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## Rearranging the desk chairs: A large randomized field experiment on the effects of close contact on interethnic relations

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**Rearranging the Desk Chairs: A large randomized field experiment on the effects of close contact on interethnic relations**

FELIX ELWERT – TAMÁS KELLER – ANDREAS KOTSADAM

**CERS-IE WP – 2020/54**  
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## ABSTRACT

Contact theory and conflict theory offer sharply conflicting predictions about the effects of interethnic exposure on prejudice. Contact theory predicts that close collaborative contact under conditions of equal status causes a reduction in inter-ethnic prejudice. By contrast, conflict theory predicts that shallow or competitive exposure causes an increase in inter-ethnic antipathy. Both theories are backed by rigorous field-experimental evidence. However, much of this evidence tests each theory under arguably extreme conditions. Therefore, the boundaries of the scope conditions for contact and conflict theory remain unclear: where is the line between close versus shallow, or collaborative versus competitive, inter-ethnic exposures? We test the consequences of inter-ethnic exposure in a natural and non-extreme setting by executing a large, well-powered, and pre-registered randomized field experiment on inter-ethnic discrimination in 40 Hungarian schools. We show that neither manipulating the closeness of interethnic contact within classrooms, nor variation in inter-ethnic exposure across classrooms, has an effect on non-Roma students' inter-ethnic discrimination. These findings suggest that inter-ethnic contact may be neutral with respect to discrimination in many everyday settings, thus failing both to fulfill the hopes of contact theory and to actualize the concerns of conflict theory.

JEL codes: C93, I21, I24, J15, J18, Z13

Keywords: Contact theory, Conflict theory, Deskmates, Inter-ethnic prejudice, Randomized field experiment

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This study was reviewed and approved by the IRB offices at the Hungarian Academy of Science and at the University of Wisconsin-Madison.

# **Csak a székek átrendezése a Titanicon? Egy nagymintás randomizált terepkísérlet tanulságai a közeli kontaktusok inter-etnikus kapcsolatokra gyakorlat hatásáról**

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## ÖSSZEFOGLALÓ

A kontaktus és a konfliktus elméletek egymásnak ellentmondó előrejelzéseket tesznek az inter-etnikus kontaktusok előítéletességre gyakorolt hatásáról. A kontaktus elmélet szerint az egyenlő státusú felek közötti szoros és együttműködő kapcsolatok csökkentik az etnikai előítéleteket. Ezzel szemben a konfliktus elmélet úgy véli, hogy a felszínes és versengő kapcsolatok fokozzák az inter-etnikus ellenszenvet. Mindkét elméletet rigorózus terepkísérletek egész sora támasztja alá. Ezek a terepkísérletek azonban vitathatatlanul szélsőséges körülmények között tesztelik az egyes elméleteket. Ezért a kontaktus és a konfliktus elméletek hatókörének határai továbbra sem tisztázottak. Keveset tudunk arról, hogy hol van a határ a szoros és a felszínes valamint az együttműködő és a versengő inter-etnikus kapcsolatoknak való kitettség között. Kutatásunkban az inter-etnikus környezetnek való kitettséget természetes és nem szélsőséges körülmények között vizsgáljuk. Egy nagymintás, előregisztrált terepkísérletet végeztünk az etnikai előítéletesség témakörében 40 magyar általános iskolában. Megmutatjuk, hogy sem az inter-etnikus kapcsolatok közelségének osztályon belüli manipulálása, sem pedig az inter-etnikus kapcsolatoknak való kitettség osztályok közötti váltakozása nincsen hatással a nem-Roma tanulók etnikai előítéletességére. Mindez azt sugallja, hogy az inter-etnikus kapcsolatok a legtöbb hétköznapi szituációban minden bizonnyal nincsenek hatással az előítéletességre. Az inter-etnikus kapcsolatoknak való kitettség így nem teljesíti be sem a kontaktus elmélet által keltett reményeket, sem a konfliktus elmélet által felvázolt aggodalmakat.

JEL: C93, I21, I24, J15, J18, Z13

Kulcsszavak: Kontaktus elmélet, Konfliktus elmélet, Inter-etnikus előítéletek, Padtársak, Randomizált terepkísérlet

# Rearranging the Desk Chairs: A large randomized field experiment on the effects of close contact on interethnic relations\*

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## Abstract

Contact theory and conflict theory offer sharply conflicting predictions about the effects of interethnic exposure on prejudice. Contact theory predicts that close collaborative contact under conditions of equal status causes a reduction in inter-ethnic prejudice. By contrast, conflict theory predicts that shallow or competitive exposure causes an increase in inter-ethnic antipathy. Both theories are backed by rigorous field-experimental evidence. However, much of this evidence tests each theory under arguably extreme conditions. Therefore, the boundaries of the scope conditions for contact and conflict theory remain unclear: where is the line between close versus shallow, or collaborative versus competitive, inter-ethnic exposures? We test the consequences of inter-ethnic exposure in a natural and non-extreme setting by executing a large, well-powered, and pre-registered randomized field experiment on inter-ethnic discrimination in 40 Hungarian schools. We show that neither manipulating the closeness of interethnic contact within classrooms, nor variation in inter-ethnic exposure across classrooms, has an effect on non-Roma students' inter-ethnic discrimination. These findings suggest that inter-ethnic contact may be neutral with respect to discrimination in many everyday settings, thus failing both to fulfill the hopes of contact theory and to actualize the concerns of conflict theory.

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## 1 Introduction

Whether ethnic intermingling exacerbates or diminishes ethnic tensions is a pressing question of our time. Social science presents sharply different predictions for the effects of interethnic exposure. On one hand, contact theory (Allport 1954) predicts that deep and cooperative interethnic exposure promotes understanding, reduces prejudice, and increases interethnic trust. On the other hand, conflict and group threat theories (Blalock 1967; Bobo 1999; Williams 1964) predict that shallow or competitive exposure spurs exclusionary attitudes on the part of the ethnic majority toward their ethnic others. Constrict theory (Putnam 2007), a variant of conflict theory, additionally posits that interethnic exposure can weaken in-group solidarity among the majority group.

These contrasting theoretical predictions beg the question of scope conditions (Paluck et al. 2019). When does interethnic contact promote trust, and when does it promote prejudice? Building on Allport's (1954) classical formulation, over time, social scientists have endorsed an increasingly expansive interpretation of the scope conditions for contact theory, reporting positive effects not only for prolonged cooperative exposure but also for indirect contact via mass media and friends of friends (Pettigrew et al. 2011). Perhaps buoyed by this confidence, "the promotion of intergroup contact has arguably become the foremost strategy for reducing prejudice" (Paluck et al. 2019:130).

At least three reasons, however, caution against expansive policy hopes for contact theory. The first concern is selection bias. The great majority of supportive research is cross-sectional and observational, i.e. compares individuals who, at least in part, self-select into, or out of, intergroup contact. It is reasonable to expect that individuals who choose to expose

themselves to ethnic others are less prejudiced against them in the first place. Separating selection on ex-ante predispositions from the causal effects of the resulting interethnic contact on attitudes and behavior is difficult (Morgan and Winship 2005). The methodological consensus in sociology is that causal claims are most credible when they are backed by field experimental evidence that eliminates selection bias through randomization (Baldassari and Abascal 2017).

The second concern is impact. Compared to the estimates of observational studies, randomized field experiments have generally found smaller—and often much smaller—positive effects of intergroup contact (see Paluck et al. 2019 for a review). Furthermore, larger experiments report smaller effects, as do experiments that curtail specification searches by following pre-registered analysis plans (Paluck et al. 2019). These regularities suggest publication bias in favor of studies that support contact theory, even among randomized experiments.<sup>1</sup>

The third concern is generalizability: the randomized field experiments that most strongly support contact theory were conducted not only under favorable, but arguably under extreme conditions. Ten of the 24 randomized field experiments supporting contact theory in Paluck et al.'s (2019) comprehensive review study the effects of enforced coresidence among a highly selected population of relative strangers. To wit, research demonstrates conclusively that interethnic exposure promotes inclusionary attitudes and behaviors when members of different ethnic groups are forced to live together in elite college dorms (Boisjoly et al. 2006), the military (Finseraas and Kotsadam 2017), or elite military college dorms (Carell et al. 2015). Clearly, if the positive effects of interethnic contact on interethnic relations hinged on coresidence, the policy potential of contact theory would be limited (Paluck and Green 2009).

Together, these concerns raise the question of when, and to what extent, interventions

that promote intergroup contact diminish intergroup discrimination, especially when these interventions occur in mundane, and hence scalable, conditions.

In this study, we test the predictions of the contact, conflict, and constrict theories under quotidian and scalable conditions. Rather than intervening on coresidence among former strangers in elite colleges or the military, we randomize the seating chart in 186 public-school classrooms in Hungary. We conduct two experiments: A vignette experiment to measure discrimination, and a field experiment that randomizes the seating chart of 186 classrooms in 39 Hungarian schools for the duration of one semester. This allows us to investigate whether close interethnic contact, defined by sitting next to a deskmate belonging to the Roma ethnic minority affects outgroup discrimination and outgroup-friendships among members of the non-Roma Hungarian majority.

Our study differs from previous randomized evaluations of contact theory in several ways. First, our intervention has the potential of universal scalability, since, in contrast to college or military service, almost all members of a birth cohort must attend school. Second, our intervention has a light touch. Rather than intervening in individuals' living arrangements, which most people regard a prerogative of intimate personal choice, we intervene in classroom seating charts, which are routinely set by teachers. Third, we study a well-established natural setting. Rather than inducing coresidence between strangers in a new environment (college freshmen or military recruits), we reseal 3<sup>rd</sup> through 8<sup>th</sup> grade students who have grown up in the same small towns and have attended school together for at least 2 years prior to the intervention. Fourth, to our knowledge, this study is by far the largest randomized field experiment of interethnic contact, involving N=3,184 students. Fifth, unlike most field experiments on interethnic contact



(Paluck et al. 2019) our study was pre-registered and closely adheres to a pre-analysis plan.<sup>2</sup>

The results of our study are unambiguous. Our randomized vignette experiment documents substantial discrimination against Roma children among non-Roma children in our sample. The probability that a non-Roma student would lend money to a classmate is reduced by 27 percent if that classmate is described as Roma. But our field experimental findings fail to lend support to *any* of the main theories of inter-ethnic exposure. First, they plainly disappoint the hopes raised by contact theory, as being randomly assigned to a Roma deskmate for an entire semester does not affect the ethnic majority's discrimination against Roma students. Indeed, being randomly assigned to a Roma deskmate does not even lead to a higher probability of having a Roma friend. Second, in contrast to conflict theory, we do not find that exposure to a Roma deskmate leads to fewer friendships or more discrimination against Roma. Third, in contrast to constrict theory, we find no evidence that exposure to Roma deskmates reduces intra-ethnic lending among non-Roma Hungarians.

Our Null results are decidedly informative, i.e. they are not due to a lack of statistical power or inadvertent averaging of heterogeneous effects. All estimates are closely centered around the Null hypothesis of no effect, and there is little indication of effect heterogeneity based on students' own characteristics, their deskmates' characteristics, or classroom characteristics, such as grades and gender. In other words, our Null results do not present lack of evidence for an effect, but evidence for the lack of an effect.

Supplementary analyses also rule out that classroom level exposure to Roma students reduce discrimination. We show this using a quasi-experimental approach that exploits plausibly exogenous variation in the number of Roma students across classrooms (or across grades) within schools

(Hoxby 2000). While the number of inter-ethnic friendships increases with the share of Roma students in the classroom, the magnitude of discrimination is unaffected. Hence, these results suggest that neither desk- nor classroom-level contact with ethnic minorities ameliorates (or exacerbates) out-group discrimination among the ethnic majority. In other words, neither contact nor conflict theory apply.

The remainder of this paper is organized as follows: Section 2 reviews prior work on interethnic exposure with a particular focus on field experiments and scope conditions; Section 3 describes the experimental design; Section 4 describes the data; Section 5 the method; Section 6 presents the field experimental results; and 7 present quasi-experimental results. Section 8 discusses our findings and concludes.

## **2 Previous findings and scope conditions**

The literature on the effects of interethnic exposure divides into two main perspectives: contact theory and conflict theory. According to contact theory, close and cooperative contact, under scope conditions detailed below, can reduce prejudice and increase trust (Allport, 1954). Such exposures may diminish interethnic animosity by fostering empathy, increasing understanding, normalizing or habituating otherness, and promoting friendship. By contrast, conflict theory posits that animosity across groups is worsened with shallow or competitive exposure (Blalock 1967; Williams 1964). Such exposures may worsen outcomes by virtue of lacking the depth to promote understanding and empathy so that the categorical otherness of the interaction partners dominates to activate aversion and perception of threat. Putnam's (2007) constrict theory adds to the conflict perspective by arguing that inter-ethnic exposure may also undermine in-group trust.

The canonic treatment of interest in these theories is the co-location of members of

different ethnic groups in the same space. The wider (especially interventionist) literature on inter-group relations sometimes additionally evaluates bundled treatments that combine interventions on co-location with mandatory perspective-taking exercises on race or ethnic relations (e.g. Sorensen 2010; Markowicz 2009). We sidestep such bundled treatments and focus on the effects of co-location under various scope conditions.

Contact theory is supported by hundreds of observational, and mostly cross-sectional, studies (see e.g., Pettigrew and Tropp 2006, Pettigrew et al. 2011 for reviews), and also by a smaller number of randomized field experiments. We found 8 randomized field experiments involving interracial or interethnic exposures (Boisjoly et al., 2006; Burns et al., 2016; Camargo et al 2010; Carrell et al., 2019; Green and Wong, 2009; Finseraas and Kotsadam, 2017; Finseraas et al. 2019; Page-Gould et al. 2008). Most field experiments that support positive effects of interethnic contacts evaluate arguably extreme interventions that enforce prolonged coresidence of young adults in college dorms or military bootcamps (Boisjoly et al., 2006; Burns et al., 2016; Carrell et al., 2019; Finseraas and Kotsadam, 2017; Finseraas et al. 2019; Camargo et al 2010). For example, Boisjoly et al. (2006) found that random assignment to African American roommates at a selective U.S. university increased white students' support for affirmative action. Carrell et al. (2019) found that assignment to African American roommates at the United States Air Force Academy increased white students' requests for African American roommates in subsequent years. Finseraas and Kotsadam (2017) found that random assignment to ethnic-minority roommates during boot camp improved Norwegian army recruits' opinion of immigrants' work ethic. In a similar spirit, Green and Wong (2009) find that white high-school students developed greater out-group tolerance after random assignment to racial or ethnic

others during a three-week long outdoor survival course.

Far fewer field experiments evaluate the consequences of contact with ethnic others under less extreme conditions. Page-Gould et al. (2008) evaluate a friendship building exercise that randomly paired white and Latinx college students during three one-hour meetings. They find no evidence of interethnic contact on initiating cross-group interactions 10 days after the end of the intervention on average, but they do report statistically significant positive effects for students with high initial prejudice. Yet other experiments report positive effects of contact between groups defined by categories arguably related to ethnicity, such as assignment to members of different castes in Indian cricket teams (Lowe 2020) or assignment of Christians and Muslims to vocational training courses in Nigeria (Scacco and Warren 2018) or to soccer teams in Iraq (Mousa 2020). Rao (2019) investigates in group bias in Indian schools and find that wealthy students discriminate poor students less after exposure.

Like contact theory, conflict theory is supported by numerous observational, and mostly cross-sectional, studies (e.g. Alesina and La Ferrara, 2002; Delhey and Newton, 2005; Dinesen and Sønderskov, 2015; Stolle, Soroka, and Johnston, 2018; Legewie and Scheffer 2016), and also by some recent field-experimental evidence. For example, in a quasi-experimental study exploiting demographic changes in Chicago neighborhoods following the demolition of large public housing complexes, Enos (2016) found that voter turnout and the vote share for conservative candidates decreased sharply among whites as African American neighbors moved away. Yet more strikingly, in one of the few randomized field experiments on the topic, Enos (2014) found that randomly placing Spanish-speaking individuals at commuter train stations in Boston significantly increased exclusionary attitudes toward immigrants among white passengers.

Constrict theory, as a variant of conflict theory, currently lacks support from high-quality evidence. Putnam, (2007) based constrict theory on the observation that less ethnically diverse neighborhoods in the United States have higher levels of in-group trust among whites. However, more ethnically diverse U.S. neighborhoods are not only less white, but also poorer and less stable than more ethnically homogenous neighborhoods (Abascal and Baldassarri 2015; Meer and Tolsma 2014). It is difficult to disentangle the effect of ethnic diversity on trust from the effects of poverty, residential mobility, and other, potentially unmeasured, correlates of diverse locales in observational studies. The only randomized field experiment testing constrict theory (Finseraas et al. 2019) fails to support it.

The apparently disparate—positive and negative—effects of inter-ethnic contact are generally reconciled by different scope conditions for contact and conflict theory (e.g. Abascal and Baldassarri 2015; Dinesen and Sønderskov 2015; and Valdez 2014). In Allport's (1954) classical formulation of contact theory, interethnic contact will promote positive intergroup relations when (i) contact is supported by an authority and both groups (ii) share equal status and (iii) cooperate (iv) in the pursuit of common goals. Subsequent research additionally emphasized the importance of (v) close and prolonged interactions (e.g., Finseraas and Kotsadam 2017) and (vi) friendship potential among the interacting individuals (e.g., Pettigrew, 1998; Laurence, 2009; Stolle, Soroka, and Johnston, 2008; Van Laar et al., 2005). The scope conditions for conflict theory include (i) fleeting, shallow, and non-repeated exposures (Enos 2014), or (ii) exposures occurring when in-group and out-group members manifestly compete over scarce resources, social rights, and social status (e.g. Bobo 1999; Semyonov et al. 2006), or (iii) exposures to out-group members that are *perceived* to pose economic or cultural threats (Blalock, 1967; Williams, 1964; Bobo

1999; McLaren, 2003; Valdez, 2014).

The scope conditions for both theories are vague. For contact theory, it is unclear how close is close enough; what characterizes friendship potential; and in what sense equal status is even possible in societies where the majority oppresses the minority. Regarding conflict theory: when do individuals not compete over some resource, and when can analysts exclude the possibility of perceived cultural threat? These problems are multiplied if each scope condition has to be evaluated along multiple dimensions: cooperation vs. competition with respect to multiple goals; status with respect to multiple criteria. Furthermore, even if individual scope conditions are unambiguously met, it is unclear to what extent scope conditions can trade off against each other: does enthusiastic cooperation in pursuit of a single overriding common goal make up for status inequality or perceived cultural threat?

The unavoidable conclusion is that most real-world settings do not constitute ideal-typical matches for either theory. This is not to say that those settings do not exist. For example, Norwegian military bootcamps may be considered well-nigh obvious settings for contact theory (Finseraas and Kotsadam 2017), because multiethnic teams spend most of their days pursuing externally mandated group goals that are evaluated at the team level, cooperation is enforced by drill sergeants, and equal status within teams is a stated policy of the military, symbolically manifested by identical rank, pay, and uniforms. By contrast, even in college dorms, which are the most common setting for field experiments on contact theory, one might wonder to what extent roommates necessarily cooperate in the pursuit of a common goal, rather than, say, fight over space and quiet hours.

This theoretical ambiguity triggers practical concerns for policy. While the field

experimental evidence demonstrates that contact *can* reduce prejudice, it also demonstrates that contact *can* increase prejudice. Absent unambiguous, let alone exhaustive, scope conditions, it is very difficult to defend ex-ante expectations about the effects of contact in new settings. This is a problem inasmuch as promoting interethnic contact is a popular strategy for improving interethnic relations in new settings where the effects of contact have not yet been evaluated. We take the view that the labor of refining the scope conditions of contact and conflict theory must take the form of building an expansive evidence base that evaluates germane interventions in new settings with dependable research designs.

### **3. Setting and Expectations**

#### **3.1. Setting**

We test the effects of interethnic exposure on interethnic friendships and discrimination in 186 3rd through 8th grade classrooms of 39 schools in rural Hungary. Hungary is an ethnically homogenous country in which the Roma ethnic minority experiences a high degree of discrimination. The Roma are Hungary's largest ethnic minority, comprising 3 percent of the total population, and 12 percent of Hungarian youth. Many Roma speak the Romani language in addition to Hungarian; and many are recognizable by appearance to non-Roma Hungarians (Kertesi and Kézdi, 2011b). Roma people suffer severe economic, social, and health disadvantages and are frequent targets of bullying, prejudice, and discrimination (Kertesi and Kezdi 2011a; Hajdu et al. 2017a,b; Simonovits et al 2018, Grow et al. 2016, Kisfalusi et al. 2018, Kisfalusi et al. 2019).

Students in Hungary attend untracked compulsory primary schools from 1<sup>st</sup> through 8<sup>th</sup>

grade, corresponding to elementary and middle school in the United States. Students attend local schools. Therefore, the ethnic composition of the student body in each school reflects the ethnic composition of the local catchment area. Students form stable classrooms that receive instruction in all core (and most other) subjects together. Furthermore, most subjects are taught in the same room (except physical education, and, depending on facilities and grade level, art, music, and the sciences). Seating charts are set by teachers and fixed for all subjects within a given room. The core subjects in primary school are Hungarian grammar (writing), Hungarian literature (reading), and mathematics. Grades in these subjects determine the subsequent allocation of students to tracked secondary schools, starting in 9<sup>th</sup> grade. Instruction is lecture based, interspersed with group work between deskmates.

Interethnic contact in schools occurs at multiple levels. Students have limited exposure to schoolmates across grades, with whom they only share recess. Students share more time with their classmates, with whom they share instruction in most subjects. Finally, students are most exposed to their deskmates, with whom they share the closest proximity throughout the school day and with whom they cooperate in group work.

### 3.2. Scope conditions in context

We test the causal effects of interethnic exposures between Roma and non-Roma Hungarians on interethnic friendship and discrimination against Roma at two different levels: first, at the desk level within classrooms; second across classrooms within schools.

Ex-ante expectations about the fit between our setting and the scope conditions for contact and conflict theory determine theoretical predictions for the effects of these exposures. We argue that deskmate exposures to ethnic others, even more so than classmate exposures,



best fit the scope conditions of contact theory, especially in the expansive interpretation of Pettigrew et al. (1998). Support for interethnic contact by an authority is self-evident: schools have assigned Roma and non-Roma students to the same classrooms, and teachers seat Roma and non-Roma students next to each other at the same desk. Equal status of students is given in the sense that Roma and non-Roma students share the same classroom as peers, have the same teacher, and are subject to the same curriculum and grading standards. Roma and non-Roma students collaborate and pursue common goals in group work. Exposure is long lasting, in that students typically stay with their classmates from 1<sup>st</sup> through 8<sup>th</sup> grade, and seating charts are set for a whole semester. Finally, classrooms are ripe with friendship potential, as we document below. We therefore expect that sitting next to, and sharing a classroom with, Roma students will increase interethnic friendships and decrease discrimination by non-Roma students, in line with contact theory.

Like most natural settings, however, one can easily argue that our setting also strains against the scope conditions of contact theory. For example, the degree of actual collaboration between deskmates and classmates in pursuit of common goals may not be extensive, just like the degree of cooperation may not be extensive between college roommates. Similarly, equal status among students within the formal framework of Hungarian education does not prevent community prejudice from inflecting interactions among students or between students and teachers, much like randomly assigning college roommates does not prevent social hierarchies from entering dorms. One might therefore also legitimately expect that sitting next to, and sharing a classroom with, Roma students might worsen inter- and intra-ethnic relations, in line with conflict and constrict theory.

Our pre-analysis plan registered positive expectations for the effects of interethnic exposure at the desk-level, because we judge the fit between our setting and the scope conditions of contact theory to be no worse than the fit in studies of college roommates, which form the backbone of field experimental support of contact theory. However, in order to honor the ambiguity of the scope conditions, we specifically pre-registered double-sided statistical tests at all levels of analysis, to enable testing the predictions not only of contact, but also of conflict and constrict theory.

Our ability to test contact and conflict theory at once means that our study tests whether our novel setting meets the scope conditions of either theory. Hence, our study generates new insights not only into the scope conditions of either theory, but into the boundary conditions between theories.

#### **4. Study design**

Our study is distinguished by three main design elements. First, we create a randomized vignette experiment to measure interethnic discrimination of non-Roma students against Roma students. Second, we randomize the seating charts within classrooms to evaluate exposure to Roma vs non-Roma students at the desk level within classrooms. Third, we exploit quasi-random variation in the share of Roma students across classrooms within schools to evaluate the effect of classroom level exposures.

##### 4.1 Vignette experiment to measure discrimination

In order to measure ethnic discrimination, we designed a two-question survey experiment that

presented students with a scenario to lend money to their classmates during a hypothetical field trip to the zoo. The first question (Question 9a) was designed to introduce the scenario by eliciting students' willingness to lend money to their deskmate. It reads (in translation):

*"Imagine that you are going to the zoo with some of your classmates. Your deskmate (whom you sat next to in Hungarian class in December) has forgotten to bring money for the entrance ticket. You have enough money for two entrance tickets. Would you lend your deskmate the money for the entrance ticket?"*

We note that this first question was designed merely to introduce the scenario; it was not designed to elicit information about effects of deskmates on ethnic discrimination. While we can use it to measure differences for people with different deskmates, we cannot use it to estimate the effect of exposure since e.g., students sitting next to a Roma student are not asked to make a decision about a non-Roma deskmate.<sup>3</sup> In order to measure discrimination, and the effects of exposure to a Roma deskmate on discrimination, we therefore conducted a survey experiment that randomly assigned students to different ethnic prompts. Question 9b reads:

*"Now imagine that it is not your deskmate, but a different classmate who has forgotten to bring money with him/her. **This classmate is a Roma/Gypsy.** Would you lend this **Roma/Gypsy** classmate the money for the entrance ticket?"*

The bold text is only presented to a random half of the students.<sup>4</sup> The answer categories were "Yes," "No," and "I do not know."

Vignette experiments are commonly used for eliciting attitudes that cannot be inferred from manifest behavior, or when researchers are worried about social desirability bias (Atzmüller and Steiner 2010; Hainmueller et al. 2015). Vignette experiments have previously been used to

study discrimination (Finseraas et al. 2016; Jakobsson et al. 2016). Since the only difference for the respondents is whether or not they are told that the classmate is of Roma ethnicity, and since this difference is randomly assigned, the difference in response to this question measures discrimination. The experiment also measures differences in in-group and out-group trust, which has previously been shown to be affected by close inter-group contact (Finseraas et al. 2019).

#### 4.2 Randomizing deskmates

The field experiment manipulated interethnic contact between Roma and non-Roma students by randomizing the seating chart within 3rd through 8th grade classrooms of schools in rural Hungary. Students were randomly allocated to freestanding front-facing desks seating two students each based on class lists provided by the schools. The intervention commenced at the beginning of the 2017 fall semester, and we encouraged adherence to the seating chart until the end of the semester in January 2018. Outcomes data were collected by the field team in the spring of 2018, including the randomized survey-vignette experiment.<sup>5</sup>

Based on a set of pre-registered inclusion criteria, our base sample consists of 3,184 Roma and non-Roma students across 186 classrooms of 39 schools. Excluding the Roma students, who serve as exposures but not as subjects in the analyses below, the final analysis sample includes 2,395 non-Roma students in 175 classrooms and 39 schools. Ex-ante power calculations were based on a sample of at least 2000 non-Roma individuals; hence, our sample is sufficiently large to detect meaningful effects.

#### 4.3. Quasi-randomization of classmates

We investigate the effect of grade or classroom-level exposure to Roma students using

quasi-experimental strategies that exploit across-classroom (grade) variation in the share of Roma students. We assume that the variation in the share of Roma is essentially random in such regressions, an assumption that is more likely to hold when we add school fixed effects and when investigating across grade variation. The variation in this latter specification will be driven by cohort-by-cohort variation at the same school.

## **5. Data and coding of main variables**

Here, we highlight the main features of our design and data collection. See Appendix B for details. A detailed pre-analysis plan was archived on March 22, 2018, before any endline data was received in June 2018. Any deviation from this plan is highlighted in the text. The data collection instruments and the pre-analysis plan [blinded for review] are reproduced in Appendix C. Replication data will be available online upon acceptance. We obtained consent from teachers and parents at multiple points. The study was approved by the IRB offices at [blinded for review].

We define three treatment variables; one for the deskmate intervention, one for the vignette experiment, and one for the cross-product of the two. The first treatment variable, *Roma Deskmate*, equals 1 if a student is randomized to sit next to a Roma deskmate at the beginning of the fall semester, and 0 otherwise. Students' ethnicity was reported by classroom teachers at the beginning of the trial. By using assigned rather than actual deskmates we perform an intention to treat analysis that is not biased by the endogenous seating choices made by students and teachers after randomization. 86 percent of students in the analysis sample were seated in compliance with assignment.<sup>6</sup>

The second treatment variable is coded from the vignette experiment conducted at endline. The variable *Roma Vignette* equals 1 if the text of the vignette (Question 9b) asks students if they would lend money to a Roma classmate, and equals 0 if the text asks students to

lend money to a classmate of unspecified ethnicity. The third treatment variable is the cross-product between the first two, i.e. *Roma Deskmate* \* *Roma Vignette*. This cross product equals 1 if a student sitting next to a Roma deskmate is asked to lend money to a Roma classmate, and 0 otherwise.

Outcome variables were collected via a 45-minute two-part student survey at endline (see appendix). The first part of the survey (20 minutes) elicited ego-centric network data, contained the survey experiment, and asked several other questions (which were pre-registered not to be analyzed in this paper).<sup>7</sup> The two versions of the endline questionnaire containing the two versions of the survey vignette (Question 9b) were given to students in random order (using a random number generator).

We pre-registered two main outcome variables: *Lend to Classmate* and *Roma friend*. The outcome variable *Lend to Classmate* is based on the survey vignette experiment (described above) and equals 1 if the respondent answers that they would, and 0 if they would not, lend money to the classmate. *Roma friend* captures whether an individual has a Roma friend among his or her best friends. Survey Question 5 generically prompts: "Now in general think of your best friends, not just in the class but EVERYWHERE." Question 5d subsequently prompts specifically: "Among your best friends, how many are Roma (gypsy)?" The outcome variable *Roma Friend* equals 1 if the individual has at least one Roma friend, and 0 otherwise.

We also pre-registered several secondary outcome variables for exploratory analyses. In addition to investigating the probability of having a Roma friend, we also investigate effects on the *Number of Roma friends*, elicited in Question 5d. We also create the variables *Deskmate among best friends* (to see if deskmate relations in general are characterized by friendship potential) and *Liked*

*sitting next to deskmate.*

We collected baseline covariates from classroom-teacher reports, including students' age (in 0.1 years), gender, and spring-semester 2017 grades in five core subjects (Hungarian literature, Hungarian grammar, mathematics, diligence, and behavior, coded on a five-point integer scale where 1 is worst and 5 is best). We filled in missing baseline grades from student self-reports at endline.<sup>8</sup> For remaining missing values, we coded the observation as zero and included dummy variables controlling for missing status in order to retain observations.

Table 1 presents descriptive statistics for the sample of 2395 non-Roma ethnic Hungarian students, separately by whether they were exposed to a Roma (treated) or non-Roma (control) deskmate. We see that 30 percent of non-Roma Hungarian students in the total sample have at least one Roma friend. This share naturally differs substantially across treated and control individuals because this descriptive comparison does not account for the substantial difference in the share of Roma students in the classroom (our subsequent analysis controls for pre-registered classroom fixed effects). We also see that 57 percent of students are willing to lend money to a classmate in the vignette experiment. The share that are willing to lend to a Roma classmate (Lend to Roma) is lower than the share willing to lend to an unspecified classmate (Lend to non-Roma). For these variables there does not seem to be any difference between treated and control students.

(Table 1 here)

Table 2, tests whether the randomization achieved covariate balance across treated and control individuals, after controlling for classroom fixed effects to account for experimental design. Columns 1-7 show the regression of treatment (having a Roma deskmate) on each of the

baseline covariates and classroom fixed effects. Column 8 include all baseline covariates simultaneously. Associations between the covariates and treatment would indicate a failure of randomization. We see that there does not appear to be any differences across the treated and control groups. Following our pre-analysis plan, we also compute an F-test for whether the control variables jointly predict treatment status in a regression of treatment on all covariates. The p-value of the F-test is 0.25, indicating that the randomization was successful in achieving balance on observable factors. In column 9 we change the dependent variable to Roma vignette instead and we note that there is balance on baseline covariates also for this variable (the p-value of the F-test is 0.83).

We also note that among the 2395 individuals in our analytical sample, only 2102 and 2124 answered the survey questions about lending and friendship respectively. Importantly, this attrition is unrelated to treatment status (the p-values are above 0.9 for Roma deskmate in regressions of attrition on Roma deskmate and class fixed effects).

(Table 2 here)

## **6. Empirical strategy**

A large methodological literature in sociology, economics, and statistics documents the severe and often unexpected challenges of identifying peer effects when social ties are not randomly formed (e.g., Angrist 2014; Ogburn and VanderWeele 2014; Manski 1993). Our field experiment avoids these difficulties by randomizing deskmate relationships. In order to focus on the effect of exposure to a stigmatized minority group on the discrimination and friendship behaviors of the ethnic majority population, we exclude the Roma students themselves as subjects from the regressions.

To analyze the effects of sitting next to a Roma deskmate and being prompted to lend money



to a Roma classmate on the chance that a non-Roma student lends money to a classmate, we estimate the following regression:

$$\text{Lend to Classmate}_{ict2} = \beta \text{Roma Deskmate}_{ict1} + \vartheta \text{Roma vignette}_{ict2} + \delta \text{Roma Deskmate}_{ct1} * \text{Roma vignette}_{ict2} + \alpha \text{Class}_{ct1} + \gamma X_{ict1} + \varepsilon_{ict2},$$

where  $i$  indexes individuals,  $c$  indexes classrooms, and  $t$  is time (either baseline=1 or follow-up=2).  $\text{Roma Deskmate}_{ict1}$  is a dummy equal to 1 if this person is assigned a Roma deskmate,  $X_{ict1}$  is a set of individual-level baseline covariates (described in section 4), and the error term,  $\varepsilon_{ict2}$ . We add the variables  $\text{Roma vignette}$ , which equals 1 if the vignette prompts the student to lend money to a Roma classmate, and the interaction between  $\text{Roma vignette}$  and  $\text{Roma Deskmate}$ . A vector of classroom fixed effects,  $\text{Class}_{ct1}$ , is included as the randomization was conducted within classrooms. A positive and statistically significant estimate for the interaction term,  $\delta > 0$ , would indicate support for the contact hypothesis that sitting next to a Roma deskmate diminishes discrimination toward Roma, whereas a statistically significant negative estimate,  $\delta < 0$ , would indicate support for conflict hypothesis that sitting next to a Roma deskmate increases discrimination.

The theories of inter-ethnic contact are thus tested by a difference in difference model. The estimate for  $\delta$  gives the differential effect of inter-ethnic contact on lending money to a classmate when that classmate is identified as Roma. We also interpret the coefficients for  $\text{Roma Deskmate}$  and for  $\text{Roma vignette}$ . In particular, we will divide the sample into classrooms that are majority Roma and classrooms that are majority non-Roma. In classes that are majority non-Roma, being prompted to lend to a classmate of unspecified ethnicity de facto prompts students to lend money to a non-Roma. Hence, the coefficient for  $\text{Roma Deskmate}$  in equation 2 tests whether

having a Roma deskmate affects in-group trust. According to conflict theory, exposure to ethnic diversity should lead to lower trust towards the in-group. The coefficient for *Roma vignette* estimates the differential willingness to lend to a Roma classmate for individuals not exposed to a Roma deskmate. Since both deskmates and the vignette are randomized, all three coefficients can be given causal interpretations.

We estimate the following regression to identify the effect of being randomly assigned to sitting next to a Roma deskmate on the probability of having a Roma friend:

$$RomaFriend_{ict2} = \beta RomaDeskmate_{ict1} + \alpha Class_{ct1} + \gamma X_{ict1} + \varepsilon_{ict2},$$

A positive and statistically significant estimate  $\beta > 0$  would support the contact hypothesis that sitting next to a Roma student increases the chance of naming a Roma among a student's best friends, and  $\beta < 0$  would lend support to conflict theory.

We present all results with and without the baseline controls; our primary specification is without controls. Control variables are included as they may increase power, although they are not necessary for identification. To avoid distortions from functional form restrictions, we add all control variables as series of indicator variables (Athey and Imbens, 2017). We report heteroscedasticity-robust Huber-White standard errors for all models. Standard errors do not need to be clustered at any level, as randomization occurred at the individual level (Abadie et al., 2017).

In the quasi-experimental analysis of classroom level exposure we conduct several analyses. First, we regress lending on the share of Roma in the class and the interactions without any controls. Second, we include school-level fixed effects to defend against the possibility that non-Roma families in high-Roma schools (and hence high-Roma classrooms) are positively selected for interethnic trust (perhaps because non-Roma families who most object to

interethnic exposure leave the school district) or negatively selected (for instance due to housing prices in such areas attract people that may have other social issues and therefore less trust).<sup>9</sup> Third, we control for classroom-average GPA to defend against the possibility that ability sorting across classrooms within grades influences the share of Roma in the classroom. This final analysis is hence expected mostly to exploit variation in the share of Roma students across grades within school. We expect this cross-cohort variation to be largely random (Hoxby 2000; Gould, Levy, Passerman 2009).<sup>10</sup>

## **6 Results**

### **6.1 Discrimination and Field experimental results**

Table 3 presents our primary results. We start by showing the effects of having a Roma deskmate on lending to a classmate using our vignette experiment. Our hypothesis was that being assigned to a Roma deskmate would increase the relative probability of willingness to lend to a Roma classmate. Column 1 shows our main pre-specified regression, with classroom fixed effects but without other covariates. We see that the coefficient for Roma vignette is negative and statistically significant, implying that non-exposed students are considerably less willing to lend to a Roma classmate than to a classmate of unspecified ethnicity. Being randomly prompted to lend to a Roma classmate lowers the probability that a non-Roma student is willing to lend by 18 percentage points. The mean in the control group (non-Roma vignette and non-Roma deskmate) is 67 percent, so the effect corresponds to a 27 percent lower likelihood of lending money to a Roma classmate. We conclude that non-Roma students strongly discriminate against Roma students.

(Table 3 here)

Our main interest lies, however, in whether sitting next to a Roma student affects

students' differential willingness to lend to a Roma student, as captured by the coefficient on the *Roma deskmate*\**Roma vignette*. We see that that there is no statistically significant effect, and the coefficient (-0.03, p=0.61) is small and even negative, suggesting that treatment did not change the differential between lending to a Roma classmate or a random classmate.

The coefficient on Roma deskmate estimates the effect of being assigned to a Roma deskmate on wanting to lend money to a random (not-specifically Roma) classmate. We see that this effect is close to zero and not statistically significant (-0.03, p=0.57). If there were no majority Roma classrooms, this effect would be a test of the constrict hypothesis. In the majority Roma classrooms, not specifying the ethnicity of the classmate would most likely be interpreted as a prompt to lend to a Roma classmate. In appendix Table A.11 we show that the coefficient is also small and not statistically significant in non-Roma majority classrooms and we can thereby reject the constrict hypothesis that exposure to the ethnic outgroup reduces trust in the in-group.

Column 3 of Table 3 presents our pre-specified estimates for the effect of having a Roma deskmate on counting at least one Roma friend among one's best friends. The estimate is very close to zero and has a small standard error (-0.03, p=0.34). Hence, we find no support for the notion that sitting next to a Roma affects inter-ethnic friendships for non-Roma Hungarian students. Adding baseline covariates in column 4 does not appreciably change the results.

Table 4 presents estimates for the effects of having a Roma deskmate on our pre-specified secondary outcomes. We find no effect of having a Roma deskmate on the students' number of Roma friends (column 1). Columns 2-4 show results intended to capture antipathy and distrust towards Roma deskmates. If the deskmate was Roma, non-Roma students were less likely to espouse a willingness to lend to their deskmate, were less likely to like sitting next to their deskmate,

and were less likely to nominate their deskmate as one of the 5 closest friends. Note that the results in columns 2-4 are not suited to detect any effects of exposure to a Roma deskmate on students' attitudes toward Roma individuals in general, as all treated individuals have a Roma deskmate and all non-treated individuals have a non-Roma deskmate. The results in columns 2-4 show, however, that there is substantial antipathy and distrust towards Roma students, even among those exposed to Roma deskmates.<sup>11</sup> The table also shows that deskmates usually like each other and that deskmates are likely to become friends in general.<sup>12</sup>

(Table 4 here)

## **6.2 Quasi-experimental results**

We move on to test whether classroom level exposure affects inter-ethnic discrimination and friendship formation. We explored several specifications and we present the results in Table 5. We create a standardized variable (with mean zero and a standard deviation of 1) of the share of Roma students in the classroom and interact this variable with our main treatment variables.<sup>13</sup> The standard errors in these regressions are clustered at the classroom level.

In column 1 we regress lending on the share of Roma in the class and the interactions without any controls. We see that there is no statistically significant relationship between the share of Roma in the class interacted with getting the Roma vignette. That is, there is no difference in reported lending for individuals having relatively more or fewer Roma students in the class. Neither is there any interaction between having a Roma deskmate and having many Roma in your class. In columns 2 and 3 we see that the results are similar if we add school fixed effects and classroom-average GPA. The results of these exploratory models consistently fail to support that greater classroom level exposure to Roma students reduces discrimination among

non-Roma students.

(Table 5 here)

We also analyze the effect of having more Roma students in the class on the probability of having a Roma as one of the best friends in the same fashion. We see in column 4 that the share of Roma in the class is itself highly correlated with the probability of having a Roma friend. We also note that there is no statistically significant effect of being assigned a Roma deskmate in classrooms with an average share of Roma on nominating a Roma student as a best friend, but the interaction term is statistically significant in column 4, showing that the effect of having a Roma deskmate is more negative in classrooms with a greater share of Roma students. To alleviate some of the endogeneity concerns we include school fixed effects and compare classrooms within the same schools. In column 5 (which includes school fixed effects) we see that the share of Roma in the class is highly predictive of nominating a Roma student as a best friend. Having a one-standard deviation greater share of Roma students in the class (about 17 percent) is correlated with a 16 percentage points higher probability of having a Roma friend as compared to having an average share of Roma classmates (12 percent). The results point to the classroom as an important arena for exposure to enhance cross-ethnic friendships and the results are robust to controlling for average class GPA (column 6). There is no sign of a differential treatment effect by Roma deskmates having different effects in classes with few and many Roma once school fixed effects are controlled for.

In total, while the share of Roma at the classroom level may be important for friendship formation it does not seem to be sufficient to affect the degree of discrimination against Roma students. Therefore, neither deskmate- nor classroom-level exposure to ethnic others appear to suffice to eliminate, or even diminish, discrimination.

## 7 Conclusion

Contact theory posits that close, collaborative contact under conditions of institutionally supported equal status will reduce prejudice and discrimination against ethnic others. By contrast, conflict theory suggests that shallow or competitive exposure may increase antipathy and discrimination. Little is known about where the lines between close versus shallow and collaborative versus competitive exposure are drawn, so it is unclear in what situations exposure produces beneficial outcomes.

Previous evidence on whether exposure reduces prejudice is mixed. Shallow exposure is generally correlated with more prejudice and less trust (e.g., Delhey and Newton, 2005; Dinesen and Sønderskov, 2015) and has been found to causally increase prejudice (e.g., Enos, 2014; 2016, Hangartner et al. 2019). On the other hand, a number of well identified studies using random assignment of peers have found positive effects of close personal contact (e.g., Boisjoly et al., 2006; Carrell et al., 2019; Dahl et al. 2018; Finseraas and Kotsadam, 2017; Finseraas et al., 2016, Finseraas et al. 2019). In a recent meta-analysis, Paluck et al. (2019) can only identify three studies that meet what they label “the very highest standards of experimental quality and research transparency” (p. 150), in particular having pre-specified analysis plans, and they argue that we need more high quality studies to learn more about whether and when contact theory operates.

In a well-powered, pre-registered, randomized field experiment, we test whether deskmate exposure to Roma students in Hungarian schools affects discrimination. Using a vignette experiment, we show that non-Roma students discriminate and trust Roma students less. We further show that being randomly assigned a Roma deskmate does not affect non-Roma students’ discrimination nor the probability of having a Roma best friend. This is quite stark as

students are sitting next to their deskmates for around 20 hours each week. In addition we show that the students exposed to Roma deskmates show substantial antipathy towards them, as they are less likely to like sitting next to their deskmate and less likely to have their deskmate as one of the 5 closest friends as compared to students with non-Roma deskmates.

We further investigated whether there are effects of inter-ethnic contact at the classroom level. Leveraging a quasi-experimental approach with school fixed effects, we find no support for this. In this setup we do find, however, that the share of Roma at the classroom level seems to be important for friendship formation. Nonetheless, even when cross-ethnic friendship levels are higher, it does not seem to be sufficient to affect the degree of discrimination against Roma students.

Our findings disappoint the hope that an easy intervention of increasing spatial proximity through deskmate assignments could ameliorate ethnic discrimination. While prior research has found that enforced ethnic intermingling in college dorm rooms or military boot camp squadrons does increase inter-ethnic trust, the intensity of such contact may be hard to replicate in more everyday settings (Paluck and Green 2009). Our results suggest that in order for close contact to reduce discrimination, it must be more intense (e.g. by sharing living quarters in college) and/or more collaborative (e.g. by sustained teamwork in military boot camp) than sharing a desk with another student. Or perhaps the contact must take place in a setting where participants enjoy more equal status than do Roma and non-Roma children in Hungarian schools, that is, perhaps the positive effects of interethnic contact presuppose relatively low levels of existing ethnic inequalities. The equal status condition may be hampered for several reasons. The school setting need not be one where status differences are minimal or the parents and the teachers may be



biased so that there is no enforcement of equal status. Prejudice against the Roma is widespread and severe in Hungary. For instance, Hajdu, et al., (2017b) report that 40 percent of Hungarians think Roma customers should be banned from bars and where 60 percent agree with the statement that the inclination for criminality is “in the blood” of the Roma. In such a context it is likely that adults, such as parents and teachers, are not enforcing equal status.

If the conditions for contact theory really need to be as stark as sharing rooms or in settings with equal status in the wider community there is little hope for large-scale social change by exposure. This raises the spectre that many well intentioned contact interventions in settings of ethnic inequality amount to cosmetics, like rearranging the desk chairs on a ship whose destination is otherwise determined.

While deskmate exposure does not lead to less ethnic discrimination among Hungarian school children, it is important to note that it does not lead to more discrimination either. As such, our paper shows that the setting is not one where the scope conditions of conflict theory applies. Neither do we find any evidence that exposure to a Roma deskmate affects trust *within* the majority group, thereby we also reject the constrict hypothesis in this setting. Future work should continue to evaluate the scope conditions of contact theory. If they do, we further urge them to pre-register the experiments so that we can start to build a truly credible research base on these issues, which are likely to be ever more contentious in the future due to our societies becoming more and more diverse.

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#### ENDNOTES

<sup>1</sup> Gerber and Malhotra (2008) document that the leading sociology journals prioritize the publication of statistically significant findings in general.

<sup>2</sup> While pre-registration does not by itself guarantee high methodological standards, it at least guards against the problem of "p-hacking" (respecifying models until results meet desired levels of statistical significance, despite the



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fact that respecification can invalidate p-values (Head et al. 2015) and “HARKing” (“hypothesizing after the results are known,” which also invalidates p-values (Christensen et al. 2019).

<sup>3</sup> Imagine that we would find that those sitting next to a Roma deskmate are less likely to lend out money to their deskmates. This tells us nothing about the effect of exposure as we do not know the counterfactual lending of this person to a Roma deskmate had they not had a Roma deskmate.

<sup>4</sup> Although the Hungarian term for “gypsy”, similar to the English term, carries derogatory connotations, it is a commonly used term in Hungary, and it is the only descriptor of this minority known to many school children. By contrast, the term “Roma” is not universally known to non-Roma school children. Hence, we prompt for both “Roma” and “Gypsy.” We did not specify the ethnicity of the hypothetical classmate when not prompting for a Roma classmate, because we did not want to make ethnicity salient for the control group.

<sup>5</sup> We pre-tested the survey instrument in one out-of-sample school in the same geographic area in the Fall of 2017.

<sup>6</sup> Compliance is not correlated with treatment status and the conclusions remain the same if we restrict the sample to classrooms with at least 90 percent compliance (see Appendix Table A.1).

<sup>7</sup> The second part of the endline survey consisted of a reading comprehension test that is not part of this project.

<sup>8</sup> We did not pre-register that we would fill in missing baseline grades from student self-reports, but the procedure does not affect conclusions.

<sup>9</sup> The use of school fixed effects was not pre-registered but we chose to report these results because they add to our understanding of the mechanisms at play.

<sup>10</sup> In Appendix Table A.12 we present results with grade level variation rather than class, and the results are very similar.

<sup>11</sup> The finding that non-Roma students are less likely to befriend their deskmate when they are assigned a Roma rather than a non-Roma deskmate reveals ethnic antipathy on the part of the non-Roma students. This finding does not, however, mean that being seated next to a Roma deskmate has caused this ethnic antipathy in the first place. Hence, it does not imply that seating non-Roma students next to Roma students makes non-Roma students less favorably inclined to Roma students in general, or even toward the particular Roma student who is their deskmate. This finding is consistent with the results of Table 3, columns 3 and 4, which does test whether sitting

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next to a Roma student increases (or decreases) discrimination, and finds no evidence that it does.

<sup>12</sup> We extensively explored the possibility of heterogeneous treatment effects but found little indication of effects for any specific group of students. Appendix Tables A.3 and A.4 show that there is no indication of heterogeneous treatment effects based on ego characteristics and Tables A.5 and A.6 show the same for deskmate characteristics (age, gender, baseline grades). All continuous variables are standardized to have a mean of zero and a standard deviation of 1 to facilitate interpretation in the heterogeneity analyses. Appendix Table A.7 shows that there are no statistically significant treatment effects for neither same sex deskmates or sex discordant deskmates. We further specified that we would investigate whether it makes a difference whether or not the person lent to his or her deskmate in an analysis of lending to Roma. In Appendix Table A.10 we show that people that are willing to lend to their deskmate are more willing to also lend to a Roma classmate but the treatment effect of exposure to Roma deskmate is not statistically significantly different between those willing to lend to their deskmates and not. As there is no heterogeneity we do not proceed with using machine learning techniques to validate the search for heterogeneous treatment effects (as pre-specified).

<sup>13</sup> Using the share instead of a majority indicator has several advantages. In particular, we only have 133 non-Roma students in majority Roma classrooms, and the share of Roma students is a likely confounder for treatment even within the groups of majority and minority Roma classrooms. We show the corresponding results with majority status of the classrooms in Appendix Table A.9.

## Tables

Table 1: Descriptive statistics

	Total		Treated		Control	
	Mean	SD	Mean	SD	Mean	SD
<i>Main dependent variables</i>						
Roma friend	0.30	(0.46)	0.52	(0.50)	0.27	(0.44)
Lend to Classmate	0.57	(0.49)	0.57	(0.50)	0.57	(0.49)
<i>Other outcome variables</i>						
Number of Roma friends	0.84	(2.60)	1.89	(3.86)	0.71	(2.36)
Lend to Roma	0.48	(0.50)	0.49	(0.50)	0.48	(0.50)
Lend to non-Roma	0.67	(0.47)	0.66	(0.48)	0.67	(0.47)
Lend to Deskmate	0.83	(0.38)	0.76	(0.43)	0.84	(0.37)
Liked sitting next to Deskmate	0.58	(0.49)	0.49	(0.50)	0.59	(0.49)
Deskmate among best friends	0.29	(0.46)	0.20	(0.40)	0.31	(0.46)
<i>Main exposure variable</i>						
Roma Deskmate	0.12	(0.33)	1.00	(0.00)	0.00	(0.00)
<i>Control variables</i>						
Age (in years)	11.83	(1.82)	11.97	(1.81)	11.81	(1.82)
Mathematics	3.83	(1.01)	3.68	(1.00)	3.85	(1.01)
Grammar	3.96	(1.00)	3.79	(1.11)	3.98	(0.98)
Literature	3.83	(1.08)	3.67	(1.13)	3.85	(1.07)
Diligence	4.15	(0.88)	4.03	(0.92)	4.17	(0.88)
Behavior	4.41	(0.76)	4.34	(0.82)	4.42	(0.75)
<i>Variables for heterogeneity analysis</i>						
GPA	3.87	(0.94)	3.71	(0.99)	3.89	(0.93)
Deskmate of same sex	0.50	(0.50)	0.46	(0.50)	0.50	(0.50)
Share of Roma in class	0.12	(0.17)	0.38	(0.24)	0.08	(0.13)
<i>N</i>	2395		291		2104	

*Notes:* All samples consist of non-Roma students that have a non-missing value on Roma deskmate. In column 2 we restrict the sample to students that have a Roma deskmate and in column 3 to students that do not have a Roma deskmate.

Table 2: Balance test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Roma Deskmate (RD)	RD	RD	RD	RD	RD	RD	RD	Roma vignette
Age (in 0.1 years)	0.0012 (0.0011)							0.00029 (0.0012)	-0.00057 (0.0024)
Girl		-0.0012 (0.012)						-0.00054 (0.013)	-0.012 (0.025)
Mathematics			-0.0071 (0.0065)					-0.00056 (0.011)	0.0070 (0.021)
Grammar				-0.0082 (0.0066)				-0.0052 (0.012)	0.0019 (0.022)
Literature					-0.0041 (0.0063)			0.0066 (0.010)	-0.023 (0.020)
Diligence						-0.010 (0.0075)		-0.019 (0.014)	-0.018 (0.028)
Behavior							-0.00051 (0.0084)	0.0093 (0.011)	0.036* (0.022)
Mean dep. var in C group	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.50
No. of observations	2360	2395	2274	2274	2277	2279	2279	2238	2101
R-squared	0.36	0.37	0.35	0.35	0.35	0.35	0.35	0.35	0.08
Class F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions control for class fixed effects. Conducting an F-test of whether the control variables jointly predict treatment status we get an F-value of 1.09 with an associated p-value of 0.2459. In the test we include all the controls as indicator variables and control for class fixed effects. In column 9 the dependent variable is Roma vignette and the F-value of predicting this variable is 0.87 with a p-value of 0.8257. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table 3: Effects of Roma deskmate on having a Roma friend among the best friends and on lending to a classmate

	(1)	(2)	(3)	(4)
	Lend to Classmate	Lend to Classmate	Roma friend	Roma friend
Roma Deskmate	-0.030 (0.053)	-0.027 (0.056)	-0.034 (0.036)	-0.036 (0.037)
Roma vignette	-0.18*** (0.023)	-0.17*** (0.023)		
Roma Deskmate*Roma vignette	-0.034 (0.067)	-0.052 (0.070)		
Mean dep. var in C group	0.67	0.67	0.27	0.27
No. of observations	2102	2102	2124	2124
R-squared	0.22	0.26	0.34	0.38
Class F.E.	Yes	Yes	Yes	Yes
Baseline Controls	No	Yes	No	Yes

*Notes:* All regressions control for class fixed effects. The baseline controls in columns 2 and 4 are indicator variables for gender, age in 0.1 year brackets, baseline grades in mathematics, grammar, literature, diligence, and behavior (all with one dummy for each level of the grade, which range from 1-5 and a dummy for not being graded). Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table 4: Effects of Roma deskmate on other outcomes

	(1)	(2)	(3)	(4)
	Number of Roma friends	Lend to deskmate	Liked sitting next to deskmate	Deskmate among best friends
Roma Deskmate	-0.042 (0.19)	-0.073** (0.034)	-0.085** (0.041)	-0.13*** (0.036)
Mean dep. var in C group	0.71	0.84	0.59	0.31
No. of observations	2124	1989	2028	2170
R-squared	0.37	0.13	0.16	0.14
Class F.E.	Yes	Yes	Yes	Yes

*Notes:* All regressions control for class fixed effects. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table 5: Effects of Roma deskmate in different classrooms

	(1)	(2)	(3)	(4)	(5)	(6)
	Lend	Lend	Lend	Friend	Friend	Friend
Roma Deskmate (RD)	0.019 (0.070)	0.0014 (0.069)	-0.00038 (0.068)	0.0014 (0.046)	-0.035 (0.046)	-0.043 (0.046)
Share Roma in class	0.0091 (0.027)	0.044 (0.039)	0.031 (0.035)	0.22*** (0.017)	0.16*** (0.033)	0.14*** (0.030)
Roma Deskmate*share Roma	-0.026 (0.044)	0.0048 (0.042)	0.016 (0.041)	-0.068** (0.027)	-0.017 (0.027)	-0.0062 (0.027)
Roma vignette (RV)	-0.19*** (0.025)	-0.19*** (0.024)	-0.18*** (0.024)			
Roma Deskmate*Roma vignette	-0.15 (0.096)	-0.13 (0.091)	-0.14 (0.090)			
RV*Share Roma	0.026 (0.029)	0.023 (0.028)	0.024 (0.028)			
RD*RV*Share Roma	0.090 (0.056)	0.055 (0.053)	0.050 (0.052)			
Average class GPA			-0.12*** (0.043)			-0.18*** (0.030)
Mean dep. var in C group	0.26	0.26	0.26	0.27	0.27	0.27
No. of observations	2102	2102	2102	2124	2124	2124
R-squared	0.04	0.08	0.09	0.16	0.21	0.23
School F.E.	No	Yes	Yes	No	Yes	Yes
Average class GPA	No	No	Yes	No	No	Yes

Notes: Robust standard errors clustered at the class level are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.



## APPENDIX: FOR ONLINE PUBLICATION

### A Regression results tables discussed in the text

Table A.1: Effects of Roma deskmate when restricting the sample to classrooms with over 90 percent compliance.

	(1)	(2)
	Roma friend	Lend to Classmate
Roma Deskmate	0.0070 (0.049)	-0.10 (0.074)
Roma vignette		-0.15*** (0.031)
Roma Deskmate*Roma vignette		-0.0034 (0.088)
Mean dep. var in C group	0.23	0.66
No. of observations	1120	1117
R-squared	0.33	0.22
Class F.E.	Yes	Yes
Sample		

*Notes:* All regressions control for class fixed effects. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.2: Effects of Roma deskmate in regressions with controls without including indicator variables for missing values

	(1)	(2)
	Roma friend	Lend to Classmate
Roma Deskmate	-0.019 (0.037)	-0.016 (0.057)
Roma vignette		-0.17*** (0.024)
Roma Deskmate*Roma vignette		-0.043 (0.071)
Mean dep. var in C group	0.26	0.67
No. of observations	2047	2021
R-squared	0.38	0.26
Class F.E.	Yes	Yes
Baseline Controls	No	Yes
Baseline*Treatment	No	No

*Notes:* All regressions control for class fixed effects. The baseline controls are indicator variables for gender, age in 0.1 year brackets, baseline grades in mathematics, grammar, literature, diligence, and behavior (all with one dummy for each level of the grade, which range from 1-5 and a dummy for not being graded). We do not include missing values as indicator variables in these regressions. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.3: Heterogeneous effects of Roma deskmates on Roma friend based on ego baseline characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Friend	Friend	Friend	Friend	Friend	Friend	Friend	Friend
Roma Deskmate (RD)	-0.035 (0.036)	-0.031 (0.048)	-0.024 (0.037)	-0.027 (0.037)	-0.021 (0.037)	-0.016 (0.037)	-0.014 (0.036)	-0.025 (0.037)
Roma Deskmate*Age	0.052 (0.033)							
Roma Deskmate*Girl		-0.0062 (0.061)						
Roma Deskmate*Mathematics			0.013 (0.034)					
Roma Deskmate*Grammar				0.0052 (0.029)				
Roma Deskmate*Literature					0.015 (0.032)			
Roma Deskmate*Diligence						0.042 (0.032)		
Roma Deskmate*Behavior							0.0097 (0.033)	
Roma Deskmate*GPA								0.013 (0.031)
Age	-0.096*** (0.035)							
Girl		-0.046** (0.019)						
Mathematics			-0.057*** (0.011)					
Grammar				-0.055*** (0.011)				
Literature					-0.049*** (0.011)			
Diligence						-0.066*** (0.011)		
Behavior							-0.052*** (0.011)	
GPA								-0.060*** (0.011)
Mean dep. var in C group	0.27	0.27	0.26	0.26	0.26	0.26	0.26	0.26
No. of observations	2124	2124	2046	2047	2048	2049	2049	2048
R-squared	0.34	0.34	0.34	0.34	0.34	0.34	0.34	0.34
Class F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions control for class fixed effects. All continuous variables are standardized to have mean zero and standard deviation 1 for ease of interpretation. Girl is kept as a dummy variable. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.4: Heterogeneous effects of Roma deskmates on Lend to classmate based on ego baseline characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lend	Lend	Lend	Lend	Lend	Lend	Lend	Lend
Roma Deskmate (RD)	-0.027 (0.053)	0.027 (0.070)	-0.032 (0.053)	-0.031 (0.054)	-0.030 (0.053)	-0.029 (0.054)	-0.031 (0.054)	-0.029 (0.054)
Roma Deskmate*Age	0.088* (0.052)							
Roma Deskmate*Girl		-0.13 (0.095)						
Roma Deskmate*Mathematics			-0.026 (0.047)					
Roma Deskmate*Grammar				-0.0022 (0.044)				
Roma Deskmate*Literature					0.070 (0.046)			
Roma Deskmate*Diligence						-0.025 (0.046)		
Roma Deskmate*Behavior							-0.068 (0.052)	
Roma Deskmate*GPA								0.018 (0.046)
Roma vignette (RV)	-0.18*** (0.023)	-0.20*** (0.031)	-0.18*** (0.023)	-0.18*** (0.023)	-0.18*** (0.023)	-0.18*** (0.023)	-0.18*** (0.023)	-0.18*** (0.023)
Roma Deskmate*Roma vignette	-0.032 (0.067)	-0.071 (0.089)	-0.027 (0.069)	-0.026 (0.068)	-0.024 (0.069)	-0.017 (0.069)	-0.013 (0.069)	-0.027 (0.069)
RV*Age	0.073*** (0.023)							
RV*Girl		0.042 (0.044)						
RV*Mathematics			0.025 (0.023)					
RV*Grammar				0.011 (0.024)				
RV*Literature					0.041* (0.023)			
RV*Diligence						0.016 (0.024)		
RV*Behavior							0.00095 (0.024)	
RV*GPA								0.030 (0.024)
RD*RV*Age	-0.078 (0.069)							
RD*RV*Girl		0.093 (0.13)						
RD*RV*Mathematics			-0.049 (0.065)					
RD*RV*Grammar				-0.059 (0.059)				
RD*RV*Literature					-0.12** (0.061)			
RD*RV*Diligence						0.0067 (0.063)		
RD*RV*Behavior							0.072 (0.067)	
RD*RV*GPA								-0.086 (0.062)
Age	-0.093** (0.038)							
Girl		-0.0050 (0.031)						
Mathematics			-0.00048 (0.017)					
Grammar				0.014 (0.017)				
Literature					-0.015 (0.017)			
Diligence						0.0082 (0.017)		
Behavior							0.0000035 (0.017)	
GPA								-0.00082 (0.017)
Mean dep. var in C group	0.26	0.26	0.26	0.26	0.26	0.26	0.26	0.26
No. of observations	2102	2102	2020	2022	2024	2025	2025	2024
R-squared	0.22	0.22	0.22	0.22	0.22	0.22	0.22	0.22
Class F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions control for class fixed effects. All continuous variables are standardized to have mean zero and standard deviation 1 for ease of interpretation. Girl is kept as a dummy variable. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.5: Heterogeneous effects of Roma deskmates on Roma friend based on deskmate baseline characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Friend	Friend	Friend	Friend	Friend	Friend	Friend	Friend
Roma Deskmate (RD)	-0.035 (0.036)	-0.043 (0.047)	-0.028 (0.050)	-0.052 (0.048)	-0.014 (0.047)	0.00062 (0.048)	-0.014 (0.045)	-0.030 (0.051)
Roma Deskmate*Age	-0.051 (0.034)							
Roma Deskmate*Girl		0.019 (0.059)						
Roma Deskmate*Mathematics			0.0088 (0.036)					
Roma Deskmate*Grammar				-0.028 (0.035)				
Roma Deskmate*Literature					0.022 (0.034)			
Roma Deskmate*Diligence						0.043 (0.033)		
Roma Deskmate*Behavior							0.028 (0.031)	
Roma Deskmate*GPA								-0.0025 (0.037)
Age	0.095*** (0.034)							
Girl		-0.017 (0.018)						
Mathematics			0.0071 (0.011)					
Grammar				0.0071 (0.011)				
Literature					-0.0010 (0.012)			
Diligence						-0.0045 (0.011)		
Behavior							-0.0016 (0.011)	
GPA								0.0057 (0.012)
Mean dep. var in C group	0.27	0.27	0.26	0.26	0.26	0.26	0.26	0.26
No. of observations	2124	2124	2015	2015	2019	2020	2020	2020
R-squared	0.34	0.34	0.34	0.34	0.34	0.34	0.34	0.34
Class F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Notes:* All regressions control for class fixed effects. All continuous variables are standardized to have mean zero and standard deviation 1 for ease of interpretation. Girl is kept as a dummy variable. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.6: Heterogeneous effects of Roma deskmates on Lend to classmate based on deskmate baseline characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lend	Lend	Lend	Lend	Lend	Lend	Lend	Lend
Roma Deskmate (RD)	-0.027 (0.053)	-0.099 (0.074)	0.0065 (0.064)	-0.015 (0.069)	0.016 (0.073)	0.0096 (0.068)	-0.0082 (0.064)	0.016 (0.071)
Roma Deskmate*Age	-0.087* (0.052)							
Roma Deskmate*Girl		0.14 (0.093)						
Roma Deskmate*Mathematics			0.021 (0.057)					
Roma Deskmate*Grammar				0.0013 (0.055)				
Roma Deskmate*Literature					0.059 (0.054)			
Roma Deskmate*Diligence						0.030 (0.051)		
Roma Deskmate*Behavior							0.0086 (0.046)	
Roma Deskmate*GPA								0.038 (0.056)
Roma vignette (RV)	-0.18*** (0.023)	-0.17*** (0.031)	-0.18*** (0.023)	-0.18*** (0.023)	-0.18*** (0.023)	-0.18*** (0.023)	-0.18*** (0.023)	-0.18*** (0.023)
Roma Deskmate*Roma vignette	-0.032 (0.067)	-0.011 (0.093)	0.029 (0.091)	0.051 (0.096)	0.016 (0.093)	0.035 (0.088)	-0.0056 (0.082)	0.041 (0.098)
RV*Age	-0.073*** (0.023)							
RV*Girl		-0.011 (0.045)						
RV*Mathematics			-0.017 (0.024)					
RV*Grammar				-0.035 (0.024)				
RV*Literature					0.017 (0.024)			
RV*Diligence						-0.0070 (0.025)		
RV*Behavior							-0.031 (0.025)	
RV*GPA								-0.013 (0.025)
RD*RV*Age	0.077 (0.069)							
RD*RV*Girl		-0.045 (0.13)						
RD*RV*Mathematics			0.044 (0.077)					
RD*RV*Grammar				0.084 (0.073)				
RD*RV*Literature					0.012 (0.070)			
RD*RV*Diligence						0.058 (0.067)		
RD*RV*Behavior							0.037 (0.062)	
RD*RV*GPA								0.055 (0.076)
Age	0.092** (0.038)							
Girl		-0.023 (0.031)						
Mathematics			0.019 (0.017)					
Grammar				0.017 (0.017)				
Literature					-0.014 (0.017)			
Diligence						0.0087 (0.017)		
Behavior							0.014 (0.017)	
GPA								0.0073 (0.018)
Mean dep. var in C group	0.26	0.26	0.26	0.26	0.26	0.26	0.26	0.26
No. of observations	2102	2102	1990	1990	1994	1996	1996	1995
R-squared	0.22	0.22	0.22	0.22	0.22	0.22	0.22	0.22
Class F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions control for class fixed effects. All continuous variables are standardized to have mean zero and standard deviation 1 for ease of interpretation. Girl is kept as a dummy variable. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.7: Effects of Roma deskmate depending on sex concordance of deskmate

	(1)	(2)	(3)	(4)
	Roma friend	Roma friend	Lend to Classmate	Lend to Classmate
Roma Deskmate	-0.043 (0.068)	-0.063 (0.049)	-0.24*** (0.087)	0.100 (0.076)
Roma vignette			-0.19*** (0.033)	-0.19*** (0.034)
Roma Deskmate*Roma vignette			0.14 (0.11)	-0.18* (0.094)
Mean dep. var in C group	0.27	0.26	0.68	0.66
No. of observations	1056	1068	1045	1057
R-squared	0.40	0.40	0.30	0.28
Class F.E.	Yes	Yes	Yes	Yes
Deskmate sex	Same sex	Different sex	Same sex	Different sex

*Notes:* All regressions control for class fixed effects. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.8: Effects of Roma deskmate in majority Roma and non Roma classrooms.

	(1)	(2)	(3)	(4)
	Roma friend	Roma friend	Lend to Classmate	Lend to Classmate
Roma Deskmate	-0.093 (0.11)	-0.028 (0.039)	0.036 (0.15)	-0.00079 (0.057)
Roma vignette			0.091 (0.15)	-0.18*** (0.023)
Roma Deskmate*Roma vignette			-0.030 (0.21)	-0.11 (0.074)
Mean dep. var in C group	0.86	0.25	0.55	0.67
No. of observations	103	2021	113	1989
R-squared	0.39	0.29	0.44	0.21
Class F.E.	Yes	Yes	Yes	Yes
Sample	Majority Roma	Majority non-Roma	Majority Roma	Majority non-Roma

*Notes:* All regressions control for class fixed effects. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.9: Effects of Roma deskmate in majority Roma and non Roma classrooms.

	(1)	(2)	(3)	(4)
	Lend	Lend	Friend	Friend
Roma Deskmate (RD)	0.018 (0.058)	0.019 (0.057)	0.17*** (0.047)	0.027 (0.046)
Roma vignette (RV)	-0.19*** (0.027)	-0.19*** (0.026)		
Roma Deskmate*Roma vignette	-0.089 (0.084)	-0.094 (0.082)		
Majority Roma class	-0.12 (0.13)	-0.13 (0.13)	0.60*** (0.053)	0.22*** (0.078)
Roma Deskmate*Majority Roma	0.037 (0.16)	0.12 (0.15)	-0.23*** (0.083)	-0.058 (0.086)
RV*Majority Roma	0.20 (0.13)	0.17 (0.12)		
RD*RV*Majority Roma	0.18 (0.21)	0.11 (0.20)		
Mean dep. var in C group	0.26	0.26	0.27	0.27
No. of observations	2102	2102	2124	2124
R-squared	0.04	0.08	0.08	0.19
School F.E.	No	Yes	No	Yes
Average class GPA	No	No	No	No

*Notes:* All regressions control for class fixed effects. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.10: Effects of Roma deskmate on lending to Roma interacted with lending to deskmate

	(1) Lend to Roma
Roma Deskmate	-0.12 (0.079)
Lend to Deskmate	0.31*** (0.045)
Roma Deskmate*Lend to Deskmate	0.087 (0.096)
Mean dep. var in C group	0.48
No. of observations	978
R-squared	0.33
Class F.E.	Yes

*Notes:* All regressions control for class fixed effects. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.11: Testing the constrict hypothesis: Effects of Roma deskmate on lending to a non-Roma for a sample of majority non-Roma students

	(1) Lend to non-Roma
Roma Deskmate	-0.0088 (0.061)
Mean dep. var in C group	0.67
No. of observations	996
R-squared	0.24
Class F.E.	Yes
Sample	Majority non-Roma

*Notes:* All regressions control for class fixed effects. Robust standard errors are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

Table A.12: Effects of Roma deskmate in different grades

	(1) Lend	(2) Lend	(3) Lend	(4) Friend	(5) Friend	(6) Friend
Roma Deskmate (RD)	0.024 (0.070)	0.0096 (0.070)	0.0058 (0.069)	0.027 (0.046)	-0.0044 (0.045)	-0.012 (0.045)
Share Roma in grade	0.014 (0.025)	0.061 (0.040)	0.046 (0.035)	0.21*** (0.019)	0.14*** (0.037)	0.12*** (0.033)
Roma Deskmate*share Roma	-0.037 (0.043)	-0.0050 (0.042)	0.0078 (0.041)	-0.070** (0.031)	-0.026 (0.030)	-0.014 (0.030)
Roma vignette (RV)	-0.19*** (0.025)	-0.19*** (0.023)	-0.18*** (0.024)			
Roma Deskmate*Roma vignette	-0.14 (0.096)	-0.13 (0.090)	-0.14 (0.090)			
RV*Share Roma	0.022 (0.028)	0.019 (0.026)	0.022 (0.026)			
RD*RV*Share Roma	0.094* (0.054)	0.058 (0.052)	0.051 (0.051)			
Average grade GPA			-0.12*** (0.044)			-0.18*** (0.031)
Mean dep. var in C group	0.26	0.26	0.26	0.27	0.27	0.27
No. of observations	2102	2102	2102	2124	2124	2124
R-squared	0.04	0.08	0.09	0.15	0.21	0.22
School F.E.	No	Yes	Yes	No	Yes	Yes
Average grade GPA	No	No	Yes	No	No	Yes

*Notes:* Robust standard errors clustered at the grade level are presented in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5% and 1% level, respectively.

## B Details about the data collection

We recruited schools from 7 contiguous counties of central Hungary (excluding the capital city Budapest). In the spring of 2017, we contacted all primary schools in these counties via the heads of the local school districts to elicit information about room layouts and seating practices. By the end of the summer vacation, we had obtained initial participation agreements with 55 schools in which most 3rd-8th grade classrooms were anticipated to meet a set of inclusion criteria. These criteria were: 1) Principals and teachers would implement our randomized seating chart in three subjects: Hungarian literature, Hungarian grammar, and mathematics. 2) All students in a classroom would receive instruction in these subjects together (e.g., no ability grouping). 3) Classroom layout would comprise free-standing desks that seat two students. In these classrooms, the students were expected to sit next to their deskmates for around 20 hours each week.

The intervention assigned students to free-standing two-person desks via unconstrained random partitioning within each classroom. We based the randomization on the class rosters from the preceding spring semester. Shortly before the start of the fall semester, we submitted the randomized seating charts to teachers for implementation. Teachers were instructed to use the charts for the duration of the fall semester, until January 2018. To account for changes to class rosters during the summer via exits and entries, we instructed teachers to fill seats vacated by exiting students with entering students from left to right, front to back, in alphabetic order of entering students' surnames. Since, (i) in expectation, students enter and exit classrooms for the same reason (repeating grades and residential moves); and (ii) student surnames are reasonably orthogonal to student grades, this replacement rule preserves randomization. We permitted teachers to reseat students where necessary, but we asked teachers to preserve the desk-mate composition by moving both deskmates of a desk together whenever possible. We measured compliance through teacher reports of the actual



seating chart for September 15, 2017. The field team again recorded the actual seating chart during school visits between October and December 2017 and verified classroom layouts through classroom photographs.

Schools and classrooms that did not meet our pre-registered inclusion criteria were dropped from the study. We excluded 133 entire classrooms for the following reasons: Withdrawal from the study (25); Less than 10 students at baseline (8); split (e.g. ability-grouped) classrooms (10); desk layouts other than free-standing two-person desks (30); Unreliable baseline reporting (7); Failure to implement the seating chart (40); Failure to report student ethnicity (13). We also pre-registered to exclude students who were randomly assigned to sit alone at a desk at baseline or who had missing outcomes.

## C Anonymized Pre-Analysis plan and Survey

# 1 Introduction

The Roma is one of the largest and poorest ethnic minority groups in Europe. In Hungary, the Roma population is estimated to constitute around 6 percent of the total population and 10 to 12 percent of the young adolescent population (Kertesi and Kézdi, 2011b). The Roma lag behind the general population in terms of health (Hajdu, Kertesi, and Kezdi, 2017a), education (Kertesi and Kézdi, 2011b), and employment (Kertesi and Kézdi, 2011a) and prejudice against the Roma is widespread (Hajdu, Kertesi, and Kezdi, 2017b). Using a large scale field experiment where we randomly assign desk mates in Hungarian schools we investigate whether close personal contact to Roma increases inter-ethnic friendship and trust.

Whether exposure reduces prejudice is an important question and previous evidence is mixed. Several empirical studies find patterns that shallow exposure is correlated with more prejudice and less trust (Alesina and La Ferrara, 2002; Delhey and Newton, 2005; Dinesen and Sønderskov, 2015; Stolle, Soroka, and Johnston, 2008). Putnam, (2007) has even proposed a constrict theory, arguing that ethnic diversity may not only lead to less trust between the majority and minority groups, it may also undermine trust within the majority group. A major limitation of these studies is the inability to control for selection issues and reverse causality (Baldassarri and Abascal, 2017). There are studies of close personal contact arguing that contact under some conditions reduce prejudice and increases trust (Allport, 1954) and well identified studies using random assignment of peers have found such effects (Boisjoly et al., 2006; Burns, Corno, and La Ferrara, 2016; Carrell, Hoekstra, and West, 2015; Finseraas and Kotsadam, 2017a; Finseraas et al., 2016). Kende, Tropp, and Lantos, (2017) randomly assign 61 non-Roma Hungarians to face-to-face interaction with a Roma person and found reduced prejudice for those exposed. The key condition for exposure to reduce prejudice has been argued to be friendship potential (Laurence, 2009; Pettigrew,

1998; Stolle, Soroka, and Johnston, 2008).

Similar people are more likely to form social ties (McPherson, Smith-Lovin, and Cook, 2001). This phenomenon, often described as *social homophily*, is consistent with a general preference for similarity and has been documented within several fields of science (Byrne, 1961, 1971). The tendency of lower probability of friendships across ethnic groups, inbreeding homophily, has been widely documented (see e.g. Jackson, (2014) and McPherson, Smith-Lovin, and Cook, (2001), also in Hungary with respect to Roma (Hajdu, Kertesi, and Kezdi, 2017b).

Homophily generally comes in two distinct forms that are hard to disentangle: Choice homophily and induced homophily (McPherson, Smith-Lovin, and Cook, 2001). The former arises as a function of individual preferences for similarity while the latter is purely a function of the opportunities people have to come into contact with each other. Exposure leads to greater opportunities for choice homophily but the degree to which exposure is causing friendship is uncertain. In previous studies it seems as if the level of analysis of the exposure is crucial and neighborhoods do not seem to be close enough, and may even lead to increased animosity, dorm rooms and army teams teams seem to be close and repetitive enough. It is an open question whether classrooms and desk mates fall in the positive contact realm or the negative conflict realm.

## **2 The field experiment and sample**

We execute a large-scale randomized field experiment in 182 classrooms of 38 Hungarian primary schools (after exclusions) containing 3539 students. The intervention consists of randomizing the seating chart within each classroom at the beginning of the fall semester, 2017, and encouraging adherence until the end of the semester in January 2018. Endline outcomes data are collected and will become available to the research team in May 2018.

In the spring of 2017, we contacted all primary schools in 7 contiguous counties of central

Hungary via the heads of the local school districts to elicit information about room layouts and seating practices. By the end of the summer vacation, we obtained initial participation agreements with 55 schools in which most 3rd-8th grade classrooms were anticipated to meet a set of inclusion criteria. These criteria were: 1) Principals and teachers would implement our randomized seating chart in three subjects: Hungarian literature, Hungarian grammar, and mathematics. 2) All students in a classroom would receive instruction in these subjects together (e.g., no ability grouping). 3) Classroom layout would comprise free-standing desks that seat two students.

The intervention assigned students to free-standing two-person desks via unconstrained random partitioning within each classroom. We based the randomization on the class rosters from the spring semester. Shortly before the start of the fall semester, we submitted the randomized seating charts to teachers and teachers were instructed to use the charts for the duration of the fall semester until January 2018. To account for changes to class rosters during the summer via exits and entries, we instructed teachers to fill seats vacated by exiting students with entering students from left to right, front to back, in alphabetic order of entering students' surnames. Since, (i) in expectation, students enter and exit classrooms for the same reason (repeating grades and residential moves); and (ii) student surnames are reasonably orthogonal to student grades, this replacement rule preserves randomization.

While teachers were expressly permitted to reseat students if they have to, we asked to preserve the desk-mate composition wherever possible. We measured compliance through teacher reports of the actual seating chart for September 15, 2017. The field team again recorded the actual seating chart during school visits between October and December 2017 and verified classroom layouts through classroom photographs.

Schools and classrooms that do not meet our conditions are dropped from the study. To date, we have dropped 133 classrooms for the following reasons: Withdrawal from the study (25); Less than 10 students at baseline (8); split classrooms (10); Not free-standing desks

that seat two students (30); Unreliable baseline reporting (7); Did not implement the seating chart (40); Does not include information on Roma ethnicity (13). Based on these school- and classroom-level exclusions, we anticipate an analysis sample of 3539 students across 182 classrooms of 38 schools.

*Decision rules for dropping future observations:* We will exclude students who are randomly assigned to sit alone at a desk at baseline and who have missing values on our outcomes.

*Decision rules for dropping variables:* If 95 percent or more of the sample answers the same value on a variable we define this as limited variation. We will drop variables with limited variation from the analysis.

*Missing values:* If we have missing values on variables we will code the variables as zero and include dummy variables controlling for missing status so that we do not lose observations. If more than 30 percent of the respondents do not answer a particular question, it will no longer be seen as a main outcome variable.

### **3 Data and coding of main variables**

We collect baseline variables via teacher reports. Outcome variables are collected via a student survey at endline. In particular, we field a 45-minute two-part in-class survey (see appendix). The first part (20 minutes) consists of a student questionnaire that elicits self-reported grades for the spring and fall semester 2017, academic self-concept, and several attitudinal measures. The second part of the endline survey consists of a reading comprehension test that is not used in this paper. Since the endline questionnaire contains a survey experiment with two vignettes, we randomly sort questionnaires, using a random number generator. Data collection will conclude in April of 2018. The research team will receive outcomes data in May, 2018.

*Treatment variables:* We define our (exposure) treatment variable, *Treatment* as equal

to 1 if a person is assigned a desk mate that is Roma and zero otherwise. We also have a treatment variable in the survey experiment that we call *Roma vignette*, which is equal to one if the vignette in question 9b includes the bold text saying that the classmate to lend money is Roma (see below).

*Primary and secondary outcome variables:* We have 2 primary outcome variables: *Roma friend* and *Lend to Classmate*.

*Roma friend* captures whether an individual has a Roma friend among his or her best friends. The variable is from survey question 5d. Survey question 5 prompts: "Now in general think of your best friends, not just in the class but EVERYWHERE.", and option d is "Among your best friends, how many are Roma (gypsy)?". We code the variable as 1 if the individual has at least one Roma friend and zero otherwise.

The variable *Lend to Classmate* is based on a survey experiment where students were presented with a scenario where they could lend money to a classmate, survey question 9b. The survey question 9b builds on question 9a, which reads: "Imagine that you are going to the zoo with some of your classmates. Your desk mate (who you sat next to in Hungarian class in December) has forgotten to bring money for the entrance ticket. You have enough money for two entrance tickets. Would you lend your desk partner the money for the entrance ticket?". Question 9b then reads: "Now imagine that it is not your desk mate, but a different classmate who has forgotten to bring money with him/her. **This classmate is a Roma/Gypsy.** Would you lend this **Roma/Gypsy** classmate the money for the entrance ticket?" The bold text is only presented to a random half of the students. The answer categories are Yes, No, I do not know. We will recode the variable to be 1 for Yes and zero otherwise.

We have several secondary outcome variables. These variables will not necessarily be analyzed as extensively nor by themselves be seen as confirmatory. Of special interest among these are the variables *Lend to Roma* and *Lend to non-Roma*. These variables take the

same values as the main variable, *Lend to Classmate*, but they are only defined for different samples. *Lend to Roma* is only defined for individuals receiving the bold text in the vignette and *Lend to non-Roma* is only defined when the vignette excludes the bold text. For all classrooms that are majority non-Roma, a random classmate will be more likely to be someone from the in-group for non-Roma respondents. Hence, if we restrict the sample to majority non-Roma, the variables can be used to test whether close exposure to a Roma desk mate affects both in out-group and in-group trust.

In addition to investigating the probability of having a Roma friend we will also investigate effects on the *Number of Roma friends*, which just counts the number of Roma friends in question 5d. We expect that we will get similar results with both variables.

*Control variables:* We only include control variables that are collected at baseline or stable over time. The variables we include are age (in 0.1 years), gender and spring 2017 grades in five core subjects (Hungarian literature, Hungarian grammar, mathematics, diligence, and behavior). These variables are obtained from the classroom teacher.

*Other variables:* There are a set of questions that will be used for supplementary analyses. Survey question 9a will be used to create a variable, *Lend to Desk mate*. We will also create other variables such as *Desk mate among best friends* (to see if desk mate relations in general are characterized by friendship potential) and *Liked sitting next to desk mate*.

*Heterogeneity:* The possibilities for heterogeneous treatment effects are endless. Both characteristics of the exposed and the exposer are likely to matter. It is likely that people that are similar to each other in other aspects have a higher likelihood of transmitting or changing attitudes of the desk mate. With so many options, the heterogeneity analysis will necessarily be seen as explorative. We here outline some of the aspects we will explore. We will interact students' own baseline GPA with Treatment in a model including GPA as well. We will control for and interact a variable for whether the desk mates are of the same sex or of different sex. At the contextual level there are also many possible moderators and we



will investigate the moderating role of *Share of Roma in class*.

## 4 Empirical strategy

Identifying peer effects is difficult as people self-select into networks and since outcomes are affected by correlated effects (Manski, 1993). With random variation in peer contact we get around most of the challenges associated with identifying network effects. In order to focus on the effect of exposure to a stigmatized minority group on the attitudes and preferences of the majority population, we exclude the Roma students themselves from the regressions.

We first estimate the following regression to identify the treatment effect on the probability to have a Roma friend:

$$(1) \text{Roma friend}_{ict2} = \beta \text{Treated}_{ict1} + \alpha \text{Class}_{ct1} + \gamma X_{ict1} + \epsilon_{ict2},$$

where  $i$  indexes individuals,  $c$  classes, and  $t$  is time (either baseline 1 or follow up 2).  $\text{Treated}_{ict1}$  is a dummy equal to 1 if this person is assigned a Roma desk mate,  $X_{ict1}$  is a set of individual level control variables either measured at baseline or reflecting stable characteristics (described in section 3), and the error term,  $\epsilon_{ict2}$ . We will present results with and without the baseline controls but the main specification is without controls. We use robust standard errors in all estimations. The standard errors do not need to be clustered at any level as the randomization is at the individual level (see Abadie et al., (2017)). The class fixed effects are included as the randomization was conducted within classes.

The vector of individual level control variables is included as they may increase power. To make the models fully saturated, we partition the covariate space and add these control variables as indicator variables rather than using their multi-valued codings and we also interact them with treatment (Athey and Imbens, 2017). We create an indicator for missing values in the controls and include the missing indicator in the regressions in order not to lose observations.

The same specification is also run for *Number of Roma friends* as the outcome variable.

For our second main outcome variable we estimate the following regression:

$$(2) \text{Lend to Classmate}_{ict2} = \beta \text{Treated}_{ict1} + \theta \text{Roma vignette}_{ict2} + \delta \text{Treated}_{ict1} * \text{Roma vignette}_{ict2} + \alpha \text{Class}_{ct1} + \gamma X_{ict1} + \epsilon_{ict2},$$

where we add the variables *Roma vignette*, which equals one if the bold text in the vignette is included, and the interaction between *Roma vignette* and *Treatment*.

We also run the same specification without the interaction term and without *Roma vignette* separately for *Lend to Roma* and *Lend to non-Roma* and for a sample restricted to non-Roma majority. In the analysis of lending to Roma we will also investigate whether it makes a difference whether or not the person lent to his or her desk mate.

To explore heterogeneity we will first interact the treatment variable with the baseline control variables (Gender and baseline grades). We will also test whether the effect is different in classes with relatively many and relatively few Roma by interacting treatment with *Share of Roma in class*. The standard errors will then be clustered at the class level.

We will also use machine learning techniques to automate the search for heterogenous treatment effects. There are many different types of machine learning algorithms and we have decided to use classification and regression trees (R package *causalTree*, (Athey and Imbens, 2016)); and random forests (R package *grf*, (Wager and Athey, 2017)). As this field is moving rapidly, however, it is possible that there will be other techniques that are relevant for us once we start analyzing the data.

*Balance tests:* To test for balance we will regress our main treatment variable on the control variables described above both individually and together, while controlling for class fixed effects. We will judge whether the randomization worked by conducting an F-test of whether the control variables jointly predict treatment status.

## 5 Hypotheses

In the literature on interethnic exposure there are, broadly speaking, two perspectives on the effects of diversity. One perspective argues that diversity leads to negative outcomes. Several empirical studies find patterns that are consistent with what is denoted conflict theory; diversity is associated with e.g. less trust (Alesina and La Ferrara, 2002; Delhey and Newton, 2005; Dinesen and Sønderskov, 2015; Stolle, Soroka, and Johnston, 2008). The other perspective is more positive. Contact theory (Allport, 1954) suggests that personal contact with members of out-groups can reduce prejudice and misperceptions, and thereby increase trust. There is ample evidence from well identified studies using random assignment, either of students (e.g. Boisjoly et al., 2006; Burns, Corno, and La Ferrara, 2016) or within the military (Carrell, Hoekstra, and West, 2015; Finseraas and Kotsadam, 2017b; Finseraas et al., 2016), showing that personal contact reduces prejudice and strengthens cooperation (Goette, Huffman, and Meier, 2006).

According to contact theory, the positive effects of personal contact are expected to apply when certain criteria are met (Allport, 1954). The contact should take place in a context with equal status, shared common goals, be cooperative, and take place under some form of authority (Pettigrew, 1998). Finally, the setting should have friendship potential, which increases the probability of affective ties and willingness to learn about out-group members (Van Laar et al., 2005). In fact, several authors argue that friendship potential is the most essential condition (Laurence, 2009; Pettigrew, 1998; Stolle, Soroka, and Johnston, 2008). Contact theory has received support in several field experiments with randomly assigned contact (e.g. Boisjoly et al., (2006), Burns, Corno, and La Ferrara, (2016), Carrell, Hoekstra, and West, (2015), Finseraas and Kotsadam, (2017b), and Finseraas et al., (2016)) but most of the evidence is based on correlational patterns (see Brown and Hewstone, (2005) and Pettigrew et al., (2011), and Paluck, Green, and Green, (2017) for reviews).

*Our first main hypothesis is that being randomly assigned to a Roma desk mate increases the probability of having a Roma as one of the best friends.* The reasons for this are that induced homophily is larger and that contact theory is likely to operate at this very personal level. The hypothesis will be seen as confirmed if  $\beta$  is positive and statistically significant in equation 1.

*Our second main hypothesis is that being randomly assigned to a Roma desk mate increases the relative probability of wanting to lend money to a Roma classmate rather than a random classmate.* The reasons for this is that contact theory is likely to operate at this level and that we expect friendship ties (as in the hypothesis above). The hypothesis will be seen as confirmed if  $\delta$  is positive and statistically significant in equation 2.

The second hypothesis is thereby tested by a difference in difference model. The estimate gives us the differential effect of contact on sending money to a classmate when being given the Roma vignette. The coefficients for *Treatment* and for *Roma vignette* will also be interesting to investigate. In particular in classes that are majority non-Roma, as the coefficient for *Treatment* will then show if having a Roma desk mate affects in group trust. Following constrict theory, exposure to ethnic diversity will lead to lower trust towards the in-group as well. However, trust may increase also to the in-group by being exposed to people that were mistakenly thought of as less trustworthy before contact (see Finseraas et al. 2018 for a similar reasoning). The coefficient for *Roma vignette* will tell us the difference in willingness to lend to a Roma classmate for individuals not exposed to a Roma desk mate. As outlined in the empirical strategy, we will also investigate these aspects by running separate regressions of *Lend to Roma* and *Lend to non-Roma* on *Treatment* for a sample of non-Roma majority.

As conflict and contact theories envisages different types of interactions between the majority and minority individuals, they may both be correct at the same time. Many contributions highlight this fact (e.g. Abascal and Baldassarri, (2015), Dinesen and Sønderskov, (2015), and Valdez, (2014)) and already Allport (1954) argued that shallow exposure may

increase rather than decrease antipathy towards minorities. Furthermore, a series of contributions argue that contact may diminish or even reverse the negative effects of exposure (Laurence, 2009; McLaren, 2003; Schneider, 2008; Stolle, Soroka, and Johnston, 2008; Uslander, 2012). The argument is that the threatening aspects of exposure are mitigated by contact or that social interactions changes the very conception of whom is considered to be in the in-group (McLaren, 2003; Stolle, Soroka, and Johnston, 2008). The empirical evidence for these claims is exclusively based on correlations whereby individuals self-select into having contact with or being friends with minorities.

We use our data to contrast and combine the conflict and contact perspective on ethnic diversity, by studying treatment heterogeneity according to previous exposure to diversity. We do not have random variation in the exposure to Roma at other levels of analyses but we will explore whether close personal exposure has a different impact in classes with more or less Roma. As this analysis is explorative we remain agnostic as to the direction of the heterogeneity in the effect. We also expect that there may be heterogeneity in the effects based on whom is exposed and based on qualities of the Roma child the person is exposed to. We will investigate this exploratively and we think that grades and gender may be important moderators. In an observational study, Hajdu, Kertesi, and Kezdi, (2017b) find that academically high achieving Roma students have more interethnic friendships. Other heterogeneity analyses are outlined in section 3.

## **6 Power calculation**

In testing our different hypotheses we are restricting the sample to non-Roma individuals. For the test of the difference in difference model we will furthermore base the power calculation on half of the sample as only a random half is assigned the Roma version of the vignette.

We also adjust the p-values for the fact that we are testing two hypotheses. We follow the

recommendations of Fink, McConnell, and Vollmer, (2014) and use a method developed by Benjamini and Hochberg, (1995) and Benjamini and Yekutieli, (2001) to minimize the false non-discovery rate (see also Almeida, (2012) and Finseraas and Kotsadam, (2017b) for pre-analysis plans with the same decision rules for correction of p-values). The main advantage of the method is that it is limiting the risk of false discoveries while only adjusting the critical values based on other true hypotheses. The false discovery rate method developed by Benjamini and Hochberg (1995) implies that the  $m$  p-values of the  $i$  hypotheses are ordered from low to high and that the critical value of the p-value is then  $p(i) = \alpha \cdot i/m$ . In our case, with 2 hypotheses and a significance level ( $\alpha$ ) of 0.05, the critical p-value would be 0.025 for the one with the lowest p-value ( $0.05 \cdot 1/2$ , which is the same as a Bonferroni correction). For the second hypothesis, the critical p-value is 0.05 ( $0.05 \cdot 2/2$ ).

Conservatively, we expect to have a sample of at least 2000 non-Roma individuals in our samples. We calculate power using the program optimal design and if we use the most conservative p-value of 0.025 we have a minimum detectable effect (MDE) of 0.14 for the Roma friend hypothesis. For the second hypothesis, wanting to lend money to a Roma classmate, we only have half as many people in each cell since it is based on an interaction term. Our calculated MDE for this hypothesis with the most conservative p-value of 0.025 is 0.2. We therefore think that our study is well powered to detect relatively small effects.

## **7 IRB approval and consent**

This study was reviewed and approved by the IRB offices at the Hungarian Academy of Science (data collection and analysis); and at the University of Wisconsin-Madison (data analysis). We obtained consent at multiple points. First, we asked school administrators and teachers to consent to participate in the study. Second, we had the teachers ask the parents to consent to data collection about their children.

## 8 Archive

The pre-analysis plan is archived before any endline data is received. We archive it at the registry for randomized controlled trials in economics held by The American Economic Association: <https://www.socialscienceregistry.org/> on March 22 2018. We will receive the endline data in May 2018.

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Write your name! Do not use your nickname!

Family Name	Christian/Given Name(s) (write all your given Names)

Which Grade/Class are you in? (e.g: 3/a)

	/	
--	---	--

When were you born?

Year: 

--	--	--	--

 Month: 

--	--

 Day: 

--	--



3<sup>rd</sup> Year

MTA Social Sciences Research

„Lendület” RECENS Research Group

„Lendület” RECENS Kutatócsoport

# DESK PARTNER RESEARCH

2018.



**YOUR TEACHER will complete these tables.**

School's name or official stamp

.....

Student ID Number (see separate sheet)

--	--	--	--	--	--	--	--

Test date

Month: 

--	--

 Day: 

--	--

Test Start Time

Hour: 

--	--

 Min.: 

--	--

Test End Time

Hour: 

--	--

 Min.: 

--	--



Comprehension

A

The research is supported by the National Research, Development, and Innovation Office in the framework of the Youth Research (FK) 125358 competition.



## General Information about the Exercises

Please read the following information carefully, and then start answering the questions in the notebook!

The test notebook consists of two parts.

In Part 1, we ask questions about you, or rather we are interested in your opinions. Here it is important for us that we get to know what you think.

In the test notebook's second part you will find comprehension exercises. Please read the assignments carefully, and answer the questions to the best of your knowledge!

Start doing the exercises from the beginning of the notebook! (i.e. start at the beginning?)

Always indicate your answer to the question by shading the corresponding circle. As shown in the image below.



Please make sure that you **only mark one answer for each question!**

If you have already marked an answer, but then change your mind, clearly cross out the first mark or put an X over it, and then shade in the answer you think is correct in the way shown below!



**Good luck (with the work)!**



Part 1  
**STUDENT**  
**QUESTIONNAIRE**







**1. What grades did you receive at the END OF LAST SEMESTER in the following? Think of the report card you received this January.**

Mark the appropriate number in each row! Only shade in one circle!

	1	2	3	4	5	I don't remember
a) Behaviour	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
b) Diligence	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
c) Hungarian Language	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
d) Literature	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
e) Mathematics	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>

**2. What grades did you get at the END OF LAST SCHOOL YEAR in the following? Think of the report card you received last summer in June.**

Mark the appropriate number in each row! Only shade in one circle!

	1	2	3	4	5	I don't remember
a) Behaviour	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
b) Diligence	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
c) Hungarian Language	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
d) Literature	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
e) Mathematics	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>

**3. How much do you like the following subjects?**

Mark the appropriate number in each row! Only shade in one circle!

	Really Don't like	Don't Like	Neutral	Like	Really Like	Don't know
a) Hungarian Language	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
b) Literature	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>
c) Mathematics	<input type="radio"/> <sub>1</sub>	<input type="radio"/> <sub>2</sub>	<input type="radio"/> <sub>3</sub>	<input type="radio"/> <sub>4</sub>	<input type="radio"/> <sub>5</sub>	<input type="radio"/> <sub>6</sub>



# 4 Szövegértés-3.évfolyam



**4. Please think of your best friends in your class. In the table below, write down who your 5 best friends are in the class**

*If you have fewer than 5 friends in your class, then write fewer names in the table. Be sure to write your friends' full names into the table, in other words both their family names and their Christian/given names. Do not use your friends' nicknames! Ask for your teacher's help if you don't know your friends' family names!*

	Family Name	Christian/given name (write in all Christian/given names, do not use nicknames!)
1.		
2.		
3.		
4.		
5.		

**5. Now in general think of your best friends, not just in the class but EVERYWHERE.**

*Write in the appropriate number in each row of the table!*

	Please write in the appropriate number to the question!
a) In total how many best friends do you have?	
b) Among your best friends, how many are boys?	
c) Among your best friends, how many are girls?	
d) Among your best friends, how many are roma (gypsy)?	

**6. Now think of that desk partner who you sat next to in December in Hungarian class. Write down the full name of this desk partner!**

*If you did not have a desk partner in December in Hungarian class, please shade in this circle, and do not fill in the table!*



Family Name	Christian/Given Name (Write in all given names, do not use nicknames!)

**7. How much did you like sitting next to your desk partner?**

*Mark the corresponding number! Only shade in one circle!*

Really Did not Really Don't know Did not have  
 Did not like like Neutral Liked liked A desk partner in December

<sub>1</sub> ----- <sub>2</sub> ----- <sub>3</sub> ----- <sub>4</sub> ----- <sub>5</sub>      <sub>6</sub>      <sub>7</sub>

8. Think of Hungarian language, literature and mathematics . The following questions relate to how good you think you are in these subjects.

In each row mark the number you consider to be true! Only shade in one circle in each row!

Let's start with HUNGARIAN LANGUAGE!

In your opinion how good are you at Hungarian language?

<b>I am very bad at Hungarian</b>	<b>I am average at Hungarian</b>	<b>I am very good at Hungarian</b>	<b>I don't know</b>				
<input type="radio"/> 1	<input type="radio"/> 2	<input type="radio"/> 3	<input type="radio"/> 4	<input type="radio"/> 5	<input type="radio"/> 6	<input type="radio"/> 7	<input type="radio"/> 8

Compared to your classmates how good are you at Hungarian language?

<b>In the class I am among the worst at Hungarian</b>	<b>In the class I am average at Hungarian</b>	<b>In the class I am among the best at Hungarian</b>	<b>I don't know</b>				
<input type="radio"/> 1	<input type="radio"/> 2	<input type="radio"/> 3	<input type="radio"/> 4	<input type="radio"/> 5	<input type="radio"/> 6	<input type="radio"/> 7	<input type="radio"/> 8

Compared to your other subjects how good are you at Hungarian language?

<b>I am much worse at Hungarian than at other subjects</b>	<b>I am as good at Hungarian as at the other subjects</b>	<b>I am much better at Hungarian than at other subjects</b>	<b>I don't know</b>				
<input type="radio"/> 1	<input type="radio"/> 2	<input type="radio"/> 3	<input type="radio"/> 4	<input type="radio"/> 5	<input type="radio"/> 6	<input type="radio"/> 7	<input type="radio"/> 8

Now think of LITERATURE!

In your opinion how good are you at literature?

<b>I am very bad at literature</b>	<b>I am average at literature</b>	<b>I am Very good at literature</b>	<b>I don't know</b>				
<input type="radio"/> 1	<input type="radio"/> 2	<input type="radio"/> 3	<input type="radio"/> 4	<input type="radio"/> 5	<input type="radio"/> 6	<input type="radio"/> 7	<input type="radio"/> 8

Compared to your classmates how good are you at literature?

<b>In the class I am among the worst at literature</b>	<b>In the class I am average at literature</b>	<b>In the class I am among the best at literature</b>	<b>I don't know</b>				
<input type="radio"/> 1	<input type="radio"/> 2	<input type="radio"/> 3	<input type="radio"/> 4	<input type="radio"/> 5	<input type="radio"/> 6	<input type="radio"/> 7	<input type="radio"/> 8

Compared to your other subjects how good are you at literature?

<b>I am much worse at literature than at other subjects</b>	<b>I am as good at literature as at other subjects</b>	<b>I am much better at literature than at Other subjects</b>	<b>I don't know</b>				
<input type="radio"/> 1	<input type="radio"/> 2	<input type="radio"/> 3	<input type="radio"/> 4	<input type="radio"/> 5	<input type="radio"/> 6	<input type="radio"/> 7	<input type="radio"/> 8

Finally, think of MATHEMATICS!

In your opinion how good are you at mathematics?

<b>I am very bad at mathematics</b>			<b>I am average at mathematics</b>			<b>I am very good at mathematics</b>	<b>I don't know</b>							
<input type="radio"/> <sub>1</sub>	-----	<input type="radio"/> <sub>2</sub>	-----	<input type="radio"/> <sub>3</sub>	-----	<input type="radio"/> <sub>4</sub>	-----	<input type="radio"/> <sub>5</sub>	-----	<input type="radio"/> <sub>6</sub>	-----	<input type="radio"/> <sub>7</sub>	-----	<input type="radio"/> <sub>8</sub>

Compared to your classmates how good are you at mathematics?

<b>In the class I am among the worst at mathematics</b>			<b>In the class I am average at mathematics</b>			<b>In the class I am among the best at mathematics</b>	<b>I don't know</b>							
<input type="radio"/> <sub>1</sub>	-----	<input type="radio"/> <sub>2</sub>	-----	<input type="radio"/> <sub>3</sub>	-----	<input type="radio"/> <sub>4</sub>	-----	<input type="radio"/> <sub>5</sub>	-----	<input type="radio"/> <sub>6</sub>	-----	<input type="radio"/> <sub>7</sub>	-----	<input type="radio"/> <sub>8</sub>

Compared to your other subjects how good are you at mathematics?

<b>I am much worse at mathematics than at other subjects</b>			<b>I am as good at mathematics as I am at the other subjects</b>			<b>I am much better at Mathematics than at Other subjects</b>	<b>I don't know</b>							
<input type="radio"/> <sub>1</sub>	-----	<input type="radio"/> <sub>2</sub>	-----	<input type="radio"/> <sub>3</sub>	-----	<input type="radio"/> <sub>4</sub>	-----	<input type="radio"/> <sub>5</sub>	-----	<input type="radio"/> <sub>6</sub>	-----	<input type="radio"/> <sub>7</sub>	-----	<input type="radio"/> <sub>8</sub>

9. Imagine that you are going to the zoo with some of your classmates. Your desk partner (who you sat next to in Hungarian class in December) has forgotten to bring money for the entrance ticket. You have enough money for two entrance tickets. Would you lend your desk partner the money for the entrance ticket?

Shade just one circle in!

- a) Yes ..... <sub>1</sub>
- b) No ..... <sub>2</sub>
- c) I don't know ..... <sub>3</sub>
- d) I didn't have a desk partner in Hungarian class December ..... <sub>4</sub>

Now imagine that it is not your desk partner, but a different class mate who has forgotten to bring money with him/her. This classmate is a Roma/Gypsy [This sentence is missing in Version B]. Would you lend this Roma/Gypsy [Roma/Gypsy omitted from Version B] classmate the money for the entrance ticket? [i.e. Version B makes no mention of Roma/Gypsy otherwise it is the same]

Shade just one circle in!

- a) Yes ..... <sub>1</sub>
- b) No ..... <sub>2</sub>
- c) I don't know ..... <sub>3</sub>

10. Now think about how good the boys and how good the girls are at Hungarian language, literature, and mathematics. In your opinion when it comes to Hungarian language, to literature and to mathematics, are the boys better, or are the girls better, or are they equally good?

In each row mark the corresponding number that you consider to be true!

	THE BOYS are much better than the girls	THE BOYS are somewhat better than the girls	The boys and the girls are EQUALLY good	THE GIRLS are somewhat better than the boys	THE GIRLS are much better than the boys
a) Hungarian -----	<input type="radio"/> <sub>1</sub> -----	<input type="radio"/> <sub>2</sub> -----	<input type="radio"/> <sub>3</sub> -----	<input type="radio"/> <sub>4</sub> -----	<input type="radio"/> <sub>5</sub>
b) Literature -----	<input type="radio"/> <sub>1</sub> -----	<input type="radio"/> <sub>2</sub> -----	<input type="radio"/> <sub>3</sub> -----	<input type="radio"/> <sub>4</sub> -----	<input type="radio"/> <sub>5</sub>
c) Mathematics -----	<input type="radio"/> <sub>1</sub> -----	<input type="radio"/> <sub>2</sub> -----	<input type="radio"/> <sub>3</sub> -----	<input type="radio"/> <sub>4</sub> -----	<input type="radio"/> <sub>5</sub>

11. Now think of the classmate of yours whom you consider to be the cleverest. Is this classmate a boy or a girl?

Shade just one circle in!

a) Boy ..... <sub>1</sub>

b) Girl ..... <sub>2</sub>

c) I can't say who is the cleverest ..... <sub>3</sub>

12. Now think of an assignment that a group of children must solve/do together. What do you think, which group would be able to do this assignment better?

Shade just one circle in!

a) A group only of boys ..... <sub>1</sub>

b) A group only of girls ..... <sub>2</sub>

c) A group with both boys and girls in it - - ..... <sub>3</sub>

d) I don't know ..... <sub>4</sub>

**The following questions (13 & 14) are administered in Grades 6-8 only**

Q13 for 6<sup>th</sup> and 7<sup>th</sup> grade [not translated yet]

[Q13 in 8<sup>th</sup> grade]

13. Please indicate whether or not you applied to grammar school in February 2018! If you applied to several high schools were any of these grammar schools?

Only shade one answer!

a) Yes ----- <sub>1</sub>

b) No ----- <sub>2</sub>

c) I don't remember ----- <sub>3</sub>



**[Q14 in 6<sup>th</sup>-8<sup>th</sup> grade]**

**14. Regardless of whether you did or did not apply to grammar school, do you think you would/will be accepted?**

*0 means that they would definitely not accept you. 10 means that they would definitely accept you. You can also use numbers between 0 and 10 where the larger the number you circle the more certain you are that they will/would accept you. Only shade **one** answer!*

**Definitely will not  
Accept me**

0    1    2    3    4    5    6    7    8    9    10

**Definitely  
Will  
accept me**

**Please continue on to the comprehension exercises!**

**8** Szövegértés–3.évfolya



