different types of recipients, involving 1807 participants from a quasi-representative sample of the German population. Remarkably, Grimalda et al. (2022) find that workers' expectations in the present experiment were correct, as allocators transferred 16.6% more when the efficiency factor was 0.5 instead of one. Furthermore, the allocators transferred 5.6% more when the efficiency factor was two than when it was 0.5 - something that workers failed to anticipate, albeit the difference in expectations between these two cases is not significant. This pattern of preferences, which disregards efficiency concerns but seemingly aims to guarantee a minimum earning level to the recipient, is compatible with a Rawlsian or a "Boulding" social welfare function (Traub et al., 2005).

Somewhat mirroring our results from the analysis of effort, on average, we do not find any significant treatment effects from the recipient's characterization as either an Asylum seeker, a German citizen, or an Economic migrant on the stated beliefs (see Table 3, column 3).

Heterogeneity within groups defined by their closeness. — However, as in the case of effort, this aggregate result masks a significant heterogeneity of treatment effects concerning participants' identification with (other) Germans. Participants who reported to be close to Germans expected relatively lower tax rates when the recipient is either a German (difference = -9.3 percentage points, p = 0.183) or an Economic migrant (difference = -6.6 percentage points, p = 0.360) in comparison with the base category of an Asylum seeker as the recipient, thereby not reaching statistical significance. There are no significant differences in tax beliefs between the treatments when the recipient is a German or an Economic migrant in this group (difference = -2.7 percentage points lower expected tax rates when the recipient is a German, p = 0.702).

On the other hand, those participants who do not report to identify with Germans, expected the allocator to impose marginally significantly higher tax rates when the recipient

was described either as a German (difference = 17.2 percentage points, p = 0.086) or as an Economic migrant (difference = 16 percentage points, p = 0.092), relative to the treatment when the recipient was an Asylum seeker.

Beliefs vs. allocators' actual choices. — We compare the allocators' actual choices concerning recipients' identities from Grimalda et al. (2022) with workers' beliefs in the present experiment. Allocators, on average, redistribute the most to German recipients (45.6 percent) followed by recipients that are Asylum seekers (41.2 percent) and Economic migrants (37.8 percent) (Grimalda et al., 2022). Hence, the beliefs of participants who reported feeling close to (other) Germans and those who did not report feeling close were incorrect concerning ordering the share to be redistributed to the three types of recipients. On the one hand, those with a strong German identity expected asylum seekers to benefit most from redistribution and German recipients to benefit the least. In contrast, the actual choices by allocators reveal that German recipients benefitted the most, and economic migrants benefited even less than asylum seekers (Grimalda et al., 2022). On the other hand, those not having a strong German identity correctly anticipated that Germans would have benefitted the most from redistribution. However, they expected asylum seekers to benefit even less than economic migrants. In contrast, it was the other way around concerning actual allocators' redistribution choices towards asylum seekers and economic migrants (Grimalda et al., 2022). It is also remarkable that those not reporting strong German identity expected asylum seeker recipients to be penalized four times more (relative to the redistribution towards German recipients) than was, in fact, the case. In general, allocators discriminated across recipients' groups at a lower rate than workers expected.

Heterogeneity between groups defined by their closeness. — As in the case of effort, only one difference between those who reported a strong identification with their ingroup compared to those who did not report strong identification reaches statistical significance. Participants who reported feeling close to their ingroup of (other) Germans expect a larger share of their earnings to be redistributed when the recipient is an Asylum seeker compared to those who did not report a strong identification (difference = 18.9 percentage points, p = 0.031). There are no significant differences between both groups' beliefs when the recipient is either a German (difference = -7.6 percentage points lower expectations among *Close* participants, p = 0.374) or an Economic migrant (difference = -3.7 percentage points lower expectations among *Close* participants, p = 0.650).

We also looked at the differences in differences in beliefs between groups defined by their identification with Germans. The result for expected tax rates is similar to what we found with the number of centered sliders as the dependent variable. Namely, participants with a strong identification with Germans relative to those without solid identification with Germans expect significantly lower tax rates when the recipient is a German compared to when the recipient is an Asylum seeker (difference = -26.5 percentage points, p = 0.029). The same

applies to the treatments where the recipient is an Economic migrant instead of an Asylum seeker but only reaches marginal statistical significance (difference = -22.6 percentage points, p = 0.058).

4   Conclusion

We report results from an experiment in which a student sample with exclusively German citizenship exert real effort in a variant of the slider task (Gill and Prowse, 2012, 2019) to study ingroup favoritism in a setting that resembles a simplified version of a welfare state. We informed participants that part of their earnings might be redistributed to a recipient, whereby the choice of the transfer-determining tax rate lies in the hands of a third-person allocator. In three treatments, administered in a between-subject design, the recipient is either (i) a German citizen, (ii) an Asylum seeker, or (iii) an Economic migrant.

The extant literature has found that ethnic heterogeneity may affect the welfare state in several dimensions, such as income redistribution and public goods provision. This paper aimed to examine whether ethnic heterogeneity may also affect workers' propensity to exert effort, knowing that earning redistribution may affect either fellow country people or immigrants, distinguishing between economic migrants and asylum seekers. We found that, even if we cannot detect an effect of the recipient's identity in the aggregate, this hides an essential difference between people who closely identify with other Germans and people who do not. The former group tends to exert less effort when the recipient is an asylum seeker than the latter group. Workers closely identifying with other Germans also tend to put more effort when the redistribution recipient is German, or an economic migrant than when the recipient is an asylum seeker, while workers not identifying with other Germans tend to do the opposite.

Our analysis shows that lower altruism toward asylum seekers rather than statistical discrimination leads to the observed discrimination. In our context, statistical discrimination would operate through the belief that the allocator will benefit asylum seekers more than others. While it is indeed the case that *Close* participants expect, on average, higher redistribution toward asylum seekers, we show that this belief does not significantly affect their effort (see Table 3 and Appendix Table B.5). While, in principle, there could be room for statistical discrimination to operate, its effect is negligible, according to our findings. Instead, the reduced effort by *Close* participants is almost entirely driven by reduced altruism or taste-based discrimination (Becker, 1974).

Of course, one should be cautious when extrapolating our findings to the real world. In particular, the stakes involved (10 Euros at maximum) were small compared to usually taxed real-world incomes. Moreover, the situation in the laboratory and the slider task are artificial. In particular, the recipient in the experiment could not undertake work and

contribute to the welfare state. This design choice was made to identify the possible effect of ethnic heterogeneity on individual effort in the neatest possible way. In reality, immigrants contribute to the welfare state. Therefore, the effect of ethnic heterogeneity we observed in the experiment may arguably be interpreted as the upper bound of what is the case in real life. Nonetheless, it is well-known that people tend to grossly underestimate the immigrants' contribution to the tax revenues and the economy. The effect in real life may thus be not so distant from the effect detected in the experiment, especially for people with a strong ingroup identity. Overall, we believe that showing that a fraction of people with a strong ingroup identification tend to sacrifice potential earnings if members of an outgroup could be beneficiaries is relevant for many societies facing increased heterogeneity due to immigration. However, further research is needed to explore how these findings may translate into the field and non-student populations.

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A Appendix

Table A.4: Detailed Sample Characteristics

	Mean	SD	Median	IQR	Min	Max	Obs
Female	0.50	0.50	0.5	1	0	1	172
Age in years	25.71	4.53	25	4.5	18	47	172
Dual citizenship	0.05	0.22	0	0	0	1	172
Participant born in Germany	0.95	0.22	1	0	0	1	172
Mother born in Germany	0.94	0.23	1	0	0	1	172
Father born in Germany	0.90	0.30	1	0	0	1	172
Political left to right	2.53	0.59	3	1	1	4	172
Closeness	2.34	0.86	2	1	1	5	172

Notes: Table displays summary statistics of sample characteristics. "Female" is the average share of females. "Age in years" is the average age in years. "Dual citizenship" is the share of participants holding a dual citizenship. "Participant born in Germany" is the share of participants born in Germany, analogously for "Mother/Father born in Germany" variables. "Political left to right" is ranging from 1 (very left) to 5 (very right). "Closeness" is a measure of closeness to Germans, ranging from 1 (very close) to 5 (very distant).

Figure A.3: Screenshot: Slider task
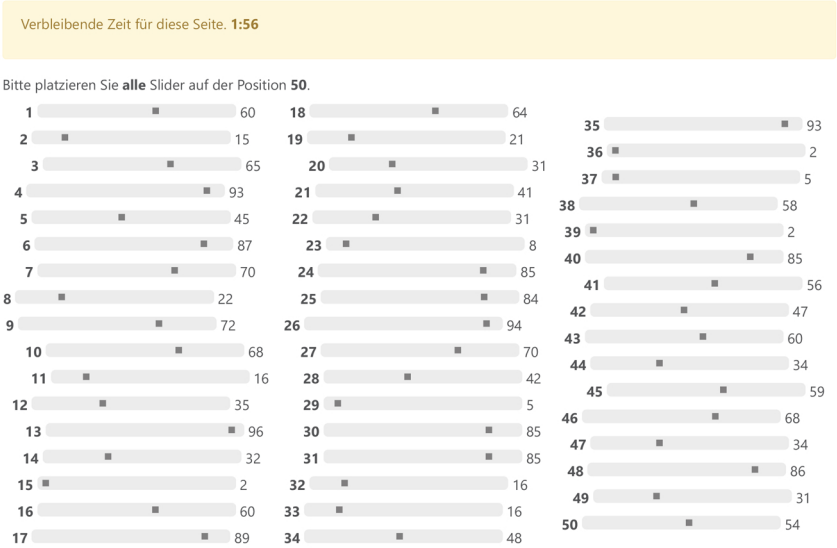
Note: This figure shows an exemplary screenshot from the variant of the slider task we used in the computer laboratory.

Figure A.4: Boxplot: Effort by Treatment and Closeness
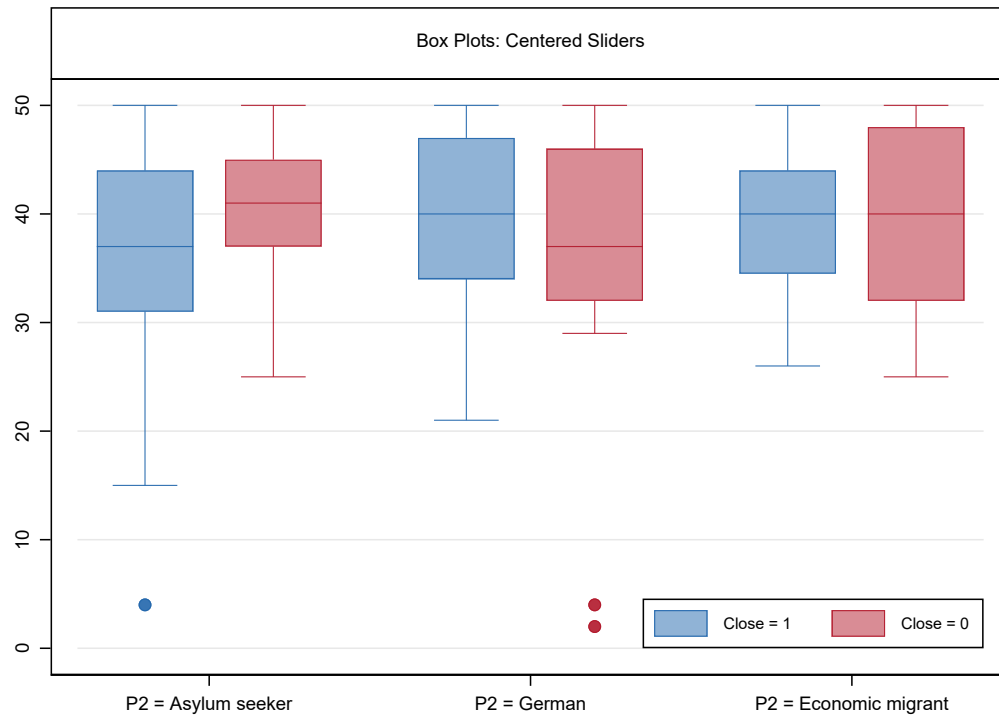
Note: This figure shows Tukey's boxplots for the effort measure by treatment and self-reported ingroup identification.

B Online Appendix

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B.1 Further Analyses

The regressions in this Section provide several additional insights that serve as robustness checks for our main results. In regressions in the first column of Table B.1, we excluded extreme values in the number of centered sliders by the method of Tukey's fences (more than 1.5 times the IQR below or above the 25th and 75th percentile over all treatment conditions, respectively). We interacted the female dummy with treatments in the second column to explore heterogeneity with respect to gender. The results are unaffected by excluding outliers. There is no significant treatment heterogeneity concerning gender. Table B.2 shows the results from the same regression models as in the main text (see Table 3) using OLS instead of Tobit random-effects. The OLS regressions deliver qualitatively equivalent results.

Table B.3 shows results from regressions where we replaced the closeness indicator to explore treatment heterogeneity with indicator variables related to the respondent's views on immigrants, Asylum seekers, and Economic migrants. The indicator variables are equal to one if the PCA-index based on the questions in B.4 concerning views on immigrants, Asylum seekers, and Economic migrants indicates views that are worse (less positive views on the groups) than the median views in the sample. We tested the index reliability using Cronbach's alpha. The three indices concerning views on immigrants in general, Asylum seekers, and Economic migrants have an alpha of 0.745 (five items, average interitem covariance = 0.326), 0.757 (six items, average interitem covariance = 0.249), and 0.548 (four items, average interitem covariance = 0.232), respectively. Unlike utilizing the closeness indicator, the results show no significant treatment heterogeneity. This finding supports the view that such questions are more likely to be prone to social desirability biases than questions focusing on closeness to specific groups, which do not imply animosity towards outgroups.

Table B.1: Robustness Checks: Outliers and Female

	(1)	(2)
	Effort	Effort
German	-3.052	0.626
	(2.14)	(1.88)
Economic migrant	-1.821	2.104
	(2.01)	(2.00)
Round 2	3.997****	3.923****
	(0.43)	(0.47)
Round 3	5.780****	5.711****
	(0.42)	(0.47)
Female	-6.078****	-5.322***
	(1.03)	(1.93)
2x Efficiency	-0.394	-0.49
	(0.40)	(0.44)
Belief about tax	0.005	-0.003
	(0.01)	(0.01)
German x Female		-0.558
		(2.69)
Economic migrant x Female		-1.476
		(2.71)
Close	-4.023**	
	(1.86)	
German x Close	5.542**	
	(2.62)	
Economic Migrant x Close	4.732*	
	(2.55)	
Constant	45.62****	42.12****
	(2.81)	(2.95)
Obs.	491	494
Right-censored	46	46
Left-censored	0	0
No. of panels	172	172
Log-likelihood	-1388.2	-1438.7
Round 2 = Round 3	p < 0.001	p < 0.001
2x Eff. = 0.5x Eff.		
German = Economic migrant	p = 0.543	p = 0.419

German $= 0$	$p = 0.154$	$p = 0.739$
Economic migrant $= 0$	$p = 0.366$	$p = 0.293$
Close $= 0$	$p = 0.030$	
Close $+$ German $\times$ Close $= 0$	$p = 0.412$	
Close $+$ Economic migrant $\times$ Close $= 0$	$p = 0.684$	
German $+$ German $\times$ Close $= 0$	$p = 0.099$	
Economic migrant $+$ Economic Migrant $\times$ Close $= 0$	$p = 0.062$	
Economic migrant $(1 + $ Close$) =$ German $(1 + $ Close$)$	$p = 0.783$	

Notes: Table shows panel data regression results from a Tobit random-effects model accounting for left-censoring at 0 and right-censoring at 50. Dependent variable is the number of centered sliders. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects older than 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task. Regression in the first column excludes extreme outliers for which the number of centered sliders lies outside the Tukey's fence defined by 1.5 times the IQR below or above the 25th and 75th percentile. Below "Log-likelihood" we report p-values from Wald tests.

Table B.2: OLS Regressions

	(1)	(2)	(3)	(4)	(5)
	Effort	Effort	Effort	Beliefs	Beliefs
German	0.444	-3.528	-3.606	-1.876	12.715*
	(1.32)	(2.51)	(2.56)	(4.51)	(6.92)
Economic migrant	1.266	-1.663	-1.735	0.078	11.839
	(1.32)	(2.08)	(2.08)	(4.74)	(7.50)
Round 2	3.694****	3.644****	3.602****		
	(0.43)	(0.43)	(0.43)		
Round 3	5.321****	5.273****	5.232****	-0.265	-0.265
	(0.47)	(0.47)	(0.47)	(2.26)	(2.26)
Female	-5.356****	-5.280****	-5.253****	-4.387	-4.448
	(1.03)	(1.03)	(1.02)	(3.72)	(3.65)
2x Efficiency	-0.661*	-0.576	-0.505	-4.719**	-4.719**
	(0.38)	(0.37)	(0.32)	(1.89)	(1.89)
0.5x Efficiency				6.878***	6.878***
				(2.16)	(2.17)
Belief about tax	-0.013	-0.006			
	(0.02)	(0.02)			
Close		-3.994**	-4.080**		14.068*
		(1.94)	(1.98)		(7.26)
German × Close		5.948**	6.080**		-21.728**
		(2.88)	(2.97)		(8.91)
Economic Migrant × Close		4.469*	4.579*		-18.196*
		(2.57)	(2.60)		(9.60)

Constant	41.865****	44.527****	44.322****	44.262****	33.675***
	(2.17)	(2.50)	(2.49)	(9.47)	(10.60)
Obs.	494	494	494	494	494
No. Clusters	172	172	172	172	172
R2	0.210	0.232	0.231	0.051	0.077
Adj. R2	0.192	0.209	0.211	0.031	0.052
Hypothesis tests (p-values)					
Round 2 = Round 3	$p < 0.001$	$p < 0.001$	$p < 0.001$		
2x Eff. = 0.5x Eff.				$p < 0.001$	$p < 0.001$
German = Economic migrant	$p = 0.473$	$p = 0.449$	$p = 0.448$	$p = 0.626$	$p = 0.893$
German = 0	$p = 0.737$	$p = 0.162$	$p = 0.161$	$p = 0.678$	$p = 0.068$
Economic migrant = 0	$p = 0.339$	$p = 0.426$	$p = 0.406$	$p = 0.987$	$p = 0.116$
Close = 0		$p = 0.041$	$p = 0.041$		$p = 0.054$
Close + German × Close = 0		$p = 0.369$	$p = 0.363$		$p = 0.143$
Close + Economic migrant × Close = 0		$p = 0.778$	$p = 0.767$		$p = 0.508$
German + German × Close = 0		$p = 0.106$	$p = 0.102$		$p = 0.110$
Economic migrant + Economic Migrant × Close = 0		$p = 0.078$	$p = 0.077$		$p = 0.282$
Economic migrant (1 + Close) = German (1 + Close)		$p = 0.752$	$p = 0.762$		$p = 0.590$
German × Close = 0		$p = 0.040$	$p = 0.042$		$p = 0.016$
Economic migrant × Close = 0		$p = 0.083$	$p = 0.080$		$p = 0.060$

Notes: Table shows OLS regression results. Standard errors (clustered at the individual level) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects older than 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task. Dependent variables are the number of centered sliders (first three columns) and beliefs about the tax rate (last two columns).

Table B.3: Treatment Heterogeneity w.r.t. Views

	(1)	(2)	(3)
	Effort	Effort	Effort
German P2	-0.075	-0.961	-0.664
	(1.93)	(1.90)	(1.91)
Economic migrant P2	2.323	2.123	0.346
	(1.82)	(1.94)	(1.80)
Round 2	3.927****	3.929****	3.915****
	(0.47)	(0.47)	(0.47)
Round 3	5.714****	5.718****	5.704****
	(0.47)	(0.47)	(0.47)
Female	-6.031****	-5.950****	-5.918****
	(1.11)	(1.10)	(1.11)
2x Efficiency	-0.493	-0.497	-0.476
	(0.44)	(0.44)	-0.437
Belief about tax	-0.004	-0.004	-0.002
	(0.01)	(0.01)	(0.01)
German P2 x H	0.55	2.369	1.882
	(2.69)	(2.68)	(2.70)
Economic migrant P2 x H	-2.304	-1.955	2.048
	(2.71)	(2.68)	(2.73)
H	0.718	-1.595	-0.807
	(1.91)	(1.90)	(2.00)
Constant	42.419****	44.243****	42.607****
	(3.02)	-3.017	-2.971
Obs.	494	494	494
Right-censored	46	46	46
Left-censored	0	0	0
No. of panels	172	172	172
Log-likelihood	-1400	-1400	-1400

Notes: Table shows panel data regression results from a Tobit random-effects model accounting for left-censoring at 0 and right-censoring at 50. Dependent variable is the number of centered sliders. H is a dummy indicating a value of worse than the median concerning the PCA-index of attitudes towards Asylum seekers (column 1), Economic migrants (column 2), and migrants in general (column 3). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects older than 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task.

Table B.4: Touched Sliders as Effort Measure

	(1)	(2)	(3)
	Effort	Effort	Effort
German P2	0.042	-3.627*	-3.574*
	(1.23)	(2.10)	(2.10)
Economic migrant P2	0.532	-2.41	-2.356
	(1.22)	(1.98)	(1.98)
Round 2	4.133****	4.114****	4.148****
	(0.47)	(0.46)	(0.46)
Round 3	5.557****	5.539****	5.571****
	(0.46)	(0.46)	(0.46)
Female	-5.956****	-5.917****	-5.938****
	(1.02)	(1.01)	(1.01)
2x Efficiency	-0.533	-0.507	-0.564
	(0.43)	(0.43)	(0.41)
Belief about tax	0.003	0.005	
	(0.01)	(0.01)	
Close		-3.740**	-3.673**
		(1.83)	(1.82)
German P2 x Close		5.534**	5.438**
		(2.57)	(2.56)
Economic Migrant P2 x Close		4.567*	4.483*
		(2.50)	(2.50)
Constant	44.605****	47.224****	47.397****
	(2.53)	(2.76)	(2.74)
Obs.	494	494	494
Right-censored	57	57	57
Left-censored	0	0	0
No. of panels	172	172	172
Log-likelihood	-1400	-1400	-1400
Round 2 = Round 3	$p < 0.001$	$p < 0.001$	$p < 0.001$
German $\times$ Close = 0		$p = 0.031$	$p = 0.034$
Economic migrant $\times$ Close = 0		$p = 0.068$	$p = 0.073$
German = Economic migrant	$p = 0.686$	$p = 0.539$	$p = 0.540$
German = 0	$p = 0.973$	$p = 0.084$	$p = 0.088$
Economic migrant = 0	$p = 0.664$	$p = 0.223$	$p = 0.233$
Close = 0		$p = 0.041$	$p = 0.044$

Close + German × Close = 0	p = 0.322	p = 0.330
Close + Economic migrant × Close = 0	p = 0.629	p = 0.636
German + German × Close = 0	p = 0.198	p = 0.208
Economic migrant + Economic Migrant × Close = 0	p = 0.159	p = 0.164
Economic migrant (1 + Close) = German (1 + Close)	p = 0.867	p = 0.862

Notes: Table shows panel data regression results from a Tobit random-effects model accounting for left-censoring at 0 and right-censoring at 50. Dependent variable is the number of touched sliders. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects older than 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task. Below "Log-likelihood" we report p-values from Wald tests.

Table B.5: Mediation of Effort Differences by Beliefs

	Model M1 (No Beliefs)	Model M2 (With Beliefs)	Difference (M1 - M2)	Mediation (Percentage)
Close to other Germans				
German = Asylum seeker	2.53	2.52	0.01	0.24%
German = Economic migrant	-0.44	-0.44	0.00	-0.23%
Economic migrant = Asylum seeker	2.97	2.96	0.01	0.17%
Not close to other Germans				
German = Asylum seeker	-4.15	-4.14	-0.01	0.22%
German = Economic migrant	-2.37	-2.37	0.00	0.00%
Economic migrant = Asylum seeker	-1.78	-1.77	-0.01	0.51%
Close = 1 vs. Close = 0				
German	2.60	2.60	0.00	0.15%
Asylum seeker	-4.08	-4.07	-0.01	0.27%
Economic migrant	0.67	0.67	0.00	0.45%

Notes: The table shows the differences in effort dependent on the recipient's type for the model without (columns 1) and with beliefs about the tax rate (column 2) included in the regression model. Differences are always calculated as the effort in the first-mentioned condition minus the second condition. Coefficients are based on the main results of columns 2 and 3 in Table 3. The mediation in percentage terms is calculated as the difference between the estimates based on the models with and without beliefs, relative to the model without beliefs.

B.2 Detailed summary statistics: Effort and beliefs

Table B.6: Detailed Summary Statistics: Effort and Beliefs

	Mean	SE	Median	IQR	Min	Max	Obs.
Efforts							
Round 1							
P2 Asylum seeker	34.96	1.08	35	10	4	50	55
P2 German	36.42	0.85	35	9	21	50	59
P2 Economic migrant	37.07	0.96	36.5	11	25	50	58
All P2	36.17	0.56	35	10	4	50	172
Round 2							
P2 Asylum seeker	39.06	1.17	41	13	15	50	51
P2 German	40.20	1.16	40.5	12	2	50	54
P2 Economic migrant	39.84	0.95	40	12	25	50	56
All P2	39.71	0.63	40	12	2	50	161
Round 3							
P2 Asylum seeker	40.16	1.17	42	12	15	50	51
P2 German	41.67	1.15	44.5	11	4	50	54
P2 Economic migrant	42.13	0.93	43.5	12	26	50	56
All P2	41.35	0.62	44	12	4	50	161
All Rounds							
P2 Asylum seeker	37.98	0.68	39	13	4	50	157
P2 German	39.34	0.63	39	13	2	50	167
P2 Economic migrant	39.65	0.57	40	13	25	50	170
All P2	39.01	0.36	39	13	2	50	494
Efficiency 2x							
P2 Asylum seeker	39.67	1.20	41	13	15	50	51
P2 German	40.37	1.19	41	13	4	50	54
P2 Economic migrant	40.73	0.93	41	12	25	50	56
All P2	40.27	0.64	41	12	4	50	161
Efficiency 0.5x							
P2 Asylum seeker	39.55	1.15	41	13	15	50	51
P2 German	41.50	1.13	44	12	2	50	54
P2 Economic migrant	41.23	0.97	41.5	13.5	26	50	56
All P2	40.79	0.62	42	13	2	50	161
Beliefs							

Table B.6: Detailed Summary Statistics: Effort and Beliefs

	Mean	SE	Median	IQR	Min	Max	Obs.
Round 1							
P2 Asylum seeker	35.60	3.83	40	58	0	100	55
P2 German	32.73	2.90	40	20	0	100	59
P2 Economic migrant	34.34	3.41	40	40	0	100	58
All P2	34.19	1.94	40	36	0	100	172
Round 2							
P2 Asylum seeker	35.27	4.42	40	58	0	100	51
P2 German	33.57	3.74	30	57	0	100	54
P2 Economic migrant	34.95	3.53	40	36	0	100	56
All P2	34.59	2.23	40	57	0	100	161
Round 3							
P2 Asylum seeker	33.55	4.47	20	58	0	100	51
P2 German	35.00	3.68	40	40	0	100	54
P2 Economic migrant	34.59	3.69	30	47.5	0	100	56
All P2	34.40	2.26	40	58	0	100	161
All Rounds							
P2 Asylum seeker	34.83	2.43	40	58	0	100	157
P2 German	33.74	1.97	40	20	0	100	167
P2 Economic migrant	34.62	2.03	40	55	0	100	170
All P2	34.39	1.23	40	57	0	100	494
Efficiency 2x							
P2 Asylum seeker	28.94	4.12	20	38	0	100	51
P2 German	29.06	3.51	20	38	0	100	54
P2 Economic migrant	28.13	2.91	20	27.5	0	100	56
All P2	28.70	2.02	20	38	0	100	161
Efficiency 0.5x							
P2 Asylum seeker	39.88	4.62	40	58	0	100	51
P2 German	39.52	3.76	40	40	0	100	54
P2 Economic migrant	41.41	4.00	40	55.5	0	100	56
All P2	40.29	2.37	40	57	0	100	161

Notes: Table shows detailed summary statistics for effort (no. of centered sliders) and beliefs about tax rates to be chosen by the third person. Efficiency factor always equal to 1 in first round.

B.3 Instructions in the experiment

We outline the structure of the experimental platform on which the participants completed the task, stated their beliefs about the share to be redistributed by the third-person allocator, and filled out a questionnaire. The whole experiment was conducted at the computer laboratory, where participants received the following (translated) instructions on their computers. The text in square brackets varies across the different treatments. Text written in italics was not shown to participants. The original instructions in the German language are available upon request.

First Part: Introduction   You are taking part in a study on economic decisions and are then asked to make several decisions. Please read the following instructions carefully. In this study, you have the opportunity to earn money, which you will be paid out individually and receive in cash at the end of the study. During the study, you are not allowed to talk to the other participants. If you have a question, we ask you to raise your hand, after which an experimenter will come to you and help you.

The study consists of two parts. In the first part of the study, you will be asked to complete three tasks. In these tasks, you have the opportunity to earn money. The amount of your earnings depends on someone else's decision. The second part of the study consists of a questionnaire. Please read the following explanations carefully.

Second Part: Explanations   In this first part of the study, two other people are involved in addition to you. We will call them Person 2 and Person 3. Person 2 and Person 3 are real people that exist in reality. Therefore, any information you receive about either person is truthful. Both individuals are not participating in this study but have already participated in another study.

We ask you to complete three tasks below. In these tasks, you have the opportunity to earn money. After processing of the tasks, a task is randomly selected as payment-relevant. Your earnings from this randomly selected task can then be transferred in whole or in part to Person 2. Person 3 decides how much is transferred. So Person 3 can transfer 0%, 20%, 40%, 60%, 80% or 100% of your earnings to Person 2. Person 3's decisions were made prior to this study in another study. We will randomly assign you a decision of a person 3. At the end of the study, your earnings and carryover will be paid to you and Person 2 according to Person 3's decision.

In each of the tasks, you will be shown 50 sliders. You can set each slider to any position between 0 and 100 by pressing and dragging the slider to the desired position with your computer's mouse. You will see the current position displayed on the right side of the slider. Your task is to place all sliders on position 50. You have 2 minutes to do this.

In the image below, you can see two examples. The top slider has a current position of 28. So, it is not correctly placed. The lower slider has the current position of 50 and is therefore correctly placed.

If you manage to place at least 25 of the 50 sliders in the correct position, you will receive €5. If you cannot do this, you will not receive any payout from the respective task. For each additionally correctly placed slider, you will receive €0.20.

Third Part: Task explanation Before you process the task, you will receive the following information about Person 2 and Person 3. Person 3 is a German citizen who earned €5 in a previous study. Person 2 is [An asylum seeker / A German citizen / An economic migrant] who has not earned €5 in a previous study.

[The amount that Person 3 transfers from you to Person 2 is transferred one-to-one in this task / The amount that Person 3 transfers from you to Person 2 is doubled in this task. Hence, twice the selected amount goes to person 2. / The amount that Person 3 transfers from you to Person 2 is halved in this task. Hence, half of the selected amount goes to person 2.]

Fourth Part: Slider task (Effort measure) and subsequent beliefs elicitation
Participants were shown a screen with 50 sliders in a randomly determined initial position in each round, as depicted in Figure A.3 above. After each round of performing the slider task, beliefs about the share redistributed by the third-person allocator were elicited.

What do you think? Which percentage of your earnings from this task will person 3 transfer from you to the [asylum seeker / German citizen / economic migrant]? If your estimate is correct, you will receive an additional €0.5.

[Remember that the amount is transferred one to one. / Remember that twice the amount is transferred. / Remember that half of the amount is transferred.]

Fifth Part: Questionnaire *We list the questionnaire's items we used in the analyses in Section B.4.*

Sixth Part: Comments and end of the study Here you have the opportunity to give us feedback on the study: *(Empty text-box where participants could provide feedback.)*

Thank you for your participation. Task [1 / 2 / 3] was randomly determined to be relevant for payout. In this task, you have correctly placed # sliders.

(Participants were informed about their earnings depending on their performance and the correctness of their beliefs.)

B.4 Items from the questionnaire

Below we show relevant questions from the post-experimental questionnaire (translated from the original German version) that we used to construct variables for our analyses. The original German version of the questionnaire is available upon request.

- How old are you? (Enter your age)

- Please enter your gender: 1 Male 2 Female 3 Non-binary

- Do you have another citizenship besides German? 1 Yes 0 No

- Were you born in Germany? 1 Yes 0 No

- Was your mother born in Germany? 2 Don't know 1 Yes 0 No

- Was your father born in Germany? 2 Don't know 1 Yes 0 No

- Were your grandparents born in Germany? 1 Yes 2 No 3 Partly 4 Don't know

- Do you belong to a religious group? If yes, which one? (1 I don't belong to any religion 2 Protestant church 3 Catholic church 4 Christian Orthodox churches 5 Islam 6 Judaism 7 Other)

- Please use the following scale to indicate how much you enjoyed the tasks: Very much 1 2 3 4 5 6 7 Not at all

- How close are you to the following groups? (Scale: 1 Very close 2 Close 3 Not decidedly 4 Distant 5 Very distant)

    - People in your city
    - Germans
    - Europeans
    - People all over the world

- Many people use the terms 'left' and 'right' to denote different political views. If you think about your own political views, where would you place them on this scale? 1 Very left 2 Left 3 Center 4 Right 5 Very right

We base our PCA-indices used in the regressions in Table B.3 on the following questions focussing on attitudes towards immigrants (in general), Asylum seekers, and Economic migrants.

- Views on immigrants (Scale: 1 Strongly agree 2 Agree 3 Neutral 4 Disagree 5 Strongly disagree)

    - Immigrants increase crime rates.
    - Immigrants are generally good for Germans Economy.
    - Immigrants are taking jobs away from people who were born in Germany.
    - The foreigners living in the Federal Republic should adapt their lifestyle to the lifestyle of the Germans.

- Germany is currently taking in too many migrants.

- Views on Asylum seekers (Scale: 1 Strongly agree 2 Agree 3 Neutral 4 Disagree 5 Strongly disagree)

  - People who have received asylum in Germany should receive financial support from the German state to ensure their livelihood.
  - People who have received asylum in Germany should receive free access to support that facilitates integration.
  - Asylum seekers who have not yet received asylum in Germany should receive financial support from the German state to ensure their livelihood.
  - Asylum seekers who have not yet received asylum in Germany should receive free access to support that facilitates integration.
  - People who apply for asylum in Germany are mainly politically persecuted people who have a right to asylum.
  - People who apply for asylum in Germany are mainly people who come to Germany for economic reasons and have no right to asylum.

- Views on Economic migrants (Scale: 1 Strongly agree 2 Agree 3 Neutral 4 Disagree 5 Strongly disagree)

  - Migrants who came to Germany for economic reasons and have no right to asylum should receive financial support from the German state to ensure their livelihood.
  - Migrants who have come to Germany for economic reasons and have no right to asylum should receive free access to support that facilitates integration.
  - Migrants who come to Germany for economic reasons are mainly citizens from other European countries.
  - Migrants who come to Germany for economic reasons are mainly citizens from non-European countries.