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

Supplementary Information (Online Appendix)

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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, in order to assess how our sample and sampling frame differs from the general public. In particular, 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. We compare average responses to these items in Table B2. The results show 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).

Table B2: Anti Roma prejudice in the HLCS and the general population

Survey	HPES	HLCS	Experiment
Year	2009	2009	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

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 for the HPES and the target cohort 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.

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.

Table C1: Predictors of attrition

	Attrition from survey 1			Attrition from survey 2		
	Treated	Control	Pooled	Treated	Control	Pooled
	(1)	(2)	(3)	(4)	(5)	(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.*

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.