Pre-analysis plan for: Close contact, Trust, and Interethnic Friendship - A large scale field experiment of Roma desk mates in Hungarian schools[*]

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Abstract

We are conducting a large scale field experiment where we randomly assign desk mates in Hungarian schools. By comparing people with Roma desk mates to people with non-Roma desk mates we investigate whether close personal contact to Roma increases inter-ethnic friendship and trust. In this plan we pre-register some key decisions to follow once we receive the data.

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 prejuice 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 teems 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 loose 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 compre- hension 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 differ-ent 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 investi-gate 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

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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) $Roma\,friend_{ict2} = \beta Treated_{ict1} + \alpha Class_{ct1} + \gamma X_{ict1} + \epsilon_{ict2},$

where i indexes individuals, c classes, and t is time (either baseline 1 or follow up 2). $Treated_{ict1}$ 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, ϵ_{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) Lend to Classmate$_{ict2}$ = $\beta Treated_{ict1}$ + θ Roma vignette$_{ict2}$ + $\delta Treated_{ict1}$*Roma vignette$_{ict2}$ + $\alpha Class_{ct1}$ + γX_{ict1} + ϵ_{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 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 β 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 δ 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

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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; Uslaner, 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) = a*i/m. In our case, with 2 hypotheses and a significance level (a) of 0.05, the critical p-value would be 0.025 for the one with the lowest p-value (0.05* 1/2, which is the same as a Bonferroni correction). For the second hypothesis, the critical p-value is 0.05 (0.05*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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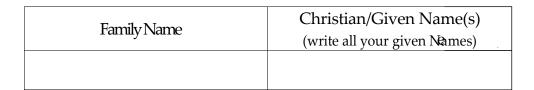
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Write your name! Do not use your nickname!

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: ☐☐

3rd Year

DESK PARTNER RESEARCH

2018.

YOUR TEACHER will complete these tables.

School's name or official stamp

Comprehension

A

………………………………………………………

Student ID Number (see separate sheet)

☐☐☐☐☐☐

Test date

Month: ☐☐   Day: ☐☐

Test Start Time

Hour: ☐☐   Min.: ☐☐

Test End Time

Hour: ☐☐   Min.: ☐☐

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.

(A)
(B)
(C)
(D)

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!

(A)
(B)
(C)
(D)

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	1	2	3	4	5	6
b) Diligence	1	2	3	4	5	6
c) Hungarian Language	1	2	3	4	5	6
d) Literature	1	2	3	4	5	6
e) Mathematics	1	2	3	4	5	6

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	1	2	3	4	5	6
b) Diligence	1	2	3	4	5	6
c) Hungarian Language	1	2	3	4	5	6
d) Literature	1	2	3	4	5	6
e) Mathematics	1	2	3	4	5	6

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	1	2	3	4	5	6
b) Literature	1	2	3	4	5	6
c) Mathematics	1	2	3	4	5	6

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!

◯ 1

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 like	Did not like	Neutral	Liked	Really liked	Don't know	Did not have A desk partner in December
◯ 1	◯ 2	◯ 3	◯ 4	◯ 5	◯ 6	◯ 7

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?

I am very bad at Hungarian						**I am very good at Hungarian**	**I don't know**
○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8

(with "**I am average at Hungarian**" labeled above circles 3–4)

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

In the class I am among the worst at Hungarian		**In the class I am average at Hungarian**			**In the class I am among the best at Hungarian**		**I don't know**
○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8

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

I am much worse at Hungarian than at other subjects		**I am as good at Hungarian as at the other subjects**			**I am much better at Hungarian than at other subjects**		**I don't know**
○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8

Now think of LITERATURE!

In your opinion how good are you at literature?

I am very bad at literature		**I am average at literature**			**I am Very good at literature**		**I don't know**
○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8

Compared to your classmates how good are you at literature?

In the class I am among the worst at literature		**In the class I am average at literature**			**In the class I am among the best at literature**		**I don't know**
○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8

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

I am much worse at literature than at other subjects		**I am as good at literature as at other subjects**			**I am much better at literature than at Other subjects**		**I don't know**
○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8

Finally, think of MATHEMATICS! ────────────────────────────

In your opinion how good are you at mathematics?

| **I am**
very bad at
mathematics | | | | | | | **I am**
average at
mathematics | | | | | | **I am**
very good at
mathematics | **I don't**
know |

$\bigcirc_1$ - - - - $\bigcirc_2$ - - - - - $\bigcirc_3$ - - - - $\bigcirc_4$ - - - - - $\bigcirc_5$ - - - - $\bigcirc_6$ - - - - - $\bigcirc_7$ $\bigcirc_8$

Compared to your classmates how good are you at mathematics?

| **In the class I am**
among the worst at
mathematics | | **In the class I am**
average at
mathematics | | **In the class I am**
among the best
at mathematics | **I don't**
know |

$\bigcirc_1$ - - - - $\bigcirc_2$ - - - - - $\bigcirc_3$ - - - - $\bigcirc_4$ - - - - - $\bigcirc_5$ - - - - $\bigcirc_6$ - - - - - $\bigcirc_7$ $\bigcirc_8$

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

| **I am much worse at**
mathematics than at
other subjects | | **I am as good at**
mathematics as I am at
the other subjects | | **I am much better at**
Mathematics than at
Other subjects | **I don't**
know |

$\bigcirc_1$ - - - - $\bigcirc_2$ - - - - - $\bigcirc_3$ - - - - $\bigcirc_4$ - - - - - $\bigcirc_5$ - - - - $\bigcirc_6$ - - - - - $\bigcirc_7$ $\bigcirc_8$

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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_____$\bigcirc_1$

b) No_____$\bigcirc_2$

c) I don't know_____$\bigcirc_3$

d) I didn't have a desk partner in Hungarian class December$\bigcirc_4$

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_____$\bigcirc_1$

b) No_____$\bigcirc_2$

c) I don't know_____$\bigcirc_3$

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	$\bigcirc_1$	$\bigcirc_2$	$\bigcirc_3$	$\bigcirc_4$	$\bigcirc_5$
b) Literature	$\bigcirc_1$	$\bigcirc_2$	$\bigcirc_3$	$\bigcirc_4$	$\bigcirc_5$
c) Mathematics	$\bigcirc_1$	$\bigcirc_2$	$\bigcirc_3$	$\bigcirc_4$	$\bigcirc_5$

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 _____ $\bigcirc_1$

b) Girl _____ $\bigcirc_2$

c) I can't say who is the cleverest_____ $\bigcirc_3$

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 _____ $\bigcirc_1$

b) A group only of girls _____ $\bigcirc_2$

c) A group with both boys and girls in it - - $\bigcirc_3$

d) I don't know _____ $\bigcirc_4$

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

Q13 for 6th and 7th grade [not translated yet]

[Q13 in 8th 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 - 1

b) No - 2

c) I don't remember - - - - - - - - - - - - - - 3

[Q14 in 6ᵗʰ-8ᵗʰ 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   **Definitely**
Accept me   **Will**
                                                                                              accept me

| 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |

Please continue on to the comprehension exercises!

8 Szövegértés–3.évfolya