Seeing the world through the other's eye: An online intervention reducing ethnic prejudice

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Abstract

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 by-product, also reduced antipathy toward refugees, another stigmatized group in Hungary and reduced vote intentions for Hungary's overtly racist, far right party by 10%-points. 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.

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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 interventions that can reduce inter-group hostility is that most proposed antecedents of prejudice (e.g. personality, the nature of inter-group 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 inter-group contact to reduce prejudice are often extremely difficult, costly or time-consuming to achieve under realistic circumstances. In this study, 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 perspective-taking intervention to reduce prejudice against an ethnic minority. The intervention was a "choose-your-own-adventure-game", which required participants to assume the role 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 Kezdi, 2016). Ethnic relations between the Roma and the majority have been wrecked by both sporadic cases of ethnic violence and the rise of

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Jobbik, a far right and overtly racist party as the second largest political force (Karacsony and Rona, 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 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 its 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 et al., 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 in order 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 several dozens of subjects per cell. Third, our statistical analysis of the intervention was fixed before the data collection in a pre-analysis plan, to minimize researcher-degrees of freedom (e.g. Franco et al., 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 in order to accurately assess both the magnitude and the duration of possible effects (c.f. Broockman et al., 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 attentions to also account for the possible spillover effects of perspective taking. On the one hand, we explored whether taking the perspective of one out-group (i.e. a Romani person) could reduce prejudice about another stigmatize out-group (e.g. 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 inter-group 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 (Karacsony and Rona, 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 towards 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 inter-group 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 (i.e. affect towards refugees) and intended behavior motivated by prejudice (i.e. 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 pre-analysis plan (PAP) were implemented and are reported in the paper. We provide the full list of departures from the PAP in the Supporting Information (SI).

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, p. 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 the one hand, previous research has identified increased empathy towards the targeted out-group (Batson, 2011). In general, such affective responses are classified as either between 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 non-dispositional *vis-a-vis* dispositional factors in explaining the behavior of out-group members (Vescio et al., 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 and coauthors (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 towards 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 Nyari, 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.

The 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 protagonists, and describes what he/she senses in his/her social and physical environment. The text is written in second person, e.g. "You are approaching the building". We provide the full script of the story in the SI (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 (e.g. 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 story line 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 is exposed to the same narrative.

Our use of a role-playing activity to enhance perspective-taking is based on a rich psychological research. First, pretense and role-playing are associated with the ability to see the world from others' viewpoint (Leslie, 1987; Kawanaugh, 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 8th-graders in Hungary in 2006 (Simonovits and Kezdi, 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 percent 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 pre-treatment 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% re-contact rate). As we show in the SI, the subjects who participated in the experiment were very similar to the initial, representative sample in terms of pre-treatment prejudice. At the same time, they scored almost 0.5SD higher on average on a standardized reading test in 8th 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 over-representation 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 SI.

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 (e.g. "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 et al., 2001; Karacsony and Rona, 2011; Simonovits and Kezdi, 2016). We describe the wording of these items and compare

their distribution to benchmarks from national surveys in the SI (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 1. (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 pre-treatment measurement. In the follow-up wave, we included the same items along with three more items measuring attitudes about the Roma (see SI). 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 towards a specific group (the Hungarian Roma) we also assessed possible transfer effects – an increase in positive attitudes towards 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 effect is 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, in order 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' vote intention 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 vote 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 Kezdi, 2016) and its supporters have been found to have high anti-Roma prejudice (Karacsony and Rona, 2011). Our outcome variable for far-right voting is a binary

indicator taking the value of 1 if a respondent chose Jobbik and 0 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 (i.e. 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 the 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 Years 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 SI 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 pre-treatment covariates. First, we demonstrate that attrition was not systematically related to the pretreatment measure of prejudice (SI, Table C1). With homogenous treatment effects that would suggest no bias in the estimates. Our main analysis presents regression estimates adjusted by pre-treatment 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 SI Table D2). This approach adjust 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 et al., 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 (i.e. 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 croups 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 applying multiple imputations for missing outcome values and inverse probability weighting. The results of these analyses are similar to those reported in the main text (SI Table C2). Second, we estimate

bounds on the treatment effect on always-reporters (i.e. 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 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	Wave 2	Wave 1	Wave 2	Wave 2
	(1)	(2)	(3)	(4)	(5)
Treatment effect	-0.298	-0.208	-0.341	-0.340	-0.119
	[0.072]	[0.054]	[0.074]	[0.056]	[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 effect 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 them to the sample of individuals who finished both surveys (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 SI.

The follow-up survey measured voting intentions, and we show results on intentions to vote for the racist far-right party Jobbik (Table 1, column 5). Stunningly, the treatment reduced far right support by 12%-points, compared to the baseline of 43% in the control group. On one hand, this results provides 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 (SI, 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 towards 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 towards the other groups, supporting the conjecture that transfer effects are conditional on both the perceived similarity of the targeted and non-targeted 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 towards the main character (*reactive empa*thy). We describe these qualitative responses in more detail in the Supporting Information.

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 towards refugees, another stigmatized out-group in present-day Hungarian society. Perhaps most importantly from a political point of view, the intervention lead to a substantial reduction in vote 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 et al., 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 towards 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 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 pre-treatment covariates) are relatively straightforward to implement in a large variety of social settings. Future research should evaluate similar interventions in order 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.

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Seeing the world through the other's eye: An online intervention reducing ethnic prejudice

Supplementary Information: Online Appendix

A: Departures from pre-analysis plan.

We registered our design in the EGAP repository (http://egap.org/design-registrations, ID: 20161128AA) before we started the data collection. All analysis specified in the pre-analysis plan (PAP) was implemented and is reported in the paper. Below we provide the full list of departures from the PAP.

1. The PAP was ambiguous about individuals with missing pre-treatment outcomes (that we used to define blocks). Eventually, we decided exclude these respondents from the sampling frame, because we could not have established as-if-random attrition for these individuals. We used these individuals (i.e. those with valid email addresses but missing pre-treatment prejudice scores) for our pre-tests exploring response rates.

2. The PAP was ambiguous about what analyses we would conduct in the case of substantial attrition. While in the PAP we wrote that we would implement extreme bounds (Horowitz and Manski, 2000), we later realized that this procedure is inappropriate in our context because (1) our main dependent measure is theoretically unbounded (its range depends on the number of questions used to construct the scale) and (2) the rate of attrition was simply to high to make extreme bounds informative even with implausibly large treatment effects.

   We eventually decided to use trimmed bounds (Lee, 2009) as well as parametric procedures that either re-weight the data by the predicted probability of attrition (Inverse Probability Weighting) or impute the missing values of the outcome variable based on pre-treatment covariates.

3. We added additional dependent measures in the follow-up wave (three additional items measuring prejudice, the feeling thermometer and vote intention). The effect of the treatment on these outcomes was analyzed the same way as for the outcomes specified in the PAP.

B: External validity

Selection into the study. We first describe the demographic composition of our sample exploring how it was affected by attrition from the Hungarian Life Course Study (HLCS) *before* our study and self-selection into our study. To do so, we rely on a set of variables that were measured for each individuals in the sampling frame such as demographic information (age, gender, ethnicity), a measure of prejudice observed in the 2009 wave of the HLCS as well as a standardized measure of reading ability available for each participant of the HLCS.

Table B1 compares our analysis sample to the first wave of the HLCS as well as our sampling frame. The HLCS was a survey representative of the population of 8th graders in Hungary as of 2006. Our sampling frame was restricted to 2,600 individuals who (1) provided their email address to HLCS and (2) had non-missing responses to the items in the 2009 wave which we used to

construct pre-treatment prejudice scores from. Comparing columns 1 and 2 reveals that our sampling frame roughly preserved the demographic composition of the HLCS with the notable exception of ethnicity, with the proportion of participants identifying as Roma reduced to a half. Possibly because our study involved a reading intensive task in the treatment group, our sample over-represents individuals with higher reading scores. Finally, in terms of the target attitude of our study (i.e. prejudice against the Roma) or sample was remarkably similar to the sampling frame as well as the baseline HLCS.

Table B1: External validity

	Baseline sample [n=10,022]	Sampling frame [n=2,610]	Opted in to study [n=579]	Finished wave 2 [n=385]
Age	24.9	24.8	24.7	24.7
Female	48.5%	45.9%	52.5%	54.3%
Roma	8.3%	3.9%	1.2%	0.8%
Prejudice (pre-treatment)	0.00	0.09	0.08	0.05
Reading score	-0.11	-0.07	0.31	0.41

Note: *Entries are means of pre-treatment variables for the baseline HLCS sample (column 1), the sapling frame (column 2), individuals who opted into the study (column 3) and individuals who finished the second wave (column 4).*

Prejudice in the sample and the population. We provide nationally-representative benchmarks for anti-Roma sentiment and support for Jobbik, in order to assess how our sample and sampling frame differs from the general public. First, we used data from the 2009 Hungarian Panel Election Study (a probability sample of Hungarian adults) which includes six of the total 9 survey measures of anti-Roma prejudice we used in the study. Second, we compared the reported vote intention among our subjects in the second wave of the experiment to corresponding numbers from a national probability sample collected by Median, a leading Hungarian polling firm.

We report these comparisons in Table B2. Comparing average agreement with these statements shows that the baseline sample (column 2) was very similar to the general Hungarian public (column 1) in terms of expressed prejudice and so was the subset of HLCS respondents who opted into our study (column 3).

Comparing far-right support in our analysis sample and the general population (columns 3 vs. 4) reveals that the proportion of those supporting Jobbik was almost twice as high in our sample than in the general public. Restricting the comparison to those in the representative survey who were below 30 years old (column 5) shows that this discrepancy is at least partly explained with younger voters supporting Jobbik at higher rates.

Table B2: Comparing the prevalence of prejudiced attitudes in the HLCS to the general population

Survey	HPES	HLCS	Experiment	Median	Median,
Year	2009	2009	2017	2017	2017
The Roma should be given more government assistance	0.21	0.14	0.15	-	-
The growth of the Roma population is a threat to the security of the society	0.67	0.63	0.59	-	-
Every Roma child has the right to study in integrated classes	0.75	0.72	0.71	-	-
Criminality is in the blood of the Roma	0.59	0.59	0.45	-	-
Many Roma do not work because they do not get jobs	0.48	0.46	0.41	-	-
It is great that there are still bars that do not admit Roma	0.43	0.55	0.5	-	-
Intends to vote for Jobbik (%)	-	-	38.48%	20.20%	28.40%

Note: *Because the same survey questions are measured using 4-point scales in the HLCS and 5-point scales in the Hungarian Panel Election Study and our experiment, we recoded responses to range from 0 to 1 and the reported values are the means of these responses.* Sampling weights are used in the first two columns to render these samples representative of their target populations (Hungarian adults in the case of the HPES and the population of 8th grader as of 2006 in the case of HLCS).

C: Attrition.

Introduction Attrition from the first survey was 34% in the treatment group compared with 9% in the control group; attrition from the second survey was 38% in the treatment group and 29% in the control group. These differences are substantial, especially in the first survey wave. Thus an important threat to our empirical strategy to estimate the causal effect of the intervention is 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). This would happen for instance, if assignment to the treatment group induced more prejudiced subjects (in the absence of treatment) to drop out of the study with a higher likelihood. To the extent that treatment effects are heterogeneous, attrition would also lead to bias if assignment to the treatment group induced subjects who would experience a smaller treatment effect to drop out of the study with a higher likelihood.

Because we can only observe the differences in post-treatment prejudice among those with non-missing outcome measures, the question such comparison yields unbiased estimates of the average effect. If attrition was completely random (i.e., unrelated to potential outcomes) in both the treatment group and the control group, our comparison would yield an unbiased estimate of the average treatment effect. If attrition was non-random in the same way in the treatment group and the control group our comparison would still yield an unbiased estimate for the average treatment effect if individual level treatment effects are constant, and an unbiased estimate of the average treatment effect for always-reporters (i.e., those that would answer the survey questions whether assigned to treatment or not) even when treatment effects are heterogeneous. However, if attrition

was non-random in different ways in the two groups, a possibility described in the previous paragraph, we would fail to identify either effects without invoking additional assumptions about the nature of attrition.

We address this issue in three ways. First, we make use of the rich set of pre-treatment variables to explore whether attrition was in fact related to observables that may proxy potential outcomes, and whether those relations appear different in the treatment group versus the control group. The results of this analysis are broadly consistent with attrition being unrelated to potential outcomes, though such test can be misleading because we may lack the power to detect small imbalances in attrition with our sample size. Second, we take the results of these regressions to estimate the average treatment effect assuming attrition on observables (i.e., that non-randomness of attrition is fully captured by the pre-treatment covariates). We show results from three procedures, each conditioning on all predictor variables: Multiple Imputation (imputing the likely values of missing outcomes), Inverse Probability Weighting (reweighting the sample by the inverse of the predicted attrition probabilities), and unweighted regression without imputations. Third, we use the bounding procedure developed by Lee (2009) to allow for differential attrition related to potential outcomes not captured by observables. With the assumption of "monotonicity" (here meaning that assignment to treatment may increase but not decrease attrition), it yields conservative lower and upper bounds to the average treatment effects for the always-reporters.

Predicting attrition Table C1 shows the results of our regressions that attempt to predict attrition with the help of pre-treatment variables. These are linear probability models with robust standard error estimates. Columns 1 and 2 show the results for attrition from the first survey for treated and control while column 3 shows the results for two groups pooled with an interaction. Comparing the coefficients in columns 1 and 2 indicate differential attrition related to the predictor variables, and the coefficients on the interaction terms in column 3 give direct estimates of those differences. Columns 4, 5 and 6 repeat the exercise for attrition from the second survey.

Several patterns are noticeable. First, our pre-treatment measure of prejudice is not related to attrition from the first survey, and there are no differences between treated and control. That suggests that one of the potential outcomes, prejudice in the absence of treatment, is not related to attrition, and is definitely not differentially related to attrition in the two groups. The same is less clearly true for attrition from the second survey, where assignment to the control group appears to have led more prejudiced people to drop out with a higher likelihood. This pattern suggests that assignment to treatment made those with lower potential non-treated prejudice more likely to drop out of survey two, leading to a selection bias *against* our findings.

Table C1: Predictors of attrition

	Attrition from survey 1			Attrition from survey 2		
	Treated (1)	Control (2)	Pooled (3)	Treated (4)	Control (5)	Pooled (6)
Unconditional attrition rate (%)	**34%**	**9%**	**22%**	**38%**	**29%**	**34%**
Predictor variables						
Pre-treatment prejudice	-0.020	-0.021	-0.021	-0.024	0.037	0.037
	[0.028]	[0.014]	[0.014]	[0.027]	[0.028]	[0.028]
Reading	-0.075	0.004	0.004	-0.076	-0.039	-0.039
	[0.035]	[0.018]	[0.018]	[0.036]	[0.033]	[0.033]
Female	-0.055	-0.047	-0.047	0.016	-0.046	-0.046
	[0.057]	[0.034]	[0.034]	[0.059]	[0.054]	[0.054]
Age	-0.107	-0.007	-0.007	0.036	-0.027	-0.027
	[0.056]	[0.027]	[0.027]	[0.060]	[0.050]	[0.050]
College	-0.008	-0.122	-0.122	0.039	-0.060	-0.060
	[0.139]	[0.080]	[0.080]	[0.144]	[0.113]	[0.113]
High school	-0.040	-0.094	-0.094	0.025	0.068	0.068
	[0.135]	[0.082]	[0.082]	[0.140]	[0.105]	[0.105]
Capital	0.126	-0.024	-0.024	0.107	-0.077	-0.077
	[0.093]	[0.040]	[0.040]	[0.091]	[0.069]	[0.069]
City	0.033	0.055	0.055	0.050	0.004	0.004
	[0.066]	[0.042]	[0.042]	[0.068]	[0.063]	[0.063]
Treated			0.162			0.013
			[0.152]			[0.171]
Treated × pre-treat prejudice			0.001			-0.061
			[0.031]			[0.039]
Treated × reading			-0.079			-0.037
			[0.039]			[0.049]
Treated × female			-0.008			0.062
			[0.066]			[0.080]
Treated × age			-0.100			0.063
			[0.062]			[0.077]
Treated × college			0.114			0.099
			[0.160]			[0.183]
Treated × high school			0.054			-0.042
			[0.157]			[0.175]
Treated × capital			0.150			0.184
			[0.102]			[0.114]
Treated × city			-0.022			0.045
			[0.078]			[0.093]
Constant	0.360	0.198	0.198	0.343	0.330	0.330
	[0.132]	[0.076]	[0.076]	[0.138]	[0.102]	[0.102]
Observations	292	287	579	292	287	579
R-squared	0.053	0.042	0.139	0.036	0.048	0.049

Notes. Estimates from linear probability models. Pre-treatment prejudice and reading are standardized; age is centered on 25. Robust standard errors in brackets.

Second, higher reading score is associated with lower attrition in the treatment group but not in the control group (survey 1), or less so in the control group (survey 2). That is a very intuitive result as the treatment tasks preceding survey 1 involved a lot more reading than the tasks in the control group. The coefficient estimates on the other predictor variables are imprecise and do not show a clear picture: older subjects are less likely to drop out in the treatment group in survey 1 but not in the control group and not in survey 2; higher education may be associated with lower attrition in the control group in survey 1 but not in the treatment group and not as clearly in survey 2; subjects living in Budapest instead of rural areas drop out from the surveys with higher likelihood in the treatment but not in the control group, but this difference is not there for subjects that live in other large cities.

Taken together, these estimates do no suggest a clear story of endogenous attrition that is different by treatment assignment. But they don't rule out that, either. On one hand, because the null-hypothesis for these models is the equality of coefficients predicting attrition across experimental groups, our sample size places limits on the statistical power of test detecting these differences. Put more simply, the same way as treatment effects are more difficult to detect with a smaller sample, systematic attrition is also harder to detect with smaller samples. On the other hand, even if attrition was not systematically related to observables, it could still be related to unobserved confounders that are not captured by the set of covariates in our regressions. Thus, we caution the reader against making strong conclusions about the validity of our estimates based on these regressions.

Treatment effects assuming attrition on observables: Our second approach makes a further step assuming attrition on observables. This amounts to assuming that attrition is non-random with potentially different patterns among treated and untreated, but all that non-randomness is fully captured by the predictor variables. We first take the estimates of the regressions, use them to impute missing values for the prejudice scores (Multiple Imputation), and estimate the average treatment effect using the observations including the imputed ones. As an alternative we use the coefficient estimates to re-weight the sample (Inverse Probability Weighting). We include the randomization blocks as well as all predictor variables as controls when estimating average treatment effects in both procedures. As yet another alternative we estimate the average treatment with those control variables without the imputed values or weights (similar to the specification in the main text with more covariates). All three methods yield consistent estimates of the average treatment effect assuming attrition on observables; they differ in terms of efficiency and small-sample properties. Table C2 shows the results.

The results from the three procedures are very similar. The average treatment effect estimates on prejudice are somewhat smaller and less precise than the baseline estimates that control for randomization blocks only (Table 1 in the main text). These estimates still rule out zero or positive immediate effects with a 95% likelihood, but they do not rule out zero or positive effects for the delayed effect measure on prejudice. The effect estimates for voting on the far-right party Jobbik are virtually unchanged from the baseline estimates: they suggest a strong negative effect and rule out zero or positive effects.

Table C2: Average treatment effect estimates assuming attrition on observables

Dependent variable	Estimates with control variables		
	Multiple imputations	Inverse probability weighting	No imputations, unweighted
Prejudice (wave 1)	-0.210	-0.256	-0.257
	[0.076]	[0.083]	[0.084]
Prejudice (wave 2)	-0.144	-0.155	-0.159
	[0.088]	[0.093]	[0.093]
Jobbik vote	-0.117	-0.108	-0.105
	[0.044]	[0.050]	[0.050]

Note: Estimates from regressions controlling for the twenty randomization blocks as well as the predictor variables used in the regressions in table C1 (mean-differenced and fully interacted with treatment). Multiple imputations use ten imputed values for each observation with missing outcome variable. Inverse probability weighting uses the inverse of the predicted response probabilities for each outcome variable from logit models with the predictor variables (fully interacted with treatment) as weights. Robust standard errors are in brackets.

Bounds on the treatment effects Our third – and most conservative – approach allows for attrition on unobservables, with a possibility for an effect of treatment assignment on those patterns. We use the bounding procedure of Lee (2009) to estimate the average treatment effect among always-reporters (those that would answer the survey questions whether they are assigned to the treatment group or the control group). The procedure needs the assumption of monotonicity, here meaning that assignment to treatment may make some subjects drop out, but assignment to the control group would not make anyone drop out.

Table C3: Bound estimates of the treatment effect on always-reporters

Dependent variable	Lee bounds		Lee bounds tightened	
	Lower	Upper	Lower	Upper
Prejudice (wave 1)	-0.780	0.167	-0.650	0.044
	[0.110]	[0.127]	[0.115]	[0.099]
Prejudice (wave 2)	-0.374	0.050	-0.336	-0.003
	[0.139]	[0.133]	[0.127]	[0.089]
Jobbik vote	-0.157	-0.029	-0.161	-0.048
	[0.070]	[0.071]	[0.058]	[0.058]

Note: We follow the procedure in Lee (2009) using the leebounds command of Stata 14. Tightened estimates use the blocks used for randomization (ten equal-sized groups of pre-treatment prejudice interacted with gender). Bootstrap standard errors are in brackets.

The lower bounds obtained from these procedures reflect a case which the treatment effects would have been systematically stronger for all subjects that dropped out. In contrast, the upper bounds assume that the treatment effects would have been weaker for all subjects that dropped out. We show results from two variants of the procedure: the first one does not include any covariates in the estimation, while the second one uses the twenty randomization blocks to "tighten" the bounds. The tightened bounds are somewhat narrower and more precisely estimated indeed, but they yield the same qualitative conclusions. In the discussion below we will focus on tightened bounds.

Table C3 shows our estimated bounds. The estimated bounds allow for zero and positive effects for immediate prejudice but do not include such effects for the delayed measure of prejudice and, especially, for voting for Jobbik. These results show that our qualitative conclusions are likely to hold even in the face of systematic attrition that work in favor of our finding, even though we found scant evidence for such patterns of attrition using a rich set of pre-treatment covariates. Moreover, to the extent that attrition is systematic and works *against* our findings, the results reported in the main text underestimate the actual impact of our intervention.

D: Additional results.

Transfer effects. Table D1 reports the transfer effects of the treatment (i.e. the impact of the intervention on attitudes towards other out-groups). It shows that while the intervention reduced prejudice towards refugees – another stigmatized group in Hungary – it failed to do so for other out-groups (such as homeless people).

Table D1: Transfer effects

Dependent variable: Affect for…	Homeless	Refugees	Roma	Old people	Politicians	Doctors
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	1.3	5.7**	3.9*	0.3	1.3	1.4
	[2.4]	[2.1]	[1.8]	[2.5]	[1.9]	[2.0]
Observations	404	404	404	404	404	404
R-squared	0.035	0.074	0.059	0.014	0.010	0.024

Note: *Estimates are from linear regressions with controlling for block fixed effects, gender, education and Roma ethnicity. Robust standard errors in brackets.*

Raw comparisons. In the main text we reported treatment effects computed via multiple regressions, controlling for pre-treatment covariates. These covariates include indicators for blocks based on pre-treatment prejudice and gender (that were used to form groups for block-randomization) as well as ethnicity (Roma vs. not Roma) and education (indicators for vocational training, high school diploma and college degree). Table D2 below reports treatment effects without covariate adjustment.

Table D2: Raw differences between treatment and control

Dependent	Prejudice		Feeling thermometer						Vote intention
Variables	(W 1)	(W 2)	Homeless	Refugees	Roma	Old people	Politicians	Doctors	far-right (%)
Treated	-0.3	-0.1	1.4	5.1	2.7	0.4	1.5	1.8	10.2
	[0.1]	[0.1]	[2.0]	[2.5]	[2.5]	[2.2]	[2.2]	[2.3]	[5.0]
Constant	0	0	41.4	25	28.9	68.7	21.8	64.2	43.3
	[0.1]	[0.1]	[1.4]	[1.6]	[1.7]	[1.6]	[1.5]	[1.7]	[3.6]
Observations	453	385	404	404	404	404	404	404	369
R-squared	0.023	0.005	0.001	0.011	0.003	0	0.001	0.001	0.01

Note: *Estimates are from linear regressions with controlling for block fixed effects, gender, education and Roma ethnicity. Robust standard errors in brackets. Or measure of prejudice is standardized to have zero mean and unit variance, affect is measured on a 0-100 scale and vote intention is an indicator variable (multiplied by 100 for ease of interpretation).*

Additional analyses comparing immediate and long-run effects. In our main analysis (Table 1 in the main text) we showed that (1) the effect of the treatment was greater immediately after the intervention than in the follow-up wave but (2) the effect was very similar among those who provided non-missing responses to our outcome measure in *both* waves (always-reporters). Our interpretation of this finding was that the initial differences are likely to reflect differential attrition rather than a decay in the impact of the intervention.

Our argument was that because our design only permitted the measurement of immediate outcome among those who completed the online game it excludes non-compliers from the calculation of the treatment effect. In our context, non-compliers are those subjects who were assigned to the treatment group but dropped out of the study before or during the intervention. In contrast, because we invited all subject who had entered the first wave of the study to complete the follow-up questionnaire, outcome measures in the second wave are also available for non-compliers (Table D3).

One way to compare the immediate and long-run effect of the intervention is to shift our focus from intent-to-treat effects (ITT, i.e. the effect of being assigned to the treated group) to the Complier Average Causal effect (CACE), that is, the effect of the treatment on those who were actually exposed to it (Gerber and Green, 2012). In our case, under the assumption that attrition is independent of potential outcomes, conditional on observed pre-treatment covariates, our estimate of the immediate effect of the treatment already reflects a CACE, as it excludes non-compliers from its calculation.

At the same time, we can estimate the long-run CACE using instrumental variables regression. Formally, we estimate CACE via two stage least squares regressions where we use the treatment assignment as an instrumental variable for actual take-up. We define take-up as an indicator that takes the value of one if a subject actually started the game. In this specification we find that the

long run CACE is −0.25 and the 95% CI for the absolute effect size is [0.03 to 0.46]. Note that this is extremely close to the confidence interval around the immediate effects that was [0.14 to 0.46].

Table D3: Compliance and observed outcomes

	All treated subjects		Treated subjects who finished wave 1		Treated subjects who finished wave 2	
Compliance	N	(%)	N	(%)	N	(%)
Non-complier	61	20.89	0	0	29	15.93
Complier	231	79.11	192	100	153	84.07

Possible mediating role of mentalising. In our pre-analysis plan we specified a hypothesis about the effect of the treatment on subjects' performance in the emotion guessing game. We conjectured that if the treatment operates by increasing subjects' willingness or ability to empathize with others, then this effect (i.e. possible mechanism) can be captured by that game. As specified in the pre-analysis plan, we measured empathy as the proportion of emotions guessed correctly by each subject (out of 10). We found that treated and control subject performed similarly on the task (66.9% vs. 64.7%) and the estimated effect of the treatment was indistinguishable from zero, both with and without covariate adjustment.

It is difficult to interpret this null finding because it is consistent with several explanations. First, it is possible that while the treatment indeed increased respondents' empathy, performance in the emotion guessing game fails to capture this effect, because of a too low signal-to-noise ratio. Second, because treated subjects completed the emotion guessing task after participating in a lengthy reading exercise (the treatment) it is possible that they exerted less effort in the task due to fatigue. Finally, it is possible that the role of empathy played a moderating rather then mediating role: we found evidence suggesting that the treatment effect was stronger among subject who performed better on the emotion guessing task. However, again, because that measure is likely to capture a combination of ability and effort it is difficult to interpret this finding.

E: Qualitative evidence on mechanism

A formal quantitative test of why our intervention reduced prejudice is not possible to conduct with the data at hand because of the compound nature of the treatment. Because we anticipated a small sample size we assigned each subject in the treatment group to the same exact game (other than matching the participant's gender). Thus, it is not possible to assess if some parts or features of the game were especially important for the observed effect. Our resources to speak to the causal mechanism are limited to experimental subjects' responses to an open-ended question that asked them about their experience with the game.

Variable construction: Out of the 209 subjects in the treatment group, who finished the study 127 gave some kind of comment about the game. Three research assistant coded these responses based on a classification scheme that we developed after an initial examination of their content. The research assistants coded theses comments to create the following variables:

• Overall assessment of the game (good, bad, mixed)

• Perceived realism of the game (realistic, unrealistic, mixed)

• Whether the subject mentioned that they took the main character's perspective

• Whether if subject mentioned a specific emotion that they felt during the game

• Whether the subject mentioned what she thought the game's goal was

• Whether the subject provided any specific feedback on how to develop the game 13Some responses were coded by multiple raters to establish inter-coder agreement. In most cases coders agreed and in cases when they did not one of the authors made a final decision.

General assessment of the game: Even though the "game" had an extremely simple design, lacked any visual elements and they choices available to subjects were quite limited, its reception was surprisingly positive. Overall, 53 respondents provided positive comments compared to 15 negative and 21 "mixed" ones. Almost 50% of responses discussed how realistic the game was. Among them, about 45% wrote that the game was an accurate representation of reality, 35% complained that it exaggerated the discrimination faced by the Roma and about 20% took a position in between.

Among a third of the subjects who responded to the open-ended question specifically men- tioned feelings or emotions that they had during the game. Several subjects (about 15%) reported that they felt sad, angry or helpless during the game. In some cases, these responses read as if they were still written from the perspective of the game's protagonist (e.g. "I felt whatever I do I keep getting rejected"). These emotional reactions were often very strong: two subjects recounted that they were at the verge of crying and almost quit the game.

Other subjects had less intense reactions to the story and in their comments they simply described how they felt about the game itself. Some participants found the game boring and others wrote that they could not really identify with the main characters. In contrast, others described the game as entertaining (one respondent compared it to the games he played on his Play Station) and wrote that they felt sympathy and compassion towards the main character (20% specifically mentioned that they took the perspective of the main character). Yet others wrote that the game made them think about racial relations in Hungary without referring to any emotions towards the game itself.

Importantly, these responses also show that many of the subjects completed the game even though they had issues with the game, and at times got quite angry. While some subjects were simply annoyed because they thought that game was too long or boring others seemed outraged about what they thought was purposefully biased depiction of racial relations. Some denied the existence of the kinds of discrimination depicted in the game, while others stressed the responsibility of the Roma too.

F: Survey materials

Questionnaire and script for the game. Table F1 explains how the experiment proceeded. First, we asked some basic demographic questions from each participants. Next, participants in the

treatment group were given instructions about the role-playing game, proceeded to play the game and were asked to write about their impressions. The outcomes of interests (i.e. the prejudice-battery) were asked after this block from 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.

Table F1: Experimental protocol

Wave 1	Control	Treatment
Introductory questions	Yes	Yes
Treatment	**No**	Yes
Outcomes	Yes	Yes
Emotions game	Yes	Yes
Wave 2	Control	Treatment
Introductory questions	Yes	Yes
Outcomes	Yes	Yes

Demographics questions (both waves).

- What is your gender? (Male/Female)

- How old are you? (18-23/23- 28/Older than 28)

- What is the highest degree you have completed? (8 grade or less/Vocational school/High school/College or university)

- What kind of settlement do you live in? (Capital/County seat/Other town/Village)

Items measuring prejudice. The last three questions were only asked in wave 2. Items in italics were reverse coded. The response options for each question are "Strongly agree", "Rather agree", "Neither agree nor disagree" "Rather disagree" and "Strongly disagree".

- *Every Roma child has the right to study in ethnically mixed classes.*

- *There are no more criminal among the Roma than among the non-Roma of similar status.*

- Criminality is in the blood of the Roma.

- *Many Roma do not work because they do not get jobs.*

- It is great that there are still bars that do not admit Roma.

- The Roma should be completely separated from the rest of the society because they cannot coexist.

- The problems that the Roma face would go away if only they started working.

- The growth of the Roma population is a threat for the security of the society.

- *The Roma should be given more government assistance so that they can catch-up faster.*

Feeling thermometer. The feeling thermometer (included in wave 2) was prefaced with the following question: "Now we would like to ask your opinion about different groups that live in Hungary. We will list a few of them and we will ask you to use the "sliders" to indicate how good or bad your opinion is about the people that belong to these groups."

Vote intention. We measured vote intention with the following question: "If the general elections were held the next Sunday, which party's list would you be the most likely to vote for? Even if you think that there is no way would vote, we ask you to pick the party you like the most." The response options were Fidesz (the governing center-right party), MSZP (the main center-left opposition party), Jobbik (the far-right opposition party), Politics can be Different (an alternative, green opposition party), Dialogue for Hungary, Democratic Coalition and Together (the latter three are small center-left opposition parties).

Additional survey items. In wave 1, we asked two questions from people assigned to the treatment group. First, we asked them what their impression was about the game and provided a large text box for open ended responses (we analyze these responses in th SI). Moreover, we asked participants about the extent to which they "identified with the main character in the game?" (response options were "Completely"; "Somewhat", "Not really", "Not at all"). Finally, as part of the placebo game we also measured respondents' ability to guess the emotions of individuals based on black and white pictures. Originally, we sought to use this variable to explore the role of increased empathy as a results of the intervention (as specified in our pre-analysis plan) but we found no differences in the "performance" of treated and control subjects in the game.

The perspective taking game.

Subjects played the game online and after a short introduction (see below) they needed to click through a series of screens displaying the story without any visuals.

Structure. After an introduction, the game consisted of three "chapters". In the first one, the main character was looking for a sublet, then he/she was doing grocery shopping and in the third one he/she was looking for a job. To enhance perspective taking, subjects were repeatedly prompted to make decisions that ostensibly influenced the plot and other times they were asked to flip a coin so that the plot branched randomly. Table F2 lists how these elements appeared in each chapters.

Table F2: Storyline and structure

Part	Screens	Decisions	Random
Introduction/Instructions	4	0	0
Apartment search	10	3	3
Grocery shopping	2 or 3	1	1
Job search	8	2	1

Script for the perspective-taking game: In this game, you are the main character. Your destiny depends on your own choices (and some luck): you decide where to go and who to make friends with. The story is directed by your own desires and dreams. You can click through chunks of the story based on your choices and the directions provided. Sometime you will read: Toss a coin. In this case you probe your luck. The coin flip represents the role of chance in life. Please respect the game and follow the story according the to the coin flip. You will take the role of a Roma youngster, live his everyday life and strive in his circumstances. You can learn about life he lives through his own perspective. There is no right path, and you will find the one you like through trial and error. If you are ready, you can start the game now!

You were born eighteen years ago, in June in a Gypsy settlement near Szekszard. Your mother, as you know, long-dead, no news about your father. You might have some siblings somewhere in the country, but you never met them. Your childhood was spent in orphanages, in recent years in an institution in Vas County. You loved living there, the community made up for the family you wanted all your life. They did not care about you being Romani, you can hardly remember an offending comment and you never cared about the Roma yourself. After elementary school you attended vocational school for two years and then made your way to a high school providing a vocational. Here, too, you've completed two years when he reached the age of eighteen. You have been waiting for this moment all your life, to stand on your own feet. Already packed at last, said goodbye to your friends and teachers, the building. Many of them walk you to the train station. After hours a rather uncomfortable journey, you slowly run into the Eastern railway station. Entangled dreams and stories swirl in your head. You are scared, but you know that now you can achieve anything. Your net worth of two hundred and ten thousand forints is all in your pocket, that is what you received from the state after all the deductions. You nervously feel your dress as the train rolls in between the platforms.

The station is not as scary as you thought based on the story you had heard. You start walking towards the exit. The station is beautiful but the sheer mass of the crowd and the scale of the building scares you. In the underpass you see a group of bouncers with dogs, fortunately on the leash, so passing by them is more uncomfortable than dangerous. You reach the street and start thinking of where to stay for the night. In the end, you decide to look for your uncle Lajos in Baross street.

You have seen your uncle twice, at the most. Two years ago he offered that you can move in to his place, though he had not live in Budapest that time. Hello, Son He say as you walk up the staircase. He seems genuinely happy about seeing you. You walk though the narrow corridor leading to the

living room; the walls are covered by a large poster depicting a forest. Uncle Lajos sits next to you and lights a cigarette. After a minute of awkward silence, he stands up, starts walking and says If you wish, you can stay in the small room. The others are going to be be back soon. There are two bunk beds in the room, leaving no space for your luggage; a strong smell of cabbage penetrates the room. You eat anything, but you loathed cabbage in the institution already. You walk to the little balcony, looking at the yard, listening to the rather intense life of the apartment building. The tenants apparently live in the shared spaces of the building and there is no separation of in and out. People are loud, and there is a constantly changing scene of chatting arguing groupings. You are about to step back inside when a small group of people passes by you and enters the apartment. You walk after them apparently they are uncle Lajos's family that is you folks.

You have lived in Budapest for a week now. You still have some money left but life with your folks gets more and more awkward. Auntie Jolan gets m ore and more intense in pointing to the fact that you have overstayed. You don't wan to make them mad at you so you decide to look for a sublet. When you inform Auntie Jolan about your decision she eases up immediately and offer that you can stay as long as you want You know exactly what it means: the sooner you are out, the better. You are browsing through some websites offering housing and feel the notes in your pocket. You pick the four sublets that look the best among those you could possibly afford. One might be a little overprices, but all of them offer immediate move in. You call each of the landlords, and agree to check each of them out. Choose which one you visit first:

- You go to the one on Nemet street
- You go to the one on Rozsa street
- You go to the one on Dob street
- You go to the slightly expensive one on Jokai street

[*In the next screens the story unfolds independently of the choices subjects make but the list of options get updated accordingly.*]

Luckily you avoid the security personnel on the tram checking for tickets. It would not hurt to buy a monthly pass but then you would not have enough for the sublet Seven stops with tram 50 you keep repeating. The tram stops on every corner and you start to get confused. Eventually, another passenger helps you and points to a bunch of old apartment buildings. It takes you 15 minutes to find the place, you walk up to the 6th floor. There is a sticker on elevator door that reads Stop gypsy-crime. You imagine the apartment: it has to be all new and fancy!
You ring the bell and a middle-aged woman opens the door. Hello, I am here to see the room, we have talked over the phone, and Sorry, we already found someone else she cuts you off. But we have just spoken an hour ago you interrupt. Sorry, we already found someone else she says, and shuts the door on you. You stare at the door, curse and realize that it might be a long way before you find a new place. You look over the alternatives again,

[DISPLAY THE REMAINING THREE OPTIONS]

You call the landlord again, and go to see the apartment. The streets are dark, full of dog poop. You find the door and enter the building. It is old, but at least there is an elevator. You are looking for Mrs Szabo. No one responds to your ringing, so you knock on the door. You are about the

leave, when the next door opens and an old lady comes out. She tells you that the landlady lives on the ground floor and offers to go and find her. They show up in five minutes.

Now, flip a coin.

When the landlady sees you she starts in a confused ramble "explaining" that her nephew just called ten minutes ago to tell her that he needed to move in. You are sad but dont feel like arguing so you leave. Where do you go next?

[DISPLAY THE REMAINING TWO OPTIONS]

You are staring the buses passing by; you have never taken them before. You watch the locals, and do some window-shopping. Finally, you get to number 119, walk up the third floor and finally find door you are looking for. You ring the bell; and as the door opens you see an old guy sitting in the kitchen, half-naked. An old lady rushes out and quickly starts to talk to you: Are you the one looking for the apartment? What are you waiting for, come in!

Now, flip a coin

As the two of you walk into the flat the woman's behavior changes abruptly. She would not let you through the small kitchen and tells you that the rent is actually the double of the one in the ad. You say goodbye and leave. You turn back and ask So how come the price jumped so much? What made it so much better in two hours? She is shouting at you; while you make your way down the stairs you hear her last sentence You filthy gypsy, did you really think you would move in here? You get out of the building and start to consider your options. There is only one room left to check.

[DISPLAY THE LAST OPTION]

You walk to Blaha square and take the tram. At first it is very crowded but eventually it starts to clear up and you immediately realize why. An elderly woman next to you I holding her purse with both hands obsessively and two guys with moustaches are mumbling something about gypsies. As soon you take a seat the women sitting next to you stands up and pushes her against the standing crowd. The two guys continue their mumbling and now you can hear it clearly: They are thieves, all of them. They should be in jail. You feel scared instead of anger. Two security guys on the tram notice you and ask for your ticket. I dont have one, I just arrived to the city and didnt know where to get one, you say. My dear God, then just get off the train one of them says. You arrive to the apartment on foot. The house is in a terrible condition, and it is so dark that you can barely find the staircase. A lady comes out to your ring. And asks if you are looking for the room.

Now, flip a coin.

Come in, look around she says. My husband will be here soon. The apartment is a small studio, the bathroom is shared, in the common area of the building. They ask you to put down three months rent as a deposit. Now. The guy helps you to get some of the furniture from the basement, and they leave you alone. You are excited to think of how to furnish your first apartment ever. You happily leave to buy some food in the deli you saw on your way there.

You think about the next steps on your way to the deli. The sublet is great, you might be able to continue school as well, though you need to find a job first. You miss your friends, but you are sure to find new ones. The deli is small but with a big traffic; most are there to buy booze. We are closed a tall shopkeeper tells you, it has passed 8. You are about to leave when you realize that the deli is 24/7. What do you mean you are close, you ask. Just that we are closed he replies.

• Do you still want to shop here?
• Or do you look for another shop?

[If first option is chosen]
You are getting angry. A woman who was behind you in the line passes you and starts shopping around. You are furious; So, how come she can come in? you yell at the shopkeeper. Because she can, you know? We are open for folks who are a little lighter-skinned, you get it, you son of a bitch? You are speechless; some customers are staring at you but most of them just turn their heads. I am coming in anyway, you say in the end.

Now, flip a coin.

[IF HEAD]

All right, come in, then. He laughs. You scared me so much I wont be able to sleep. He says, but give you way. Do your shopping, but I will watch you and call the police if I see you are up to something. You pick up the groceries you need and keeping turning your head to see if they are watching you. On your way home, you wonder how come they let you in eventually. Maybe they actually got scared. Anyway, this place is screwed-up I am not going to come back anyway. Not that you got scared but it would be hellish if grocery shopping would be like that every time. You decide that you will find another deli and also start to look for a job the next day.

[IF TAIL]

The shopkeeper is now furious Just try you little piece of shit, he yells at you. You are preparing for a fight when the manager steps out and starts shouting at you; You will not make a scene here, I will call the police and tell them that some gypsy is stealing from my deli. You have a minute to get out of here. You turn around and walk home. You decide that you will find another deli and also start to look for a job the next day.

[If second option is chosen]

You are furious but realize that it is better to stay out of trouble and you look for another store. You find one nearby, enter and start shopping. You realize that one of the shopkeepers is staring at you and even asks if he can help you. You shake your head, and go to pay. You decide that if this happens ever again you will make a scene; show them that you cannot be treated like this. You decide that you will find another deli and also start to look for a job the next day.

You are excited about the job ads; there are not many places that do not require a high school degree but you still manage to find some. You start calling potential employers and you find to places that you could interview today. Which one you pick

• Warehouse job
• Machinist

[If first option is chosen]

You get to the factory fast and notice a familiar face: it is Peti whose little brother was your classmate back in the days. At that time Peti was not in the orphanage anymore and you might have met him three times. It is still nice to catch up with him, you entertain each other with some old stories. Then you start to talk about jobs: he tells you that he still has not gotten a job even though he had enrolled in several job training programs. No one hires gypsies. In the waiting room you are joined by a third guy, you wait for the manager together. He finally shows up, asks for your papers and you follow him to his office. He explains what the job is and after glancing over your papers he says that he can only hire one of you: the guy who showed up last. He is the only one with the necessary qualifications. The qualified applicant is a redhead. You feel an urge to yell at the manager but at that point Peti loses it: This was an ad for an unqualified job? What the hell do you mean that I dont have the qualification? I have what he has.. He slowly calms down, and the manager mumbles something about rules. The third applicant looks embarrassed and walks away. You leave the factory without a job, Peti is cursing but you feel more like crying. You say goodbye to each other and you decide that you will not give up on getting a job that easy.

[If second option is chosen]
The ad is for positions for an unskilled job as machinist. It is 5 to 9am; you were early. Maybe it is a good dream lingering in your head or the nice weather but you feel really good about this job. There are six guys sitting in the waiting room; the atmosphere is rather tense. A secretary glances out of her office and tells everyone with a smile that they are in bad need of workers and they are planning to hire at least 20 people. They call people in one-by-one; you wait 35 minutes but it feels like hours. The secretary show you the way, you walk along a neon-lit corridor and make your way up the stairs. You try your most self-confident smile when you knock on the door. The door opens and when the manager (about 35 years old) sees you he makes a strange face and even utters a quite Wow. He says in an easy-going fashion: Sorry, the position is filled. You are holding on the rail in the staircase, trying to lose it: But there are still folks down there and they told us you were still hiring you interrupt. I am really sorry Maybe next time he replies and disappears in his office. You hang out in the corridor for a little, disappointed but too tired to argue. You leave the factory.

You decide to check out the other job.
 [DISPLAY THE OPTION NOT CHOSEN]

The next day you find two more job ads. You are still traumatized by your experience yesterday but you have got to keep going. Which one do you choose to check out?

• Job in supermarket

• Cleaner job

[If first option is chosen]

The job in the supermarket doesn't look too bad. There are many people in the waiting room, apparently they have posted ads for multiple jobs. An HR person named Ica is giving some instructions to the applicants; you make your way towards the crowd around her. She asks for your papers and tells you to sit down and wait till you are called. Later you go to another room to talk to the manager; he asks you some questions and tells you to wait outside.

Now, flip a coin.

There are posters hanging on the wall some of them look like the ones you have seen at the optometrists. For a little while you entertain yourself by trying to decipher the letters that get smaller and smaller down the poster. You start to get hungry. Finally, the door opens and Ica comes along holding your papers. Sorry, maybe some other time she says. She smiles at you and hurries back to her office. You stand there, with your papers alone in the corridor. You try to think of where you might have screwed up. Maybe you said something inappropriate when talking to the manager? You leave the building and go to see the other job.
You enter the storage room in the warehouse and look at the piles of cheap boxes of laundry detergent. These are the ones you will need to pack up a older guy wearing suit tells you and pats your shoulder, I mean, you are the one looking for the job, right?. That is correct you reply But how did you know?. Well he says dryly, Folks who look like you are either here to apply for jobs, or than he cracks up at this own joke. Do not be offended, this was just a joke. You say you are okay and follow him in another room where you meet your boss-to-be. You do the paper work and go home.

[If second option is chosen]

The cleaning job doesn't look too bad. There are many people in the waiting room, apparently they have posted ads for multiple jobs. An HR person named Ica is giving some instructions to the applicants; you make your way towards the crowd around her. She asks for your papers and tells you to sit down and wait till you are called. Later you go to another room to talk to the manager; he asks you some questions and tells you to wait outside.

Now, flip a coin.

There are posters hanging on the wall some of them look like the ones you have seen at the optometrists. For a little while you entertain yourself by trying to decipher the letters that get smaller and smaller down the poster. You start to get hungry. Finally, the door opens and Ica comes along holding your papers. Sorry, maybe some other time she says. She smiles at you and hurries back to her office. You stand there, with your papers alone in the corridor. You try to think of where you might have screwed up. Maybe you said something inappropriate when talking to the manager? You leave the building and go to see the other job.

You enter the storage room in the supermarket and look at the piles of cheap boxes of laundry detergent. These are the ones you will need to pack up a older guy wearing suit tells you and pats your shoulder, I mean, you are the one looking for the job, right?. That is correct you reply But how did you know?. Well he says dryly, Folks who look like you are either here to apply for jobs, or than he cracks up at this own joke. Do not be offended, this was just a joke. You say you are okay and follow him in another room where you meet your boss-to-be. You do the paper work and go home.