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Seeing the World Through the Other's Eye: An Online Intervention Reducing Ethnic Prejudice

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We report the results of an intervention that targeted anti-Roma sentiment in Hungary using an online perspective-taking game. We evaluated the impact of this intervention using a randomized experiment in which a sample of young adults played this perspective-taking game, or an unrelated online game. Participation in the perspective-taking game markedly reduced prejudice, with an effect-size equivalent to half the difference between voters of the far-right and the center-right party. The effects persisted for at least a month, and, as a byproduct, the intervention also reduced antipathy toward refugees, another stigmatized group in Hungary, and decreased vote intentions for Hungary's overtly racist, far-right party by 10%. Our study offers a proof-of-concept for a general class of interventions that could be adapted to different settings and implemented at low costs.

Intergroup prejudice has been recognized as one of the most important social problems, leading to discrimination, inequality, and violence in countries across the world. Understanding the mechanisms behind and reducing prejudice are thus of eminent interests for scientific as well as political reasons. Decades of research have accumulated a vast array of knowledge about the origin of prejudice, ranging from accounts emphasizing the role of personality (Adorno et al. 1950), to the structure of interaction between groups (Allport 1954), to cognitive processes (Tajfel 1970). At the same time, there are few convincing empirical studies demonstrating successful large-scale interventions that reduce prejudice in practice (Paluck and Green 2009).

The key challenge in designing programs that can reduce intergroup hostility is that most proposed antecedents of prejudice (for example, personality, the nature of intergroup contact, or social categorization) are simply very resistant to change. For instance, numerous experimental studies have supported the contact hypothesis (Al Ramiah and Hewstone 2013; Pettigrew and Tropp 2006), and the main tenets of this paradigm have been corroborated by field studies as well (e.g., Shook and Fazio 2008; Samii 2013; Broockman and Kalla 2016). Yet, the implementation of large-scale programs based on this paradigm has lagged behind (Paluck and Green 2009) because the conditions needed for intergroup contact to reduce prejudice are often extremely difficult, costly, or time-consuming to achieve under realistic circumstances. In this article, we report the results of an intervention that was both effective in reducing prejudice and appears to be easily implementable in a broad class of settings.

We designed and evaluated an online intervention to reduce prejudice against an ethnic minority. The intervention was a choose-your-own-adventure-game, which required participants to take the perspective of a member of a marginalized ethnic minority group. We evaluated the effects of this intervention by a randomized placebo-treatment design with several hundred young adult participants. We measured outcomes immediately after the intervention as well as with a one-month delay. Our outcome measures included a battery of questions on prejudice as well as intentions of voting for a far-right party with an explicitly racist rhetoric.

The political context of our study was Hungary, and we examined prejudice against the country's Roma minority, one of the largest ethnic minorities in Europe, characterized by widespread poverty and social exclusion (Simonovits and Kézdi 2016). Ethnic relations between the Roma and the majority have been wrecked by both sporadic cases of ethnic violence and the rise of Jobbik (in Hungarian, "Better"), a far-right and overtly racist party, as the second-largest political force (Karácsony and Róna 2011). Within this political landscape, studying the determinants of ethnic prejudice is necessary to understand the spectacular success of radical politics, which, in turn, is also indicative of a more general phenomenon across most parts of Europe (e.g., Ivarsflaten 2008; Lucassen and Lubbers 2012).

Our intervention is based on the psychological theory of perspective taking, which proposes to reduce prejudice by changing "people's perspectives so that they are coordinated with the experiences of members of other groups" (Dovidio et al. 2004; see also Broockman and Kalla 2016). This paradigm offers a great deal of promise given that even brief interventions

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encouraging perspective taking (without the physical presence of a member of an out-group) have been shown to improve intergroup attitudes (Todd and Galinsky 2014). So far, however, both the way perspective-taking interventions were implemented and evaluated raises concerns about their practical viability in the field.

Most laboratory experiments induce perspective taking through instruction, making this approach difficult to implement on a general population and raising concerns about demand effects (Batson, Early, and Salvarani 1997; Galinsky and Moskowitz 2000). In contrast, approaches that cast these interventions in the form of entertainment, and target individuals who are unaware of being studied, have proved less effective (Paluck 2010). Moreover, typical evaluations of these interventions used very small samples that revealed little about the precise magnitude of treatment effects. Finally, the effect of perspective-taking interventions had been typically evaluated only minutes after the intervention, giving little information on long-run effects (e.g., Bruneau and Saxe 2012).

Taken together, while the existing literature in social psychology provides strong support for the relationship between perspective taking and prejudice (Todd and Galinsky 2014), this same literature fails to provide practical guidance on how perspective taking could be enhanced to achieve long-term attitude change. Our innovation is to deploy an intervention that required participants to assume the role of a member of a marginalized ethnic minority group. We hypothesized that this form of intervention would prove more efficient compared to most lab studies, as perspective taking was facilitated not by experimental instruction but by the rules and structure of the game that participants voluntarily played. On the other hand, we improve over field studies (Paluck 2010) by providing a more powerful intervention centered on interaction rather than passive listening. Finally, as the intervention was implemented online, its total costs were largely independent of the number of participants.

We also sought to improve upon the previous literature with respect to the evaluation of our intervention. First, the impact of the perspective-taking game was assessed both immediately and a month after the experiment. Second, in contrast to lab-based studies, we used a much larger sample size: several hundred instead of dozens of subjects per cell. Third, our statistical analysis of the intervention was fixed before the data collection in a pre-analysis plan (PAP), to minimize researcher-degrees of freedom (e.g. Franco, Malhotra, and Simonovits 2016). Finally, our use of pretreatment data allowed us to both assess and address potential inferential issues resulting from attrition from the study. These design features, while typically absent in experimental studies on prejudice, are indispensable to accurately assess both the magnitude and the duration of possible effects (c.f. Broockman, Kalla, and Sekhon 2017).

While much of the social psychological research has focused its attention on the effect of perspective-taking interventions on attitudes about the targeted out-group, we expanded our focus to also account for the possible spillover effects of perspective taking. On one hand, we explored whether taking the perspective of one out-group (that is, a Romani person) could reduce prejudice about another stigmatized out-group (for example, homeless people or refugees). Similar transfer effects (Pettigrew 2009) have been found by studies in the contact-hypothesis paradigm, a phenomenon explained by attitude generalization and the similarity of the contacted and the secondary group (Pettigrew 2009; Schmid et al. 2012; Tausch et al. 2010). Such generalization of improved intergroup relations, however, has not been detected by studies of perspective taking (Todd and Galinsky 2014). Additionally, because studies have linked far-right voting to anti-Roma prejudice (Karácsony and Róna 2011), we also tested if a potential reduction in prejudice would also spill over to political preferences that are at least partly driven by such attitudes.

Our results show that the intervention proved effective in reducing prejudice. The immediate effect of the treatment on anti-Roma attitudes was 0.3 standard deviations, more than the difference between supporters of the center-right and the far-right party in the control group. Importantly, a follow-up survey conducted more than a month after the intervention revealed that this effect showed little decay. We also demonstrate that changes in attitudes toward the Roma minority also increases in sympathy for other marginalized groups, such as the refugees. Perhaps the most important result from a political point of view is a robust effect on voting intentions: the intervention reduced vote intentions for the racist party Jobbik by 12 percentage points (to be compared to a control mean of 43%).

While these results are consistent with the recent psychological literature connecting perspective taking to intergroup prejudice (Galinsky and Moskowitz 2000), we also advance this research by pointing to the possible "downstream" consequences of perspective taking. The impact of our intervention spilled over to attitudes both horizontally related to anti-Roma prejudice (that is, affect toward refugees) and intended behavior motivated by prejudice (that is, far-right voting). Perhaps more importantly, though, our contribution is practical: we provide the blueprint for a class of interventions that can durably reduce prejudice at extremely low costs to a broader population than existing approaches.

RESEARCH DESIGN

The study employed a treatment-placebo encouragement design in which participants were invited to try an online game. Subjects were randomly assigned to either the treatment group (and played the choose-your-own-adventure game) or a placebo group (and played an emotion-guessing game). We assessed the effect of the intervention by comparing the responses to a battery of questions tapping anti-Roma prejudice across those in the treatment and the control group both immediately after the intervention

and after a buffer period of one month. We registered our design in the EGAP repository (http://egap.org/design-registrations, ID: 20161128AA) before we started the data collection. All analyses specified in the PAP were implemented and are reported in the paper. We provide the full list of departures from the PAP in the Supplementary Material (SM).

Theoretical background

Our intervention is based on the psychological paradigm of perspective taking, defined as the "active consideration of out-group members' mental states" (Todd and Galinsky 2014, 374). A rich experimental literature in social psychology has provided evidence that inducing people to perspective taking can reduce many of the biases that characterize intergroup encounters (Galinsky and Ku 2004; Galinsky and Moskowitz 2000). A recent review of the literature by Todd and Galinsky (2014) points to multiple mechanisms through which perspective taking affects intergroup attitudes, which can be divided to affective and cognitive processes.

On one hand, previous research has identified increased empathy toward the targeted out-group (Batson 2011). In general, such affective responses are classified as between either parallel or reactive types of empathy (Todd and Galinsky 2014). These two reactions involve experiencing the same emotions as the member of the out-group and a heightened concern for their well-being (Todd and Galinsky 2014). Experimental studies of perspective taking have found evidence of both forms of empathic responding.

On the other hand, research has also identified cognitive mechanisms through which enhanced perspective taking can influence attitudes. First, some studies have found that treated individuals assigned a greater weight to nondispositional vis-à-vis dispositional factors in explaining the behavior of out-group members (Vescio, Sechrist, and Paolucci 2003). Second, other works showed that the effect of perspective-taking interventions is mediated by *self-out-group merging*, or the increase in the overlap in the cognitive representation of the self and the out-group category (Galinsky and Moskowitz 2000).

The intervention

Most experimental studies (e.g. Dovidio et al. 2004) follow the footsteps of the seminal study of Batson, Early, and Salvarani (1997). The typical sequence of experimental studies is to (1) expose subjects to a story (in the form of a vignette, audio recording, video etc.) about a member of an out-group, (2) instruct a subset of individuals in the treatment group to listen to the story while taking the person's perspective (as opposed to no instruction), and (3) compare attitudes toward that group via a post-stimuli survey.

In contrast to these approaches, we encourage perspective taking through the treatment activity itself rather than through a combination of an activity with instruction. The treatment is an online role-playing game, utilizing parts of a choose-your-adventure book, *Gypsy Maze* (Kardos and Nyári 2004). The original role-playing book was edited for style and brevity to be more suitable for an online game. We also produced two separate versions of the story to match the gender of the participant.

Gypsy Maze recounts the story of an 18-year-old Roma adolescent who arrives in Budapest, Hungary to start a new life. The narrative features a realistic account of what life might be like for a Roma adolescent: the story consists of vignettes in which the main character tries to find a sublet, buys groceries, and looks for a job. Crucially, the story is written from the perspective of the Roma protagonist, and describes what he/she senses in his/her social and physical environment. The text is written in second person, for example, "You are approaching the building." We provide the full script of the story in the SM (Section F, see also Table F2).

Two additional features of the game enhance perspective taking. At several points of the story, participants can make decisions that ostensibly affect the narrative (for example, they can choose which job ad to respond to or which apartment to see). At other times, participants are asked to flip a coin and continue the storyline based on the coin flip, aiming to simulate how chance influences the fortune of a Roma person with the deck stacked against them. In fact, the narrative was unrelated to these choices, but this remained unknown to participants. We chose to fix the narrative so that each participant in the treatment group was exposed to the same narrative.

Our use of a role-playing activity to enhance perspective taking is based on rich psychological research. First, pretense and role playing are associated with the ability to see the world from others' viewpoints (Leslie 1987; Kavanaugh 2006) and enhanced perspective taking (Bergen 2002). Second, instructed role-playing trainings have been successfully used to increase perspective-taking abilities (Marsh, Felicisima, and Barenboim 1980). Taken together, we expected that the readers would identify with the imagined character through their own actions and decisions, and thus invoke both empathy and the cognitive mechanisms of perspective taking.

Subjects

Our subjects were participants of the Hungarian Life Course Survey (HLCS), a longitudinal study that was administered annually in six waves to a sample randomly selected from the population of eighth graders in Hungary in 2006 (Simonovits and Kézdi 2016). Three features of the HLCS render it suitable for our purposes. First, as a result of the sampling process, the HLCS population is homogeneous in terms of age (97% between 24 and 26 years of age), which made it easier to design a single role-playing game that facilitates perspective taking. On a more practical level, we were able to secure a large enough sample without any compensation for participation, reducing the threat of demand effects. Finally, for each individual in the HLCS study, we had access to a rich universe

of background information, most importantly a pre-intervention measure of prejudice.

While face-to-face interviews were discontinued after 2012, email addresses were obtained from participants in 2014 that were reached at their most-recent address. Our sampling frame consisted of the 2,610 individuals who had provided their email address and responded to the full battery of questions about ethnic prejudice in the 2009 wave of HLCS. We restricted our sampling frame to these individuals because the availability of this pretreatment variable was crucial both to increase the precision of our estimates and to test balance after attrition. In particular, we stratified individuals in our sampling frame into 20 blocks based on this earlier prejudice measure and their gender. Upon entering the study, respondents were block-randomized to either the treatment group (and played the perspective-taking game) or the control group (and played an unrelated game). We invited participants via email and asked them to participate in the testing of an online game.

Response rates to the invitation emails were relatively modest: 579 individuals opened the first wave survey (a 22% response rate) and only 385 subjects participated in the follow-up survey (a 66% recontact rate). As we show in the SM, the subjects who participated in the experiment were very similar to the initial, representative sample in terms of pretreatment prejudice. At the same time, they scored almost 0.5 standard deviation higher on average on a standardized reading test in eighth grade. (Reading scores were linked from administrative records to all respondents in the HLCS). Participants also included much fewer respondents of Roma descent (1.2% compared to an 8.3% baseline). Because the intervention was highly reading intensive, the overrepresentation of subjects with high reading ability in the analysis sample may lead to higher treatment effects compared to what we would find on a sample that is representative of our sampling frame. We report these comparisons in Table B1 in the SM.

Outcomes

The main outcome of interest is a battery of six questions tapping anti-Roma prejudice, chosen from a battery of items that were available for each individual in the sampling frame measured in 2009. These attitude questions (for example, "The inclination to criminality is in the blood of gypsies." "All gypsy children have the right to attend the same classes as non-gypsies.") have been utilized in most of the research on anti-Roma prejudice in Hungary (e.g., Székelyi, Örkény, and Csepeli 2001; Karácsony and Róna 2011; Simonovits and Kézdi 2016). We describe the wording of these items and compare their distribution to benchmarks from national surveys in the SM (Table B2).

We defined the dependent variable of our experiment as a simple index of the responses to these six survey items standardized by the mean and variance in the control groups at wave one. (We reversed the scoring for the questions where agreement indicated no prejudice against the Roma.) We opted for this simple measurement strategy for clarity and because of the extremely high correlation of this measure with other versions estimated via factor analysis in the pretreatment measurement. In the follow-up wave, we included the same items along with three more items measuring attitudes about the Roma (see SM). We added these items to reduce the possibility of demand effects resulting from survey takers responding to the same exact items twice. Our results are identical when we construct our second wave measure of prejudice only based on the six items in both waves (and pre-registered in the PAP).

While the intervention was designed to target attitudes toward a specific group (the Hungarian Roma), we also assessed possible transfer effects—an increase in positive attitudes toward other groups. Existing studies about perspective taking have found no evidence for spillovers to other groups, possibly because the treatments they used were too specific to the targeted out-group (Todd and Galinsky 2014). On the other side, secondary transfer effects are well-documented in intergroup contacts (Pettigrew 2009).

To measure transfer effects, we included a feeling thermometer tapping attitudes about homeless people and refugees. We included similar questions for three other groups—old people, doctors, and politicians—for which we expected no effect, to test for placebo effects. For each of these groups, we asked participants to indicate how good or bad their opinion was about them using "sliders" on top of each other with labels "Very Bad," "Neutral," and "Very good". We measured these variables on 100-point scales (with the numerical values not shown to respondents).

Finally, we also measured experimental subjects' voting intentions to assess the possible downstream effect of the intervention on far-right voting. In the context of Hungary's multiparty system, we were particularly interested in the effect of the intervention on voting intentions for Jobbik, an overtly racist far-right party in Hungary. While Jobbik has moderated its platform since its founding in the early 2000s, it broke into national politics in the general election of 2010 after an explicitly racist campaign (e.g. Simonovits and Kézdi 2016) and its supporters have been found to have high anti-Roma prejudice (Karácsony and Róna 2011). Our outcome variable for far-right voting is a binary indicator taking the value of one if a respondent chose Jobbik and zero otherwise.

Experimental procedure

The experiment proceeded as follows. First, we asked some basic demographic questions from each participant. Next, participants in the treatment group were given instructions about the role-playing game. They proceeded to play the game and they were asked to write about their impressions. The outcomes of interests (that is, the prejudice battery) were asked after this block to members of the treatment group and

immediately following the demographic question in the case of the control group. Finally, all respondents were asked to play the placebo game.

The placebo game was an emotion-guessing task in which subject were asked to choose an adjective that they thought best described how a person depicted in a photo was feeling. The photos were taken directly from the Reading the Mind in the Eyes test (Baron-Cohen 1995). We asked participants in the treatment group to also play the placebo game, so that the only difference between the experimental procedures across treatment groups is the perspective-taking game. Thus, observed differences between treated and control group can be interpreted as the "marginal effect" of playing the perspective-taking game rather than the effect of playing the perspective-taking game compared to the placebo game. Note that individuals in the treatment group spent more time in the study before their outcomes were measured; fatigue might have influenced their responses.

The follow-up survey was fielded a month after the end of the first period, with Christmas and New Year's holidays in between the waves. We invited all participants who opened the invitation email to the first wave and thus were assigned to an experimental group. The follow-up included the same questions as the outcomes measured in the first wave as well as some additional items that we included to explore transfer effects and voting intentions. Asking similar questions in consecutive waves of a study like ours might increase demand effects. We believe that this may be less of a concern in our case. Participants in our experiment had completed six waves of the HLCS that included many of the same items in each wave and thus were used to answer survey questions of the same form and content over time. Moreover, some of the items asked at follow-up were new, including the three additional items on the prejudice score, the feeling thermometers measuring transfer effects, and voting intentions. Table F1 in the SM summarizes this information.

Statistical analysis

The key threat to identify the causal impact of our intervention was the considerable attrition from the study in both waves. Out of the 579 individuals that started the study and thus were assigned to treatment and control groups, we observed the outcomes for only 453 in the first wave, and 385 in the follow-up. Attrition was more severe in the treatment group, especially in the first survey: 34% compared with 9% in the control group. Attrition from the second survey was 38% in the treatment group and 29% in the control group. Higher attrition in the treatment group was likely caused by higher workload: median completion time was 24 minutes in the treatment group and 5 minutes in the control group. These differences highlight the possibility of differential attrition in the treatment group versus the control group that is related to potential outcomes (Gerber and Green 2012, chapter 7), which may bias our estimates.

Our research design permits us to explore and address this issue using pretreatment covariates. First, we demonstrate that attrition was not systematically related to the pretreatment measure of prejudice (SM, Table C1). With homogenous treatment effects that would suggest no bias in the estimates, our main analysis presents regression estimates adjusted by pretreatment covariates that were declared in a pre-analysis plan and include indicators for blocks based on the deciles of the outcomes variable measured in an earlier wave of the HLCS as well as gender, education, and Roma ethnicity (we report raw comparisons in SM, Table D2). This approach adjusts for all potential imbalance in baseline prejudice resulting from attrition that is captured by these covariates.

While this approach is both more conservative and better powered than most applications (see Broockman, Kalla, and Sekhon 2017), it still allows for potential bias resulting from attrition if treatment effects are heterogeneous. Covariate adjustment in our main regressions accounts for imbalance in terms of baseline prejudice (that is, potential outcomes when untreated), but it may not account for a possible relationship between the propensity to drop out of the study and potential treatment effects. In that case, differential attrition in the treatment and control groups could lead us to either overestimate or underestimate the true average treatment effect.

We employ two strategies to address this issue. First, assuming attrition that is random conditional on observables, we re-estimate the effects conditioning on all predictors of attrition, and apply multiple imputations for missing outcome values and inverse probability weighting. The results of these analyses are similar to those reported in the main text (SM, Table C2). Second, we estimate bounds on the treatment effect on always-reporters (that is, subjects who would not drop out in either experimental group) following the procedure by Lee (2009). The results (Table C3) are, again, consistent with our main estimates, although the point estimates for these bounds include zero in the case of the immediate measure of prejudice. Taken together, our analysis detects scant evidence of selective attrition and provides evidence that our results are robust to—even extreme—forms of selective attrition working in favor of our results.

RESULTS

The first column of the table shows that the immediate effect of the treatment was large, amounting to about a third standard deviation in terms of our prejudice scale [95% CI for the absolute effect size is (0.14 to 0.46)]. Substantively, this difference is comparable to the difference between voters of the far-right and the center-right party in the control group. The second column reports the long-term effect of the treatment on the full sample of subjects that provided responses in the follow-up wave. The estimated effect is only a little smaller, amounting to two-thirds of the immediate effect [95% CI for the absolute effect size is (0.09 to 0.33)].

TABLE 1. The Effect of the Intervention on Anti-Roma Prejudice

	Dependent variable:				
	Standardized prejudice score in . . .				Vote intention far-right
	Wave 1 (1)	Wave 2 (2)	Wave 1 (3)	Wave 2 (4)	Wave 2 (5)
Treatment effect	–0.298 [0.072]	–0.208 [0.054]	–0.341 [0.074]	–0.340 [0.056]	–0.119 [0.053]
Control mean	0.000	0.000	0.008	0.003	0.432
N	453	385	328	328	369
R-squared	0.079	0.051	0.081	0.067	0.072
Participants with non-missing outcomes in . . .	W 1	W 2	W 1 & 2	W 1 & 2	Wave 2

Note: Estimates are from simple linear regressions. Each specification controls for block fixed-effects, gender, ethnicity and education (three indicators for high-school, vocational, and college degrees). Robust standard errors in brackets.

On one hand, the reduction in the magnitude of the treatment effect could be due to decay. On the other hand, it is also possible that the difference in the point estimates is due to differential attrition across the two waves. To explore this possibility, in columns (3) and (4) we report estimates from the same specifications as before, but we restrict the sample to only include subjects who completed both waves (328 individuals). When comparing immediate and long-term effects on this sample, we find absolutely no evidence of decay: the point estimates are virtually identical. This suggests that differences across immediate and long-term estimates on the full sample are driven by subjects who dropped out of the study before or during the treatment and, as a result, they were excluded from the immediate comparisons but not from the follow-up survey. We present additional analysis supporting this interpretation in the SM.

The fifth column of the Table 1 shows that the treatment reduced far-right support by 12%, compared to the baseline of 43% in the control group. On one hand, these results provide strong causal evidence that support for the far-right (at least in our sample) is rooted in anti-Roma prejudice. On the other hand, to our knowledge, this constitutes the first evidence of a perspective-taking intervention impacting political preferences.

Beyond the main effects on prejudice and voting intentions, our results (SM, Table D1) show clear evidence of both the existence and the limits of transfer effects. On one hand, we find that the intervention increased affect toward refugees. The magnitude of the effect is 5.7 points (on a 0–100 scale), or 23% of the control mean, similar to our estimate for the Roma (a 3.9 points effect, or 14% of the control mean). On the other hand, the treatment did not improve attitudes toward the other groups, supporting the conjecture that transfer effects are conditional on both the perceived similarity of the targeted and nontargeted out-groups (Pettigrew 2009) and the baseline affect for these groups (Todd and Galinsky 2014).

While our design is not well suited to identify the exact mechanism through which the intervention operated, we elicited treated participants' thoughts through an open-ended question immediately following the game. Without baseline measurement and measures for the control group, we are unable to carry out formal tests, but these qualitative responses are indicative of several mechanisms at play. The answers suggest that most subjects were indeed "immersed" in the experience, and many of them tried to take the perspective of the game's protagonist. Many of these responses were consistent with the affective mechanisms corroborated by previous research on perspective taking: several subjects mentioned that they experienced the same emotions as the character they were asked to play (*parallel empathy*), while others wrote about their sympathy toward the main character (*reactive empathy*). We describe these qualitative responses in more detail in the SI.

CONCLUSIONS

This study documented the effects of an intervention that targeted ethnic prejudice using an online game that facilitated perspective taking. We found robust evidence that participation in the game led to a large reduction in anti-Roma sentiment that persisted for at least one month. The effect also spilled over to attitudes toward refugees, another stigmatized out-group in present-day Hungarian society. Perhaps most importantly from a political point of view, the intervention led to a substantial reduction in voting intentions for the racist far right party Jobbik.

Our results have important implications for the comparative politics literature on ethnic conflict. Much of this literature focuses on the structural factors that drive conflict between groups such as the relative size of ethnic groups (Posner 2004), geography (Enos and Gidron 2016), or the role of elites in maintaining ethnic cleavages (e.g., Eifert, Miguel, and Posner 2010). Given the rigidity of these factors, it appears that ethnic conflict is inevitable. In contrast, decades of research in social psychology indicates that intergroup attitudes and behavior react to even subtle cues (e.g., Tajfel 1970). We believe that shifting a focus toward the individual level determinants of prejudice could provide fruitful

avenues to design interventions that can ultimately reduce prejudice. This is all the more important since, like most experimental work, the generalizability of our findings is limited by certain features of our research design.

First, our focus on a single case (a specific out-group in a specific political and cultural context) certainly limits the breadth of the inferences we can make about how to reduce prejudice in general. Second, to the extent that prejudiced attitudes are more malleable among young adults, our results might overstate the possible effect of a similar intervention implemented on an older sample. Third, given that individuals in the sampling frame with lower reading ability (likely reflecting lower educational attainment later) were less likely to participate in the study, it is possible that online interventions that rely on reading would be less effective in reducing prejudice among less-educated people. Finally, because our intervention was complex and our study provides relatively little direct empirical evidence on the exact mechanism through which our treatment reduced prejudice, it is difficult to tell which element of the intervention was the key to its effectiveness.

These limitations notwithstanding, the key building blocks of our design (entertainment-based but interactive intervention, conducted online, and evaluated using a large sample with available pretreatment covariates) are relatively straightforward to implement in a large variety of social settings. Future research should evaluate similar interventions to investigate their effects on prejudice in other contexts, test their political significance, and reveal the underlying theoretical mechanisms. Such an endeavor would be important not just to improve our understanding of ethnic prejudice but, perhaps, even more importantly, to help in reducing it.

SUPPLEMENTARY MATERIAL

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