

Queen Bee Immigrant: The Effects of Status Perceptions on Immigration Attitudes

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Abstract

This work examines a seemingly counter-intuitive phenomenon observed in many Western democracies, whereby parts of the immigrant population oppose new immigration waves. I propose a theory based on group status distribution to explain these preferences, arguing that relative status deprivation — the degree to which a group is ranked low in the host country's ethnic hierarchy — negatively affects its members' attitudes toward even lower-ranked groups. In an experiment in Germany, immigrant participants from over 30 countries residing receive from natives either a positive or a negative evaluation of their national/ethnic ingroup, while outgroup evaluations are held fixed. Receiving a negative evaluation leads participants to express more negative views of refugees from the Middle East and to donate less to an organization supporting them, without altering generosity in unrelated contexts. Participants in negative treatment also update their perceptions of norm surrounding prejudice expression and become less willing to conceal critical views publicly.

1 Introduction

In the 2024 U.S. presidential election, National Election Pool exit survey estimated that 46% of Latino voters cast their ballots for Donald Trump, marking the strongest Republican performance among this electorate in exit polling since the 1970s. Given his explicitly restrictionist immigration agenda, this remarkable level of support appeared surprising, quickly becoming one of the most discussed aspects of Trump’s victory.¹ Indeed, this is seemingly counter-intuitive—why would groups who themselves have a history of immigration, and are also after naturalization largely perceived as immigrants, support anti-immigration platforms?

This is, however, far from the only example of opposition to immigration voiced by immigrants. In the 2017 federal election in Germany, Alternative für Deutschland (AfD), which based the core of its platform on opposing immigration, reached higher support among Russian-speaking Germans compared to the national average (Goerres et al., 2020). The “Leave” option in the Brexit referendum garnered significant support among the British South Asian population, with accounts often pointing to concerns over new waves of immigration as explaining this preference (Shackle, 2017; Dunin-Wasowicz, 2017, 2016). In Switzerland, voters with an immigrant background supported the referendum “against mass immigration” as much as the native majority did (Strijbis and Polavieja, 2018). Prominent politicians of immigrant descent, such as Suella Braverman, Nicky Haley, and João Varela, who built careers as outspoken opponents of immigration, further exemplify the phenomenon.² Challenging the common expectation that immigrants hold generally positive views of immigration (Krejčova et al., 2025; Neureiter and Schulte, 2024; Hindriks et al., 2017; Sirin et al., 2017; Just and Anderson, 2015; Dandy and Pe-Pua, 2010; Berry, 2006), these examples point to a broader question: how do immigrants form their immigration

¹See Election 2024: Exit Polls available at: <https://www.cnn.com/election/2024/exit-polls/national-results/general/president/7>

²Further examples are provided by, e.g., Pettersson et al. (2016) in Sweden, or Meeusen et al. (2019) in case of Belgium.

attitudes, what shapes them, and when do they turn negative?

While a vast literature in political science, economics, and related disciplines studies immigration attitudes from the perspective of native majorities (for a survey of this literature, see, e.g., [Alesina and Tabellini \(2024\)](#) and [Hainmueller and Hopkins \(2014\)](#)), with some notable exceptions, considerably less attention has been devoted to understanding how immigrants themselves view immigration. This gap merits attention, given that immigrants constitute an increasing share of the (voting) population in virtually all western democracies and considering the importance of immigration attitudes in shaping broader political preferences. The limited existing work on this question relies primarily on correlational designs—linking immigrants’ attitudes to factors such as time spent in the host country, labor market competition, or naturalization ([Hindriks et al., 2014](#); [Just and Anderson, 2015](#); [Kolbe and Crepaz, 2016](#); [Krejčová et al., 2025](#); [Neureiter and Schulte, 2024](#); [Politi et al., 2020](#); [Šedovič and Dražanová, 2025](#))—making causal inference difficult given endogeneity and self-selection concerns.

This paper studies immigration attitudes among immigrants and proposes a theory based on group status distribution that (complementarily to other considerations) can help to explain them. Immigrant communities are not monolithic; they vary in the social status and reception they experience in the host society. I argue that relative *status* deprivation, that is, the degree to which an immigrant group is ranked low in the ethnic status hierarchy of the host country, has a negative impact on its members’ attitudes toward groups ranked even lower.³ In other words, I suggest that immigrant groups encountering an unfavorable reception in the host society respond to this experience by “kicking down the ladder”. In a frequently encountered situation, where the newcomer immigrants are perceived more critically and occupy a lower status position compared to the established ones, this amounts

³Whereas status as a concept has been used to designate group’s ranking along different valued dimensions (e.g., socio-economic status), in this work, I rely on the definition of status as a ranking in terms of social esteem, honor, and respect accorded to them, distinct from wealth or power ([Ridgeway \(2019\)](#), [Weber \(1968\)](#)). Therefore, the status position of an immigrant group here refers to its position in the ethnic hierarchy of the host society, i.e., how socially desirable the group is perceived to be relative to other ethnic groups (importantly, including the native majority).

to status assigned to the established immigrants shaping their attitudes towards new waves of immigration.

This expectation is based on two complementary arguments. First, drawing on Social Identity Theory (Turner, 1975; Tajfel, 1969), belonging to a low-status group generates an identity threat, prompting individuals to repair their group’s standing by derogating groups ranked even lower and emphasizing favorable comparison of their group relative to them. Such derogation is not merely an expression of hostility; it serves to reshape the perceived status hierarchy by pushing lower-status groups further down, thereby effectively improving the ingroup’s relative position.

Second, while prejudice directed at one’s own ingroup motivates defensive derogation of even lower-status groups, whether and to what extent individuals act on this motive is conditioned by the normative context. Derogating other out-groups entails drawing on generalizations and ethnic or racial stereotypes, and the legitimacy of doing so is strongly shaped by prevailing social norms (Álvarez-Benjumea, 2025, 2023; Álvarez-Benjumea and Winter, 2018; Hartevelt and Ivarsflaten, 2018; Munger, 2017; Blinder et al., 2013; Zitek and Hebl, 2007). I argue that immigrants exposed to prejudice from natives—despite being its targets—infer from this experience that such expressions are common and socially tolerated, leading them to update their perceptions of prevailing norms regulating expression of prejudice. Thus, prejudice directed at one’s own group operates both as an identity threat motivating derogation of lower-status groups and as a signal that reduces the perceived social cost of doing so.

To investigate this idea, I field a preregistered survey experiment among immigrants in Germany originating from over 30 countries and covering main sources of immigration to Germany. The design experimentally manipulates participants’ perceptions of *their ingroup’s* status by presenting them with either a positive or a negative evaluation of their own immigrant group, drawn from a separate pre-study conducted with native German respondents. To ensure that this information is not interpreted as a general signal about the majority

population’s attitudes toward all immigrants (and participants’ ingroup coincidentally mentioned), respondents in both treatments view two additional evaluations of other immigrant outgroups—one positive and one negative—kept constant across treatments. This establishes a minimal, two-tier status hierarchy in which only the position of the participant’s ingroup varies experimentally. I then assess how this manipulation shapes participants’ attitudes toward a salient lower-status immigrant outgroup—refugees from the Middle East—and their readiness to publicly express them. This is a theoretically and empirically meaningful test case. Since the 2015 “refugee crisis”, refugees from the Middle East have been the focus of sustained public debate in Germany, and policies toward asylum seekers remain among the most contested and politically responsive components of immigration policy. Furthermore, prior research shows that the Middle Eastern immigrants are consistently viewed less favorably than other immigrant groups, placing them at the lower end of the perceived ethnic hierarchy⁴, thus representing a natural benchmark for testing downward-directed responses predicted by the theory.

The findings support the proposed hypothesis. Participants who received a negative evaluation of their own ingroup donate significantly less to an organization supporting refugees. Further, while the treatment variation does not affect participants’ beliefs relating to topical concerns frequently discussed in the context of the immigration of refugees (e.g., their contribution to criminality, unemployment, or fiscal burden that they impose), exposure to negative prejudice leads to a decrease in willingness to allow more refugees into the country. These results are robust to controlling for a set of individual characteristics, including age, gender, education, household income, as well as fixed effects of the region of residence (in Germany), and immigrant region of origin.

I test several alternative mechanisms that could explain these results. I show that the effect on donations does not stem from the treatment altering participants’ generosity in contexts unrelated to immigration, nor is it driven by the perceived economic threat associ-

⁴See e.g., [Froehlich and Schulte \(2019\)](#); [Meidert and Rapp \(2019\)](#); [Sahgal et al. \(2018\)](#). This pattern is also confirmed in the present study.

ated with refugee inflows. I further provide evidence against the possibility that immigrants express negative views about other immigrant groups as a strategy of distancing from them out of fear of being perceived and treated as essentially the same.

Beyond privately held attitudes, I study how exposure to prejudice directed at one's own ingroup shapes immigrants' perceptions of the social norms governing prejudice expression in the host society, and their readiness to publicly voice anti-immigrant views. Consistent with the proposed theory, participants exposed to a negative evaluation of their ingroup expected natives to express more negative views of other low-status immigrants (but not high-status ones). This provides support for the idea that immigrants extrapolate from their own exposure to prejudice to update their perception of the norm regarding discriminating downwards. Additionally, I collect a behavioral measure capturing participants' readiness to publicly express their views of the Middle Eastern refugees. The design subtly offers participants a chance to modify their previously stated evaluation of refugees in a setting where their answer will be seen by natives, allowing me to examine treatment effects on preference falsification ([Kuran, 1997](#)). This analysis yields two insights. First, contrary to the hypothesis that immigrants may adopt negative stances toward other stigmatized groups in an effort to appeal to the native majority ([Pérez et al., 2023](#); [McClain et al., 2006](#)), participants on average express more favorable attitudes toward refugees when their responses are observable to natives. Second, although participants falsify their attitudes in a positive direction overall, they do so to a lesser extent when exposed to negative evaluations of their own ingroup. Therefore, exposure to prejudice not only fosters negative affect toward lower-status groups, but also signals that expressing such views is more socially acceptable than previously assumed, thereby shaping the public expression of attitudes.

This paper contributes to the extensive body of work studying immigration attitudes and extends it in at least two directions. First, this paper redirects attention from the well-studied attitudes of natives to the comparatively understudied attitudes of immigrants. I extend the limited extant work by providing a causal test based on an experimental design

with a diverse immigrant sample limiting the role of group-specific idiosyncrasies. Second, this paper goes beyond commonly studied explanations of immigration attitudes that emphasize perceived economic ([Haaland and Roth, 2020](#); [Pardos-Prado and Xena, 2019](#); [Scheve and Slaughter, 2001](#); [Facchini and Mayda, 2009](#); [Mayda, 2006](#)) or cultural threats ([Tabellini, 2020](#); [Hainmueller et al., 2015](#); [Hainmueller and Hangartner, 2013](#); [Card et al., 2012](#)) posed by incoming immigration, and instead examines how immigrants’ position in the host society’s status hierarchy shapes their immigration attitudes. In doing so, it contributes to the literature studying the importance of individuals’ identity and belonging to social groups for their political preferences. The works of [Fouka et al. \(2021\)](#) and [Fouka et al. \(2020\)](#) engage with a setting closely resembling the one studied here — a high-status majority and a lower-status established minority encountering a new outgroup perceived even more critically than the minority itself. They show that such entries can prompt the native majority to re-evaluate the established minority more positively. Taking a complementary perspective, I show that the negative treatment the established minority receives from that same majority triggers defensive attitudes directed downward toward the newcomers.

[Pérez et al. \(2023\)](#) study a related case, showing how inducing Latinos to feel less American heightens anti-Black racism. While their analysis focuses on an established racial minority, this paper examines a setting where a shared migration experience defines a superordinate immigrant category within which groups occupy distinct status positions — generalizing the logic to show that low-status positioning generates downward exclusion and alters norms governing prejudice expression.

Finally, this work contributes to the literature on social norms and preference falsification ([Kuran, 1997](#)). Prior work shows that social norms regulate political behavior by shaping the perceived costs of expressing controversial preferences — for instance, the electoral success of radical platforms signals that xenophobic views have become socially acceptable, increasing individuals’ propensity to express them ([Valentim, 2021](#); [Bursztyn et al., 2020](#); [Dinas et al., 2020](#); [Crandall et al., 2002](#)). I build on these findings and show that even those individuals

who are direct targets of stigmatized behavior learn from this experience that it is more prevalent and tolerated in their society, and become less willing to conceal their critical views in public.

In what follows, I develop two complementary theoretical mechanisms linking immigrants' exposure to prejudice to downward exclusion and derive testable hypotheses, before presenting the study design and empirical results.

1.1 Status Hierarchies and Downward Prejudice

A central tenet of Social Identity Theory holds that individuals in part define their own identities with regard to the social groups that they belong to (Tajfel, 1969). As people are motivated to maintain a positive view of themselves and their position in the society, they seek favorable comparisons between their own group and relevant outgroups (Turner, 1975). Assignment to a low-status group therefore presents an identity threat for its members, who are in turn predicted to engage in defensive strategies in order to cope with it. One such defensive mechanism proposed by the theory is “downwards comparison”, whereby individuals recover self-esteem by focusing on comparison with an even lower-status group and emphasizing their own group's positive distinction relative to this new basis of comparison. By derogating an even lower-status group, such as seeking or attending to unfavorable evaluations of it, members of a low-status group effectively extend the perceived hierarchy downward, thereby improving the relative position of their own ingroup. Applied to the inter-ethnic context studied here, this logic implies that established immigrants who encounter a negative evaluation of their own group from the native majority will direct more negative attitudes toward a lower-status outgroup, such as refugees from the Middle East (H1).

This logic parallels the so-called Queen Bee phenomenon, which—also grounded in Social Identity Theory—describes how high-status women in male-dominated environments sometimes display bias against lower-status, junior women (Ellemers et al., 2004). Subsequent

work shows that this behavior reflects a response to gender bias and identity threat rather than a trait specific to women (Derks et al., 2016), and that the negative bias is directed only toward junior women rather than other senior women (Faniko et al., 2017). Drawing an analogy, one might ask whether a similar “Queen Bee–Immigrant” dynamic exists: do established immigrants facing skepticism from the native majority respond by expressing negative bias toward newly arriving, lower-status immigrant groups?

1.2 Social Norms and Prejudice Expression

Whereas Social Identity Theory explains downward competition as a response to identity threat, a distinct yet complementary mechanism, grounded in individuals’ perceptions of the social norms surrounding the expression of prejudice, may further facilitate negative attitudes toward lower-status groups. Previous work on the emergence of social norms shows that individuals in part infer the group’s descriptive norms (what others are doing) from other individuals’ behavior to which they are incidentally exposed. Particularly in those situations where the behavior of interest does not produce an easily observable outcome (such as discriminatory behavior), people in part rely on their own direct experiences to learn the descriptive norm regarding this behavior (Kwan et al., 2015; Kashima et al., 2013). Therefore, groups socialized in the presence of a steep ethnic hierarchy that are themselves exposed to prejudiced treatment might learn from this experience that discriminating downwards (i.e. against groups ranked in the status hierarchy lower than one’s own group) is widespread and perhaps even socially acceptable behavior in the host society, and are more likely to apply it toward the lower-ranked groups once they encounter them. Accordingly, I hypothesize that exposing low-status immigrants to a negative prejudice directed at their ingroup, leads them to perceive expressing negative prejudice toward other low-status groups (but not high-status ones) as more frequent among the native majority (H2).

Such updates in beliefs about the social environment bear implications for how political attitudes are expressed and acted upon. Previous work on how private opinions translate into

publicly stated ones shows that individuals tend to adjust their expressed views toward positions they believe to be socially appropriate (Valentim, 2024; Dinas et al., 2020; Enikolopov et al., 2020; Bursztyn et al., 2018; Perez-Truglia and Cruces, 2017). Preference falsification (Kuran, 1997), that is, the resulting discrepancy between privately held and publicly expressed preferences is consequential: studying the rise of Donald Trump, Bursztyn et al. (2020) show that the public revelation of controversial attitudes, such as xenophobic views, can make observers themselves more willing to express—and less willing to condemn—such attitudes. Understanding how established immigrants falsify their preferences is therefore important not only because their public positions may differ from their private ones, but also because public expression of attitudes can be consequential in their own right.

I focus on one aspect of preference falsification and investigate whether established immigrants change their statements when their answers might be observed by a participant from the majority population, and whether this tendency changes with the exposure to prejudice toward their ingroup. If exposure to prejudice signals that exclusionary views toward low-status groups are more common in the host society than previously assumed, established immigrants may feel less compelled to censor their public statements. Consequently, I hypothesize that immigrants exposed to negative prejudice about their ingroup should be more willing to express their true (potentially negative) views regarding refugees when being observed by natives (H3).

2 Experimental Design

The study consists of two phases — a pre-study and a main experiment — both implemented as online surveys.⁵

⁵The study received the approval of the ethics committee of [removed for anonymization]. The survey was distributed by the a panel company CINT (<https://www.cint.com/>).

2.1 Pre-study

The pre-study was conducted with a sample of native Germans ($N = 125$), with the sole purpose of collecting majority-population evaluations of different immigrant groups for use in the main experiment. To do so, for each of the several regions/countries, participants were asked to evaluate whether people immigrating from the given region/country contribute rather positively or rather negatively to “socio-economic and cultural life in Germany” (participants selected one of the two options as an answer). Each question was accompanied by a highlighted map of the relevant region, with all corresponding countries listed explicitly. Participants received a fixed fee upon completion.

2.2 Main Experiment

The main part of the experiment was conducted with a sample of 1.159 participants with an immigration background residing in Germany. Participants’ (own and parental) country of birth is used to match participants to one of the eleven regions of origin (see Figure 2).

Treatment provision Participants were informed about the pre-study and told that some of the evaluations of immigrant groups collected from native Germans would be shown to them. They were then randomly assigned (conditional on the region that they were matched to) to one of two treatments.

In the **Positive treatment**, participants were shown an answer that evaluated the impact of their own immigrant ingroup positively, whereas in the **Negative treatment**, participants were shown an answer that negatively evaluated the impact of their ingroup. Here, the positive and negative evaluations refer to the group being evaluated as “contributing rather positively”, and respectively as “contributing rather negatively”, to socio-economic and cultural life in Germany.

To ensure the ingroup evaluation was not interpreted as a general signal about majority attitudes toward all immigrants, both treatments additionally showed two outgroup evaluations held constant: immigrants from Western EU countries (positive) and immigrants

from Lebanon (negative). The only difference between treatments is thus the evaluation of participants’ own ingroup, situating it within a fixed two-tier status hierarchy where only the ingroup’s position varies (See Figure 1 for an example).

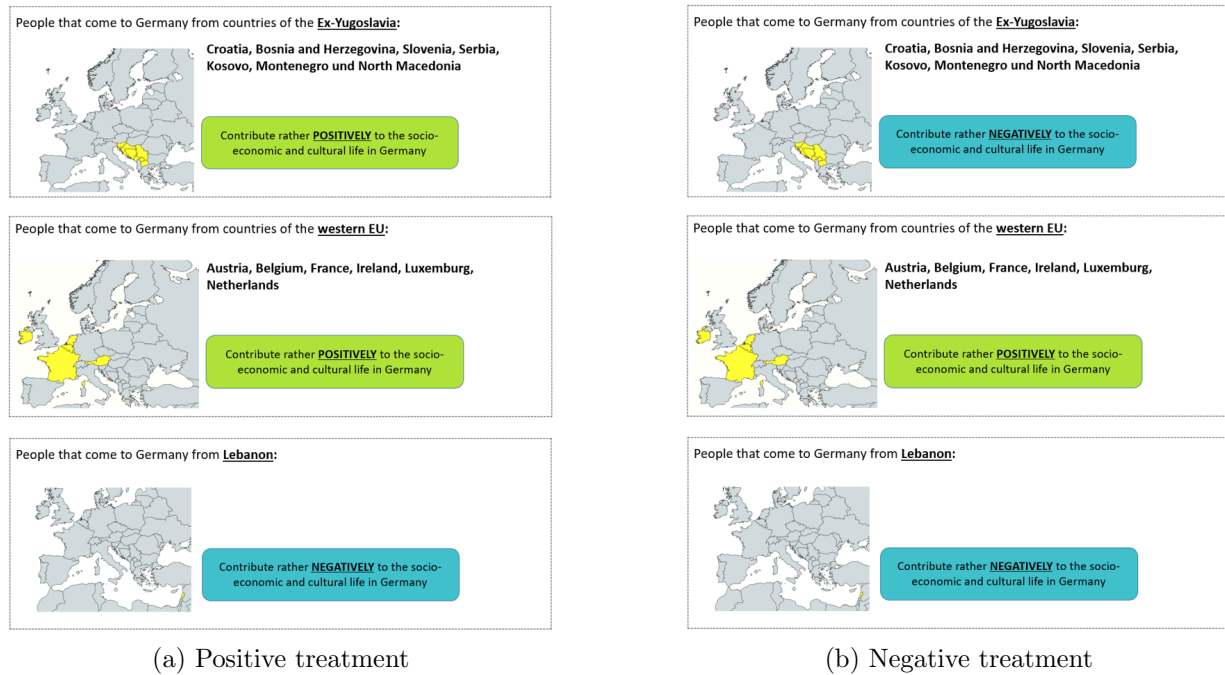


Figure 1: Treatment provision - example

The figure depicts an example of a screen that a participant, whose immigration origin was matched to the region of Ex-Yugoslavia, would see in the treatment provision phase if they were allocated to the Positive treatment (panel a), and that if they were allocated to the Negative treatment (panel b). Participants are informed that they would see a subset of answers collected in the pre-study. Treatment variation is based on randomly matching participants to an answer from the pre-study evaluating the participant’s own (parental) region of origin either positively or negatively while keeping the evaluations of the other two outgroups constant.

Elicitation of attitudes toward refugees Thereafter, participants are asked to share their own opinion regarding immigration to Germany, and in particular regarding persons “currently requesting the right to asylum in Germany”. Two measures of participants’ support of refugees were then elicited.

The main behavioral measure of participants’ support for refugees was captured by the willingness to donate to the United Nations High Commissioner for Refugees (UNHCR). Participants were told that one randomly selected participant would win 100 euros, and

were asked how much of this prize — if they won — they would like to donate to the UNHCR, described as a global organization supporting refugees. Any pledged amount would be deducted from their winnings and donated accordingly.

Moreover, an attitudinal measure of support was constructed by collecting participants’ answers to a set of seven questions. Participants provided their views on (among others) whether Germany should increase or decrease the number of people it grants asylum to, refugees’ influence on the labor market, the welfare state, probability of a terrorist attack, criminality, etc. Table 1 provides a full list of questions.

Table 1: Attitudinal questions

q1	Do you think Germany should increase or decrease the number of people it grants asylum to?
q2	Refugees are a burden on our country because they take our jobs and social benefits.
q3	The money spent on the accommodation of refugees in our country could have been spent better to cover the needs of Germans.
q4	Refugees will increase the likelihood of a terrorist attack in our country.
q5	Refugees in our country are more to blame for crime than other groups.
q6	Is Germany made a worse or a better place to live by refugees who are granted asylum in Germany?
q7	Among the following options, which one best explains why refugees leave their country? (1) To flee war; (2) To improve their economic conditions; (3) To avoid political persecution; (4) To gain access to host country’s social benefits.

In case of the first five questions, participants answered on a 5-point scale (q1: from “greatly increase” to “greatly decrease”; q2-q5: from “completely agree” to “completely disagree”). Participants answered to question q6 by selecting a position on a 11 point scale (from “worse place to live”, to “better place to live”).

Extended dictator game In this part of the study, participants played an extended version of the dictator game, allowing for a simultaneous elicitation of two measures of interest. First, participants’ willingness to share income with an anonymous other provides a measure of generosity in an immigration-unrelated context, serving as a check on whether the treatment altered general prosociality rather than refugee-specific attitudes. Second, the game captures indirect upstream reciprocity — the tendency to exhibit prosocial (antisocial) behavior toward others because somebody else has exhibited prosocial (antisocial) behavior

toward them.⁶

Participants were randomly assigned one of three roles in an extended dictator game — player A, B or C — with most assigned role B ($n = 1,150$). Player A sends some portion of a budget to Player B, and this amount is multiplied by an uncertain factor (either high or low) before being received. Player B observes only the total received but not the realization of the factor, and thus cannot infer how generous A was. Player B then decides what share of the received amount to send to Player C, separately for each of the two scenarios — one in which A sent more and the factor was low, and one in which A sent less and the factor was high. I take the difference in Player B’s transfers across the two scenarios as a measure of indirect upstream reciprocity.

Mood elicitation In order to be able to control for the treatments’ potential effect on participants’ mood, a measure of mood is elicited via the Self-Assessment Manikin questionnaire (Bradley and Lang (1994)) capturing three major affective dimensions - pleasure, arousal, and dominance using five-point manikin scales.

Descriptive norms To study treatment effects on participants’ perceived descriptive norms regarding the expression of prejudice towards low-status groups, participants are asked to guess what percentage of the 125 native participants in the pre-study evaluated negatively each of several immigrant groups. Each participant is asked to guess the share of of negative evaluations of the impact of people immigrating to Germany from: the participant’s own (parental) region of origin, Western EU countries, Lebanon, Turkey, countries of Southern Africa and that of refugees immigrating from the Middle East. The answer closest to the true collected values was rewarded with an additional 25 euros bonus.

Preference falsification This part of the experiment aims to capture a potential difference in attitudes expressed by established immigrants when they expect these attitudes to be observed by a majority population, as compared to when this is not the case. Participants

⁶The pre-analysis plan included an additional hypothesis related to indirect upstream reciprocity, which predicted stronger responses to negative ingroup evaluation among individuals higher on this trait. While the results provide partial support for this prediction, indirect reciprocity does not offer a systematic account of the main findings. For clarity of presentation, these analyses are reported in the Appendix.

were reminded that all their previous responses would be available only to the researchers in anonymized form. In contrast, their answer to the next question might be shared anonymously in a future study conducted with German citizens, who would learn the participant’s answer along with their country of origin. They were then asked to answer the question “Is Germany made a worse or a better place to live by refugees who are granted asylum in Germany?” — previously asked in the attitudinal elicitation phase.

The session ended with debriefing — participants were shown the true pre-study percentages for each immigrant group — followed by a final set of demographic questions.

2.3 Data and Sample Description

The pre-study sample comprised 125 adult residents of Germany with no immigration background. For the main experiment, 1,176 adults with an immigration background were recruited; 17 were removed for inconsistent demographic responses, yielding a final sample of 1,159.⁷

Participants with an immigration background were matched to what I refer to for simplicity’s sake as “region of origin”, indicating one of the eleven regions encompassing their, or parental, country of birth. The regions selected to be targeted in this study, depicted in Figure 2, encompassed all countries within Europe (except for the Western European countries), all ex-Soviet countries, Turkey, and five northern African countries (Egypt, Tunis, Morocco, Algeria, and Libya). Table A1 in the Appendix provides an overview of all regions (and constituent countries), along with the share of participants matched to each region and their basic demographic characteristics across treatments. The regions were selected to reflect the most represented immigrant groups in Germany⁸, while focusing on those immigrants that are likely to occupy a lower status position than the native majority in German society (thus the exclusion of the Western European countries), while still being (on average) perceived

⁷Following the Federal Statistical Office of Germany, immigration background is defined as having been born outside Germany, or having at least one parent born outside Germany. ([Statistisches Bundesamt \(Destatis\), 2022](#))

⁸See Federal Statistical Office of Germany (Genesis-Online Database, code: 12211-0202)

more favorably compared to the refugees from the Middle East. As shown in Figure A1 in the Appendix, this objective is met: the only group evaluated more negatively than Middle Eastern refugees in the pre-study was immigrants from Lebanon, who were not eligible to participate.⁹

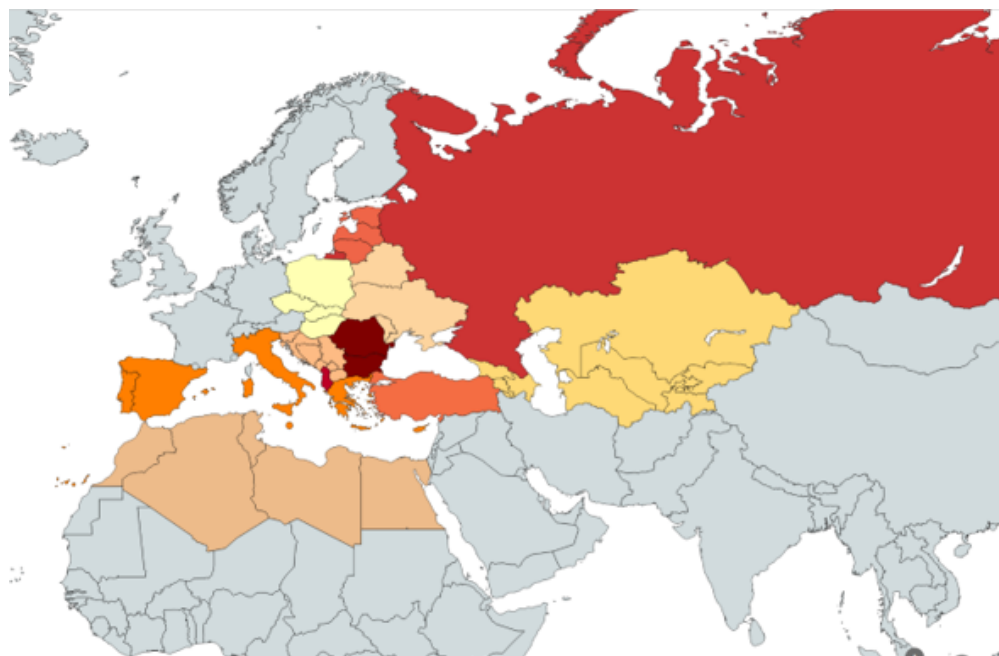


Figure 2: Participant’ immigration origins

The figure displays participants’ regions of (immigration) origin. Individuals from countries shown in gray were not eligible to participate in the study. Each participant was matched to a single country of origin based on their own or their parents’ country of birth. These countries were then grouped into 11 “regions of origin,” as indicated in the figure, with countries sharing the same color belonging to the same region. The purpose of this classification is to reduce complexity while maintaining sufficient internal homogeneity so that participants can reasonably identify with their assigned region. To this end, countries that are particularly large or have a sizable and salient diaspora in Germany were retained as stand-alone regions (e.g., Russia and Turkey). The same applies to countries that, for cultural or political reasons, are unlikely to identify with their geographic neighbors or be perceived as representative of the broader region (e.g., Albania).

⁹This hierarchy is also understood by immigrant participants in the Main experiment. When asked to guess the share of natives who negatively evaluated several immigrant groups (as detailed in descriptive norms task), participants (in both treatments) expected refugees from the Middle East to be by far the most negatively evaluated group.

3 Results

In what follows I examine the effect of exposing participants to a negative (rather than positive) evaluation of their own immigrant ingroup on their support for refugees, before turning to the treatment effects on norms related to prejudice expression and preference falsification.

Having read the displayed evaluations stemming from native Germans, participants were first asked to indicate to what extent they agree with the stated opinions. As expected, participants in the Negative treatment stated significantly lower level of agreement with the evaluations compared to those in Positive treatment.¹⁰ This difference in agreement in the first line provides a straightforward manipulation check, confirming that participants perceived the intended valence of the treatment. At the same time, it offers a proxy for the identity threat induced by negative status information.

3.1 Pledged Donation to the UNHCR

I first examine the treatment effect on the behavioral measure of participants' support for refugees captured by the amount that participants committed to donate to the United Nations High Commissioner for Refugees (UNHCR), from a 100-euro prize that is raffled among all participants. On average, respondents pledged to donate 36.86 euros, with individual contributions across the full range of possible donations (see Figure A2 in the Appendix for the distribution).

Table 2 reports the regression results for the pledged donation amount (Columns 1–2) and for an indicator capturing whether a participant made any positive donation (Columns 3–4). The results provide support for the hypothesized effect of the exposure to prejudice (H1). Participants who received a negative evaluation of their immigrant ingroup on average

¹⁰Participants answered on a 5-points scale (from strongly disagree to strongly agree). While in the Positive treatment 55% of participants state that they agree or strongly agree with the evaluations, in Negative treatment this figure drops to 24%. Comparing answers using the Mann–Whitney U-test shows that the average agreement significantly differs between the two treatments ($p < 0.01$).

donated 4.38 euros less to UNHCR relative to those who received a positive evaluation of own ingroup ($p < 0.01$). This effect is sizable, amounting to around 12% of the average pledged donation. Participants allocated to the Negative treatment were also around 5.3 percentage points less likely ($p < 0.01$) to make any positive donation. Columns (2) and (4) show that these findings are robust to controlling for respondents’ sociodemographic characteristics¹¹, and all specifications include fixed effects for both the German federal state and the participants’ (parental) region of origin.

Table 2: Treatment effects: Pledged donation to the UNHCR

	(1) Pledged donation	(2) Pledged donation	(3) Pr(Donation>0)	(4) Pr(Donation>0)
Negative treatment	-4.383** (2.004)	-4.316** (2.003)	-0.0534** (0.0242)	-0.0528** (0.0242)
Constant	45.80*** (4.778)	50.50*** (5.523)	0.848*** (0.0524)	0.955*** (0.0635)
Individual controls	No	Yes	No	Yes
Observations	1159	1159	1159	1159
R-squared	0.0402	0.0444	0.0351	0.0440

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

Column (1) and column (2) depict the results of the OLS regression of the pledged donation to the UNHCR on the treatment variable and the set of individual controls. Columns (3) and (4) show the results of the OLS regression of the dummy variable that takes value one if a participant pledged to donate a value larger than zero on the treatment variable and the set of individual controls. Negative treatment indicates receiving negative status information regarding own in-group (with Positive treatment serving as a baseline). Individual controls include age (in years), gender, equivalent household income tertile, and an indicator for completion of at least some tertiary education (See Table A2 in Appendix for results with the full covariate set). All regressions include fixed effects for the federal state of residence in Germany and for participants’ (parental) region of origin. Robust standard errors in parentheses.

One concern that might arise here, given the way the treatment was administered, is that receiving a starkly negative (positive) evaluation of one’s ingroup may simply have put participants in a bad (good) mood thereby affecting their prosociality more generally.

¹¹Table A2 in the Appendix reports the association between demographic variables and donation amounts. Apart from age—which is negatively associated with pledged donations—none of the demographic characteristics exhibit significant effects.

Should this be the case, the observed difference in pledged donations would not necessarily reflect the specific change in attitude toward the refugees, but rather a more general change in participants’ willingness to share income.

To account for this possibility, I examine participants’ behavior in the dictator game. As detailed in experimental design, participants are assigned a bonus budget and are asked to decide what share (if any) thereof they wish to send to an anonymous other participant under two possible scenarios - one in which another anonymous participant had acted more generously toward them, and one in which that participant had acted less generously. Participants know nothing about the recipient other than that this individual did not receive any bonus. Contrary to the idea that the treatment possibly affected participants’ generosity more generally—i.e. in cases unrelated to immigration—Table A8 in Appendix shows that treatment variation has no discernible effect on the amount participants chose to send to the anonymous recipient (in both scenarios).

Still, one might worry that the null effect on dictator giving reflects a compensatory response: participants who donated less to UNHCR give more in the dictator game to compensate. Contrary to this idea, dictator giving is positively correlated with pledged donations in both scenarios ($\rho_1 = 0.20$, $\rho_2 = 0.22$, $p < 0.01$), ruling out this mechanism. This is intuitive—more generous participants tend to share more in both tasks—and confirms that the absence of a treatment effect in the context unrelated to immigration is not an artefact of respondents offsetting their UNHCR donations.¹²

Furthermore, the results provide evidence against the mechanism whereby immigrants treat lower-status groups negatively in order to avoid being perceived as similar to them (Kaesler and Tani, 2023). If this concern plays a role, participants in the Negative treatment — exposed to a negative evaluation of both their own ingroup and of immigrants from

¹²A further test is provided by comparing the collected measures of participants’ mood across treatments. However, the distribution of all three measured affective dimensions - pleasure, arousal, and dominance, elicited via Self-Assessment Manikin questionnaire (Bradley and Lang (1994)), did not differ significantly between the two treatments (Kolmogorov–Smirnov test for equality of distribution in both treatments, for each of the three affective dimensions - pleasure: $p > 0.6$; arousal: $p > 0.9$; dominance: $p > 0.9$).

Lebanon — might feel concerned about being viewed as similar to the latter, who in turn may be perceived as culturally proximate to Middle Eastern refugees. A negative attitude toward refugees would then partially reflect an attempt to reinforce intergroup boundaries rather than a response to identity threat. However, if so, the strongest treatment response should be observed among participants most plausibly perceived as similar to Middle Eastern refugees — in the German context, those from predominantly Muslim countries, given that religious denomination is a salient marker shaping perceptions of group similarity. As shown in Table A9 in the Appendix, although participants from these backgrounds donate on average less in both treatments, the interaction term is insignificant, indicating that they do not react differently to the treatment.

3.2 Attitudinal Measures

In addition to the behavioral measure of support for refugees, participants answered seven attitudinal questions concerning refugees requesting asylum in Germany. The questions, among others, regressed participants’ views of the influence of refugees on employment, risk of terrorism, criminality. The exact formulation of all seven questions is provided in the experimental design description.

The first six columns of Table 3 report regression results of each attitudinal item on the treatment indicator and socio-demographic controls (higher values denote more supportive attitudes).¹³ Column (1) shows that participants in the Negative treatment were significantly more likely to favor reducing the number of granted asylums. For the remaining items, treatment effects mostly point in the expected direction—less supportive responses in the Negative condition—but none reach statistical significance. Thus, taken together, while the treatment shifts overall attitudes toward refugees, as reflected in the donation measure and

¹³Question q7 asked participants to provide their opinion on the primary reason why refugees abandon their countries among the following options: “to flee war”, “avoid political persecution”, “improve their economic conditions” and “obtain access to social security payments in the destination country”. I construct a dummy variable that takes value one if a participant selected one of the first two choices and show in column (7) the results of the Probit regression of this variable.

Table 3: Treatment effects: Attitudinal questions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	q1 Allow more	q2 Jobs	q3 Expense	q4 Terrorism	q5 Crime	q6 Impact	q7 Reason
Negative treatment	-0.147** (0.0596)	-0.0627 (0.0714)	-0.0229 (0.0680)	-0.0289 (0.0720)	0.0179 (0.0702)	-0.238 (0.156)	-0.0163 (0.0274)
Constant	3.350*** (0.147)	3.495*** (0.179)	3.126*** (0.170)	3.628*** (0.194)	3.335*** (0.187)	6.171*** (0.409)	0.889*** (0.0744)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1159	1159	1159	1159	1159	1159	1159
R-squared	0.102	0.0669	0.104	0.103	0.0617	0.0827	0.0642

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

Results of the OLS regression of participants' answers responding to the respective 6 attitudinal questions on the treatment variable and the set of individual controls. All answers are re-coded such that a higher value indicates higher support for refugees. Outcome in the last column takes value 1 if a participant selected "To flee war" or "Avoid political persecution" as the primary reason why refugees leave their countries, and 0 otherwise. All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Individual controls include age (in years), gender, equivalent household income tertile, and an indicator for completion of at least some tertiary education (See Table A3 in Appendix for results with the full covariate set and Table A5 for results excluding individual controls). Robust standard errors in parenthesis. Results with p-values adjusted for multiple hypothesis testing using the Benjamini–Hochberg false discovery rate procedure are reported in Table A10 in the Appendix.

preferred asylum inflow, it does not appear to alter how participants evaluate specific topical concerns often raised in debates about refugee immigration.¹⁴

The answers to attitudinal questions additionally allow assessing a another alternative mechanism. Although the evaluative statements were intentionally framed in broad terms to convey a general positive or negative assessment of participants' ingroup, one might still worry that they altered perceptions of the ingroup's socio-economic standing or increased its salience.¹⁵ Such a shift could, in turn, imply that assignment to the Negative treatment reinforced perceived competition in the labor market or for welfare benefits arising from

¹⁴In line with this idea, it is interesting to note that the only other case where the treatment effect is only marginally insignificant is the question asking participants if the refugees from the Middle East make Germany a better or a worse place to live ($coef. = -0.238$, $p = 0.128$), thus again capturing the general attitude rather than a particular topical concern.

¹⁵As a reminder, the statements read: People immigrating to Germany from (a given region) contribute (rather positively)/(rather negatively) to socio-economic and cultural life in Germany.

refugee inflow.

The results provide little evidence for this mechanism. Responses to question q2—asking whether refugees “take away our jobs and social benefits”, and explicitly designed to capture economic competition concerns—exhibit no discernible treatment effects. Moreover, interacting the treatment indicator with income or educational attainment and including these interactions in the donation regressions (Table A7 in Appendix) yields insignificant coefficients of both interaction terms. To the degree that income and education successfully proxy individuals’ vulnerability to economic competition, these null interactions indicate that the treatment did not differentially affect those more exposed to economic threat. Finally, if the treatment altered participants’ broader sense of economic security—and therefore their perceived ability or obligation to help others—we would expect this to affect dictator giving, which is not the case. This reinforces the conclusion that the treatment did not operate through shifts in perceived economic threat or personal economic capacity.

3.3 Norms Governing Prejudice Expression and Preference Falsification

While Social Identity Theory explains why identity threat creates incentives for members of low-status groups to engage in downward comparison, whether and to what extent individuals do so will arguably depend on its perceived social costs. I hypothesize that low-status immigrants learn from discrimination directed toward their own ingroup that discriminating downwards (i.e. against groups ranked lower than one’s own group) is more widespread (and possibly also acceptable) behavior in the host society than previously assumed.

I conduct two tests to examine this prediction. First, I examine whether receiving a negative evaluation of one’s own immigrant ingroup leads participants to expect more frequent expressions of prejudice by the native majority toward *other* low-status immigrant groups (but not toward high-status ones) (H2). Second, using a behavioral measure, I examine whether participants—conditional on their privately held preferences—alter their expressed

views of refugees when these are observable to the native majority. I expect that assignment to the negative treatment increases participants’ readiness to publicly express negative views (H3).

To conduct the first test, I use participants’ incentivized estimates of the share of native Germans who evaluated each immigrant group negatively (see Experimental Design). Recall that in the treatment provision, besides the evaluation of participant’s own immigrant ingroup, both treatments included a positive evaluation of Western EU immigrants and a negative evaluation of immigrants from Lebanon.

Table 4 reports treatment effects on participants’ norm expectations. As a first check, participants who received a negative ingroup evaluation expected natives to be more critical toward their own region of origin — confirming that participants extrapolated from the individual signal they received to the broader group average, and thus confirming successful treatment manipulation.

More interestingly, the same applies to participants’ expectations of evaluations of all other low-status immigrant groups. Particularly, in accordance with H2, participants in the Negative treatment expected a significantly more negative evaluation of refugees from the Middle East, as well as of people immigrating from Turkey, Lebanon and from countries in the south of Africa. This is not the case for the expected evaluation of high-status immigrants, that is, those coming to Germany from Western EU countries, indicating that this is not a consequence of expecting the majority population to be more skeptical toward immigrants in general. Instead, as proposed by H2, the results suggest that receiving a negative evaluation of their own ingroup led participants to expect more critical views only of those immigrant groups that were of a lower status than those who are providing the evaluation.¹⁶

While the literature documents how descriptive norms can shape injunctive norms (Bicchieri et al., 2020, 2023), the evidence here speaks directly only to the former. Nevertheless,

¹⁶Recall that treatment assignment was randomized conditional on participants’ region of origin. All specifications include region-of-origin fixed effects and thus exploit within-group variation. The results cannot be attributed to any particular immigrant group or to differential responses across groups of varying status.

Table 4: Treatment effects: Empirical expectations

Elicited expectation:	What percentage of majority population participants evaluated negatively the impact of people coming to Germany from:					
	(1)	(2)	(3)	(4)	(5)	(6)
	Own (parental) region of origin	Refugees	Turkey	Lebanon	Countries in Southern Africa	Western EU countries
Negative treatment	5.863*** (1.544)	4.921*** (1.643)	3.524** (1.722)	4.184** (1.709)	5.112*** (1.657)	-1.463 (1.715)
Constant	50.24*** (4.023)	53.23*** (4.377)	50.91*** (4.293)	38.81*** (4.619)	37.49*** (4.501)	41.06*** (4.489)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1159	1159	897	1159	1159	1159
R-squared	0.0378	0.0348	0.0397	0.0481	0.0372	0.0362

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

Results of the OLS regression of the elicited guess (of participants with an immigration background) of the share of majority population participants who evaluated negatively the impact on socio-economic and cultural life in Germany of people immigrating from countries/regions shown in columns' headers. The first column regards the evaluation of the people immigrating to Germany from the country/region of participants' origin (or that of their parent(s) if the participant was born in Germany). In questions that regarded immigrants from a region (rather than a country) all countries within the region were listed in the question. All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Robust standard errors in parenthesis. Individual controls include age (in years), gender, equivalent household income tertile, and an indicator for completion of at least some tertiary education (See Table A4 in Appendix for results with the full covariate set). Results with p-values adjusted for multiple hypothesis testing using the Benjamini–Hochberg false discovery rate procedure are reported in Table A11 in the Appendix.

descriptive norms have been shown to independently influence behavior across a wide range of domains (Krupka and Weber, 2009; Bardsley and Sausgruber, 2005), and particularly in value-laden settings where the two dimensions of norms conflict, perceived descriptive norms predict behavior better than injunctive ones (Álvarez-Benjumea and Winter, 2018; Bicchieri and Xiao, 2009). Definitively establishing the behavioral pathway would require a norm-manipulation experiment and is beyond the scope of this work, but this literature suggests that the observed norm updates may have downstream implications for exclusionary behavior.

The norm elicitation additionally offers a further piece of evidence in support of the sta-

tus manipulation’s effectiveness. To capture the treatment effect on the *relative* status of participants’ ingroup, I examine the difference between participants’ expectations regarding the share of natives who negatively evaluate their immigrant ingroup and their expectations of the share who negatively evaluate a reference low-status immigrant group. Given that the test only asks whether treatment-induced updating was disproportionate for participants’ ingroup relative to other low-status immigrant groups, the choice of the particular reference group is, in principle, inconsequential as long as it credibly occupies a lower status than the native majority and is sufficiently salient in public discourse. To avoid conflating the result with direct information provision, I focus on groups not previously mentioned either in the treatment delivery or in any prior survey question, and take as the reference group Turkish immigrants — the largest and highly salient immigrant community in Germany. For participants with a Turkish immigration background, I instead take as a reference immigrants from Romania and Bulgaria — a group that attracted considerable public scrutiny in Germany following the Eastern enlargement of the EU. The results in Table A12 in the Appendix indicate that the treatment shifted norm expectations disproportionately for participants’ own group relative to the reference group. Together with the results in Table 4, this indicates that, beyond inducing a uniform downward shift of expected evaluations of all low-status groups, the treatment was additionally processed as a signal specifically about the participant’s own group’s standing in the status hierarchy.

Turning to the second test, I rely on one of the attitudinal questions (q6) to study whether participants engage in preference falsification when under observation of native Germans, and how treatment variation affects their propensity to do so. This question asked respondents to rate whether refugees who obtain the right to asylum in Germany make Germany a worse or a better place to live. Participants answered by selecting a number on an 11-points number line, where 0 was indicated as “worse place to live”, and 10 as “better place to live”. At the end of the experiment, the same question is asked again, but this time participants were informed that their answer might be shown to a participant in a future study. Specifically,

participants knew that, if used, their response would be provided to a future participant (in an anonymized form) along with an indication of whether the participant had an immigration background and, if so, from which country, and that the person observing their answer would be a German citizen.

I denote the two answers as private (a_p) and observable (a_o). Note that a higher answer indicates a more supportive attitude toward refugees, and that participants had a possibility to provide a neutral answer by selecting 5 on the number line, exactly in the middle between the two extremes. I construct $\Delta_o = a_o - a_p$ to capture the “additional” support expressed in the observable relative to the private scenario.

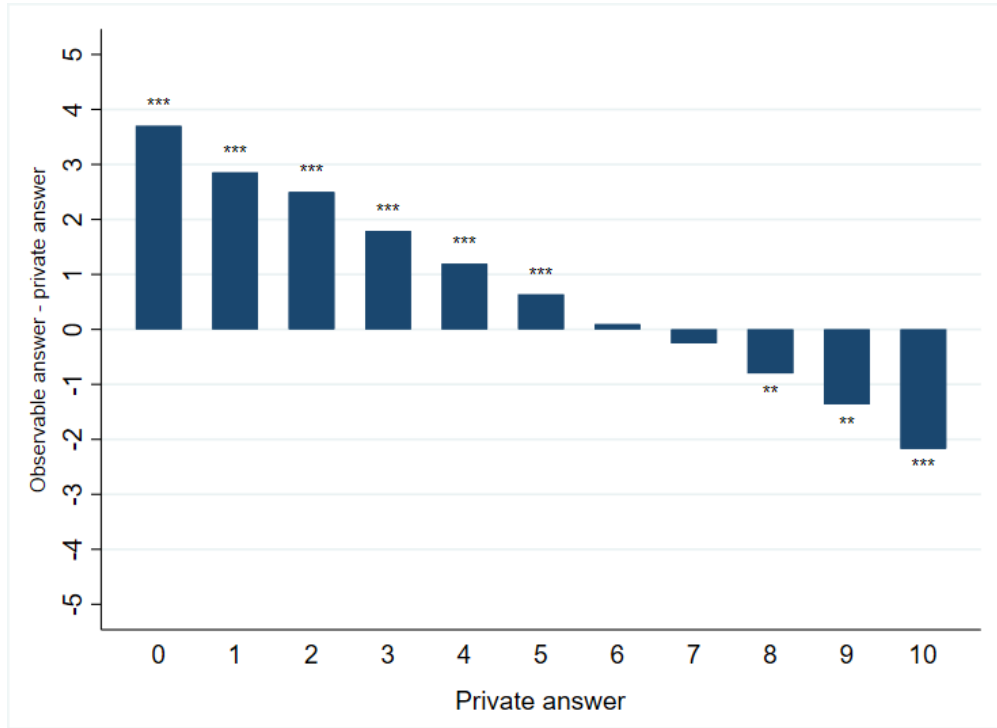


Figure 3: Difference in expressed attitudes - observable vs. private scenario

The figure depicts the average preference falsification, captured as a difference between the answers provided in the “observable” and in the “private” scenario ($\Delta_o = a_o - a_p$). The average difference between the answers is depicted per answer provided in the private scenario. A positive (negative) value indicates that, on average, participants provided an answer implying a more (less) supportive attitude toward refugees when their answer will possibly be observed by a future participant (German citizen) than when answering privately. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ in sign test for $H_0 : \text{median}(\Delta_o) = 0$

Figure 3 plots the average value of Δ_o for each level of a_p . The figure shows that the difference between observable and private answers varies strongly with the attitude initially expressed in the private scenario. Participants who reported lower support in private (any response up to the neutral midpoint of 5) gave, on average, higher answers when their response was observable. More interestingly, participants who expressed highly supportive attitudes in private ($a_p > 7$) systematically reduced their support in the observable scenario. One possible concern here is that the pattern observed in Figure 3 could result from simple mean reversion if participants were selecting answers at random in both scenarios. I discuss this possibility in Appendix section A.11 and provide multiple pieces of evidence speaking against it—most importantly, that the direction of preference falsification changes systematically around the neutral point, a pattern inconsistent with the simple mean reversion. Although this downward adjustment among the most pro-refugee respondents suggests that some immigrants may strategically moderate their views to avoid appearing excessively tolerant in front of natives, the average degree of preference falsification remains positive in both treatments (Positive treatment: mean $\Delta_o = 0.52$, $s.d. = 2.72$; Negative treatment: mean $\Delta_o = 0.56$, $s.d. = 2.55$). These results provide evidence against the notion that immigrants strategically adopt negative stances toward disfavored groups as a form of perverse integration strategy—that is, in an effort to appeal to natives. To the contrary, those most inclined to express negative views appear to believe that doing so will not result in more favorable treatment by the native majority.

Building on the patterns depicted in Figure 3, I next examine how treatment-induced exposure to prejudice shapes preference falsification separately for participants with critical ($a_p < 5$) and supportive ($a_p > 5$) private views. Within both groups, I apply Lee (2009) bounds to correct for bias arising from differential selection into the subsample induced by the post-treatment nature of a_p . Table 5 reports Lee (2009) bounds on the treatment effect on Δ_o . While participants with supportive private attitudes remain unaffected by treatment variation, among those who in private scenario expressed critical attitudes towards refugees

($a_p < 5$), Negative treatment had a negative effect on preference falsification (both bounds are negative, i.e. the identified set excludes zero). Table A13 in the Appendix shows that this effect increases further in size when restricting the sample to progressively more critical individuals (those with $a_p < 4$ and $a_p < 3$). In other words, whereas critical participants falsify their attitudes on average upward—so as to appear more supportive when observed by natives—those exposed to prejudice directed to their immigrant ingroup do so significantly less.

Taken together, these results suggest that expressed prejudice not only negatively affects the privately held attitudes toward refugees of those exposed to it, but also increases the readiness to publicly present biased views, thereby weakening the effect of the social norm against xenophobic expressions.

Table 5: Treatment Effect on Preference Falsification

	Critical private views $a_p < 5$ (1)	Supportive private views $a_p > 5$ (2)
Lower bound	−0.761 [−1.505, −0.016]	−0.140 [−0.658, 0.379]
Upper bound	−0.153 [−0.822, 0.516]	0.197 [−0.299, 0.692]
Selected obs.	394	510
Trimming prop.	0.063	0.033

Notes: The table reports Lee bounds (Lee, 2009) on the effect of Negative treatment on $\Delta_o = a_o - a_p$. The outcome variable is set to missing outside each subsample, allowing Lee bounds to account for different selection rates into the respective subsamples. 95% bootstrap confidence intervals in brackets (1,000 replications). Total $N = 1,159$ in all specifications. The interval defined by the bounds excludes zero in column (1), indicating a negative treatment effect on preference falsification among participants with critical immigration attitudes (as expressed in private scenario). The treatment did not affect preference falsification among participants with supportive views of immigration.

4 Discussion

Several design aspects merit explicit discussion before turning to the broader implications of the findings.

First, while the paper proposes two complementary mechanisms — identity threat and norm updating — precisely quantifying the relative contribution of each is difficult, and this difficulty is not unique to the present design. Any exposure to prejudice directed at one’s group simultaneously constitutes an identity-relevant experience and a piece of social information about the prevalence and tolerance of such behavior in the host society. These two aspects of the same experience generate conceptually distinct predictions — identity threat motivates downward derogation as a compensatory strategy to restore positive ingroup distinctiveness, while norm updating reduces the perceived social cost of expressing such derogation — but both channels operate in the same direction for the outcomes of interest. It is straightforward to envision an alternative design delivering a norm update free of any status signal, as is already done in the literature on discriminatory norms ([Bursztyn et al., 2020](#); [Álvarez-Benjumea and Winter, 2018](#); [Álvarez-Benjumea, 2023](#)) — but this approach would respond to a different research question than the one posed here. On the other hand, delivering a status shock free of prejudiced content is considerably more difficult: defined as the degree of social esteem, honor, and respect accorded to a group ([Ridgeway, 2019](#); [Weber, 1968](#)), status is by construction an evaluative concept, and a group cannot learn that it occupies a low position in the social hierarchy without simultaneously learning that it is regarded with less esteem by those above it. Taken together, while the present design cannot quantify how much each channel contributes to the observed effects, but it can establish that both are activated.

Second, the choice of refugees from the Middle East as the target group is motivated by the salience and political relevance of this group in the German context and, more importantly, by their consistently low position in the perceived ethnic hierarchy. However, the theoretical prediction is not specific to this group — it is expected to apply to any setting in which the target group credibly occupies a lower position than the respondent’s own group in the status hierarchy. A natural extension of the results presented here would therefore involve implementing a similar test with alternative low-status target groups, which would

also alleviate concerns that refugee status entails specific features limiting the generalizability of the results. The key challenge in designing such an extension lies in maintaining the diversity of the respondent pool while ensuring the status ordering remains credible for each participant. One approach would be to tailor the target group to each respondent’s immigrant origin, at the cost of introducing heterogeneity in the target outgroup across respondents. Another possibility is to select a group widely perceived as low-status relative to most established immigrants but without refugee background. Yet in such cases, the very characteristic that places the group at the bottom of the hierarchy may itself be seen as a distinctive group feature — much like refugee status in the present setting — thus replacing one group-specific feature with another rather than eliminating it.

Third, the design does not include a neutral control condition. This is a deliberate design choice: what constitutes a genuine neutral baseline in this setting is not straightforward. Presenting participants with evaluations of other immigrant groups while withholding any evaluation of their own ingroup cannot prevent participants from wondering what native Germans would say about their group — a counterfactual that varies with each participant’s prior beliefs about their group’s standing and remains unobserved. The two-arm design avoids this by ensuring that in both conditions the researcher knows precisely what signal participants receive regarding the standing of their own group, allowing the effect of that signal’s valence to be cleanly identified.

These considerations notwithstanding, the findings speak to a broader pattern that is expected to extend well beyond the specific setting studied here: that the treatment immigrants encounter in the host society shapes not only their own sense of belonging but also how they relate to those positioned even lower in the social hierarchy. Probing the scope conditions and disentangling the contributing mechanisms in greater detail provide natural next steps that build on the results presented here.

5 Conclusion

The largest share of scholarly work on immigration attitudes focuses on native majorities, in part because immigrants are typically assumed to hold more pro-immigration views (Just and Anderson, 2015; Berry, 2006). Recent examples — from Latino support for Trump to immigrant-origin voters backing anti-immigration referendums — raise questions about when and why immigrant-origin populations endorse positions seemingly at odds with their own migration histories.¹⁷

This article shifts the focus in studying immigration attitudes to immigrants themselves and asks how the reception they encounter in the host society shapes their views of other immigrant groups. I propose a theory that holds that encountering critical treatment in the host society leads immigrants to “kick down the ladder”—that is, to direct negative attitudes toward groups occupying an even lower position in the local ethnic hierarchy. I formulate the theoretical expectation on the basis of two complementary mechanisms that generate this prediction and demonstrate empirical support for both.

Using an experiment with immigrants in Germany from a wide variety of sending countries, I show that a simple piece of information about natives’ evaluation of one’s own immigrant group has meaningful downstream effects. Receiving a negative evaluation leads immigrants to express less support for refugees from the Middle East and donate less to an organisation supporting them. Participants also update their norm perceptions accordingly, expecting the native majority to express more negative views of other low-status groups (but not of high-status ones) — and, among those with privately critical views, become less willing to conceal those views when observed by native Germans.

Immigrants of the first and second generation make up a growing share of the population in most Western societies and are likely to become increasingly electorally consequential in the decades to come. Beyond voting, attitudes toward immigration shape a wide range

¹⁷Artificial intelligence tools—specifically OpenAI’s ChatGPT and Anthropic’s Claude—were used exclusively for minor language editing and stylistic reformulation of selected passages. All substantive content, analysis, and interpretations are the sole work of the author.

of political behaviors, from protest participation to engagement in violence, and can spill over into broader political preferences (Alesina et al., 2023). Moreover, recent work shows that immigrants’ political attitudes can, through social interaction, influence those of native populations over time, potentially altering political equilibria (Bazzi and Fiszbein, 2025). These dynamics underscore the importance of understanding the sources of immigration attitudes among immigrants themselves. This study contributes to this effort by showing that the common assumption that immigrants are systematically more supportive of left-wing platforms (Moriconi et al., 2022) and more favorable toward immigration (Berry, 2006; Dandy and Pe-Pua, 2010; Hindriks et al., 2017) cannot be taken for granted. Instead, immigrants’ attitudes toward other migrant groups are malleable and respond in systematic ways to the treatment they encounter in the host society.

Finally, these results carry broader policy implications. Debates over political correctness have long centered on concerns that social pressure may induce individuals to publicly espouse views they do not privately hold, potentially obscuring genuine preferences and rendering public discourse less informative. Such concerns are understandable: identifying an appropriate trade-off between protecting individuals from exposure to stereotyping and prejudice, on the one hand, and allowing space for genuine disagreement and open deliberation, on the other, is inherently difficult. The findings of this study point to an additional—and often overlooked—benefit of these norms. By constraining the public expression of prejudice, norms of political correctness may not only shield groups that are currently exposed to it, but also limit the cascading spread of exclusionary attitudes across groups.

Supplementary Material

Queen Bee Immigrant: The Effects of Status Perceptions on Immigration Attitudes

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A Appendix

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A.1 Pre-study evaluations

Figure A1 shows the share of the Pre-study participants who evaluated *negatively* the contribution of listed immigrant groups to the “socio-economic and cultural life in Germany”.

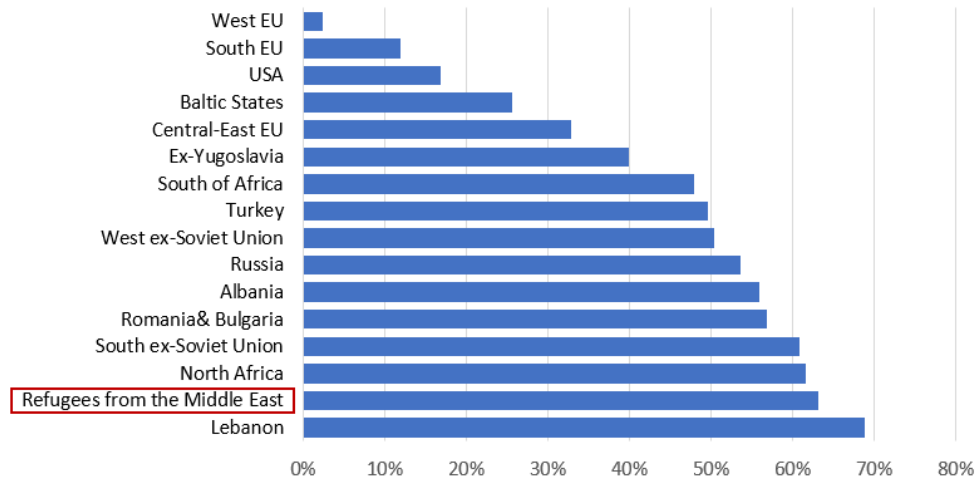


Figure A1: Native participant’ evaluations of immigrant groups

The figure displays for each depicted immigrant group, the share of native German participants who evaluated as “rather negative” the contribution of this immigrant group to the “socio-economic and cultural life in Germany”.

A.2 Sample description

Table A1 shows the distribution of the sample across the regions of origin and the basic demographic characteristics of the sample.

Table A1: Sample description: Regions of origin

	Positive treatment	Negative treatment	Total
Region of (parental) origin			
Bulgaria & Romania	0.065	0.078	0.072
Central-Eastern European Union (Czech Republic, Slovakia, Poland, Hungary)	0.182	0.172	0.177
Baltic states (Estonia, Lithuania, Latvia)	0.011	0.007	0.009
Ex-Yugoslavia (Bosnia and Herzegovina, Croatia, Kosovo, Montenegro, North Macedonia, Serbia, Slovenia)	0.058	0.084	0.072
North Africa (Morocco, Algeria, Lybia, Tunesia and Egypt)	0.078	0.068	0.073
Southern European Union countries (Greece, Italy, Portugal, Spain, Cyprus and Malta)	0.106	0.145	0.127
Turkey	0.249	0.205	0.226
Southern Ex-Soviet union (Tajikistan, Turkmenistan, Georgia, Kazakhstan, Kyrgyzstan, Armenia and Azerbaijan)	0.063	0.073	0.068
Western Ex-Soviet union (Ukraine, Moldova, Belarus)	0.033	0.028	0.030
Russian federation	0.092	0.084	0.088
Albania	0.063	0.056	0.060
Age			
[18-24]	0.338	0.349	0.343
[25-34]	0.300	0.284	0.292
[35-44]	0.182	0.175	0.179
[45-54]	0.108	0.116	0.112
[55-64]	0.060	0.063	0.061
[65-74]	0.011	0.010	0.010
[75-84]	0.002	0.003	0.003
Gender			
Male	0.457	0.438	0.447
Education			
Primary or lower secondary	0.354	0.331	0.342
Secondary	0.233	0.238	0.236
Tertiary	0.413	0.431	0.423
Observations	554	605	1,159

Notes: Regions of participants' own or parental origin across treatments.

A.3 Distribution of pledged donations

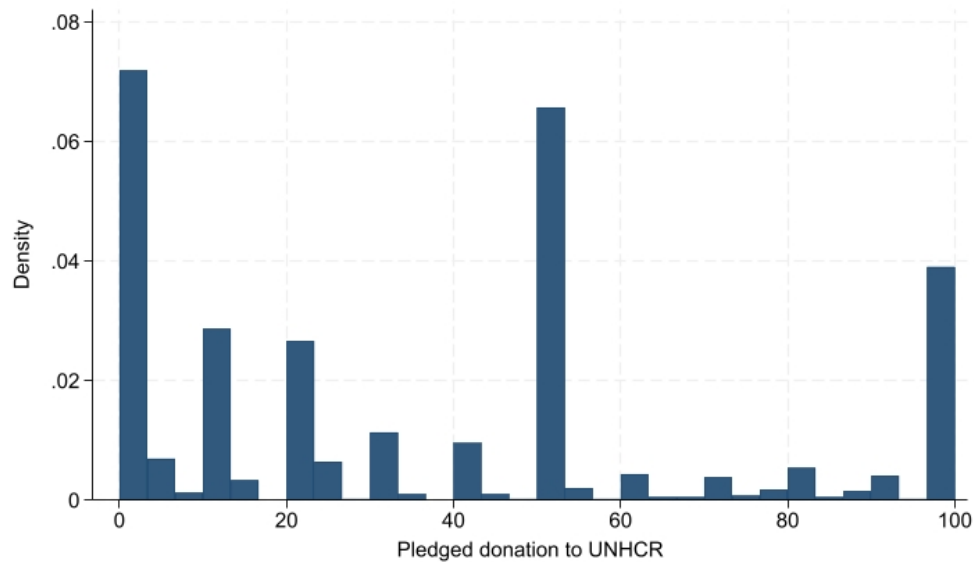


Figure A2: Distribution of pledged donations to UNHCR

A.4 Main results with full covariate specification

This subsection reproduces the results reported in tables 2 through 4, including the full set of covariates. All specifications include fixed effects for participants' region of origin and federal state of residence in Germany, omitted from the tables for brevity. Table [A5](#) reproduces the results regarding treatment effect on attitudinal measures reported in Table [3](#), while excluding individual controls from estimation.

Table A2: Treatment effects: Pledged donation to the UNHCR (Full covariate list)

	(1)	(2)	(3)	(4)
	Pledged donation		Pr(Donation>0)	
Negative treatment	-4.383** (2.004)	-4.316** (2.003)	-0.0534** (0.0242)	-0.0528** (0.0242)
Age		-0.187** (0.086)		-0.003*** (0.001)
Tertiary education		1.320 (2.509)		-0.002 (0.030)
Equivalised income tertile		0.795 (1.350)		-0.008 (0.017)
Male		0.675 (2.027)		0.018 (0.025)
Constant	45.80*** (4.778)	50.50*** (5.523)	0.848*** (0.0524)	0.955*** (0.0635)
Individual controls	No	Yes	No	Yes
Observations	1159	1159	1159	1159
R-squared	0.0402	0.0444	0.0351	0.0440

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

Column (1) and column (2) depict the results of the OLS regression of the pledged donation to the UNHCR on the treatment variable and the set of individual controls. Columns (3) and (4) show the results of the OLS regression of the dummy variable that takes value one if a participant pledged to donate a value larger than zero on the treatment variable and the set of individual controls. Negative treatment indicates receiving negative status information regarding own in-group (with Positive treatment serving as a baseline). All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Robust standard errors in parentheses.

Table A3: Treatment effects: Attitudinal questions (Full covariate list)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	q1 Allow more	q2 Jobs	q3 Expense	q4 Terrorism	q5 Crime	q6 Impact	q7 Reason
Negative treatment	-0.147** (0.0596)	-0.0627 (0.0714)	-0.0229 (0.0680)	-0.0289 (0.0720)	0.0179 (0.0702)	-0.238 (0.156)	-0.0163 (0.0274)
Age	-0.017*** (0.003)	-0.016*** (0.003)	-0.017*** (0.003)	-0.018*** (0.003)	-0.013*** (0.003)	-0.024*** (0.007)	-0.008*** (0.001)
Tertiary education	0.088 (0.075)	0.268*** (0.085)	0.233*** (0.085)	0.210** (0.090)	0.166* (0.087)	0.308 (0.195)	0.022 (0.034)
Equivalised income tertile	0.033 (0.040)	0.048 (0.049)	0.012 (0.047)	-0.049 (0.050)	-0.028 (0.047)	-0.023 (0.107)	-0.014 (0.018)
Male	0.046 (0.060)	-0.047 (0.073)	-0.010 (0.069)	0.023 (0.074)	-0.029 (0.072)	0.243 (0.158)	0.017 (0.028)
Constant	3.350*** (0.147)	3.495*** (0.179)	3.126*** (0.170)	3.628*** (0.194)	3.335*** (0.187)	6.171*** (0.409)	0.889*** (0.0744)
Observations	1159	1159	1159	1159	1159	1159	1159
R-squared	0.102	0.0669	0.104	0.103	0.0617	0.0827	0.0642

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

Results of the OLS regression of participants' answers responding to the respective 6 attitudinal questions on the treatment variable and the set of individual controls. All answers are re-coded such that a higher value indicates higher support for refugees. Outcome in the last column takes value 1 if a participant selected "To flee war" or "Avoid political persecution" as the primary reason why refugees leave their countries, and 0 otherwise. All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Standard errors in parentheses are clustered on the level of region of participants' (parental) origin. Robust standard errors in parenthesis.

Table A4: Treatment effects: Empirical expectations (Full covariate list)

	(1)	(2)	(3)	(4)	(5)	(6)
Participant's estimation of the percentage of majority population participants who negatively evaluated the impact of people coming to Germany from:						
	Own (parental) region of origin	Refugees	Turkey	Lebanon	Countries in Southern Africa	Western EU countries
Negative treatment	5.863*** (1.544)	4.921*** (1.643)	3.524** (1.722)	4.184** (1.709)	5.112*** (1.657)	-1.463 (1.715)
Age	-0.0136 (0.0639)	0.0773 (0.0700)	-0.0449 (0.0714)	0.187** (0.0731)	0.177** (0.0720)	-0.0368 (0.0738)
Tertiary education	-2.190 (1.838)	1.441 (1.986)	2.258 (2.080)	3.205 (2.070)	1.233 (2.031)	-1.457 (2.149)
Equivalised income tertile	-0.990 (1.037)	-0.701 (1.087)	0.272 (1.171)	0.740 (1.141)	0.208 (1.124)	-1.605 (1.198)
Male	0.237 (1.583)	-3.234* (1.681)	-0.459 (1.761)	-2.645 (1.752)	-2.360 (1.693)	1.891 (1.755)
Constant	50.24*** (4.023)	53.23*** (4.377)	50.91*** (4.293)	38.81*** (4.619)	37.49*** (4.501)	41.06*** (4.489)
Observations	1159	1159	897	1159	1159	1159
R-squared	0.0378	0.0348	0.0397	0.0481	0.0372	0.0362

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

Results of the OLS regression of the elicited guess (of participants with an immigration background) of the share of majority population participants who evaluated negatively the impact on socio-economic and cultural life in Germany of people immigrating from countries/regions shown in columns' headers. The first column regards the evaluation of the people immigrating to Germany from the country/region of participants' origin (or that of their parent(s) if the participant was born in Germany). In questions that regarded immigrants from a region (rather than a country) all countries within the region were listed in the question. All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Robust standard errors in parenthesis.

Table A5: Treatment effects: Attitudinal questions (Excluding individual controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	q1 Allow more	q2 Jobs	q3 Expense	q4 Terrorism	q5 Crime	q6 Impact	q7 Reason
Negative treatment	-0.152** (0.0607)	-0.0655 (0.0723)	-0.0256 (0.0690)	-0.0305 (0.0733)	0.0175 (0.0707)	-0.246 (0.157)	-0.0174 (0.0279)
Constant	2.868*** (0.125)	3.094*** (0.145)	2.659*** (0.139)	3.084*** (0.160)	2.950*** (0.154)	5.573*** (0.342)	0.644*** (0.0644)
Observations	1159	1159	1159	1159	1159	1159	1159
R-squared	0.0667	0.0370	0.0720	0.0726	0.0443	0.0698	0.0281

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

Results of the OLS regression of participants' answers responding to the respective 6 attitudinal questions on the treatment variable and the set of individual controls. All answers are re-coded such that a higher value indicates higher support for refugees. Outcome in the last column takes value 1 if a participant selected "To flee war" or "Avoid political persecution" as the primary reason why refugees leave their countries, and 0 otherwise. All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Standard errors in parentheses are clustered on the level of region of participants' (parental) origin. Robust standard errors in parenthesis.

A.5 Indirect reciprocity

Upstream indirect reciprocity designates a tendency of individuals to exhibit prosocial behavior toward others because somebody else has exhibited prosocial behavior toward them (Alexander (1987), Nowak and Sigmund (2005)). Previous studies have provided evidence for upstream indirect reciprocity, both in the laboratory (Greiner and Levati (2005)) and in field experiments (Mujic and Leibbrandt (2018)). Exhibiting upstream indirect reciprocity in the context of inter-minorities relations would suggest that receiving a less (more) favorable evaluation from an out-group might translate into a less (more) favorable view of another out-group. As such, indirect reciprocity might amplify the effects of the treatment—we would expect more reciprocal participants to react more negatively (positively) in terms of their support for refugees if they were assigned to the Negative treatment (Positive treatment)—or even operate as an alternative explanation for the detected results.

To assess this possibility, I construct a measure of upstream indirect reciprocity based on an extended dictator game. In this task, participants decide how much to transfer to an anonymous recipient under two conditions: one in which *a different participant*, who I will refer to as the “sender”, had previously acted generously toward them, and another in which that participant had acted less generously (for details, please see description of the extended dictator game in section on experimental design). I take the difference in the amount sent to anonymous recipient in the former scenario and latter scenario as a measure of participant’s indirect upstream reciprocity. In order to collect this measure for as many participants as possible, most of the participants were assigned the role of dictator ($n = 1150$), and the rest was distributed among the other two roles (sender or recipient).

From the received transfer (which for all participants equaled 32 euros), participants on average sent 1.21 euros more to the anonymous recipient when sender sent them a higher amount compared to when they sent a lower amount (average amounts sent in two cases were 13.99 and 12.79 euros). This difference is significant (Wilcoxon signed-rank test: $z = 9.544$, $p < 0.001$), providing evidence for behavior consistent with indirect upstream reciprocity.

Furthermore, the distribution of the measure of indirect reciprocity does not differ among treatments (Kolmogorov–Smirnov test: $p = 0.785$), supporting the view of reciprocity as a basic preference.

Table A6: Treatment effects: The role of indirect reciprocity

	(1)	(2)	(3)	(4)
	Pledged donation		Pr(Donation>0)	
Negative treatment × Reciprocity	-0.529 (0.393)	-0.508 (0.390)	-0.00892** (0.00404)	-0.00823** (0.00400)
Negative treatment	-3.515* (2.061)	-3.464* (2.060)	-0.0412* (0.0246)	-0.0410* (0.0245)
Reciprocity	0.335 (0.293)	0.304 (0.291)	0.00248 (0.00309)	0.00198 (0.00306)
Age		-0.183** (0.0861)		-0.00336*** (0.00113)
Tertiary education		0.585 (2.510)		-0.00176 (0.0307)
Equivalised income tertile		0.941 (1.359)		-0.00529 (0.0167)
Male		0.614 (2.040)		0.0195 (0.0247)
Observations	1150	1150	1150	1150
R-squared	0.0414	0.0454	0.0398	0.0491

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

Column (1) shows the results of the Tobit regression of the amount that participants pledged to donate to the UNHCR on the treatment variable, a measure of upstream indirect reciprocity (denoted Ind. reciprocity), and their interaction. Column (2) shows the results of the Probit regression of the dummy variable that takes value one if a participant pledged to donate a value larger than zero on the same set of regressors. All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Standard errors are clustered on the level of participants' (parental) region of origin.

Table A6 provides the results of the regression of the amount pledged to donate and that of the dummy variable indicating that the participant pledged a positive donation on the treatment variable, measure of indirect reciprocity and their interaction. The results indicate that indirect reciprocity indeed had some role in determining the decision to donate. Whereas indirect reciprocity in the Positive treatment increased, albeit insignificantly, the pledged donation and the probability to donate, it significantly reduced both values in the Negative treatment. However, although providing some evidence for the role of indirect upstream

reciprocity, these effects are relatively small and do not provide a systematic explanation of the found treatment effects—the treatment variable remains significant.

A.6 The role of participants' socio-economic characteristics

Table A7 restates the results of the regression of participants' pledged donation to the UNHCR as depicted in Table 2, but with the addition of controlling for the interaction of the treatment variable with the measure of income (column (1)), and its interaction with the indicator of participants having completed any post-secondary education (column (2)).

Table A7: Treatment effects: Role of participants' socio-economic characteristics

	(1) Pledged donation	(2) Pledged donation	(3) Donation>0	(4) Donation>0
Negative treatment×Equivalised income tertile	-1.984 (2.574)		0.0119 (0.0319)	
Negative treatment×Tertiary Education		-1.906 (4.724)		0.0129 (0.0573)
Negative treatment	-2.554 (2.945)	-3.856* (2.287)	-0.0634* (0.0363)	-0.0559** (0.0281)
Equivalised income tertile	1.809 (1.961)	0.776 (1.352)	-0.0142 (0.0227)	-0.00792 (0.0167)
Age	-0.185** (0.0856)	-0.189** (0.0857)	-0.00324*** (0.00112)	-0.00321*** (0.00112)
Tertiary education	1.237 (2.508)	2.325 (3.700)	-0.00113 (0.0305)	-0.00841 (0.0416)
Male	0.716 (2.032)	0.676 (2.028)	0.0175 (0.0246)	0.0177 (0.0246)
Constant	49.53*** (5.648)	50.32*** (5.551)	0.961*** (0.0644)	0.956*** (0.0638)
Observations	1159	1159	1159	1159
R-squared	0.0449	0.0446	0.0442	0.0441

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

Column (1) shows the results of OLS regression of the pledged donation to the UNHCR on the treatment variable, equivalent household income tertile, and their interaction. Column (2) shows the results of OLS regression of the amount pledged to donate to the UNHCR on the treatment variable, indicator variable for tertiary education, and their interaction. Columns (3) and (4) show the results of the same analysis with an outcome set equal to a dummy variable that takes value one if a participant pledged to donate a value larger than zero. All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Robust standard errors in parenthesis.

A.7 Treatment effect on dictator giving

Table A8 reports regressions of dictator giving in each of the two scenarios—when “person A” was more generous (columns 1–2) and when they were less generous (columns 3–4). Across all specifications, the treatment has no discernible effect on the amount participants chose to send to the anonymous recipient.

Table A8: Treatment effects: Dictator giving

	(1)	(2)	(3)	(4)
	Scenario 1		Scenario 2	
Negative treatment	0.0663 (0.543)	0.101 (0.543)	0.172 (0.522)	0.208 (0.521)
Age		-0.0454** (0.0226)		-0.0655*** (0.0211)
Tertiary education		-0.0236 (0.683)		0.518 (0.653)
Equivalised income tertile		0.210 (0.370)		0.536 (0.350)
Male		0.930 (0.566)		0.506 (0.541)
Constant	14.54*** (1.381)	15.50*** (1.558)	16.06*** (1.309)	17.36*** (1.462)
Observations	1150	1150	1150	1150
R-squared	0.0309	0.0364	0.0272	0.0370

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

The table shows the results of OLS regressions of dictator giving. The dependent variable is the amount that a participant in the role of “person B” (i.e. the dictator) sent to “person C” (i.e. the recipient). As participants made decisions in two different scenarios, the first two columns show the regression of dictator giving in scenario where “person A” sent lower amount to the participant. Correspondingly, in the last two columns the dependent variable equals dictator giving when “person A” sent the participant a higher amount. Negative treatment indicates receiving a negative evaluation of the own in-group (with Positive treatment serving as a baseline). Reported marginal effects represent the average marginal effect of being allocated to Negative treatment on the dictator giving. All regressions include fixed effects for the federal state of residence in Germany and for participants’ (parental) region of origin. Robust standard errors in parentheses.

A.8 The role of participants' ethnicity

Table A9 repeats the analysis of pledged donation to UNHCR, while adding an indicator set to one for participants with an immigration background in countries with predominantly Muslim populations and its interaction with the treatment variable.

Table A9: Treatment effects: Muslim background

	(1)	(2)	(3)	(4)
	Pledged donation		Pr(Donation>0)	
Negative treatment×Majority Muslim	-0.700 (4.460)	-0.674 (4.467)	0.0273 (0.0524)	0.0259 (0.0522)
Negative treatment	-4.172* (2.384)	-4.112* (2.379)	-0.0617** (0.0294)	-0.0606** (0.0294)
Majority Muslim	-8.425* (5.077)	-8.908* (5.106)	-0.0679 (0.0553)	-0.0811 (0.0556)
Age		-0.187** (0.0857)		-0.00322*** (0.00112)
Tertiary Education		1.321 (2.510)		-0.00167 (0.0304)
Equivalised income tertile		0.790 (1.350)		-0.00786 (0.0167)
Male		0.673 (2.028)		0.0178 (0.0246)
Constant	45.70*** (4.795)	50.41*** (5.538)	0.852*** (0.0527)	0.958*** (0.0638)
Observations	1159	1159	1159	1159
R-squared	0.0402	0.0444	0.0353	0.0442

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

Column (1) and column (2) show the results of the OLS regression of the amount that a participant pledged to donate to the UNHCR on the treatment variable and the set of individual controls. Negative treatment indicates receiving a negative evaluation of the own in-group (with Positive treatment serving as a baseline). is an indicator coded for participants with an immigration background in countries with predominantly Muslim populations (Algeria, Egypt, Libya, Morocco, Tunisia, and Turkey). Reported marginal effects represent the average marginal effect of being allocated to Negative treatment on the pledged donation value. Columns (3) and (4) show the results of OLS regression of the dummy variable that takes value one if a participant pledged to donate a value larger than zero on the treatment variable and the set of individual controls. All regressions include fixed effects for the federal state of residence in Germany and for participants' (parental) region of origin. Individual controls (included in columns (2) and (4)) include age, gender, equivalent household income tertile, and indication of tertiary education. Robust Standard errors in parentheses.

A.9 Multiple Hypothesis Adjusted Results

Tables A10 and A11 report estimated treatment effects from regressions of immigrants' attitudinal responses (Table A10) and empirical expectations (Table A11), with p-values adjusted for multiple hypothesis testing using the Benjamini–Hochberg false discovery rate procedure.

Table A10: Treatment effects: Attitudinal questions with BH-adjusted p-values

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	q1 Allow more	q2 Jobs	q3 Expense	q4 Terrorism	q5 Crime	q6 Impact	q7 Reason
Negative treatment	-0.147** [0.097]	-0.063 [0.799]	-0.023 [0.799]	-0.029 [0.799]	0.018 [0.799]	-0.238 [0.448]	-0.016 [0.799]
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1159	1159	1159	1159	1159	1159	1159
R-squared	0.102	0.067	0.104	0.103	0.062	0.083	0.064

Notes: Entries are estimated coefficients from OLS regressions of the outcome indicated in each column on the treatment variable (Negative treatment). Significance levels and bracketed p-values are adjusted for multiple hypothesis testing using the Benjamini–Hochberg false discovery rate procedure.

Table A11: Treatment effects: Empirical expectations

Elicited expectation:	What percentage of majority population participants evaluated negatively the impact of people coming to Germany from:					
	(1)	(2)	(3)	(4)	(5)	(6)
	Own (parental) region of origin	Refugees	Turkey	Lebanon	Countries in Southern Africa	Western EU countries
Negative treatment	5.863*** [0.001]	4.921*** [0.006]	3.524** [0.049]	4.184** [0.022]	5.112*** [0.006]	-1.463 [0.394]
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1159	1159	897	1159	1159	1159
R-squared	0.038	0.035	0.040	0.048	0.037	0.036

Notes: Entries are estimated coefficients from OLS regressions of the outcome indicated in each row on the treatment variable (negative treatment). Significance levels and bracketed p-values are adjusted for multiple hypothesis testing using the Benjamini–Hochberg false discovery rate procedure.

A.10 Relative expectation updating

Table A12: Differential updating of empirical expectation: Own ingroup relative to the reference group

	(1)	(2)
	Δ Emp. expectation (Own ingroup – Reference group)	
Negative treatment	3.180*	3.185*
	(1.925)	(1.926)
Age		0.00251
		(0.0828)
Tertiary education		-3.361
		(2.293)
Equivalised income tertile		-1.672
		(1.247)
Male		2.554
		(1.970)
Constant	-3.064	-1.385
	(3.897)	(4.753)
Observations	1159	1159
R-squared	0.0210	0.0264

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

OLS regression of the outcome set equal to the difference in expected share of natives who evaluated *negatively* participant’s own immigrant ingroup minus the share who negatively evaluated the reference outgroup. Turkish immigrants serve as a reference outgroup for all participants except for those themselves of Turkish origin, for whom immigrants from Romania and Bulgaria serve as the reference. Negative treatment indicates receiving a negative evaluation of the own in-group (with Positive treatment serving as a baseline). All regressions include fixed effects for the federal state of residence in Germany and for participants’ (parental) region of origin. Robust standard errors in parentheses.

A.11 Average preference falsification

One concern regarding the results on average preference falsification is that the pattern observed in Figure 3 could reflect statistical mean reversion rather than genuine preference falsification. Several findings argue against this interpretation.

First, the distributions of a_p and a_o both differ significantly from the uniform distribution (Kolmogorov-Smirnov tests, both $p < 0.001$), and a_p is significantly correlated with a_o , with answers to all other attitudinal questions (Pearson ρ between 0.40 and 0.47, $p < 0.001$ in all

pairwise tests), and with the donation measure ($\rho = 0.23$, $p < 0.001$). This rules out the possibility that participants selected answers randomly.

Second, as shown in Figure A3, the observed degree of preference falsification is systematically and significantly lower in magnitude than what would be expected under pure mean reversion — that is, participants adjust their observable answer toward the center, but far less than a random resampling of a_o from the observed distribution would predict.

Third, and most directly, both pure mean reversion and any mixture of mean reversion with a constant falsification tendency predict that the probabilities of upward and downward falsification should change *smoothly* as a_p moves away from the grand mean — with no reason for a discontinuous jump at any particular value. Figure A4 shows instead that both probabilities change sharply and discontinuously precisely at the scale midpoint. The probability of upward falsification ($\Delta_o > 0$) drops by 24 percentage points between $a_p = 4$ and $a_p = 5$ (from 0.63 to 0.39, $z = 4.16$, $p < 0.001$), while no other adjacent transition is significant at conventional levels. Symmetrically, the probability of downward falsification ($\Delta_o < 0$) rises by 20 percentage points between $a_p = 5$ and $a_p = 6$ (from 0.14 to 0.34, $z = 4.38$, $p < 0.001$), again with no comparable transition elsewhere. In other words, participants appear considerably more compelled to present more supportive views than they privately hold as long as their private answer was below the neutral midpoint — and more compelled to conceal privately positive attitudes as long as their private answer was above it. The concentration of both breaks uniquely at $a_p = 5$ is consistent with participants anchoring their observable response to the scale midpoint as a social norm, and is inconsistent with any model in which the observable answer is generated by some combination of reversion toward the grand mean and a constant falsification tendency.

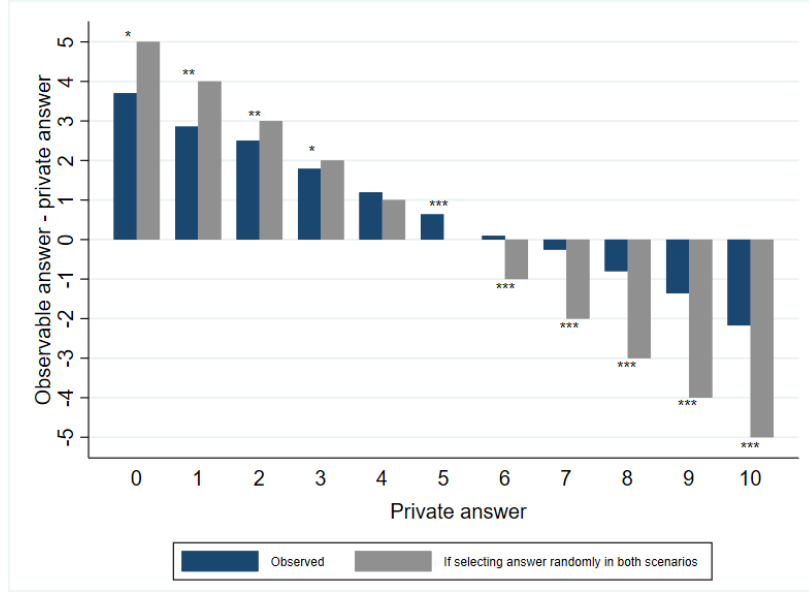


Figure A3: Observed and theoretical preference falsification

The figure depicts the average preference falsification, captured as a difference between the answers provided in the “observable” and in the “private” scenario ($\Delta_o = a_o - a_p$). The blue bars depict the average preference falsification observed in the sample. The grey bars depict preference falsification that would be expected if both a_p and a_o were selected randomly. Both values are depicted per answer provided in the private scenario. Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ in sign test for the equality of medians of observed (Δ_o) and the one that would result under random selection of a_o and a_p .

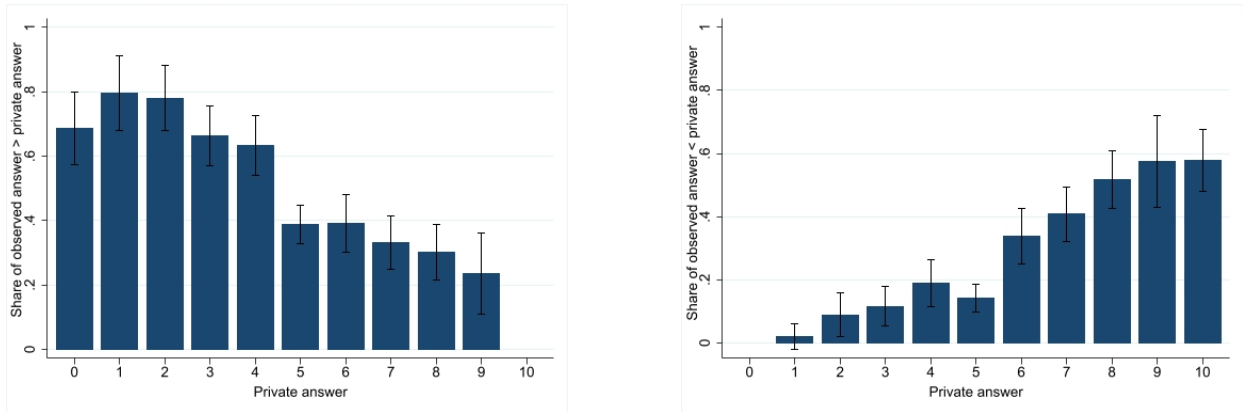


Figure A4: Share of participants with $\Delta_o > 0$ (left) and participants with $\Delta_o < 0$ (right), per private answer

The upper panel depicts the share of participants who provided a higher answer (a more positive view) in “observable” than in the “private” scenario ($a_o > a_p$). The lower panel depicts the share of participants who provided a lower (a more critical view) answer in “observable” than in the “private” scenario ($a_o < a_p$). Vertical lines designate the 95% confidence intervals.

A.12 Treatment effect on preference falsification

Table A13 restates the main result from Table 5 using Lee bounds (Lee, 2009) across progressively narrower subsamples. Among participants with critical private views, both bounds remain negative and the identified set excludes zero across all three specifications, with the magnitude increasing as the subsample is restricted to more extreme private views (columns (1)–(3)). No treatment effect is detected among participants with supportive private views (columns (4)–(6)).

Table A13: Treatment Effect on Preference Falsification

	Critical private views			Supportive private views		
	$a_p < 5$ (1)	$a_p < 4$ (2)	$a_p < 3$ (3)	$a_p > 5$ (4)	$a_p > 6$ (5)	$a_p > 7$ (6)
Lower bound	-0.761 [-1.505, -0.016]	-0.649 [-1.463, 0.165]	-1.648 [-2.824, -0.472]	-0.140 [-0.658, 0.379]	-0.448 [-1.096, 0.200]	-0.258 [-1.048, 0.532]
Upper bound	-0.153 [-0.822, 0.516]	-0.377 [-1.179, 0.424]	-0.546 [-1.650, 0.559]	0.197 [-0.299, 0.692]	0.186 [-0.395, 0.766]	0.338 [-0.433, 1.108]
Selected obs.	394	288	184	510	395	265
Trimming prop.	0.063	0.023	0.122	0.033	0.070	0.063

Notes: The table reports Lee bounds (Lee, 2009) on the effect of Negative treatment on $\Delta_o = a_o - a_p$. The outcome variable is set to missing outside each subsample, allowing Lee bounds to account for different selection rates into the respective subsamples. 95% bootstrap confidence intervals in brackets (1,000 replications). Total $N = 1,159$ in all specifications. The interval defined by the bounds excludes zero in columns (1)–(3), indicating a negative treatment effect on preference falsification among participants with (progressively more) critical immigration attitudes. The treatment did not affect preference falsification among participants with supportive views of immigration.

B Pre-Analysis Plan

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B.1 Introduction

The indications that AfD, an Euro-sceptic right-wing party in Germany that based the core of its platform on opposing immigration, had reached higher electoral support in the 2017 federal election among the so-called Russian-speaking German community compared to the national average ([Goerres et al., 2020](#)), attracted a lot of media attention in Germany. Indeed, this is seemingly counter-intuitive – why would groups who themselves have a history of immigration and are also largely perceived by natives as immigrants support anti-immigration platforms? This is however not a sole example of such dynamics. Cases of negative immigration attitudes expressed by groups of immigrants were also found for example in Switzerland ([Strijbis and Polavieja, 2018](#)), Belgium ([Meeusen et al., 2019](#)) and Austria ([Neuhold, 2020](#)).

The question that arises in this context is that of the expected position of established immigrants, that is those who have already resided in the host country for a longer time, toward new flows of immigration, and the drivers behind these positions. This work focuses on studying the dynamics of inter-minority relations and attempts to uncover the influence of the minority group’s status position in the host country on its members’ attitudes towards other minorities. I hypothesize that relative status deprivation, that is the negative difference in status between one’s own ethnic/national group and that of the native majority (or other, more favourably perceived minorities), has a negative impact on group members’ attitudes toward an even lower-ranked status group (e.g. refugees).

The tendency of individuals to classify themselves and others into in- and out-groups as well as the competition for status is a well-documented and seemingly universal characteristic of human societies. In the context of a country populated by a high-status majority and some immigrant minority groups, the appearance of a new immigrant group can trigger a redistribution of status and open a possibility for groups to be re-positioned in the hierarchy. For example, the results of [Fouka et al. \(2021\)](#) and [Fouka et al. \(2020\)](#) show, on two separate examples, how an influx of new and salient immigration groups (African Americans during the Great Migration and Mexicans in the nineties) in the U.S. led native whites to improve attitudes towards minorities present prior to the influx (European immigrants and African Americans respectively). It is argued that the appearance of the new group, perceived to be more distant from the majority than the existing minorities, led the white majority to perceive a lower social distance to established immigrants and re-categorize them as in-group.

What remains unanswered is whether established minorities also perceive the appearance of a new group as a possibility for improving their own status, and respond by assigning higher value to characteristics they share with the majority group (e.g. common religion) and lower value to characteristics they share with the new group (e.g. being an immigrant), specifically when the majority shows scepticism toward the newcomers. Furthermore, one could hypothesize that minorities residing in contexts with particularly salient ethnic/national hierarchies, as well as those occupying lower positions in such a hierarchy, would have more to gain from an upward move and would thus be particularly eager to engage in inter-minority competition.

Even in the absence of strategic behavior on the part of established immigrants, it could be hypothesized that groups socialized in the presence of a steep ethnic hierarchy, and exposed to unfavorable treatment in the course of their integration, grow to normalize inter-ethnic competition and hostility as legitimate social dynamics, and are thus more likely to apply them toward lower-ranked groups once encountered. In a certain sense, requesting newcomers to have it at least as difficult as one had it themselves would amount to a request

for a perverse version of procedural fairness.

To test this idea, I use a survey experiment with a sample of participants with an immigration background residing in Germany and experimentally vary the status of the participants' in-group. In a separate pre-study, a smaller group of participants from the majority population (those with no immigration background) is asked to assess to what degree they see different immigrant groups (structured along the region of their origin) as contributing to or undermining the socio-economic and cultural life in their country. Participants in this phase can evaluate any of the suggested groups positively or negatively, so that the hierarchy among immigrant groups is not implied by construction of the design.

In the second and main part of the experiment, a sample of participants with a migration background is presented a subset of answers elicited in the first phase. Participants are randomly assigned to be presented a subset of answers evaluating their in-group either positively or negatively, while holding the evaluation of other out-groups constant. The main outcome of interest – captured by respondents' willingness to forgo some part of their experimental earnings to secure a donation to a refugee-supporting programme – is elicited thereafter. Additionally, several attitudinal measures of participants' positions toward refugees (as well as some other immigrant groups) are also elicited.

While a considerable body of scientific literature has studied the determinants of attitudes of the majority population toward migration (for a survey see e.g. [Hainmueller and Hopkins 2014](#)), significantly less attention has been paid to the political positions of immigrants, and specifically to their positions toward new flows of immigration. On the one hand, considering the shared experience of immigration and exposure to immigration policies, one might expect people with a migration background to develop a sense of “common fate” and hold more favourable attitudes toward other immigrants than the native population. Some empirical studies indeed confirm this expectation (e.g. [Van der Zwan et al., 2017](#); [Just and Anderson, 2015](#); [Hjerm and Nagayoshi, 2011](#)).

On the other hand, in most environments with a substantial immigrant population, the

group of immigrants is itself very heterogeneous, consisting of subgroups stemming from different socio-economic and cultural backgrounds, and with different histories of immigration and integration in the host country’s society. Factors as diverse as those found to impact the immigration attitudes of the majority population could be affecting the immigration attitudes of immigrants. Notwithstanding the importance of these group-specific factors, I explore an alternative and complementary mechanism based on group status.

Establishing causality using empirical and observational data has challenges, as several unobservables are correlated. Running an experiment provides a unique opportunity to circumvent these challenges. The literature in social psychology has provided some theoretical insights relevant to the proposed hypothesis. For example, in their seminal paper, [Jost and Banaji \(1994\)](#) laid out the concept of system justification theory, which builds upon social identity theory and extends it in a way that rationalizes negative in-group bias as well as positive bias toward the high-status out-group. The theory proposes that the need to reduce feelings of uncertainty, threat, and social discord tends to lead people in environments with pronounced social inequalities to adopt beliefs and stereotypes that help rationalize (and thus justify) the inequalities inherent to the societal status quo.

Importantly, the theory and the experimental works that followed (surveyed in [Jost, 2019](#)) demonstrated the readiness of disadvantaged groups in society to adopt negative stereotypes of their own in-group (and positive stereotypes of advantaged out-groups) in order to explain the prevailing hierarchy. Moreover, as [Jost et al. \(2003\)](#) demonstrated in a correlational study, and [Van der Toorn et al. \(2015\)](#) confirmed with experimental evidence, the groups that are more disadvantaged in the social hierarchy, more dependent and powerless, are particularly susceptible to developing system-justifying beliefs.

The mechanism proposed here to explain the opposition of established immigrants toward newcomers resembles closely the idea developed in the research on the so-called Queen Bee phenomenon. The term, as described in [Ellemers et al. \(2004\)](#), designates women occupying positions in male-dominated environments who express a gender bias when evaluating lower-

ranked female subordinates – sometimes even more so than their male colleagues – while distancing themselves from their own gender by expressing masculine self-descriptions. The subsequent literature (for review, see e.g. [Derks et al., 2016](#)) has relied on both social identity theory and system justification theory to argue that the Queen Bee behavior is itself a response to gender bias and identity threat in male-dominated environments.

Drawing a parallel with the question considered here, one might wonder whether there exists a Queen-Bee-Immigrant phenomenon: do established immigrants, in an environment sceptical toward immigrants, distance themselves from immigrant status and express negative bias toward other immigrants? If identity threat is indeed the channel, we would expect immigrants from groups assigned a lower status to be particularly susceptible. Whereas the Queen Bee literature considers a bias of females toward other females (i.e. their own in-group), reacting by being more suspicious of another out-group should arguably be even less psychologically costly, and thus a more likely strategy.

B.2 Experimental Design

The experiment is run on a target sample size of 1,000 individuals with a migration background residing in Germany. A participant is considered to have a migration background if they or at least one of their parents was born in a country different from their country of residence. The experiment was programmed in Qualtrics and the link to the experiment was distributed by a panel company. The experiment is split into two phases, referred to here as the pre-study and the main experiment respectively.

B.2.1 Pre-study

The pre-study is conducted with the purpose of collecting responses from the majority population regarding their position toward different immigration groups, which are later used in the main experiment. The pre-study involves a small sample of participants (target sample size of 150 individuals) residing in Germany with no migration background.

Phase 1: Demographics. At the beginning of the survey, information about respondents' gender, age, state of residence (within Germany), education level, and family income is collected. Participants are also asked to state their own and parental country of birth, which is used to ensure that only participants from the majority population (those with no migration background) participate in the pre-study.

Phase 2: Elicitation of immigration attitudes. Participants are told that they would be asked to evaluate each of several immigration groups defined on the basis of their country/region of origin. In order to facilitate attitude elicitation and reduce ambiguity, two questions used in the European Value Survey are reformulated into statements. Particularly, participants are told:

Some people residing in Germany have a migration background, that is, either they or their parents were born in a different country. People coming to live in Germany come from a number of different countries and regions. Data collected as a part of the European Values Survey, a large-scale, cross-national study, show that people vary widely in whether they see that it is generally good or generally bad for Germany's economy and cultural life that people from other countries come to live here.

Thereafter, for each of the several immigration-origin regions/countries, participants are asked to evaluate whether Germany's socio-economic and cultural life is generally undermined or enriched by people immigrating from this region. To avoid confusion in terms of which countries are encompassed by a given region, each question is accompanied by a simple political map of the relevant part of the world with the region of interest and corresponding countries clearly indicated. Participants in the pre-study are paid only the participation fee; their answers are not incentivized.

B.2.2 Main Experiment

The main part of the experiment is conducted with a sample of 1,000 participants with a migration background residing in Germany. Participants for this phase were selected to be first- or second-generation immigrants.

Phase 1: Demographics. At the very beginning of the session, participants answer questions regarding their demographic characteristics: gender, age, state of residence (within Germany), education level, family income, and their own and parental country of birth.

Phase 2: Experimental treatments and information. Participants are told that, in a study conducted at an earlier point in time,¹⁸ a group of participants from Germany with no migration background were asked to evaluate whether Germany’s socio-economic and cultural life is generally undermined or enriched by people immigrating from several different regions or countries. They are informed that they would be presented with a subset of the collected answers pertaining to some immigrant groups.

Participants are randomly divided into two treatment groups. In both treatments, participants are presented with the evaluation of three groups: one representing their own in-group and two representing out-groups. In both treatments, one out-group is always evaluated positively and the other negatively (with positive/negative referring to contributing to vs. undermining socio-economic and cultural life). The only difference between treatments is the evaluation of the own in-group: in the *Positive status treatment*, participants are shown an answer that evaluates their own in-group positively; in the *Negative status treatment*, participants are shown an answer that evaluates their own in-group negatively.

Including the two consistently evaluated out-groups eliminates the risk that receiving negative information about one’s in-group leads to more negative views of immigration in general simply by reminding participants of integration difficulties, rather than by targeting their in-group specifically. In this way, the presence of one positively and one negatively

¹⁸The month and year in which the responses in the pre-study were collected is shown to the participants.

evaluated group fixes the hierarchy; the only element that changes is the position of the participant's in-group.

Phase 3: Elicitation of attitudes towards refugees. Participants are asked to provide their views on refugees in Germany. Following the approach of [Dinas et al. \(2021\)](#), participants answer six questions targeting attitudes toward refugees who arrived in Germany during the recent refugee influx, fleeing wars in the Middle East:

1. Do you think Germany should increase or decrease the number of people it grants asylum to? (1 = Greatly increase; 5 = Greatly decrease)
2. Refugees are a burden on our country because they take our jobs and social benefits. (1 = Completely agree; 5 = Completely disagree)
3. The money spent on the accommodation of refugees in our country could have been spent better to cover the needs of Germans. (1 = Completely agree; 5 = Completely disagree)
4. Refugees will increase the likelihood of a terrorist attack in our country. (1 = Completely agree; 5 = Completely disagree)
5. Refugees in our country are more to blame for crime than other groups. (1 = Completely agree; 5 = Completely disagree)
6. Among the following options, which one do you think best explains why refugees from Syria and other countries leave their country? (1 = To flee war; 2 = To improve their economic conditions; 3 = To avoid political persecution; 4 = To gain access to host country's social benefits.)
7. Is Germany made a worse or a better place to live by refugees who are granted asylum in Germany? (0 = Worse place to live; 10 = Better place to live)

In the next step, participants are informed that a lottery will be administered whereby one randomly selected participant will be awarded 100 Euros. They are asked whether, if they win, they would like to dedicate some percentage of the prize to help refugees, with such amounts automatically deducted from their prize and donated to a refugee-supporting organization.

To capture potential spillover effects on attitudes toward other immigrant groups, participants are also asked to evaluate several other groups of immigrants.

Phase 4: Mood elicitation. A self-reported measure of participants' mood is elicited via the Self-Assessment Manikin questionnaire ([Bradley and Lang, 1994](#)).

Phase 5: Indirect upstream reciprocity. This part aims to elicit a measure of participants' upstream reciprocity, which captures the tendency to reciprocate treatment received from one individual in interactions with another individual. All participants are assigned the role of person A, person B, or person C in a modified dictator game. Person A is given a budget and can decide to send some amount to person B, which is then multiplied by a factor of either 2 or 4. Person B, knowing only the total amount received (not the factor), then decides how much to send to person C. Most participants are assigned the role of person B. At the end of the experiment, one triplet of participants is randomly selected and payments are made accordingly.

Phase 6: Process fairness. This part tests participants' preference for equity of treatment – specifically, whether participants exposed to relatively unfavorable conditions would find it unfair if conditions for other participants are improved. Participants answer one logical-mathematical question for additional earnings. They may use a “hint” button that eliminates one incorrect answer. After answering, participants are asked whether future participants should receive more assistance (the hint deleting three incorrect answers instead of one) or whether the prize should be reduced. Their votes determine the implemented

conditions for future participants.

Phase 7: Attitudes under observation. This part captures potential differences in attitudes expressed by established immigrants when they expect those attitudes to be observed by the majority population, compared to when this is not the case. Participants are reminded that all previously provided answers will be delivered only to researchers in anonymized form. They are then asked to provide an answer to the question “Is Germany made a worse or a better place to live by refugees who are granted asylum in Germany?” (0–10 scale), which they are told may be shown to participants in a possible future study run with German citizens.

Phase 8: Perceived norm elicitation and debriefing. Participants are asked to guess what proportion of all 150 participants in the pre-study evaluated positively each of several immigration groups, including refugees from the Middle East, the participant’s own in-group region, one high-status immigrant group (western EU), and two lower-status groups. The participant whose guess is closest to the true answer receives an additional monetary reward. Collecting this information allows for a check of treatment effectiveness with respect to the evaluation of the own in-group, and for testing potential spillover effects on beliefs regarding other out-groups. At the end of the survey, participants are shown the true percentages, thereby eliminating the possibility that participants maintain false beliefs based on the experimental prime.

B.3 Hypotheses

Hypothesis 1. Being assigned to the Negative status treatment leads to a decrease in the amount donated to UNHCR and a more negative evaluation of refugees as measured by the attitudinal questions (as described in Phase 3).

Hypothesis 2. Being assigned to the Negative status treatment leads participants to expect

a higher percentage of negative evaluations of refugees' impact on socio-economic and cultural life in Germany among majority participants (in the pre-study). Furthermore, assignment to the Negative status treatment leads participants to expect a higher percentage of negative evaluations of their own in-group as well as of other low-status groups among majority participants.

Hypothesis 3. Participants with higher indirect reciprocity react more strongly to being assigned to the Negative status treatment, that is, express more negative evaluations of refugees.

Hypothesis 4. The distribution of answers provided to question 7 in Phase 3 differs from the distribution of answers provided to the same question (but under potential observation) in Phase 7. Furthermore, being assigned to the Negative status treatment leads participants to provide a less favourable opinion toward refugees in Phase 7.

C Experimental Instructions

The following pages contain the English translation of the experimental instructions as administered to participants.

Instructions

We are a group of scientists from [REDACTED] This survey should last (on average) around 15-20 minutes. Your answers will be available only to the researchers, in anonymized form and there will be no possibility to identify the respondents. The data will be kept on our servers and will be treated confidentially. Anonymized data can be provided to other researchers. You can decide at any point of time to leave the survey.

Please select whether you would like to participate in this survey.

- ☐ Yes, I would like to participate in this survey
- ☐ No, I would not like to participate in this survey

Q: Are you male or female?

☐ Female

☐ Male

Page Break

Q:

Do you speak or understand a language other than German?

Please select all that apply.

Names of languages in the list are in English.

☐

No, I speak and understand only German

☐

ALBANIAN

☐

ARABIC

Note: a total of 79 languages were listed

Q:

What is the country of your birth?

Please select from the list below.

Names of countries in the list are in English

▼ Afghanistan ... Zimbabwe

Page Break

Q:

What is the country of birth of your **mother**?

Please select from the list below.

Names of countries in the list are in English

▼ Afghanistan ... Zimbabwe

Page Break

Q:

What is the country of birth of your **father**?

Please select from the list below.

Names of countries in the list are in English

▼ Afghanistan ... Zimbabwe

Page Break

Q: How old are you?

Page Break

Q:

In one previous study, 125 German participants (without migration background) were asked to evaluate the impact of immigrants coming to live in Germany from different regions/countries on socio-economic and cultural life in Germany.

Participants were asked to evaluate the impact of several immigrant groups.

On the next screen, you will be shown a subset of collected answers.
Please read these answers carefully.

Note: This page depicts what a participant matched to region of Ex-Yugoslavia would see if they were allocated to the Negative treatment

Q:

If participant was matched to region of Ex-Yugoslavia and was allocated to the Negative treatment:

People that come to Germany from countries of the Ex-Yugoslavia:



Croatia, Bosnia and Herzegovina, Slovenia, Serbia, Kosovo, Montenegro und North Macedonia

Contribute rather **NEGATIVELY** to the socio-economic and cultural life in Germany

People that come to Germany from countries of the western EU:



Austria, Belgium, France, Ireland, Luxemburg, Netherlands

Contribute rather **POSITIVELY** to the socio-economic and cultural life in Germany

People that come to Germany from Lebanon:



Contribute rather **NEGATIVELY** to the socio-economic and cultural life in Germany

Note: This page depicts what a participant matched to region of Ex-Yugoslavia would see if they were allocated to the Positive treatment

Q:

People that come to Germany from countries of the Ex-Yugoslavia:



Croatia, Bosnia and Herzegovina, Slovenia, Serbia, Kosovo, Montenegro und North Macedonia

Contribute rather **POSITIVELY** to the socio-economic and cultural life in Germany

People that come to Germany from countries of the western EU:



Austria, Belgium, France, Ireland, Luxemburg, Netherlands

Contribute rather **POSITIVELY** to the socio-economic and cultural life in Germany

People that come to Germany from Lebanon:



Contribute rather **NEGATIVELY** to the socio-economic and cultural life in Germany

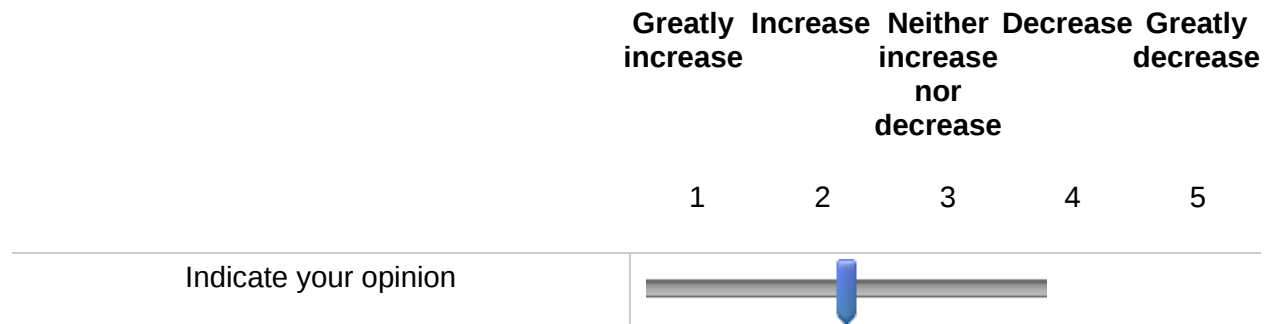
Q: To which degree do you agree with the stated results?

Strongly agree	Agree	Neither agree nor disagree	Disagree	Strongly disagree
1	2	3	4	5

Q: We would now like to ask **your opinion regarding immigration to Germany**.
Particularly, we would like to ask your opinion about today's asylum seekers in Germany.

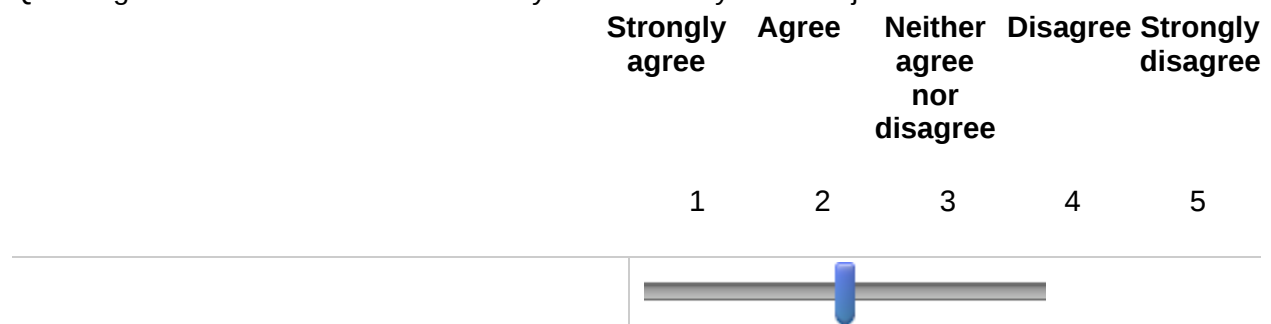
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Q: Do you think Germany should increase or decrease the number of people it grants asylum to?



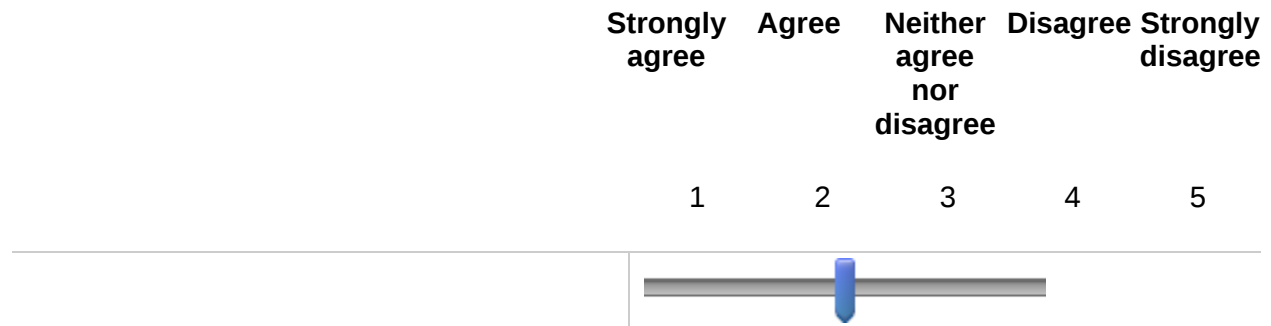
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Q: Refugees are a burden on our country because they take our jobs and social benefits.



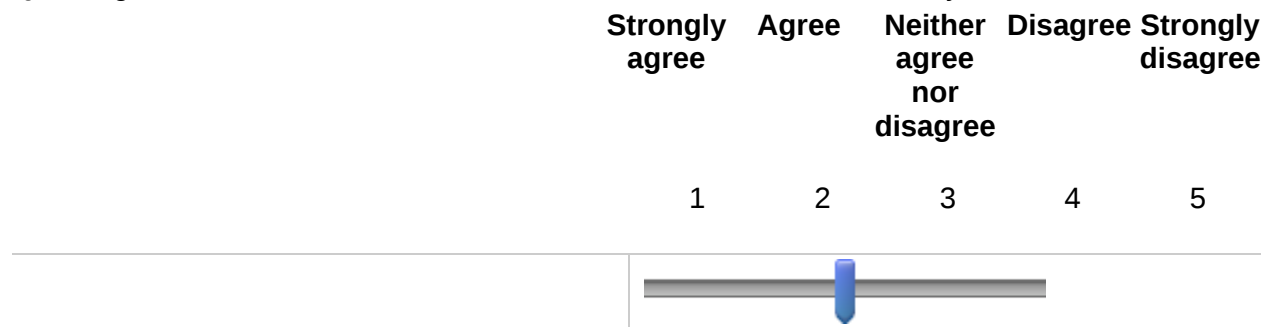
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Q: The money spent on the accommodation of refugees in our country could have been spent better to cover the needs of Germans.



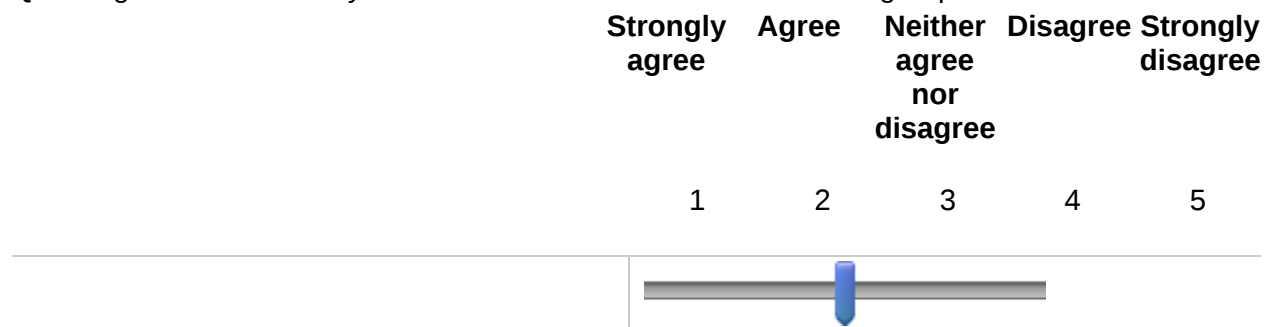
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Q: Refugees will increase the likelihood of a terrorist attack in our country.



Page Break

Q: Refugees in our country are more to blame for crime than other groups.

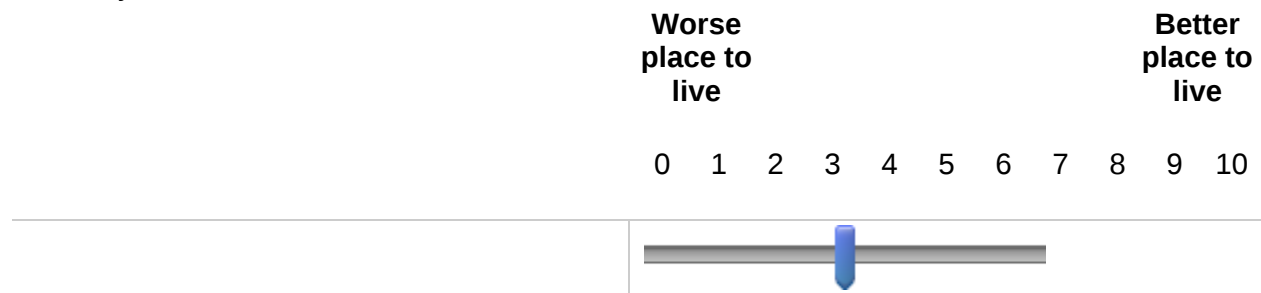


Q: Among the following options, which one do you think best explains why refugees from Syria and other countries leave their country?

- ☐ To flee war
- ☐ To improve their economic conditions
- ☐ To avoid political persecution
- ☐ To gain access to host country's social benefits.

Page Break

Q: Is Germany made a worse or a better place to live by refugees who are granted asylum in Germany?



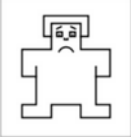
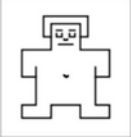
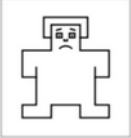
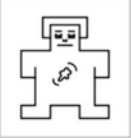
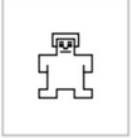
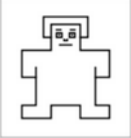
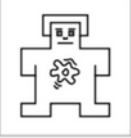
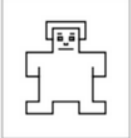
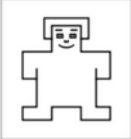
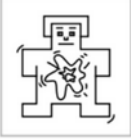
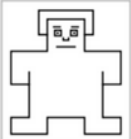
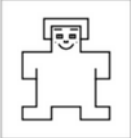
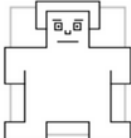
Q: As a part of this study, we will run a lottery. All participants who complete the survey will automatically be included as participants in the lottery and everyone has the same chance to win. The winner of the lottery will receive a prize in value of **100 Euros**, which will be paid out in addition to their other earnings from this survey to their panel account.

However, you can also choose to donate one part of the lottery prize that you might win to the United Nations High Commissioner of Refugees (UNHCR), a global organization dedicated to helping refugees. If you win the lottery, the donation amount will be automatically deducted from the 100-EUR prize, and a donation in this value will be made to UNHCR. The remaining part of the 100-EUR prize will be paid out to you.

Please indicate below if you would like to donate some part of your 100-EUR prize in case you win the lottery, and if so, how much.

If I win the lottery, I would like to donate the following amount from my 100-EUR prize
(Please enter an amount in EUR, between 0 and 100)

Q: Please select in each of the three sections (boxes) below the figure that describes your current mood the best. If you feel that your mood lies in between two figures, please select the point between them.

	☐		☐		☐
	☐		☐		☐
	☐		☐		☐
	☐		☐		☐
	☐		☐		☐

Q: At the beginning of the survey, we have told you about the earlier study, where 150 German participants (without migration background) were asked to evaluate the impact of immigrants coming to live in Germany from different regions/countries on socio-economic and cultural life in Germany. In that study participants were asked for each of several countries/regions to evaluate whether people coming to Germany from this country/region:

A: "contribute rather *POSITIVELY* to socio-economic and cultural life in Germany", or

B: "contribute rather *NEGATIVELY* to socio-economic and cultural life in Germany"

You have been shown some of the collected answers.

Can you try to guess **how many percent of all asked participants evaluated NEGATIVELY socio-economic and cultural impact of people immigrating to Germany from each of the following countries/regions?** (In other words, what percent of participants selected option B)

The respondent whose answers are the closest to the true outcome collected in that study which will be shown on the next screen) will receive an additional **25 EUR** to their panel account when the survey is completed.

0 10 20 30 40 50 60 70 80 90 100

<u>Participant's region of origin</u>	
<u>Refugees from the Middle East and Asia:</u> Syria, Iraq, Afghanistan and Pakistan	
<u>Turkey</u>	
<u>Countries of western EU:</u> Austria, Belgium, France, Ireland, Luxemburg, Netherlands	
<u>Lebanon</u>	
<u>South Africa:</u> South Africa, Namibia, Botswana, Eswatini and Lesotho	

Indirect reciprocity if participant is matched to the role of „Person B”

Q: In this part of the study, you will interact with two other participants of this study, but you will not know who these participants are. Let us call the three participants person A, person B and person C. **You are person B.**

The rules are the following:

Person A is given a budget of **30 EUR**. Person A can decide to send from this budget to person B (i.e. to you) some amount. The amount that person A sends to you will then be multiplied by a **factor**, and the resulting amount will be paid out to you. The value of the factor is not known to any of the three persons, but it can either be equal to 2 or 4.

Person A will keep for themselves the rest of the budget, that is the amount that they did not send to you.

----- Example -----

For example, if person A sends you 5 EUR, then person A keeps for them 25 EUR and you will receive:

*If **factor=2**: you will receive $5 \text{ EUR} \times 2 = 10 \text{ EUR}$*

*If **factor=4**: you will receive $5 \text{ EUR} \times 4 = 20 \text{ EUR}$*

You will know the total amount that you received from person A. However, you will not know the value of the factor by which the amount that person A sent to you was multiplied.

After you learn the total amount that you receive from person A, you can decide whether you want to send some part of that amount to person C, and if so, how much. This amount will not be multiplied by the factor. The amount that you send is paid out to person C. You will keep the rest (that is, the part that you do not send to person C) for yourself.

All participants in this study are assigned one of the roles (person A, person B or person C) and matched into groups of three participants. When all participants complete the survey, one group will be randomly selected and, taking into account their decisions (how much person A sent to person B, and person B to person C), the payments will be made to these participants. Person A with whom you are matched has already completed the survey and decided how much to send to you. Please proceed to the next screen to learn about the amount that you receive and to make the decision regarding the amount that you want to send to person C.

Q:

You received the amount of 32 EUR

As stated on the previous page, you don't know what was the value of the multiplication factor.

That means that one of the following scenarios happened:

Scenario1: Person A sent you **8 EUR** and the factor equals **4**. This is why you received **8 EUR*4=32 EUR**.

Scenario2: Person A sent you **16 EUR** and the factor equals **2**. This is why you received **16 EUR*2=32 EUR**

For each of the two scenarios, please make a decision how much of the received 32 EUR (if any) would you like to send to person C.

Scenario 1:

If person A sent me **8 EUR**, and the **factor=4**, I would like to send to person C the following amount:

	0	32
Please select in EUR		

Scenario 2:

If person A sent me **16 EUR**, and the **factor=2**, I would like to send to person C the following amount:

	0	32
Please select in EUR		

Indirect reciprocity if participant is matched to the role of „Person A”

Q: In this part of the study, you will interact with two other participants of this study, but you will not know who these participants are. Let us call the three participants person A, person B and person C. **You are person A.**

The rules are the following:

Person A (that is, you) is given a budget of **30 EUR**.

You can decide to send from this budget some amount to person B (person B will not receive the budget). The amount that you send to person B will then be multiplied by a **factor**, and the resulting amount will be paid out to person B. The value of the factor is not known to any of the three persons, but it can either be equal to 2 or 4.

You will keep for yourself the rest of the budget, that is the amount that you don't send to person B.

----- Example -----

For example, if you send 5 EUR to person B, then you keep for yourself 25 EUR and person B will receive:

*If **factor=2**: you will receive **5EUR*2=10 EUR***

*If **factor=4**: you will receive **5EUR*4=20 EUR***

Person B will know the total amount that they received from you. However, they will not know the value of the factor by which the amount that you sent was multiplied. Person B will then be asked if they want to send some amount from the received sum to person C. The amount that person B sends to person C will not be multiplied by the factor. Instead, it will simply be paid out to person C.

All participants in this study are assigned one of the roles (person A, person B or person C) and matched into groups of three participants. When all participants complete the survey, one group will be randomly selected and, taking into account their decisions (how much person A sent to person B, and person B to person C), the payments will be made to these participants.

Please indicate below (in EUR), how much would you like to send to person B from your 30 EUR budget.

- ☐ 0 EUR
- ☐ 8 EUR
- ☐ 16 EUR
- ☐ 30 EUR

Please note that all the answers that you provided so far will be seen (in anonymized form) only by the researchers. **On this page, we would like to ask you to provide an answer to the question: *"Is Germany made a worse or a better place to live by refugees who are granted asylum in Germany?"*, that we can show to participants in a study that might be conducted in future.**

Please complete the form with your answer to the stated question.

Q: On the next page, you will be asked to answer one question. The question is aimed to measure numerical ability.

You will have 1 minute to provide the answer to this question. There are 6 offered answers, and only 1 of those is correct.

The participant who provides the **correct answer in the shortest time** will be paid **30 EUR** in addition to other earnings from this study.

Page Break

Q: A bat and ball cost \$1.10. The bat costs one dollar more than the ball. How much does the ball cost?

Please enter your answer below in cents.

Page Break

Q: Before you proceed, we would like to ask your opinion on the question that you have seen. We might use the same question in a future study to measure the numerical ability of other participants. Any participant who took part in this survey would be excluded from the future survey.

If we decide to use this question, participants in that study will be provided as here 60 seconds, to answer the questions.

We would like to ask your opinion: If we decide to use this question in the future study, do you think that we should reward by 30 EUR not only the fastest correct answer, but also the second- and/or third-fastest correct answer? Alternatively, do you think that 30 EUR is a too high prize for this task and should be reduced to 20 EUR?

We will collect the opinions of all participants in this study. The option that receives the most votes will be implemented.

Please vote below.

- ☐ Reward **only the fastest** correct answer by **30 EUR**.
- ☐ Reward **the fastest and second-fastest** correct answer by **30 EUR each**.
- ☐ Reward the **fastest, second-fastest and third fastest** correct answer by **30 EUR each**.
- ☐ Reward **only the fastest** correct answer by **20 EUR**.

Q: If you were not born in Germany, since when do you live in Germany?

▼ I was born in Germany ... 1950

Page Break

Q: Do you have a German citizenship?

- ☐ Yes
- ☐ No
- ☐ I have a German and other citizenship(s)

Page Break

Q: How many persons live in your household (including yourself):

- ☐ Adults _____
- ☐ Children (below 18 years old) _____

Q7 What ist he highest level of schooling that you completed (allgemeinbildend oder beruflich)?

Note: the options shown below designate educational levels common in Germany

- ☐ Ich habe die Schule vor Ende der 9. Klasse / ohne Hauptschulabschluss verlassen
- ☐ Hauptschulabschluss
- ☐ Realschulabschluss (Mittlere Reife)
- ☐ Hochschulreife / Abschluss des Abiturs (Gymnasium bzw. EOS)
- ☐ Lehre / Berufsausbildung
- ☐ Bachelor
- ☐ Master / Diplom / Magister / Staatsexamen oder vergleichbarer Abschluss
- ☐ Promotion

Q: Please select below federal state of your residence and your municipality / city / region:

Federal state

Municipality / city / region

▼ Baden-Württemberg ... Thüringen ~ Weimarer Land

Q: What was the monthly net income of your household in the last month? By monthly net income we mean the sum that you received through wages, labor income, Income from self-employment, retirement income, pension, rent, Income from public subsidies, income from renting, leasing, housing benefit, child benefit and other income **after deducting taxes and social security contributions**.

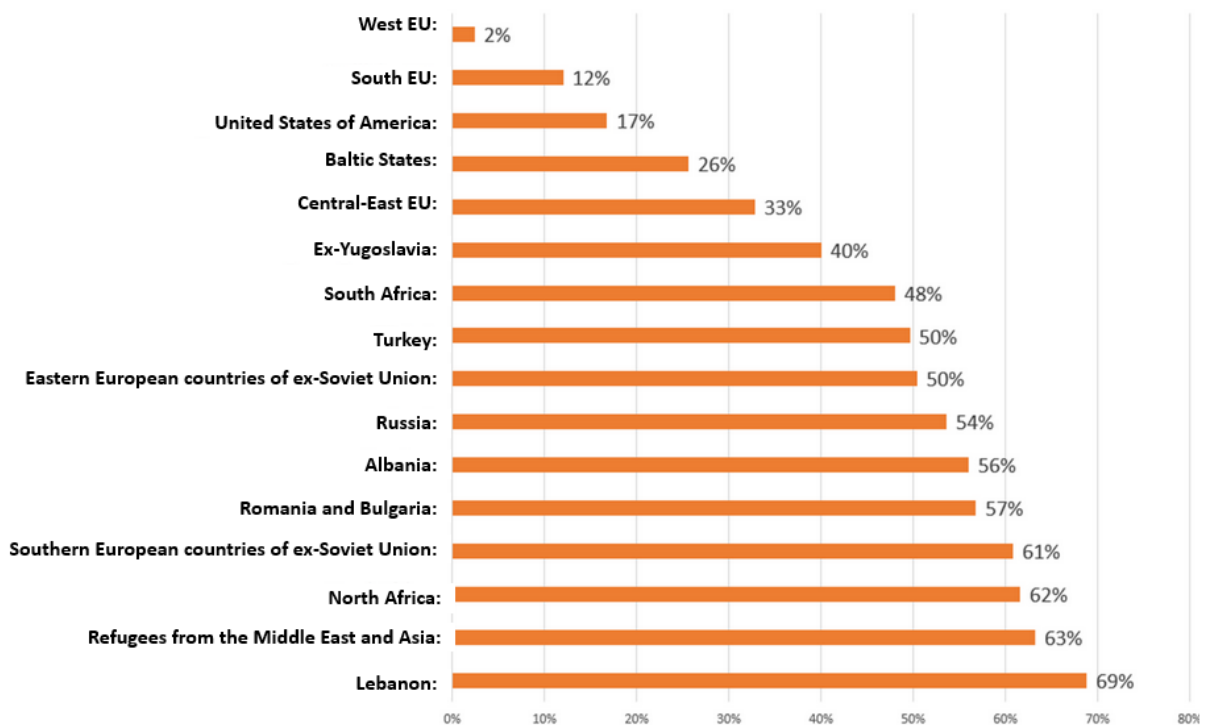
- ☐ < 1000€
- ☐ 1000€-1999€
- ☐ 2000€-2999€
- ☐ 3000€-3999€
- ☐ 4000€-4999€
- ☐ 5000€-7500€
- ☐ > 7500€

Q: As was explained before, you will now be shown the full results of the previously conducted study with 125 participants without immigration background. For each depicted immigrant-group you will be shown the percent of participants who evaluated negatively the impact of this group on socio-economic and cultural life in Germany.

Page Break

Q:

Percent of 125 participants without immigration background who evaluated negatively the impact on socio-economic and cultural life in Germany of immigrants coming to Germany from following regions/countries.



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