

Recognition of Collective Victimhood and Outgroup Prejudice

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Abstract

Groups that have experienced collective suffering are sometimes more sympathetic towards outgroups, while other times they display higher outgroup prejudice. What can account for these contradictory observations? We use a unique historical episode of forced displacement to examine how perceptions of recognition of the ingroup's victimhood affect views towards outgroups. We collect data on descendants of ethnic Germans ousted from Central and Eastern Europe after the end of World War II, and examine their attitudes towards Syrian refugees today. We use both observational data and an experiment to test the role of victimhood recognition. When they learn that their suffering is more appreciated by other Germans than what they expected, descendants of expellees become more positive towards refugees. Interestingly, this effect is not symmetric. When recognition of suffering is revealed to be lower than respondents' expectations, their sympathy towards refugees does not decrease. We document that this effect is not present among respondents without a family background of forced expulsion, and also provide evidence for the underlying mechanism at work.

Keywords: victimhood recognition; forced displacement; outgroup prejudice; German expellees

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Human history is marked by episodes of collective suffering. Be it the result of intergroup conflict or natural disasters, communities around the world witness violence, forced displacement and other forms of victimization. How do these experiences affect individual attitudes towards others? A voluminous literature in political science (Balcells, 2012; Hadzic, Carlson and Tavits, 2017; Hartman and Morse, 2018), social psychology (Noor et al., 2017; Bilewicz and Stefaniak, 2013) and economics (Bauer et al., 2016; Miguel, Saiegh and Satyanath, 2011) has examined this question, with ambiguous results.

Collective suffering tends to increase ingroup unity and to induce hostility targeted against the group to which the inflicted pain is attributed (Rozenas, Schutte and Zhukov, 2017; Fouka and Voth, 2019). This effect can extend to members of the group that did not directly experience victimization (Lickel et al., 2006), as the media (Paluck, 2009), schools (Angvik and von Borries, 1997) and family (Lupu and Peisakhin, 2017; Wohl and Van Bavel, 2011) transmit the memory of collective trauma from one generation to the next. Less clear is how collective victimhood in the past affects attitudes towards unrelated groups that undergo episodes of suffering today.

On the one hand, a spiral of *competitive victimhood* can induce prejudice and hostility against other victimized groups. Groups seek the moral and sometimes even material benefits emerging from victimhood status and compete among each other about the magnitude and pervasiveness of their suffering (Pettigrew et al., 2008; Noor et al., 2012; Shnabel, Halabi and Noor, 2013). On the other hand, when reminded about their own suffering, members of victimized groups may also react by demonstrating higher levels of outgroup empathy. Besides enabling perspective taking (Gerace et al., 2015), the shared experience of victimhood can reduce prejudice via the adoption of a more inclusive group identity. Referred to as *inclusive victimhood* (Vollhardt, 2015), this channel implies enhanced understanding of the collective suffering of others (Vollhardt, Nair and Tropp, 2016). Acknowledging others' pain, in turn, leads to identity recat-

egorization. Both the ingroup and the outgroup can now be linked under a common superordinate category, that of victimized groups (Gaertner and Dovidio, 2014).

What determines the direction of victimhood effects? We argue that a key factor that can turn competitive victimhood into inclusive is the perception of victimized groups that others acknowledge their collective suffering. The importance for reconciliation of recognition of victimhood by the perpetrator is well established. Theories of reconciliation point to the victim's need for *truth and justice* (Gibson, 2006). As Staub (2008, 5) puts it, victims want "to have the truth of what was done to them be established and their suffering acknowledged." Recognition of past victimization can facilitate reconciliation even when past victimization is not related to the current conflict (Andrighetto, Halabi and Nadler, 2018).

What is unknown so far is how broader recognition of past suffering affects attitudes towards other victimized groups. We hypothesize that this effect is positive. Third-party recognition of a group's past suffering asserts the status of the group and reduces the need for other group enhancement strategies such as victimhood competition (Turner and Tajfel, 1986; De Guissmé and Licata, 2017). By strengthening self-esteem derived from group belonging, recognition can in turn facilitate perspective taking (Galinsky and Ku, 2004; Vorauer and Quesnel, 2013), which has been shown to reduce outgroup prejudice (Galinsky and Moskowitz, 2000).

To test this idea, we use as a case at hand the forced relocation of ethnic Germans from the territories that formed part of the German Reich until World War II. An estimated 12 to 16 million people of German ethnic origin fled or were expelled from those territories after their annexation by Poland and the Soviet Union. A majority of those *Heimatvertriebene* or *expellees*, as they are known, relocated to Germany, where they were given German citizenship. We conduct an online survey targeting descendants of expellees. We examine how the attitudes of this population towards present day refugees in Germany respond to recognition of the significance of the historical experience of expulsion by the broader German population.

We first show that measures of victimhood competition and of lack of recognition of the experience of expulsion predict negative attitudes towards today’s refugees. To provide causal evidence for the role of recognition as a mechanism mediating outgroup attitudes, we elicit respondents’ prior beliefs about the share of Germans who consider the historical experience of the expellees as a very important part of German history. We then provide a random subset of respondents with information on the actual share, so that respondents who had underestimated (overestimated) the initial degree of recognition receive a positive (negative) recognition signal. Consistent with our expectations, we find that a positive recognition signal significantly improves outgroup sympathy, as measured by donations to the UNHCR. Surprisingly, this effect does not extend to expressed attitudes, and it does not seem to be symmetric: learning that recognition of the group’s suffering is in fact lower than initial expectations does not trigger anti-refugee responses.

Methods

Sampling

We collect data as part of a larger survey conducted with the help of Infratest dimap, using their standing online panel. We oversampled descendants of ethnic Germans ousted from Central and Eastern Europe after the end of WWII. To achieve roughly equal shares of respondents with and without expellee background, our survey targeted seven federal states: Bavaria, Baden-Württemberg, North Rhine-Westphalia, Lower Saxony, Hesse, Mecklenburg-Vorpommern, and Schleswig-Holstein.¹ We focus on these federal states because they had the highest proportions of expellees in the census of expellees of the Federal Republic of Germany conducted in 1952 (Statistisches Bundesamt, 1953).

¹The distribution of respondents across states in our final sample is as follows: Bavaria (23.9%), Baden-Württemberg (6.8%), North Rhine-Westphalia (15.8%), Lower Saxony (26.1%), Hesse (8.4%), Mecklenburg-Vorpommern (13.0%), and Schleswig-Holstein (5.8).

Our final sample consists of 2362 respondents, all German citizens aged 18 and above.² Of those, 1024 (43.35%) have an expellee background in the family. We matched expellee descendants and other respondents on the basis of federal state, gender, and age categories (in ten year increments). Our design did not require that the sample be representative, neither on average, nor within subgroups (expellee/non-expellee). Nonetheless, sample characteristics are roughly comparable to those of the German population as a whole.³ We focus on expellee descendants in most of our analysis, but use respondents without expellee background as a comparison group to address potential confounders.

Additional details on sampling and on the screening of respondents with an expellee background can be found in section A of the Online Appendix.

Survey design and recognition treatment

The survey started with a small set of questions on demographics, household income and interest in politics. Respondents were then informed that followup questions would deal with the topic of asylum seekers. All respondents were primed with a mention of parallels between the expulsion of ethnic Germans and present refugee waves into Europe, so that followup victimhood-related questions would appear natural to them. Next, participants were randomly assigned into three groups, stratified by expellee status.

The first group constitutes our control group, and was directly asked questions about today's asylum seekers, without any additional manipulation. The second and third group instead were first asked to estimate the importance that present day Germans

²A total of 143 respondents were removed from the dataset because they failed Infratest's attention check, by having a survey duration less than half of the median per treatment group. In our resulting sample, the minimal time spent on the survey was 187 seconds and the median time was 408 seconds.

³The average age in our sample is 54.6, compared to 49.5 in the German sample of the 2016 European Social Survey (ESS). 22.4% of our sample have a university degree. The equivalent figure in the ESS is 26.2%.

assign to the historical episode of expulsion for the country’s history. Specifically, they were asked the following questions:

From 0 to 100, what share of people in Germany do you think have ever heard about the experience of German expellees, i.e. ethnic Germans who lived in Eastern and Central Europe but were expelled after the end of WWII? What share of Germans do you think consider their story a very important part of German history?

Throughout, we refer to respondents’ guesses provided as answers to the second question as *recognition estimates*, or *priors*. After providing their estimates, respondents in the second group moved directly to the next survey questions. Respondents in the third group instead had their priors updated, by receiving information about the true share, as elicited in a nationally representative online survey we conducted prior to the experiment ($n = 400$).⁴ The informational update read as follows:

“According to a survey conducted by *Anonymized University*, 30% of people consider the history of expellees as a very important part of German history.”

This update is our *recognition treatment*. We label respondents with recognition estimates higher than 30% as having *optimistic priors*, and those with estimates lower than 30% as having *pessimistic priors*. Figure 1 shows the distribution of recognition estimates together with the true value among expellee descendants. Interestingly, the mean of the estimates is close to the true score ($\bar{x} = 32.77$). We create a binary indicator denoting respondents with optimistic priors, i.e. those whose recognition estimate was higher than 30%.

In sum, one third of respondents was not asked about perceptions of recognition of expellees’ experience. Another third was asked this question, but was not updated with

⁴The wording of the question to elicit the true share was “Would you refer to the history of expellees as a very important, important, less important or unimportant part of German history?”

information about the true value among the German population. The last third was updated with the true value, as measured in our nationally representative pre-survey. Table C.1 in the Online Appendix shows the balance between the three groups along a list of covariates. Randomization appears to have worked as expected, with a small statistically significant imbalance in self-reported income. To account for this, as well as improve precision, all reported analyses control for baseline covariates.⁵

[Figure 1 about here.]

Outcomes

Our primary outcome of interest is a quasi-behavioral question on respondents' willingness to make a monetary donation to the United Nations High Commissioner of Refugees (UNHCR). In particular, we asked if respondents wanted to donate to the UNHCR part of a potential lottery gain, worth 100 euros, raffled off after the survey. Use of behavioral or quasi-behavioral outcomes in surveys minimizes the likelihood of social desirability bias, which is an inherent concern with self-reported measures of attitudes. Donations are a costly measure of sympathy for refugees, and thus more likely to reflect survey participants' true preferences (Camerer and Hogarth, 1999).⁶

If respondents' answer to this question was positive, we elicited the desired donation amount. We use the binary donation decision (*Donate*) and the logarithm of the amount of the donation (*Log amount*) as separate outcome measures.⁷

We additionally collected two sets of attitudinal measures. The first set elicits

⁵These are indicators for gender, age (in ten-year brackets), nine categories of educational attainment, and four categories of self-reported financial assessment. Response scales and value labels are listed in Section B of the Online Appendix.

⁶After response collection was completed, we implemented the lottery. The respondent randomly chosen to receive the raffled amount had elected to transfer 0 euros to the UNHCR. Infratest dimap transferred the full amount to the participant in the form of Payback points, the method commonly used by the company to compensate respondents for their participation in surveys.

⁷More precisely, we use a Box-Cox transformation of the form $\ln(x + \lambda)$, λ being a small amount, to avoid loss of observations with zero donations. Results are not sensitive to the choice of λ .

respondents' agreement with the following statements on a 5-point Likert scale:

1. (*Increase number*) Over the last two years, Germany received 968,000 asylum applications. Do you think Germany should increase or decrease the number of people it grants asylum to?
2. (*Take jobs*) Refugees are a burden on our country because they take our jobs and social benefits.
3. (*Money to Germans*) The money spent to fund the ongoing presence of refugees in Germany could be better spent on the needs of Germans.
4. (*Increase terrorism*) Refugees will increase the likelihood of a terrorist attack in our country.
5. (*Increase crime*) Refugees in our country are more to blame for crime than other groups.

The order of the five statements was randomized. We create binary indicators out of these measures, by assigning the value one to the two most positive responses to each statement, and zero to the rest.

The second set of attitudinal outcomes asked respondents to choose the primary reason why refugees abandon their countries among the following alternatives, whose order was also randomized in each interview:

- Flee the war
- Improve their economic conditions
- Avoid political persecution
- Obtain access to social security payments in the destination country

This latter set of questions is of particular importance in the current setting. Responding that refugees flee war or persecution is an indicator of increased capacity of

expellee descendants to link Syrians' experience to their own group's past suffering. We create binary variables for each alternative indicating if respondents selected it as the primary reason refugees leave their countries. We recoded all outcomes so that higher values indicate higher support for refugees. Table 1 reports summary statistics for all outcomes.

[Table 1 about here.]

The standardized Cronbach's alpha across attitudinal measures is equal to 0.766, well above the commonly used cut-off of 0.7 (Cronbach, 1951). This indicates a reasonably high internal consistency of our measures. We use the first principal component of standardized attitudinal measures as a summary index (*Attitudes*), to reduce noise and avoid multiple hypotheses testing (Ansolabehere, Rodden and Snyder, 2008; Broockman, Kalla and Sekhon, 2017).

Finally, we collected information on potential mechanisms. We measured victimhood competition as agreement with the statement that German expellees in the past suffered more than Syrian refugees nowadays (*Victimhood competition*), and feelings of unrecognized past suffering as agreement with the statement that the suffering of German expellees has been ignored outside of Germany (*Recognition abroad*). Agreement with both statements was elicited using a 5-point Likert scale. We convert each measure to a binary indicator equaling one for respondents who agree or strongly agree with the statement and zero otherwise. The exact wording of all survey items is displayed in Section B of the Appendix.

Empirical strategy

Our analysis consists of two parts. We begin by focusing on expellees only. First, using only the control group, we examine whether victimhood competition and perceptions about victimhood recognition are good predictors of outgroup sympathy. Since perceptions of recognition are not random, these correlations cannot be interpreted causally. In the second part we demonstrate causality by experimentally manipulating percep-

tions of victimhood recognition. For that purpose we turn to the second and third group of survey respondents. We compare, among individuals with recognition estimates in the same range, respondents who receive an informational update to those who do not. We expect that updating a pessimistic prior will lead to increased sympathy for asylum seekers in terms of both behavior and attitudes. We expect the opposite effect when an optimistic prior is updated. Finally, we compare treatment effects between expellee descendants and other respondents. We expect the treatment effect of updating to be stronger for expellees than for non-expellees across all outcomes and priors.

The design is based on two assumptions. First, we assume that group identities and the memory of past suffering travels from one generation to the next. Most respondents with an expellee background are first or second generation descendants (>95%). We assume that they are aware of their ancestors' suffering and that their identity as expellee descendants is formed around the experience of that suffering. Dinas, Fouka and Schläpfer (Forthcoming) provide evidence in favor of this assumption by showing that expellee descendants are more likely to prioritize their ancestors' history in school textbooks than other topics of German 20th century history. The second assumption is that recognition of the suffering of one's own group is more important than that of another group's suffering, as suggested by social identity theory (Turner, Brown and Tajfel, 1979). This assumption motivates our expectation that effects of recognition will be weaker or absent among non-expellees.

The treatment may also have effects unrelated to expellee status. For example, learning what others think about a past episode of forced displacement can drive inference about how others see today's asylum seekers, triggering social desirability effects. Alternatively, mentioning a historical episode of collective suffering experienced by Germans may increase nationalism and thus reinforce ingroup boundaries. We have no reason to expect that these or other mechanisms are more or less pronounced for expellees (compared to non-expellees). To account for them, we use a triple interaction framework (comparing the treatment effect across types of priors and expellee background).

This gauges the net effect of ingroup victimhood recognition, after partialing out other potential effects of the treatment that work in the same way on expellee descendants and other respondents.

Results

Figure 2 shows the relationship between support for refugees and perceptions of relative suffering (top panel) and of broad recognition (bottom panel) among expellee respondents in the control condition. Consistent with our expectations, we find that respondents who agree with either of the victimhood statements are less likely to make monetary donations and, if they do, they donate smaller amounts. They also express more negative attitudes towards today’s refugees. This provides evidence that victimhood and perceptions of its insufficient recognition go together with lower levels of support for refugees. This result cannot be interpreted causally, since perceptions of recognition may be correlated with other characteristics of respondents. To overcome this problem, we experimentally manipulate perceptions of victimhood recognition.

[Figure 2 about here.]

Figure 3 presents the results. Point estimates correspond to the effect among expellee descendants of being updated with actual levels of recognition, conditional on respondents’ priors. All estimated effects are reported relative to the outcome’s standard deviation among respondents in the control group. Respondents with a pessimistic prior respond to the update with increased donations in favor of today’s refugees. The estimated effects are statistically significant at the 1% level and non-trivial in magnitude. The treatment increases the probability of a donation by more than 30% compared to expellees with a pessimistic prior who were not updated. Donated amounts experience a similarly large change, increasing by 0.32 of a standard deviation ($p < 0.01$). For context, we compare the effect size to differences in donations by party affiliation, as measured by reported voting during the 2017 national election in the control group. The effect is

more than twice as large in magnitude as the difference in donations between a voter of the Social Democratic party (SPD) and a voter of the Christian-Democratic party (CDU/CSU).⁸ Attitudinal outcomes do not follow the same pattern and show only a small, insignificant response to the informational treatment. While surprising, this is in line with recent studies that find prejudice reduction interventions to affect behavior, but not attitudes (Scacco and Warren, 2018; Mousa, 2019).

These results are not symmetric. We find no significant impact of the treatment on respondents with optimistic priors. While updating pessimistic priors about the degree of recognition of the ingroup's victimhood boosts outgroup empathy among expellee descendants, updating optimistic ones does not appear to undermine it.

[Figure 3 about here.]

How relevant is the forced displacement background of respondents for these results? We shed light on this question by comparing expellee respondents to respondents without expellee background. Figure 4 plots differential treatment effects between expellees and non-expellees. Among respondents with pessimistic priors, the difference between the two groups is statistically significant and large in magnitude for all outcomes. The positive effects of receiving the informational update are systematically larger among expellees than among non-expellees, both for behavioral ($p < 0.05$) and for attitudinal outcomes ($p < 0.1$). We find no systematic difference among respondents with an optimistic prior.⁹

[Figure 4 about here.]

Evidence on the underlying mechanism driving these asymmetric results is presented in Figure 5. We examine how measures of victimhood competition and of broad recognition respond to the information treatment. As expected, among expellee descendants

⁸In the control condition, (log) donations of SPD voters are larger than those of CDU voters by 0.12 of a standard deviation.

⁹Regressions underlying Figures 3 and 4 are presented in Tables C.2 and C.3 in the Online Appendix.

with a pessimistic prior, those who receive the information treatment express less competition with today's refugees than those who were not updated ($p < 0.05$). This effect is once again substantive in magnitude: the informational update reduces expressed competition by 0.21 of a standard deviation, which is equivalent to the difference between expellees with pessimistic and those with optimistic priors in the group that did not receive the information treatment. Respondents also express less concern about recognition of their experience outside Germany, though this effect is less precisely estimated. We find no such effect on either measure among expellee descendants with an optimistic prior. This is consistent with our findings in Figures 3 and 4. Updating optimistic priors downwards does not seem to negatively impact feelings of recognition or victimhood, and consequently does not worsen attitudes towards outgroups. One explanation for this pattern could be that the initial guesses of respondents include a high degree of uncertainty. It can then be comforting to have factual information that recognition is at 30%, even though one's initial point estimate was higher (but more uncertain).

[Figure 5 about here.]

Discussion

Taken together, these results show that when members of a group that has experienced collective suffering learn that their trauma is more widely recognized than they expected, they become more positive towards other victimized groups. While positive shocks to the perceived recognition of the ingroup's victimhood boost empathy, negative ones do not increase outgroup prejudice among the descendants of the ingroup.

Our findings can reconcile the ambiguous results of a large literature that studies how collective suffering affects attitudes towards outgroups. Existing studies find that collective suffering sometimes induces prejudice while other times mitigates it. We show that one reason for this ambiguity is the ingroup's perceptions of how established

their suffering is outside the group itself.¹⁰ For instance, Wayne and Zhukov (2019) document higher sympathy towards Syrian refugees among Holocaust survivors and their descendants in the US. This result echoes the historically supportive stance of the Jewish American community towards marginalized groups, such as African Americans during the struggle for civil rights, as well as the more general narrative among Jews to “never be a passive bystander.” These observations are in line with our results—the suffering of Jews during the Holocaust stands out among experiences of collective victimization for its high level of general recognition. Even so, Jews’ attitudes are sensitive to the way in which the experience of the Holocaust is framed (Ariely, 2018; Rosenfeld, 1999). For example, Vollhardt (2012) shows that Jewish respondents are significantly less likely to support other victimized groups when the Holocaust is cast as “crime against humanity” than when it is cast as “crime against Jews”.

In cases where actual or perceived recognition is less wide than that of the Holocaust, victimhood competition and negative attitudes towards victimized others are more likely to ensue. An Armenian American participant in Mazur, Nair and Vollhardt (2015) expresses frustration that the Armenian genocide is less acknowledged than the Holocaust by stating:

“Everyone talks about the Holocaust, but nobody talks about the [Armenian] genocide, and I guess [...] we kind of resent that a little. Even though I said we are similar and I like the Jewish people, but, we are not recognized, [...].”

Corbel et al. (2004) discusses how Algerian pupils in France contested the inclusion of the Shoah in the curriculum without a corresponding provision for the victims of

¹⁰An alternative illustration of this result is presented in Figure C.1 in the Online Appendix, which compares outcomes between expellees and non-expellees. Within the group that did not receive the informational update, priors positively (though not significantly) correlate with expressed outgroup sympathy. Among respondents with an optimistic prior, expellee descendants are more sympathetic towards present day refugees than non-expellees. The opposite pattern is found among respondents with a pessimistic prior. However, among respondents who have learned the true recognition value, expellee descendants are friendlier towards refugees across the board, and significantly so within the group of respondents with pessimistic priors.

colonization. In Greece, in order to pass a bill that penalized Holocaust denial, MPs demanded that it also apply to the genocide of the Christian populations of Asia Minor, persecuted by Kemal Ataturk in the early 20th century (Antoniou, Dinas and Kosmidis, 2020).

These examples suggest that recognition of past suffering allows victimized groups to more objectively process and sympathize with similar experiences of others. Examining various instances of collective victimhood, such as the Armenian genocide, the Kielce Pogrom, and the Holocaust, Vollhardt 2015 finds that acknowledgement of past victimization is associated with higher wellbeing and higher tendency of members of victimized groups to reconcile with the perpetrator. Our findings add to these results and point to broader positive effects of recognition for intergroup relations.

We conclude with potential implications of our results for policies of remembrance. Our study highlights the importance of recognition of a group's suffering not by the group itself, but by others outside it. This points to a central role for the state in facilitating broad recognition for all subgroups in a society. An obvious means of achieving that is through public education. The content of school curricula and the teaching of history in public schools can be pivotal in transmitting balanced and inclusive narratives of suffering, with consequences for broader attitudes towards present or future others (Antoniou, Dinas and Kosmidis, 2020; Angvik and von Borries, 1997).

A similar role can be played by official recognition of victimized groups' suffering. Fouka and Voth (2019) show that state recognition of past victimization, even if not accompanied by any material benefits for the acknowledged group, has a significant impact on attitudes and behavior, which surpasses the impact of remembrance activities organized by the group itself. The latter paper, as well as other existing work (Belmonte and Rochlitz, 2019; Ochsner and Roesel, 2017) ties remembrance of past violence to conflict. Our study complements this work by highlighting instead the positive effects of remembrance for intergroup relations and prejudice reduction.

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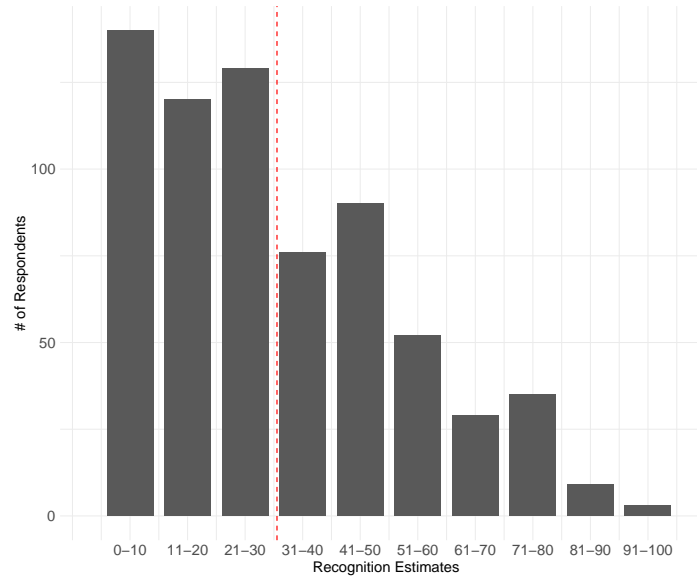
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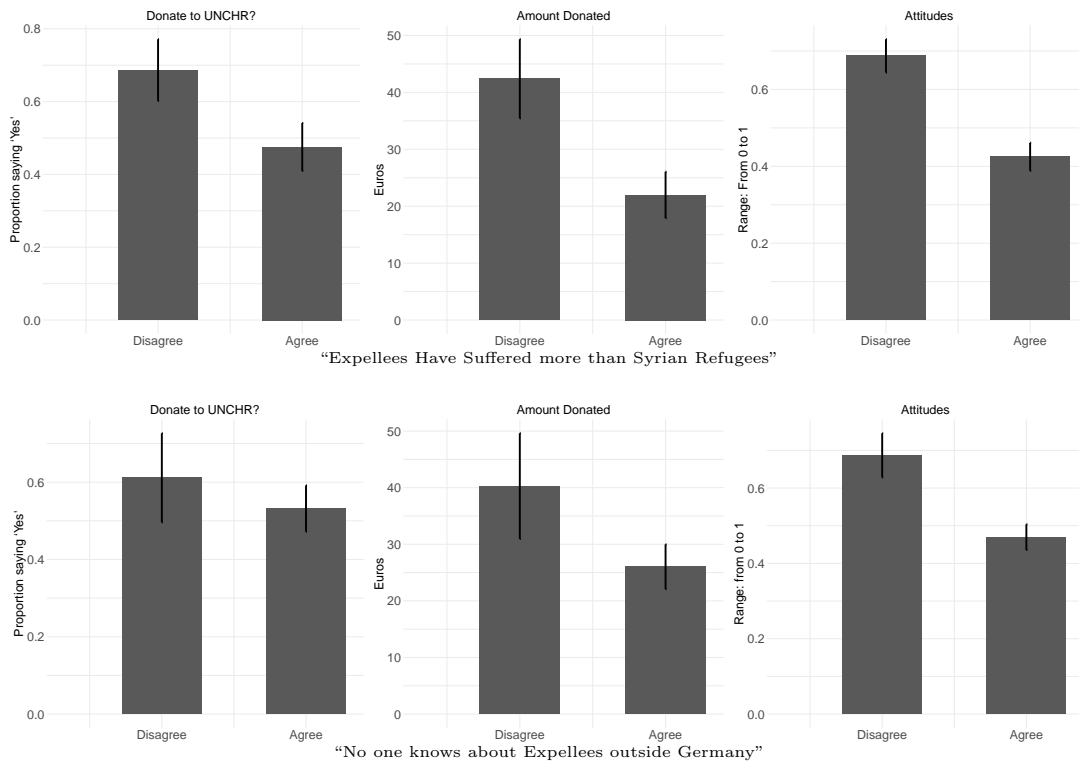
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Figure 1. Respondents' guesses of victimhood recognition.



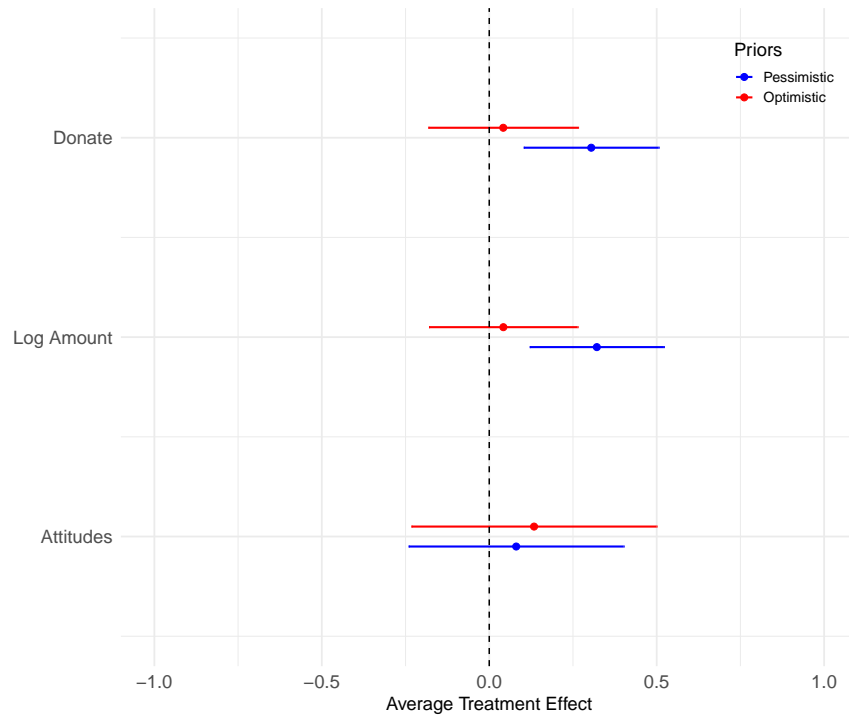
Note: The figure plots the distribution of expellee descendants' estimates of the percentage of Germans who consider the experience of expulsion an important part of German history. The vertical red line denotes the true percentage, as provided by a nation-wide survey conducted before the main study.

Figure 2. Victimhood recognition and support towards refugees.



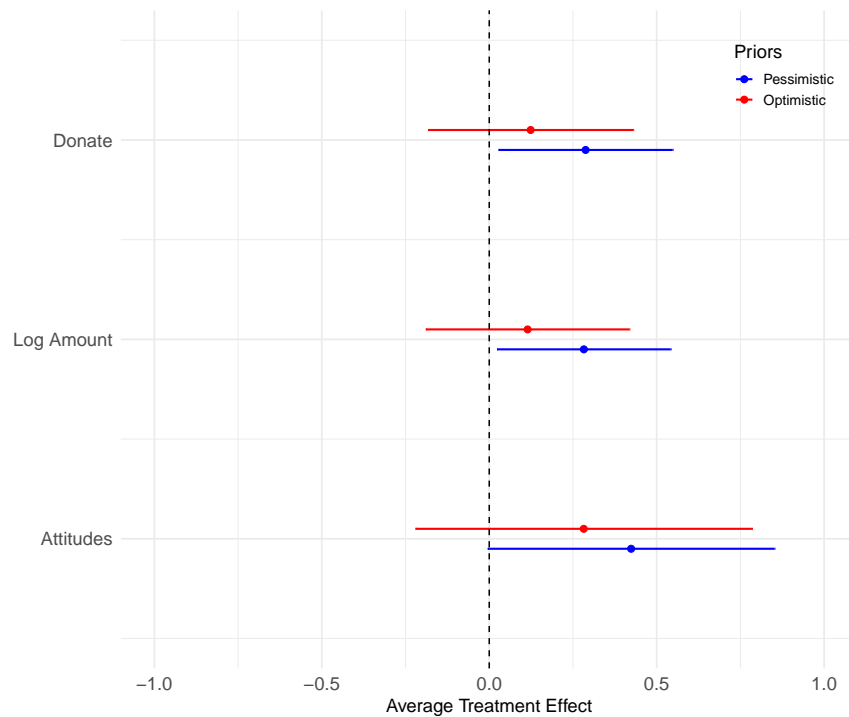
Note: The sample consists of expellee descendants. Vertical spikes denote 95% confidence intervals.

Figure 3. The impact of the recognition treatment on sympathy for refugees.



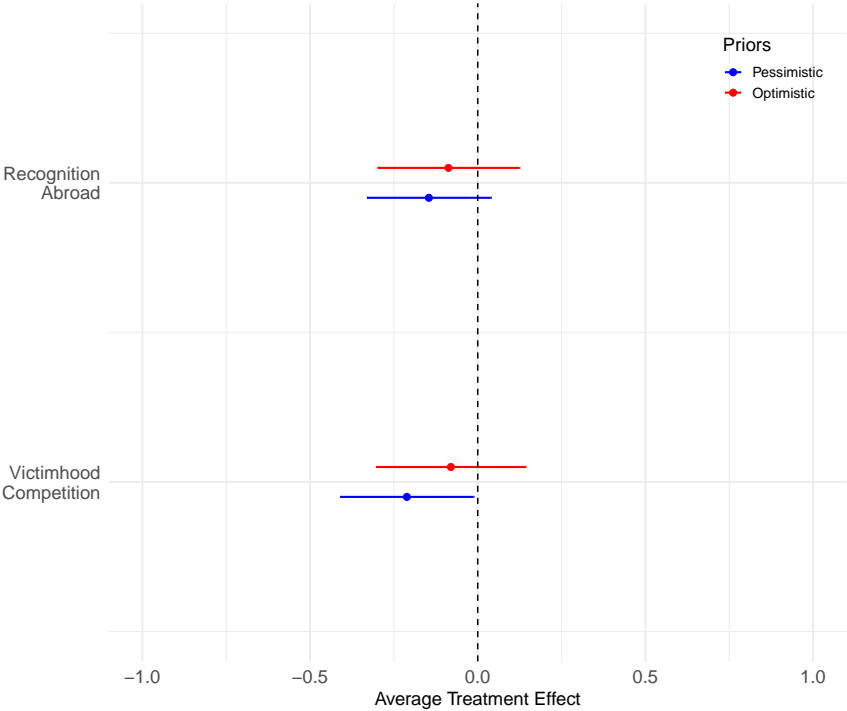
Note: The sample consists of expellee descendants. Horizontal lines denote 95% confidence intervals.

Figure 4. Difference in treatment effects between expellees and non-expellees.



Note: The sample consists of respondents with and without expellee background. Horizontal lines denote 95% confidence intervals.

Figure 5. The impact of the recognition treatment on perceptions of victimhood recognition.



Note: The sample consists of expellee descendants. Horizontal lines denote 95% confidence intervals.

Table 1. Summary statistics

	Mean	S.D.	N
Donate	0.489	0.500	2,362
Amount	24.390	32.343	2,362
Log amount	-0.548	4.178	2,362
Increase number	0.070	0.255	2,362
Take jobs	0.790	0.407	2,362
Money to Germans	0.345	0.475	2,362
Increase terrorism	0.623	0.485	2,362
Increase crime	0.423	0.497	2,362
Reason to leave: flee war	0.564	0.496	2,362
Reason to leave: economic	0.766	0.424	2,362
Reason to leave: political persecution	0.077	0.266	2,362
Reason to leave: social benefits	0.874	0.331	2,362

Online Appendix

A Survey design

A.1 Additional details on sampling and treatment

The full sample consists of 3155 respondents, 1367 of which (43.3%) were expellee descendants. Approximately 75% of the total sample were randomly assigned to a “parallels treatment”, i.e. a prime that links the historical episode of mass expulsion to today’s migrant crisis. This treatment was intended to answer a distinct and independent research question, analyzed in a companion paper. For the present study we only use the subset of respondents who received the parallels treatment ($n = 2362$). Since the parallels treatment was assigned randomly, we eventually end up using a random subset of the original sample.

The exact wording of the parallels prime, administered to all respondents considered in this study, is provided below:

We would now like to turn to a series of questions about asylum seekers, people who left their home countries and request legal safe-haven in Europe on the basis of fearing persecution in their home countries. The current refugee crisis is not the first time Germany has had to accommodate forcedly displaced populations. Other examples include Bosnians and Croats during the Yugoslav war as well as Germans from Eastern and Central Europe who came to Germany after WWII.

A.2 Identifying respondents with a forced relocation background

Respondents were screened for eligibility three weeks before the actual survey. At that point we also identified their expellee background. There was no direct connection be-

tween the screening and the survey that would be obvious to respondents. Respondents were not told that another interview would follow up the screening, nor were they reminded about the screening when re-contacted. An average member of the Infratest panel receives approximately 20 survey invitations per year, which makes it unlikely that participants make connections across the various surveys. During the screening, respondents were asked to state their birthplace as well as birthplaces of parents and grandparents. Birthplaces were chosen from a predetermined set of regions, containing Germany and the former German territories in Eastern Germany as separate entries. The foreign-born and descendants of the foreign-born were then asked to state the year of immigration to Germany of themselves or their parents or grandparents. We identify respondents who themselves or whose ancestors were born in the former German territories and immigrated to Germany between 1944-1950 as individuals with an expellee background. German-born respondents of German-born ancestors were included in the study sample as comparison group.

B Survey Instrument

Introductory block

Thank you so much for your willingness to participate in our survey. This is a survey about economic, social and political issues in Germany. To start, we would like to ask you a few questions about your personal situation.

- Q.1 What is your gender?
- Q.2 In what year were you born?
- Q.3 What is the highest general education degree you have achieved? (1 = Elementary school not completed; 2 = Elementary school completed, no secondary education; 3 = Secondary school with 8th or 9th grade degree; 4 = Secondary school with 10th grade degree; 5 = Technical high school (Fachoberschule); 6 = High school or extended secondary school; 7 = University / Technical college; 8 = Doctorate / Habilitation; Other _____)
- Q.4 Which of the following statements comes closest to how you feel about your household's income nowadays? (1 = We live comfortably; 2 = We make ends meet; 3 = We have difficulties; 4 = We have major difficulties)
- Q.5 How interested would you say you are in politics? Would you say you are very interested, fairly interested, not very interested, or not at all interested?

We would now like to turn to a series of questions about asylum seekers, people who left their home countries and request legal safe-haven in Europe on the basis that they fear persecution in their home countries. The current refugee crisis is not the first time Germany has had to accommodate forcedly displaced populations. Other examples include Bosnians and Croats during the Yugoslav war as well as Germans from Eastern and Central Europe that came to Germany after WWII.

Elicit recognition estimates (Treatment groups 1 and 2)

- Q.6 From 0 to 100, what share of people in Germany do you think have ever heard about the experience of German expellees, i.e. ethnic Germans who lived in Eastern and Central Europe but were expelled after the end of WWII?
- Q.7 What share of Germans do you think consider their story a very important part of German history?

Update recognition estimates (Treatment group 2 only)

According to a survey conducted by *Anonymized University*, 30% of people consider the history of expellees as a very important part of German history.

Attitudes toward refugees

Now, some questions about today's asylum seekers, people who left their home countries and request legal safe-haven in Europe on the basis of fearing persecution in their home countries. Over the last two years, Germany received 968.000 asylum applications.

- Q.8 Do you think Germany should increase or decrease the number of people it grants asylum to? (1 = Greatly increase; 5 = Greatly decrease)
- Q.9 Refugees are a burden on our country because they take our jobs and social benefits. (1 = Completely agree; 5 = Completely disagree)

Cont.

-
- Q.10 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)
- Q.11 Refugees will increase the likelihood of a terrorist attack in our country. (1 = Completely agree; 5 = Completely disagree)
- Q.12 Refugees in our country are more to blame for crime than other groups. (1 = Completely agree; 5 = Completely disagree)
- Q.13 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.)
-

Voting behavior

- Q.14 Some people choose to vote in elections whereas other choose to abstain. What about you? Did you vote in the last federal election? (Yes/No)
- Q.15 [If (2) in Q.14] And which party did you vote for? (1 = CDU/CSU; 2 = SPD; 3 = AfD; 4 = Greens; 5 = FPD; 6 = Die Linke; 7 = Other _____)
- Q.16 And what about the 2013 election, did you vote in that election? (Yes/No)
- Q.17 [If (2) in Q.16] And which party did you vote for? (1 = CDU/CSU; 2 = SPD; 3 = AfD; 4 = Greens; 5 = FPD; 6 = Die Linke; 7 = Other _____)
-

Behavior toward refugees

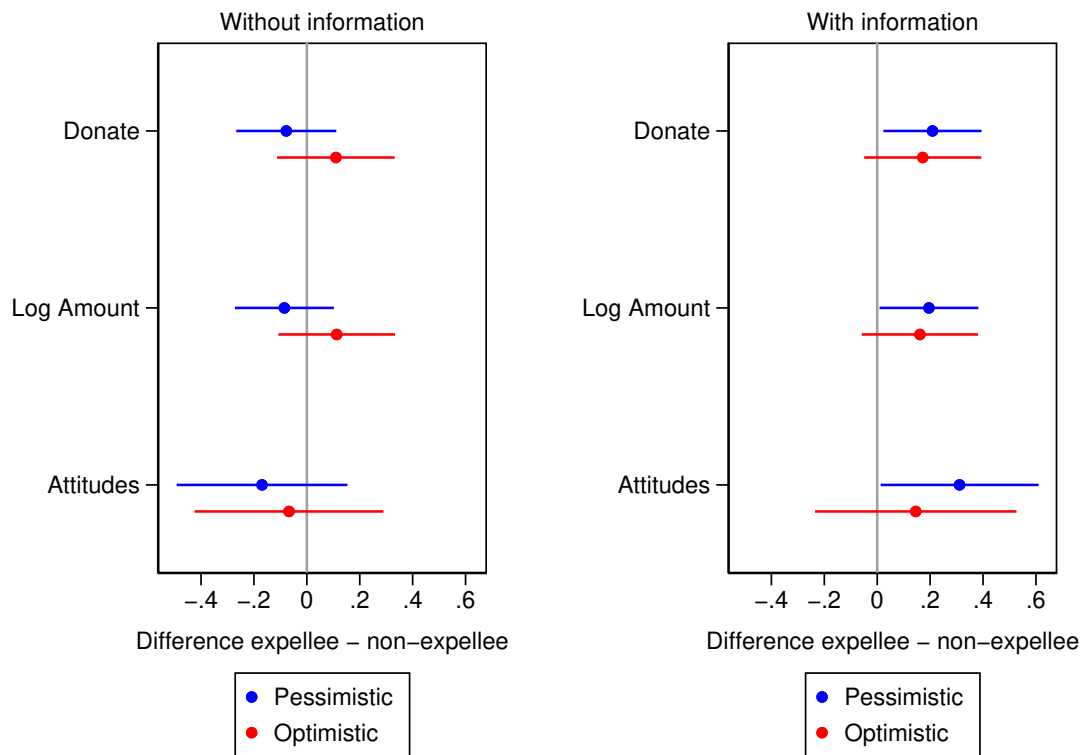
- Q.18 Among all participants of the survey we raffle off a 100-euro voucher. Every respondent has an equal chance of winning the voucher. However, you can also choose to donate a percentage of your winnings to the United Nations High Commissioner of Refugees (UNHCR). If you win the voucher, the donation amount will be deducted from the voucher, the remaining part will be transferred to you panel account. Would you like to donate some part of the 100-euro voucher, and if so, how much?
-

Victimhood competition and recognition

- And one last question: Some people agree with some statements whereas others disagree. How much you agree or disagree with the following statements?
- Q.19 German expellees during WWII have suffered more than Syrians suffer now (1 = Completely agree; 5 = Completely disagree)
- Q.20 No one outside Germany has paid attention to how much German expellees have suffered after the WWII. (1 = Completely agree; 5 = Completely disagree)
-

C Additional figures and tables

Figure C.1. Difference between expellee descendants and non-descendants.



Notes: Differences between descendants of expellees and other respondents, by initial recognition estimates. The left (right) panel shows differences for respondents who were not (were) updated on the true share of Germans who consider the experience of expellees an important part of German history. Lines denote 95% confidence intervals.

Table C.1. Randomization check

	Control	Treatment 1 (recognition estimate)	Treatment 2 (recognition estimate and update)	P-value
Expellee	0.429 (0.018)	0.432 (0.018)	0.439 (0.018)	0.916
Age	54.2 (0.649)	55.2 (0.576)	54.5 (0.566)	0.300
Male	0.587 (0.017)	0.575 (0.018)	0.543 (0.018)	0.189
<i>Education</i>				
Elementary Education	0.013 (0.004)	0.006 (0.003)	0.009 (0.003)	0.441
Secondary up to 8th degree	0.145 (0.012)	0.159 (0.013)	0.145 (0.013)	0.689
Secondary completed	0.407 (0.017)	0.420 (0.018)	0.437 (0.018)	0.478
Technical school	0.071 (0.009)	0.079 (0.010)	0.083 (0.010)	0.628
High school	0.113 (0.011)	0.104 (0.011)	0.107 (0.011)	0.844
University degree	0.223 (0.015)	0.202 (0.014)	0.203 (0.014)	0.525
<i>Income</i>				
Live comfortably	0.432 (0.018)	0.479 (0.018)	0.482 (0.018)	0.079
Make ends meet	0.462 (0.018)	0.439 (0.018)	0.413 (0.018)	0.170
Have difficulties	0.088 (0.010)	0.073 (0.009)	0.087 (0.010)	0.501
Major difficulties	0.018 (0.005)	0.008 (0.003)	0.018 (0.005)	0.164
<i>Federal state</i>				
Baden-Wuerttemberg	0.069 (0.009)	0.067 (0.009)	0.068 (0.009)	0.984
Bavaria	0.224 (0.015)	0.265 (0.016)	0.229 (0.015)	0.110
Hessen	0.088 (0.010)	0.073 (0.009)	0.091 (0.010)	0.410
Mecklenburg-Vorpommern	0.142 (0.012)	0.128 (0.012)	0.122 (0.012)	0.477
Lower-Saxony	0.257 (0.016)	0.256 (0.016)	0.270 (0.016)	0.780
North Rhine-Westphalia	0.160 (0.013)	0.147 (0.013)	0.168 (0.013)	0.516
Schleswig-Holstein	0.059 (0.008)	0.063 (0.009)	0.052 (0.008)	0.619
<i>Party Voted in 2017</i>				
CDU/CSU	0.271 (0.016)	0.241 (0.015)	0.259 (0.016)	0.397
SPD	0.205 (0.015)	0.231 (0.015)	0.213 (0.016)	0.460
AfD	0.076 (0.009)	0.059 (0.008)	0.052 (0.008)	0.134
FDP	0.072 (0.009)	0.073 (0.009)	0.068 (0.009)	0.917
Die Linke	0.053 (0.008)	0.063 (0.009)	0.057 (0.008)	0.680
Grüne	0.068 (0.009)	0.086 (0.010)	0.059 (0.008)	0.105
Observations	794	776	792	

Notes: Entries in first three columns are sample means by subgroups (standard errors of sample means in parenthesis). The last column reports the p-value from an F-test for joint orthogonality.

Table C.2. Difference in differences estimates among expellee descendants

	Donate	Log Amount	Attitudes	Victimhood competition	Recognition abroad
	(1)	(2)	(3)	(4)	(5)
Optimistic	0.194* (0.109)	0.205* (0.107)	0.0203 (0.177)	-0.0393 (0.106)	-0.00883 (0.0952)
Treatment 2	0.305*** (0.103)	0.321*** (0.102)	0.0805 (0.163)	-0.211** (0.101)	-0.146 (0.0940)
Treatment 2 \times Optimistic	-0.263* (0.153)	-0.279* (0.151)	0.0533 (0.248)	0.131 (0.152)	0.0584 (0.144)
Observations	683	683	683	683	683
R-squared	0.0660	0.0716	0.110	0.0903	0.0550

Notes: The sample consists of expellee descendants whose prior recognition estimates were elicited (Treatment groups 1 and 2). Dependent variables are coded so that higher values indicate higher victimhood competition and lower perceptions of recognition. All regressions include indicators for federal state, gender, education, self-reported financial situation and 10-year age brackets. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.3. Triple differences

	Donate	Log Amount	Attitudes	Victimhood competition	Recognition abroad
	(1)	(2)	(3)	(4)	(5)
Treatment 2 \times Expellee	0.288** (0.132)	0.283** (0.132)	0.424* (0.218)	-0.307** (0.133)	-0.123 (0.127)
Optimistic \times Expellee	0.176 (0.146)	0.186 (0.144)	0.112 (0.238)	-0.0453 (0.144)	0.132 (0.134)
Treatment 2 \times Optimistic \times Expellee	-0.164 (0.205)	-0.168 (0.204)	-0.141 (0.338)	-0.00389 (0.202)	-0.0927 (0.195)
Expellee	-0.0918 (0.0946)	-0.0978 (0.0936)	-0.198 (0.159)	0.208** (0.0942)	0.117 (0.0880)
Optimistic	0.0256 (0.0985)	0.0270 (0.0976)	-0.0756 (0.161)	-0.00962 (0.0995)	-0.133 (0.0976)
Treatment 2	0.0282 (0.0853)	0.0512 (0.0855)	-0.346** (0.145)	0.0768 (0.0883)	-0.0252 (0.0844)
Treatment 2 \times Optimistic	-0.0906 (0.137)	-0.102 (0.136)	0.227 (0.228)	0.140 (0.135)	0.143 (0.133)
Observations	1568	1568	1568	1568	1568
R-squared	0.0548	0.0590	0.0874	0.0561	0.0489

Notes: The sample consists of respondents with and without an expellee background whose prior recognition estimates were elicited (Treatment groups 1 and 2). Dependent variables are coded so that higher values indicate higher victimhood competition and lower perceptions of recognition. All regressions include indicators for federal state, gender, education, self-reported financial situation and 10-year age brackets. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.