



Research Paper

The effect of brief cooperative contact with ethnic minorities on discrimination

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ABSTRACT

We randomly assigned students to an ethnically homogeneous or an ethnically mixed team for a cooperative task. One month later we used behavioral games to measure students' tendency to discriminate against ethnic minorities. We find that, overall, students did not discriminate and that participation in an ethnically mixed team did not have an effect on their behavior. We also find that prior exposure to ethnic minorities served as a moderator: students who had no classmates from a minority group in their regular classes did display discrimination and participation in an ethnically mixed team removed it. For students exposed to minority classmates, there was no effect, but they had displayed no discrimination in the first place. The results show that a relatively simple intervention that induces interethnic cooperative interaction may reduce discrimination among individuals who lack such interaction in their daily lives.

1. Introduction

“A fundamental threat to the quality of the rule of law is that too often, still, a person's skin color or name determines his or her opportunities in life. That is unacceptable.” In his 2020 Throne Speech (a kind of State of the Union), King Willem-Alexander van Oranje Nassau of the Netherlands expressed deep concern about continued discrimination and unequal treatment of ethnic minorities in the country.¹ His concern does appear to be justified. Ethnic minorities in many countries face discrimination in numerous domains, including labor markets (Bertrand and Mullainathan, 2004; Dur et al., 2022; Heath et al., 2013; Hedegaard and Tyran, 2018; Zschirnt and Ruedin, 2016), housing markets (Bartoš et al., 2016), consumer markets (Ayres et al., 2015; List, 2004), and the judicial system (Anwar et al., 2012; Shayo and Zussman, 2011). Given that unequal treatment of minority groups in societies is both unjust and inefficient, a key question is how it can be overcome.

We report on a field experiment that tested the effect of an intervention aimed at reducing ethnic discrimination, inspired by the idea that cooperative intergroup contact is crucial in reducing discrimination (Allport, 1954). The main subjects were 16- or 17-year-old high school students from the Dutch majority group who participated in an educational program at Tilburg University.

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¹ Ethnic minorities constitute about 14 percent of the population in the Netherlands, of which the majority are first- or second-generation immigrants of Turkish or Moroccan descent. The unemployment rate among ethnic minorities is more than twice that among native Dutch, and the poverty rate is more than five times as high (Eurostat, 2021). The situation is similar in other Western European countries.

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They visited the campus at least twice. The intervention was integrated in a single 45-minute lecture that took place during the first visit. During the lecture, the high school students worked on a group assignment in randomly formed teams consisting of three or four members that included one university student who was visibly a member of either the majority group or a minority group.² This assignment generated exogenous variation in interethnic contact between a *Control* group and a *Treated* group of high school students. Students and lecturers were unaware of being part of a research study.

During the second campus visit, which took place a month after the intervention, we measured ethnic discrimination among the participants. To do so, we had the high school students make choices in two behavioral games with monetary incentives (an ultimatum game and a sender-receiver game) as part of a lecture on economic games.³ In the ultimatum game, the students played the role of responder and in the sender-receiver game, they played the role of sender.⁴ In both games, the students were randomly matched to an anonymous partner with either a minority or majority background, who was a student in a school that did not participate in the educational program and was thus unknown to the decision making students. The partner's first name was communicated, which signaled ethnic background in an unobtrusive way.⁵ Ethnic discrimination in the ultimatum game implies that offers from minority proposers are less likely to be accepted than offers from majority proposers. Discrimination in the sender-receiver game refers to senders being less willing to lie for a minority receiver than for a majority receiver (see also Danilov and Saccardo, 2019). Since we made sure that students matched to a partner with a minority background in the ultimatum game were matched to a majority partner in the sender-receiver game, and *vice versa*, we also have a within-subjects measure for discrimination as a dependent variable.

We find that, on average, students did not discriminate against (nor in favor of) partners with an ethnic minority background, and were not affected by the contact intervention. A heterogeneity analysis shows that cooperative interethnic contact with peers established prior to the intervention was potentially an important moderator. Specifically, students in the *Control* group who had no minority classmates in their regular classes displayed significant discrimination against minority partners whereas students in the *Treated* group did not differentiate according to ethnic background. The contact intervention thus eliminated discrimination among these students. In contrast, students who had minority peers in their regular classes, and were thus almost certainly exposed to cooperative interethnic contact prior to the intervention, did not discriminate, and were not affected by the intervention.⁶

The current paper contributes to the literature on the causal effects of intergroup contact on ethnic prejudice or attitudes toward ethnic minorities. One branch of this literature uses laboratory experiments that typically involve a short intervention and measurement of outcomes immediately afterward. In general, studies of this kind provide strong support that contact reduces prejudice (Pettigrew and Tropp, 2006), even if the contact is only imaginary (Crisp and Turner, 2009; Miles and Crisp, 2014). However, the light touch nature and artificiality of most laboratory contact interventions raise concern that the findings do not apply outside the lab (Paluck and Green, 2009; Paluck et al., 2021). In the current study the contact intervention and the outcome measurement are tightly controlled, as in the laboratory, but are at the same time unobtrusively integrated into a natural setting.

A second branch of the literature uses more natural settings. These studies typically observe college students who are randomly assigned roommates with a majority or ethnic minority background. A common finding is that the students exposed to minority peers are less likely to maintain stereotypes about minority groups, have more interethnic friendships and are more likely to marry someone with a minority background later in life (Boisjoly et al., 2006; Carrell et al., 2019; Corno et al., 2019; Marmaros and Sacerdote, 2006). Field experiments that involve other types of interethnic contact, for example, in the context of a military training camp (Finseraas et al., 2019; Finseraas and Kotsadam, 2017), in the classroom (Scacco and Warren, 2018; Elwert et al., 2023), or in sports competitions (Lowe, 2021; Mousa, 2020) show weaker but overall still positive effects (except Elwert et al., 2023), with effects depending on the specific outcome variable or the context. Moreover, the effects tend to be smaller when the outcomes are measured with a delay (see the meta-studies of Lemmer and Wagner, 2015; Paluck et al., 2019).

Whereas studies based on natural experiments or interventions in the field have, by and large, focused on the effects of relatively intense, long-run contact experiences, the current study investigates whether a brief but meaningful interaction can also generate positive effects. This could be particularly relevant for policy makers who do not always have the option of facilitating extended contact. Since almost everyone attends high school at some point, high school students are potentially an ideal target population for anti-discrimination policies. Remarkably, only 10 percent of the studies involving prejudice reduction intervention are based on high school students (Paluck et al., 2021). Our study is one of the first steps in this direction and shows that a contact intervention targeted at high school students who have not been exposed to cooperative interethnic contact has the potential to be effective and deserves further investigation.

² The teams were randomly formed with the restriction that high school students from the same school could not be on the same team. The majority and minority university students did not differ in terms of sex or level of education (Bachelor vs. Master).

³ The second lecture was organized differently than the first, with different lecturers and slides, so that the students would not discern a connection between the two.

⁴ In the ultimatum game, the students were asked how much they would be willing to accept from an anonymous partner-proposer who was meant to distribute an amount of money (40 euros) between the two of them. In the sender-receiver game, the students were informed about the outcome of a die roll and were asked to report the number to an anonymous partner who was asked to choose a number between 1 and 6 upon receiving the message. If the partner chose the same number as the outcome of the roll, then they would each receive 20 euros. If the numbers differed, then the student would receive 18 euros and the partner would receive 30 euros.

⁵ It is common in experiments on ethnic discrimination to use the name to signal ethnic background (e.g., Bertrand and Mullainathan, 2004; Bouckaert and Dhaene, 2004; Cettolin and Suetens, 2018; Fershtman and Gneezy, 2001).

⁶ Notice that the heterogeneous effects are robust to different clustering methods, are not driven by outliers, and are not due to other differences between *Control* and *Treated*.

The paper proceeds as follows. In Section 2, we provide details of the experimental design and procedures. In Sections 3 and 4, we discuss the descriptive statistics and the empirical strategy. Section 5 presents the results and Section 6 concludes.

2. Experimental design and procedures

The subjects in the experiment were 16- or 17-year-old high school students, who took part in a biennial educational program called Econasium. The program is organized by the School of Economics and Management at Tilburg University in cooperation with a range of secondary schools in the Netherlands that offer preparatory education for university. About 71 percent of the schools offer this type of education.⁷ Students attending the program typically have good grades in mathematics and they are interested in studying economics at the university when they finish high school; they follow the Economics & Society track.⁸ The program consists of a lecture on statistics at their school that is not part of the standard curriculum and a series of lectures at the university. Other elements of the program include excursions to, for example, the Dutch Central Bank or a well-known company, support while preparing a high school thesis and participation in an entrepreneurial game. Any school that is ready to engage in these activities can sign up to participate by contacting the director of the program at Tilburg University. The interested students in such schools can then voluntarily decide to participate in the program.

Most of the participating schools are located in the south of the Netherlands, as shown in Fig. B.1 in the appendix. Population size, household size, age, and income are similar in the districts in which the participating schools are located as in the typical district in which a school offering VWO is located. The share of citizens with a non-Western immigration background is lower than in the typical district (9.5% versus 14.2%) reflecting the fact that in the south of the Netherlands the share of ethnic minorities in the population is lower than in the major cities, located further north.⁹ Yet, the share of students with an ethnic minority background following the Economics & Society track is very similar to that in the other tracks (Culture & Society, Nature & Health, Nature & Physics). To illustrate, 5.7 percent of the high school students in the Economics & Society track have at least one parent born in either Morocco, Turkey, Surinam, Indonesia or the Caribbean (covering the largest ethnic minority groups in the Netherlands) whereas this percentage equals 5.9 among students choosing any of the other tracks.

The students were all, except one, members of the Dutch majority group, as was indicated by their first and second name.¹⁰ They visited the campus at least twice for half a day each time, and on both occasions they attended lectures in two large lecture halls. The experiment consisted of three phases: an intervention phase and a decision-making phase, plus a debriefing phase, which were all integrated within the Econasium program. Neither the high school students nor the other participants knew that they were part of an experiment. The intervention phase took place on February 14, 2019, and was designed to bring the high school students into contact with peers who are members of an ethnic minority. In the decision-making phase, which took place on March 12, 2019, the students participated in two economic games designed to elicit their attitude toward ethnic minorities. Two debriefing sessions took place in May–June, 2019. We received approval from the TiSEM IRB (protocol no. 201901) to conduct the research without informed consent.¹¹ We pre-registered the design after the intervention phase, on February 28, 2019, in the AEA RCT Registry under study number ID AEARCTR-0003955.¹² In what follows, we explain the procedures followed in each of the three phases in detail.

2.1. Intervention phase

During the first campus visit, the high school students attended a lecture on *Strategic Thinking* (see Section A.1 in the appendix for the slides). Before the lecture started, we recruited university students registered at Tilburg University, who would form teams with the high school students. We recruited mostly bachelor university students with the purpose of minimizing differences in status between the high school and university students. The share of bachelor students was 85 percent.

The university students were assigned a code (e.g. A2) and were dispersed in the two lecture halls where the lectures were being given. They were instructed to make the code visible when the high school students entered the room. The high school students, who also had been assigned a code upon arrival at the university, were instructed to find a seat in the lecture hall next to the university student with a matching code. The teams were randomly constructed, conditional on the members not being from the same school. Each team consisted of two or three high school students and a university student and they were meant to work together on a task.

⁷ The statistics in this section are from Ministry of Education Culture and Science (2024) and Statistics Netherlands (2023). In Dutch, the type of education is called VWO which stands for “Voorbereidend wetenschappelijk onderwijs”.

⁸ After the third of the six years of secondary education preparing for university entrance, students have to choose one of four tracks: Economics & Society, Culture & Society, Nature & Health, or Nature & Physics. Some courses are mandatory for all tracks (Dutch, English, Mathematics, Sociology, Arts, Gymnastics, and a second foreign language). In addition, each track has a selected set of mandatory courses (e.g., Economics and History for the Economics & Society track, and Biology and Chemistry for the Nature & Health track), and a restricted set of electives (e.g., Geography, Business Economics, and Social Science for the Economics & Society track).

⁹ Districts refer to four-digit postal codes (see Table S.1 in the appendix).

¹⁰ We received information about the names of the participants before executing the experiment. To ensure the reliability of the results, we excluded the team that included the minority high school student in the data analysis.

¹¹ The reason why we did not want to have subjects (or their parents) sign a consent form is that we feared that doing so would destroy internal validity: knowing that one is part of a study on ethnic discrimination may lead to social desirability bias and may lead subjects to behave differently than they would if they would not be aware (Agan et al., 2023; Murad et al., 2023).

¹² The experimental design and primary outcome variables were pre-registered but not the empirical strategy or the post-decision-making questionnaire (which we added right before the decision-making phase took off).

Because some of the university students had a majority background while others had an ethnic minority background, some teams were homogeneously Dutch (the *Control* group) while others were ethnically mixed (the *Treated* group).

The task consisted of choosing a successful strategy in a repeated prisoner's dilemma game among six available strategies, namely *always defect*, *always cooperate*, *coin toss*, *tit-for-tat* (start with cooperate and then imitate partner), *defect tit-for-tat* (start with defect and then imitate partner), or *grim trigger* (start with cooperate and then cooperate if the partner cooperates, otherwise defect forever after). After listening to an introductory lecture on the topic, the teams were given 15 minutes to discuss and prepare their answer. The task of the university students was to guide the discussion and submit a strategy by means of an online quiz platform. The lecture took place in two different rooms, and was accompanied by slides in both. Two native Dutch teaching assistants, who were also unaware of the experiment, served as instructors. Their task was to show the slides and answer questions if needed, as well as to present the distribution of answers at the end of the lecture. The lecture ended with the explanation of a smart repeated-game strategy.

The university students were recruited via email invitations signed by the director of the Econasium program.¹³ They were offered 40 euros to help with an introductory lecture for high school students that would take about an hour. To ensure fluency in the Dutch language—since all the lectures were conducted in Dutch—those chosen were born in the Netherlands. Furthermore, to guarantee that they had basic knowledge in strategic thinking, we only invited students registered in a business or economics program. The email invitations were sent to university students with a minority background (with a Moroccan, Turkish, or Arab first and second name), and to a random subset of the other university students (mainly Dutch). After double-checking during the session whether the minority or majority background was visibly recognizable, we coded all university students with a Moroccan, Turkish, or Arab sounding name, except for one, as minority. The exception was coded as majority since his minority background was not visible. In addition, we coded students with an Asian background and one dark-skinned student as minority. In total, 51 university students were recruited, of which 18 were coded as ethnic minority. One week before the lecture, we sent out detailed instructions about the task they were going to be involved in (see Section A.2 in the appendix). After the lecture, the university students were invited to fill out a short survey of the level of engagement among the high school students and the discussion held in the team framework (see Section A.3 in the appendix).

2.2. Decision-making phase

During the second campus visit, the high school students attended a lecture called *Participation in Economic Games* (see the accompanying slides in Section A.4 in the appendix). The high school students were first assigned to a seat.¹⁴ There was a different arrangement and different instructors than in the first lecture so that the subjects would not suspect a connection between the two lectures. Two university professors were present in the classrooms and read the instructions for the decision-making tasks.¹⁵ We used two decision-making settings that had been shown to be conducive to ethnic discrimination in a context similar to our context (see Danilov and Saccardo, 2019).¹⁶

Ultimatum game The main task for the high school students was to make a choice as a responder in an ultimatum game, which was referred to as a *Distribution Game* in the instructions (see Section A.5 in the appendix). The high school students were explained that a student from a school that is not part of the Econasium program (the partner-proposer) had been given 40 euros and asked to propose a division of the amount between himself and another person. They were each communicated the first name of the matched proposer in private, and were asked to write down the minimum amount they would be willing to accept on an answer sheet. They were told that if their answer would be lower than the proposed amount, the proposed allocation would be implemented. Otherwise, neither the responder nor the proposer would receive anything.

The proposer's ethnic background was randomly varied across the high school students and it was signaled to the student through the name on the answer sheet. The proposer was one of eight male university students recruited prior to the task. Four were members of the Dutch majority (Daan, Koen, Max and Tom) and the other four were of Turkish or Moroccan descent (Abderrahman, Mohamed, Yassin and Younes).¹⁷ They were recruited via email invitations sent by the Economics Department at Tilburg University. The email solicited participants for an online survey on economic decision-making with the possibility of earning up to 40 euros, depending on the choices made. All eight were paid for their choices. The high school students were unaware of the total number of proposer participants, did not know they were all male, and did not know they were selected according to their ethnic background.

We say there is discrimination against (in favor of) ethnic minorities if the minimum amount required from a minority proposer is, on average, higher (lower) than that required from a majority proposer; this would make it more likely that responders reject (accept) a given amount if proposed by a minority proposer versus a majority proposer.

¹³ The director was aware that an experiment was taking place but did not know what the research question was.

¹⁴ Seating was arranged in alphabetical order (using name tags) both between and within the two lecture halls. Space between seats was maximized.

¹⁵ The professors were informed that the lecture was part of an ongoing experiment but they were not aware of its purpose or the research question. One of the professors was the director of the Econasium program.

¹⁶ Decision-makers in Danilov and Saccardo (2019) were German students who were faced with a native German or Turkish partner.

¹⁷ In the decision-making phase none of the high school students had a game partner with the same name as the team member they encountered in the intervention phase.

Communication game After collecting the answer sheets with the minimum acceptable offer, the instructions and answer sheets for the second decision task, called *Communication Game*, were handed out and explained (see Section A.6 in the appendix). The second task made it possible to measure discrimination within subjects, because students who had a majority partner in the ultimatum game were now assigned to a minority partner, and *vice versa*. The partner was selected from among the same eight university students mentioned above (and the high school students were of course unaware of this). The high school students were again communicated the first name of their partner and recorded their answers on the answer sheet which was collected right after the decision was made.

The second task consisted of making a choice in the role of sender in a sender-receiver game (Danilov and Saccardo, 2019). Specifically, the high school students were informed about the outcome of a die roll and were asked to report the outcome to an anonymous partner. They were told that their partner had to choose a number between 1 and 6 upon receiving their message and that he received no further information. If the number that the partner chose was the same as the outcome of the roll, then the high school student and the partner would each receive 20 euros. If the number that the partner chose differed from the outcome of the roll, the high school student would receive 18 euros and the partner would receive 30 euros. We identify discrimination if the high school students are more willing to lie for a majority partner than for a minority partner. The rationale is that lying implies a willingness to sacrifice a small amount of money to generate a large benefit for the partner.

Post-experimental survey At the end of the decision-making phase, the students participated in an online survey. A QR code and a link to the online survey (run on Qualtrics) were given to them after they had played both games and handed in their answer sheets. The survey consisted of questions concerning risk attitude, general trust, and opinions about ethnic diversity and the presence of immigrants in the Netherlands. They were also asked to report the number of classmates and friends with an immigration background (either zero, between 1 and 5, between 5 and 10, or more than 10).¹⁸ Details are in Section A.7 in the appendix.

Payment The high school students were informed that eight of them would be randomly selected to implement the choices made in the games for real (four for each game). They were told they would learn whether they were among the selected students at a later stage. The selected students were instructed to collect their money in cash during a third campus visit in May or June 2019, or, if they could not attend, it would be collected for them by their teacher on the third visit.

2.3. Debriefing session

During a third campus visit, which took place on May 9 or June 13, 2019, (most of) the high school students who participated in the experiment attended a lecture entitled “Unknown is unloved?” (see Section A.8 in the appendix for the slides). The lecture introduced them to the ultimatum game and how it can be used to measure unequal treatment and discrimination. They were also shown the results of the experiment in which they themselves had participated. The session served two purposes. The first was to give them a first-hand look at research done at the university and the process that researchers go through, from data collection to communicating the results. The second was to explain the experiment to the high school students, which up to that point had not been aware of. The session provided time for discussion, which the high school students indeed took advantage of by asking questions. They were also given the contact information of a person outside the research study who they or their parents could contact in case they felt uncomfortable with the study’s outcome or wished to share concerns (an opportunity which none of the students used).

3. Descriptive statistics

3.1. Number of observations

In each of the two main phases (intervention and decision-making) 136 high school students participated; 124 of them participated in both lectures.¹⁹ We omitted the data for one team of 4 students and one team of 3 students, leaving us with observations for 117 high school students organized into 49 teams, of which 114 also answered the post-experimental questionnaire.²⁰ Eighty of them were assigned to the *Control* group, and thus had a majority university student on their team. Out of these 80, 45 of were matched to a *Majority* proposer and 35 to a *Minority* proposer in the ultimatum game (and *vice versa* in the communication game). The remaining 37 students were assigned to the *Treated* group, and thus had a minority university student on their team. Out of these 37, 16 were matched to a *Majority* proposer and 21 to a *Minority* proposer in the ultimatum game (and *vice versa* in the communication game).

¹⁸ Not all the students answered the survey. In particular, we miss survey data from 3 students who were assigned to the *Control* group.

¹⁹ 12 students only participated in the first lecture, and another 12 participated only in the second lecture. Furthermore, the number of participants was substantially lower than the pre-registered number (174) due to one school with many students deciding last-minute not to participate and a number of students not showing up for the specific lecture(s).

²⁰ During the first session, three high school students from the same school joined a different team than they were assigned to. Since previous connections among students might have altered the group dynamics, we omitted this team (of 4 high school students in total) from the dataset. In a different team, one of the high school students had an ethnic minority background. Even though our treatment manipulation was by means of the university students, the minority background of a high school student could also influence the behavior of the other students in the team. Thus, we omitted that team (of 3 high school students) as well. The main results remain unchanged if both teams are included, when either of them is included, and when the two high school students on the team of the minority high school student are recoded as members of the *Treated* group.

Table 1
Comparison of *Control* and *Treated*.

	<i>Control</i>	<i>Treated</i>	Normalized difference	<i>p</i> -value
<i>A. Team-related variables</i>				
Male university students (yes = 1)	0.47	0.41	0.11	0.769
Bachelor's degree university students (yes = 1)	0.84	0.88	0.11	0.720
Team size	2.72	2.47	0.30	0.327
Team strategy:				
Tit-for-tat	0.22	0.12	0.27	
Grim trigger	0.34	0.35	0.02	
Defect + tit-for-tat	0.16	0.24	0.20	0.815
Always defect	0.28	0.29	0.03	
Observations	32	17		
<i>B. Survey responses university students</i>				
Response rate	0.69	0.53	0.32	0.284
Time for task completion				
Less than 5 minutes	0.41	0.44		
5-10 minutes	0.50	0.44	0.02	1.000
More than 10 minutes	0.09	0.11		
Active discussion (O)	4.27	4.56	0.38	0.315
Interesting discussion (O)	3.86	4.00	0.21	0.786
Discussion leader (O)	3.91	4.33	0.52	0.244
University student enjoyment (O)	4.73	4.67	0.13	0.740
Observations	22	9		
<i>C. High school student characteristics</i>				
Male (yes = 1)	0.57	0.49	0.18	0.427
Population density school district	1716	1735	0.03	0.873
Share non-Western immigrants school district	0.12	0.12	0.01	0.977
Observations	80	37		
<i>D. Survey responses high school students</i>				
Fun lecture (O)	4.19	4.19	0.01	0.801
Risk taking (O)	3.14	3.27	0.14	0.649
Level of trust (O)	3.14	2.97	0.18	0.456
Pro-multiculturalism (O)	4.35	4.22	0.19	0.297
Accepting of foreigners (O)	3.57	3.73	0.17	0.270
Minority friends				
None	0.27	0.38	0.23	
Between 1 and 5	0.60	0.43	0.33	
Between 6 and 10	0.10	0.14	0.10	0.655
More than 10	0.03	0.05	0.14	
Minority classmates				
None	0.19	0.27	0.18	
Between 1 and 5	0.73	0.70	0.05	
Between 6 and 10	0.08	0.03	0.23	0.225
More than 10	0.00	0.00	0	
Observations	77	37		

Notes: The reported *p*-values are based on tests of differences between *Control* and *Treated*: Fisher's exact tests for categorical variables, independent-samples *t*-tests for continuous variables and Mann-Whitney tests for ordinal variables. Notice that for ordinal variables measured at a 5-points Likert scale (O) means of the scale are reported.

3.2. Confounding effects

We wish to determine whether in the case that the contact intervention indeed had an effect, we can be confident that it is due to the induced variation in interethnic contact between *Treated* and *Control* rather than to other differences between the teams with minority and the teams with majority university students. Overall, we find no differences in any of the variables measured. The gender distribution was similar across the two groups of university students, with males representing 41% of the minority students and 47% of the majority students, as was the share of bachelor students (respectively 88% and 84%). The team size did not substantially differ between *Treated* and *Control*, nor did the teams chose very different strategies in the repeated prisoner's dilemma task. In both cases, the most frequently chosen strategy was the *grim trigger* strategy, followed by the *always defect* strategy. An overview of the team-related variables, the normalized difference between the *Treated* and *Control* groups, as well as the *p*-values for the tests of differences are shown in panel A of Table 1.

Moreover, evidence from a survey among the university students shows that the majority and minority students did not rate their experiences with carrying out the team task differently with respect to participation and engagement in the discussion and enjoyment of the session (see panel B in Table 1). Overall, these findings suggest that minority and majority university students do not differ in respects other than ethnic background.

3.3. Balancing tests

We show that the treatments are also balanced in terms of the high school students' background characteristics and post-experimental survey responses. Panels C and D of Table 1 show the means of the variables we measured and a number of relevant administrative variables, along with the normalized difference and p -values of the associated tests of differences between *Control* and *Treated*. As can be seen, the two conditions are well-balanced in terms of all measured characteristics (i.e., gender, risk-taking, level of trust, preferences with regard to multiculturalism and foreigners, number of minority friends and classmates, etc.). Moreover, the population density and the share of the population with a non-Western immigration background are of a similar order of magnitude across the two conditions. A joint test of orthogonality based on an ordered logit regression points out that the treatment allocation is balanced in terms of the variables shown in panels C and D ($p = 0.765$).

4. Empirical strategy

4.1. Main analyses

To answer the question of whether the contact intervention reduced the level of ethnic discrimination, we first perform a difference-in-differences analysis of responder behavior in the ultimatum game. In particular, we regress the minimum acceptable offer (MAO) made by the high school students on a binary variable for being matched to a *Minority* partner-proposer, a binary variable for being part of the *Treated* group, and an interaction term. The estimated effect of *Minority* should reflect the extent of discrimination against minorities in *Control*. A positive effect would imply that a higher MAO is required from minority proposers than from majority proposers, indicating the presence of discrimination. Also of key interest is the estimated effect of the interaction term, which if negative would indicate that the contact intervention has reduced discrimination.

The analysis of behavior in the communication game is similar to that in the ultimatum game, with the only difference being that the dependent variable is now the tendency to lie. Specifically, a binary variable indicating whether or not the student was honest about the outcome of the die roll (equal to 1 if he was and 0 otherwise). Honesty corresponds to an unwillingness to sacrifice a small amount of money to generate a large benefit for the partner. We again perform a difference-in-differences analysis and focus on the estimated effect of the interaction between *Treated* and *Minority*, the latter now referring to the ethnic background of the partner in the communication game.

In another analysis, we combine the data from the ultimatum game and the communication game to construct an index of discrimination measured at the individual level.²¹ The index measures the kindness shown by a high school student towards a minority partner in the ultimatum game (sender-receiver) game minus his/her kindness towards a majority partner in the sender-receiver (ultimatum) game. Kindness towards the proposer in the ultimatum game is measured by the standardized value of the minimum acceptable offer, with lower values implying a lower probability to reject and thus greater generosity towards the partner. Kindness towards the receiver in the sender-receiver game is measured by a binary variable indicating whether the student was dishonest about the outcome of the die roll, given that by lying the payoff of the partner could be increased at a small cost for oneself. A negative value of the discrimination index is an indication of ethnic discrimination and indicates that the high school student was less kind towards a minority partner than towards a majority partner (the minimum value is -1). A positive value refers to positive discrimination (the maximum value is 1). A value of zero means equal treatment of majority and minority partners. To study whether the contact intervention reduced ethnic discrimination, in our main analysis we regress the discrimination index on a binary variable indicating whether the high school student was in *Treated*.

4.2. Prior interethnic contact

Since we are investigating the effect of interethnic contact, it is natural to look at heterogeneous effects driven by previous cooperative contact with ethnic minorities. Thus, in a second step of our analysis of the behavioral games, we use information gathered in the survey on the number of classmates with a minority background and distinguish between two groups of high school students depending on the presence of classmates with a minority background in their regular classes. Overall, 22% of the students had no classmates with a minority background at the time of the experiment, and respectively 72% and 6% had between 1 and 5 and between 6 and 10. No student reported more than 10. This distribution of responses led us to merge the three latter categories, giving us an indicator variable equal to zero for students without classmates with a minority background and equal to one for students with one or more minority classmates.²² It can be reasonably assumed that the students who had been exposed to minority classmates in their regular classes, had already experienced cooperative interethnic contact prior to our intervention.

²¹ Recall that the high school students were matched with partners with a different ethnic background across the two games.

²² As can be seen in panel D of Table 1, the distribution is not significantly different between *Control* and *Treated*.

Table 2
Effect of the intervention in the ultimatum game.

Dep. var.: Minimum accepted offer	(1)	(2)	(3)	(4)
<i>Minority</i>	0.35 (1.39)	-0.25 (1.56)	8.20 (2.70)***	7.60 (2.64)***
<i>Treated</i>	0.93 (1.75)	1.09 (1.79)	1.05 (2.79)	1.18 (2.78)
<i>Minority</i> × <i>Treated</i>	-2.40 (2.19)	-2.84 (2.31)	-12.95 (3.66)***	-12.74 (3.40)***
Prior contact			1.80 (2.19)	2.67 (2.19)
<i>Minority</i> × Prior contact			-9.43 (3.82)**	-9.52 (3.61)**
<i>Treated</i> × Prior contact			-0.22 (3.73)	-0.23 (3.57)
<i>Minority</i> × <i>Treated</i> × Prior contact			13.28 (5.06)**	12.85 (5.05)**
Constant	8.51 (0.92)***	15.39 (4.95)***	7.20 (1.68)***	13.44 (5.58)**
Controls		✓		✓
Number of observations	117	114	114	114
Number of clusters	49	49	49	49

Notes: *Minority* is a binary variable equal to one if the high school student is matched with a minority partner. *Treated* is a binary variable equal to one if the student is in the *Treated* group. *Prior contact* is a binary variable equal to one if the student has at least one classmate from an ethnic minority. Controls include the sex of the high school student, trust and risk attitudes, the team's university student's sex and degree, the team size, and the population density in the school district. Standard errors (in parentheses) clustered at the team level. *** - significant at 1 percent, ** - significant at 5 percent, * - significant at 10 percent.

The division into the two groups is backed up by the observation that students with no regular contact with minorities in the classroom tend to live in municipalities with a lower share of residents with a non-Western immigration background ($p = 0.001$) and have fewer minority friends ($p = 0.006$) than students with regular contact. Interestingly, they are also more likely to report that there are too many foreigners in the Netherlands ($p = 0.052$), have somewhat lower levels of trust ($p = 0.072$), live in less urbanized areas (but $p = 0.126$) and tend to be more negative about a multicultural society (but $p = 0.175$). More statistics are shown in Table S.2 in the appendix.²³

Using the prior contact indicator, we estimate interactive regression models with the three dependent variables introduced in Subsection 4.1, to investigate whether students exposed to interethnic contact prior to the contact intervention differed in their tendency to discriminate and whether they reacted differently to the intervention. Notice that the background characteristics of the high school students and the team-related variables are balanced between *Control* and *Treated* within the group of high school students with prior contact and within the group of students without prior contact with ethnic minorities, as confirmed in joint tests of orthogonality ($p = 0.838$ if prior contact and $p = 0.977$ if no prior contact). More details are available in Table S.3 in the appendix.

5. Results

5.1. The ultimatum game

Table 2 presents the regression results for the minimum accepted offer by responders in the ultimatum game: general results appear in column 1 (without controls) and column 2 (with controls) while results from the fully interactive model that allows for heterogeneous effects depending on prior contact appear in column 3 (without controls) and column 4 (with controls). According to the results in columns 1 and 2, there is no discrimination present in the *Control* group (the effect of *Minority* is close to zero).²⁴ Moreover, the interaction *Minority* × *Treated* is negative but qualitatively small and statistically not significant, indicating that the contact intervention did not have a general effect on behavior.

If we focus on the heterogeneous effects depending on prior interethnic contact, then interesting patterns can be detected. To visualize the patterns, the results in column 3 of Table 2 are presented graphically in Fig. 1. The right-hand panel shows the coefficient and p -value for the difference-in-differences effect in the case of students who had prior contact with minority peers (lower panel) and those who did not (upper panel). As can be seen, the minimum accepted offer (MAO) for high school students without prior contact is particularly sensitive to the contact intervention. In particular, in the *Control* group, these students expect more than 8 additional euros from a minority proposer than from a majority proposer, providing evidence of a substantial degree of discrimination. The discrimination completely disappears, however, as a result of the contact intervention. Thus, students in *Treated* are willing to accept almost 13 euros less than students in *Control* when matched to a minority proposer, whereas the MAO is similar when matched to a majority proposer. High school students with prior contact tend not to discriminate: the MAO of students in *Control* with a minority

²³ We explored whether a variable based on the median split of the share of non-Western immigrants in the school district population could be used as an indicator of prior interethnic contact. Yet this variable did not generate a significant difference in the number of friends with a minority background or in the normative statements regarding foreigners or multiculturalism. Moreover, high school students do not necessarily live in the district where the school is located, making this variable a noisy proxy for prior interethnic contact. Reassuringly, though, qualitative results based on this variable are similar to those based on our individual-level proxy for prior interethnic contact (see Tables S.8 to S.10). Notice further that we performed other heterogeneity analyses but none of these led to robust heterogeneous effects (see same tables).

²⁴ This is in contrast to the results in Danilov and Saccardo (2019). Overall, the rejection rate is in the same ballpark as typically observed in experiments (Oosterbeek et al., 2004).

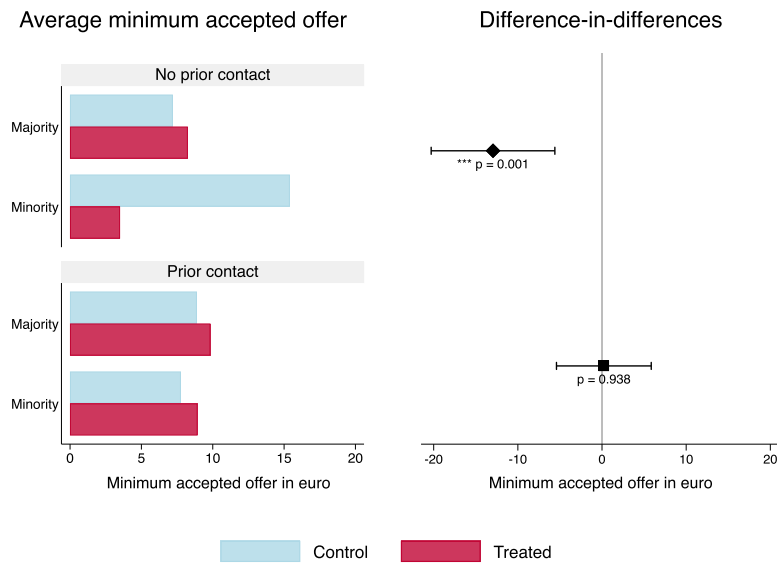


Fig. 1. Effect of intervention on minimum accepted offer by prior contact. Notes: The p -values are based on regressions reported in column 3 of Table 2. *** - significant at 1 percent, ** - significant at 5 percent, * - significant at 10 percent.

partner is of the same order of magnitude as the MAO in the case of a majority partner and is almost 9 euros lower than that made by high school students without prior contact. Moreover, given that the effect size of the triple interaction term corresponds very closely to that of *Minority* \times *Treated*, we conclude that the contact intervention has no effect on students with prior contact.

As shown in column 4 of Table 2, the results also hold when controlling for the background characteristics of the high school students (gender, measures of trust levels and risk attitudes, all based on the survey questions), team characteristics (gender and degree of the university student, and team size), and characteristics of the school’s district (its population density).²⁵

5.2. The communication game

Table 3 reports the treatment effects and the difference-in-differences effect in the communication game where the dependent variable is a binary honesty indicator. As shown in columns 1 and 2, the general effects are qualitatively small and not statistically significant, as in the ultimatum game. The heterogeneous effects are reported in columns 3 and 4, and show that, in contrast to the ultimatum game, discrimination is not present among the high school students without prior contact (the *Minority* variable is small in size and not statistically significant). Nonetheless, the difference-in-differences effect for this group of students is qualitatively similar to that in the ultimatum game, albeit not statistically significant. Among students with prior contact, treatment effects are qualitatively small and not statistically significant.²⁶ Fig. 2 provides a visualization of the results reported in column 3.

Although the difference-in-differences results in the communication game are qualitatively similar to those in the ultimatum game, the levels of discrimination are quite different. A possible explanation for the lack of negative discrimination among students without prior contact in *Control* and the positive discrimination in *Treated* may have to do with the within-subjects switch of partner.²⁷ The switch in ethnic background of the game partner may have made the high school students in the communication game, being the second game played, more aware that anti-social behavior towards someone with a minority background is inappropriate. This may have induced the students matched to a minority partner to act more pro-socially towards their partner than they would have otherwise (see also Barr et al., 2018; Agan et al., 2023; Murad et al., 2023).

5.3. Individual-level index of discrimination

Table 4 reports the regression results when the dependent variable is the discrimination index, with the general effect shown in columns 1 and 2 and the effect interacted by prior contact shown in columns 3 and 4. Fig. 3 visualizes the results reported in columns 1 and 3 of the table. Overall, basically no ethnic discrimination took place in *Control*, as shown by the estimated constant term in column 1, and the contact intervention had a small and positive but statistically not significant effect on the discrimination index.

If we look at the effects depending on prior interethnic contact in the classroom, then it can be seen that high school students who did not have minority classmates, negatively discriminated against ethnic minority partners, as shown by the significantly negative constant term in column 3, whereas prior contact offsets this discrimination, as shown by the associated significantly positive

²⁵ The results are also robust to estimating p -values of the regression estimates using randomization inference (see Table S.4 in the appendix).

²⁶ We get qualitatively similar results when estimating p -values of the regression estimates based on randomization inference (see Table S.5 in the appendix).

²⁷ Recall those students with a majority partner in the ultimatum game have a minority partner in the communication game and *vice versa*.

Table 3
Effect of the intervention in the communication game.

Dep. var.: Honesty	(1)	(2)	(3)	(4)
<i>Minority</i>	-0.14 (0.10)	-0.13 (0.10)	0.00 (0.22)	0.10 (0.22)
<i>Treated</i>	0.02 (0.12)	-0.02 (0.13)	-0.30 (0.31)	-0.26 (0.31)
<i>Minority × Treated</i>	-0.06 (0.22)	0.02 (0.22)	-0.50 (0.33)	-0.54 (0.35)
Prior contact			-0.07 (0.20)	0.03 (0.18)
<i>Minority × Prior contact</i>			-0.20 (0.22)	-0.29 (0.23)
<i>Treated × Prior contact</i>			0.43 (0.35)	0.33 (0.34)
<i>Minority × Treated × Prior contact</i>			0.59 (0.42)	0.72 (0.45)
Constant	0.74 (0.07)***	0.92 (0.36)**	0.80 (0.19)***	1.00 (0.35)***
Controls		✓		✓
Number of observations	117	114	114	114
Number of clusters	49	49	49	49

Notes: *Minority* is a binary variable equal to one if the high school student is matched with a minority partner. *Treated* is a binary variable equal to one if the student is in the *Treated* group. *Prior contact* is a binary variable equal to one if the student has at least one classmate from an ethnic minority. Controls include the sex of the high school student, trust and risk attitudes, the team’s university student’s sex and degree, the team size, and the population density in the school district. Standard errors (in parentheses) clustered at the team level. *** - significant at 1 percent, ** - significant at 5 percent, * - significant at 10 percent.

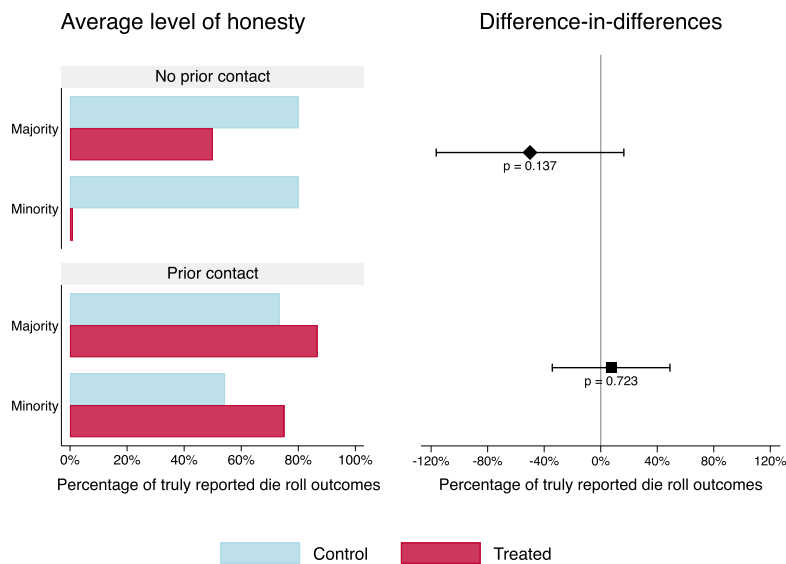


Fig. 2. Effect of intervention on honesty by prior contact. Notes: The *p*-values are based on the regressions results reported in column 3 of Table 3. *** - significant at 1 percent, ** - significant at 5 percent, * - significant at 10 percent.

coefficient. Among the students without prior contact, the contact intervention increased the discrimination index by 0.67 points, as is shown in the estimated coefficient of *Treated*, thereby eliminating all negative discrimination and even generating positive ethnic discrimination. The latter result come from the communication game, as mentioned in Subsection 5.2, and can be interpreted in light of the switch in ethnic background of the game partner in the second game, which could have made the high school students more prone to adopt a behavior in line with societal expectations (Barr et al., 2018; Agan et al., 2023; Murad et al., 2023). Among the students with prior contact, the intervention did not have a significant effect; the effect of *Treated* is offset by the interaction between *Treated* and prior contact. But these students did not negatively discriminate in the first place, of course.

The general and heterogeneity results are robust to including controls in the regression (columns 2 and 4), do not depend on the clustering method (see Table S.6 in the appendix), and hold if removing observations one at the time.²⁸ Whereas the contact intervention did not generate a general behavioral change, the heterogeneity results suggest that contact induced in the experiment and contact experienced in the classroom may have been substitutes in removing ethnic discrimination.

²⁸ The results are also robust to estimating *p*-values of the regression estimates based on randomization inference (see Table S.7 in the appendix).

Table 4
Regression results on the effect of the contact intervention.

Dep. var.: Discrimination index	(1)	(2)	(3)	(4)
<i>Treated</i>	0.17 (0.13)	0.16 (0.13)	0.67 (0.19)***	0.74 (0.22)***
Prior contact			0.42 (0.16)**	0.52 (0.19)***
<i>Treated</i> × Prior contact			-0.66 (0.27)**	-0.72 (0.30)**
Constant	0.03 (0.06)	0.15 (0.51)	-0.30 (0.14)**	-0.41 (0.48)
Controls		✓		✓
Number of observations	117	114	114	114
Number of clusters	49	49	49	49

Notes: *Treated* is a binary variable equal to one if the student is in the *Treated* group. *Prior contact* is a binary variable equal to one if the student has at least one classmate from an ethnic minority. Controls include the sex of the high school student, trust and risk attitudes, the team's university student's sex and degree, the team size, and the population density in the school district. Standard errors (in parentheses) clustered at the team level. *** - significant at 1 percent, ** - significant at 5 percent, * - significant at 10 percent.

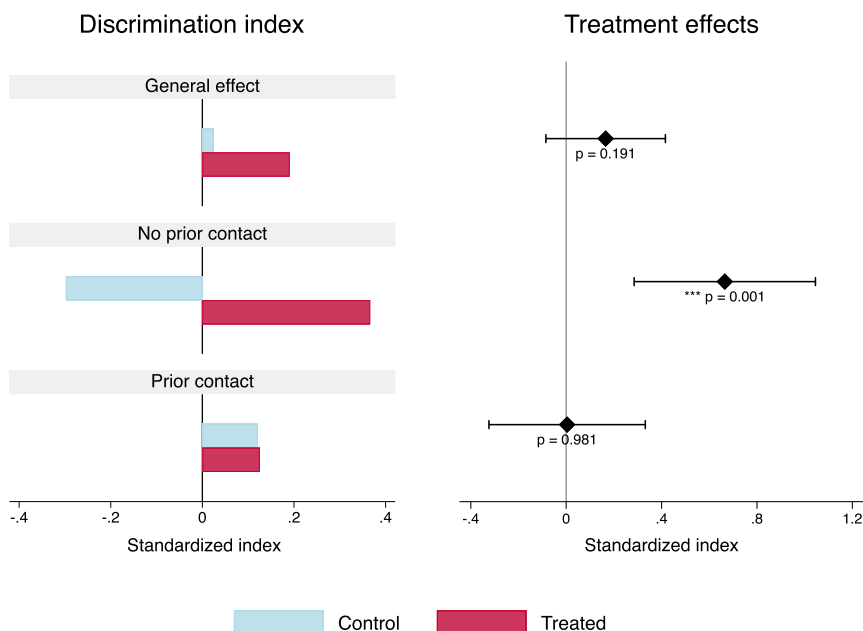


Fig. 3. Contact intervention effects. Notes: The *p*-values are based on the regressions results reported in columns 1 and 3 of Table 4. *** - significant at 1 percent, ** - significant at 5 percent, * - significant at 10 percent.

6. Conclusion

We show that an intervention consisting of cooperative interaction with someone from an ethnic minority group did not have a general effect on ethnic discrimination by high school students from the majority group, perhaps because, overall, the students did not discriminate in the first place. This result is driven by students who had been exposed to minority peers in their regular classes. Yet, a heterogeneity analysis reveals that students who had no minority peers in their regular classes at the time of the intervention did react to the intervention; whereas students who were part of a control condition negatively discriminated against an ethnic minority partner in behavioral games, there was no such discrimination among those who underwent the intervention. The results indicate that the students who had no minority classmates, thus arguably had fewer opportunities for cooperative contact with minorities, viewed minorities as an *out-group*. The contact intervention may have been the first time they worked together with someone from a minority group. In contrast, students who were used to interacting with minority classmates did not think or behave according to minority-majority categories.

What made our intervention work among students not exposed to ethnic minorities in their regular classes? We speculate about the channels but further research is needed to provide a final answer. A first possible mechanism is that the discussions and joint problem solving thinking that took place in the ethnically mixed teams may have induced the team members to think more from the out-group's perspective. Perspective-taking interventions have been shown to reduce social exclusion and tighten interethnic social ties (Alan et al., 2021; Kalla and Broockman, 2020, 2021). Alternatively, the high school students may have viewed the university students as role models, given they had more of a leading role in the team tasks and were further advanced in their educational

training. Exposure to a minority role model may have induced more respect or may have further increased the likelihood of taking the perspective of the minority student.

Although the scale of the experiment is too small to warrant direct policy conclusions, our study is a first step into the investigation of the efficacy of short, feasible interethnic contact interventions. Essentially, the findings demonstrate that, if targeted at high school students in schools with a low representation of ethnic minority groups, such interventions may be a promising avenue for future research and ultimately for policy makers combating discrimination. Such an intervention can be implemented relatively easily at a large scale without affecting the curriculum or the ethnic composition of the classes. All that is required is an extra-curricular activity that involves interethnic cooperation. The activity can be educational in nature, or related to sports or culture. In fact, such activities already exist in many schools, sometimes in the form of exchange programs between schools. Several secondary schools have active alumni societies whose members participate as mentors and moderators in school activities. For the intervention program it could be a possibility to oversample alumni with an ethnic minority background. Alternatively, given that some universities in the Netherlands recruit students to visit secondary schools to introduce their educational programs, university students with such background could be involved as well in moderating the extra-curricular activities. The program can be marketed as a program that stimulates the development of collaborative skills and fosters social cohesion by having students work together on challenging tasks (as in Alan et al., 2021).

A possible concern is that brief contact interventions are unlikely to have any long-lasting effects (beyond, say, one month). Although future research is needed to alleviate this concern, there are reasons for optimism. First, intergroup contact that satisfies Allport's contact theory conditions (personal interaction, shared goals, a common project, equal status, and approval by a recognized authority) has been shown to have long-lasting effects in other contexts (for example, Bagues and Roth, 2020; Bazzi et al., 2019). Second, as discussed by Paluck (2016), other short but meaningful interventions have been shown to have long-lasting effects on prejudice. For example, Broockman and Kalla (2016) show that a 10-minute conversation encouraging actively taking the perspective of transgenders can markedly reduce antitransgender prejudice for at least 3 months.²⁹ Although our contact intervention may seem trivial, for a majority student who has never had a meaningful conversation with a minority student, let alone worked together with one toward a common goal, a short positive personal experience may have a major impact, especially in comparison to common policy interventions, such as mass media campaigns (Gerber et al., 2011).

On a final note, we designed the intervention with the purpose of tailoring the background characteristics of the “to be studied subjects” (the high school students) and the “intervention subjects” (the university students) as much as possible, apart from the ethnic group to which they belong. They were all interested in economics at a university level (they planned to study or studied it), they all spoke Dutch, they all lived in the south of the Netherlands etc. Doing so allowed us to focus on the effect of interethnic contact rather than of, e.g., interstatus contact or interlanguage contact etc. (see Allport, 1954, who emphasized the importance of equal status for interethnic contact to reduce prejudice). Further research is needed to discover whether the results generalize to less selective groups of students. Also, the fact that the intervention subjects did not have a lower social status than the to be studied subjects in our experiment may have facilitated cooperative and common-goal-oriented interethnic contact and may thus have been a necessary condition for reduced discrimination among the group without prior classroom contact. To answer this question, further research is needed here as well.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

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²⁹ Broockman and Kalla (2016) also included a sort of (very) brief contact intervention because they varied the identity of the canvassers (transgender or not). Our contact intervention differed markedly from this intervention, though, since it was designed to involve cooperation towards a common goal, defined by Allport as one of the key conditions for intergroup contact to reduce discrimination.

Appendix A. Supplementary material

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jebo.2024.04.003>.

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