With a Little Help From My Friends: Medium-Term Effects of a Remedial Education Program Targeting Roma Minority*

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Abstract

A poor-performing student can achieve better results by following the footprints of an older friend. In this paper, we study a remedial education program that takes advantage of this phenomenon. Introduced in Serbian primary schools in 2009, the Roma Teaching Assistant Program targets underachieving students belonging to the Roma minority. It assigns one person, usually Roma, to each school participating to provide support to targeted pupils and create a bridge with their community. We estimate its medium-term effects on educational attainments at the end of primary school by comparing students in schools participating and in schools that applied, but were not selected, before and after the introduction of the program. The impacts on marks and standardized test scores are modest, although positive and bigger in schools with a lower percentage of Roma. Roma students are however more likely to choose longer secondary school tracks, a requirement for entering higher education.

Keywords: remedial education, Roma, ethnic minority, standardized test scores

JEL classification codes: I21, J15, D04

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1 Introduction

Over the last decades, inequality within countries is rising both in the developed and developing world (Keeley, 2015) and high-quality education for all children independently of their socio-economic background is one important lever to reverse this trend (Rodríguez-Pose and Tselios, 2009; Abdullah et al., 2015). The provision of high quality education to all pupils can help to reduce the perpetuation of inequalities from older to younger generations and bridge the achievement gaps between advantaged and disadvantaged students. Attaining more education implies getting higher salaries (Ashenfelter and Krueger, 1994; Harmon et al., 2003) and thus education can help disadvantaged students escape the vicious cycle of poverty and get better opportunities and jobs than the generation of their parents. One of the most disadvantaged group in Europe is the Roma ethnic minority. In comparison to the majority of the population in the developed and middle-income countries where they mainly live, most of its members have extremely low educational attainment. A necessary condition to ensure a better life to the young generation of Roma is to provide them with an education that can help rise the economic ladder (Kertesi and Kézdi, 2011).

The Roma Teaching Assistant (RTA) program is one of the main initiatives in Europe targeting this population and aiming at increasing their educational attainment. It assigns one person, usually Roma, to each school participating to provide additional in-class and out-of-class support to students and build a bridge between their community and the school. This paper focuses on the RTA Program in Serbia,[1] where it was first introduced in 2009, and estimates its impacts on affected students in the medium-term. In the short-term, one year after its implementation, the program improved school attendance and benefited younger children in terms of educational achievements (Battaglia and Lebedinski, 2015). Medium-term effects, which in our case refer to 5 to 8 years of treatment, that is at the end of the primary school cycle, can substantially differ from short-term effects and it is not clear in which direction they can go: we can expect either stronger positive effects or they can dissolve. On the one hand, first, the teaching assistants gain experience over the years and their learning curve is steep in the first few years.[2] Second, schools might need some time to adapt to changes and find ways to use the newly available resources optimally. Third, in the medium-term children are exposed longer to the program and we can expect that longer exposure has a stronger cumulative effect.

[1] Roma Teaching Assistants exist in some form in most countries where Roma live (Rus, 2006).

[2] Similar to teachers who on average improve their teaching skills in the first few years (Hanushek, 2011).

On the other hand, the intervention may be less relevant at later ages or assistants may lose their initial dedication and enthusiasm and as a result short-term effects can dissipate.

In order to investigate the RTA effects in the medium-term, we combine two sources of data: (i) the primary school final examination dataset for the years 2008 to 2018 and (ii) the list of schools that applied for the RTA Program in 2009 and 2010. Both information is provided by the Serbian Ministry of Education, Science and Technological Development (MoESTD). The final examination dataset includes the whole population of pupils attending the eighth and therefore the last grade of primary school in a given year.[3] The final examination is an external standardized test that all students who are finishing primary school take to formally complete it. The school application list contains information on school size and share of Roma in school and reports whether the school received the program and at which point in time.

Schools needed to apply to be part of the program. We exploit the fact that, among those applying, some schools were admitted to the program while others were not. We use a difference-in-difference strategy and define as treated the schools which applied either in 2009 or in 2010, got selected and still had an assistant five to six years later.[4] We define as control the schools which applied in any of the two years and never got selected. Since treated and comparison (not admitted) schools may differ in their pretreatment characteristics, we pre-process the data with the entropy balancing method (Hainmueller, 2012) and obtain a balanced sample in observable characteristics. We use the percentage of Roma in the school at the time of application and all outcomes (test scores, marks and dropouts, enrollment in secondary school and school choice) in the pre-treatment year to balance the data set. In our final sample there are 64 treated schools and 39 control schools. It is important to bear in mind that not all Roma students in treated schools work with the assistant. Since there is only one of them per school, she might decide to help only some students. For this reason, we also exploit the intensity of the program that depends on the number of Roma students in each school. Our results are intention-to-treat effects.

Our outcomes of interest are marks in Language and Math, the probability of not sitting

[3]In Serbia, primary education lasts eight years. In the first four grades pupils get one teacher who teaches all compulsory subjects, except English, and in the upper four years one teacher per subject. School is compulsory until age 15 and children first enroll when they are aged 6.5 at the beginning of the scholastic year, in September.

[4]We consider five to six years later, that is 2015, because this is the only year for which we have information on assistants. This is not problematic for two reasons. First, we can assume that the assistant in 2015 is the same schools hired in either 2009 or 2010. Second, even if we cannot know whether schools had assistants in the period after 2015, all pupils in our sample had assistants in the lower four grades throughout their education, that is in the grades in which the pupils are most exposed to the program. More details on the RTA in Section 2.

the final examination and therefore complete primary school, and standardized test scores in Language and Math. We also look at secondary school enrollment and secondary school study track. Overall, the RTA does not have statistically significant effects on educational outcomes of students in treated schools, except for those in schools where the percentage of Roma at application is low and where therefore the program is more intensively implemented. In these schools, Roma students receive a 0.405 standard deviation more in the Math test than their counterparts in control schools. Moreover, there is evidence that students at the margin of passing a course are less likely to be over-graded and are better prepared in the subjects examined. The program was yet 16.8 percentage points more successful in making Roma students choose longer and more demanding secondary education tracks. The assistants do not work with Non-Roma children and none of these students are directly treated by the RTA. However, in treated schools there are positive spillovers that are reflected in higher educational attainments of Non-Roma, especially in schools with fewer Roma. We also look at the characteristics of the assistant and if their interaction with students' traits is relevant for our outcomes of interest. Overall, we observe that if students interact with an assistant who has a university degree or more years of experience than the median in the school, they are more likely to keep studying in secondary education.

This paper speaks to the literature on remedial education programs and the effectiveness of such programs to improve the educational outcomes of a marginalised group.[5] The evidence for different settings suggests that in the short-term remedial education programs are effective in raising educational outcomes. A well-known remedial education program in India called Balsakhi targeted third and fourth graders in primary schools and provided every day two hours remedial classes during regular school time (Banerjee et al., 2007). The short-term impact on average test scores was substantial, 0.14 standard deviations in the first year and 0.28 in the second year, but the large effects of the program did not persist after pupils left the program. Other remedial education programs, such as the Program for School Guidance in Spain or a remedial math course in Mexico, have found similar effects sizes in different settings (Gutiérrez and Rodrigo, 2014; García-Pérez and Hidalgo-Hidalgo, 2017). The Spanish Program for School

[5]There are other programs targeting disadvantaged pupils, which cannot be categorized as remedial education programs, and have been analyzed extensively in the literature. These studies include merit pay for principals, teachers, and students (Podgursky and Springer, 2007; Fryer, 2010), professional development for teachers (Boyd et al., 2009), getting parents to be more involved (Domina, 2005), placing disadvantaged students in better schools through desegregation busing (Angrist and Lang, 2004) or altering the neighborhoods in which they live (Jacob, 2004; Sanbonmatsu et al., 2006). Their findings suggest that there is no panacea for improving educational outcomes of disadvantaged pupils: some programs work and others do not in a specific context.

Guidance, implemented in both primary and secondary schools, was successful in raising test scores in reading by 0.09 to 0.17 standard deviations (García-Pérez and Hidalgo-Hidalgo, 2017). In Mexico City a low-cost intervention targeting low-performing students in secondary schools had an impact of 0.21 and 0.26 standard deviations (Gutiérrez and Rodrigo, 2014).

The analysis of medium-term or even long-term effects of educational programs is however less common in the literature. One of the most studied programs is the Head Start program. The experience with Head Start suggests that short-term findings might not necessarily translate to long-term effects. For instance, Currie and Thomas (1995) find that the short-term effects on test scores in the case of Head Start are quickly lost for the disadvantaged African-American group of pupils. On the other hand, Lavy et al. (2018) analyze the effect of a high school remedial education program in Israel, almost two decades after its implementation. The program provided low-performing students with additional instruction in order to prepare them for the matriculation exam and succeeded in raising the matriculation rate by 3.3 percentage points in the short-term (Lavy and Schlosser, 2005). The long-term results suggest that the early positive effects persisted and treated students experienced an increase in completed years of college schooling, in annual earnings, in months employed, and in intergenerational income mobility, especially if coming from below median income families. In our paper we contribute to this strand of the literature providing new evidence on the medium-term effects of a remedial education program. Our outcomes are not limited to standardized tests, but we also examine the impact on completion of primary school and secondary school enrollment.

Furthermore, we contribute to the literature on discrepancies between teacher assessment and standardized test scores by providing evidence on how teachers raise expectations and start grading more strictly as a response to the intervention. This literature examines predominantly differences along two dimensions: first, between natives and immigrants and second, between girls and boys. Burgess and Greaves (2013) show evidence of differential grading among immigrants in England, over- and under-assessment by teachers, and they argue that such behavior is motivated by stereotypes. Conditional on standardized test scores, teachers over-grade ethnic groups that scored higher in the previous years, and under-grade ethnic groups that scored lower. Conversely, Diamond and Persson (2016) show, with Swedish data, that teachers may inflate grades in high stakes exams for students who had a "bad day", but do not discriminate on immigrant status or gender. Such discretion has long term consequences in terms of level of education and earnings. Alongside, Calsamiglia and Loviglio (2019) provide evidence that a student in a classroom with better peers receives lower grades from the teacher than an identical student with worse peers, that is, teachers grade on a curve leading to a negative source of distortion. With respect to the differences between genders, the literature suggests that in some settings boys are under-graded while girls can be over-graded (Cornwell et al., 2013; Lavy, 2008; Lindahl, 2007). Lavy (2008) provides evidence that the differential grading is correlated with teacher's characteristics suggesting that the bias is driven by teachers' discrimination.

Finally, studies on same-race teachers are relevant for our study. While the RTA program is unique in targeting the largest ethnic minority in Europe, and there is no study on the importance of having a co-ethnic as a Roma assistant in the long-run, we conjecture that the fact that Roma are helped by same-race teachers can make an important difference. Gershenson et al. (2018) show that African-American students with a same-race teacher in early grades of primary school have higher graduation rates in high school and are more likely to enroll in college. They provide evidence that this positive effect is explained by the presence in the classroom of a role model of the same background. Battaglia and Lebedinski (2017) show in the same context of this paper that when pupils are taught by someone from their community, parents' expectations on their returns to education and on secondary school achievement increase. A poor-performing student can improve her educational attainments by following the footprints of an older friend or neighbor, especially in an extremely socially and economically deprived environment.

The paper proceeds as follows. In Section 2 we describe the RTA Program, Section 3 describes the dataset, Section 4 explains the empirical strategy and Section 5 reports our results. Section 6 concludes.

2 Roma Teaching Assistant Program in Serbia

The Roma minority arrived in Europe from India between 6th and 11th century (Kenrick and Taylor, 1998), but they have preserved their traditions and they have rarely assimilated to the majority population. In most of the countries where they reside, Roma households are poorer than Non-Roma households, making decent quality housing less affordable to them (Perić, 2012). One of the reasons for the poverty-stricken housing conditions of Roma, is their situation in the labor market: Roma are severely disadvantaged, with women facing even more difficulties than men (O'Higgins, 2012), and this is mainly caused by their low educational background. Due to their modest education, Roma often work in the informal labour market where job quality is low

(O'Higgins, 2012; Lebedinski, 2019). Policies aiming at improving their educational outcomes are important to lift them out of poverty. However, disparities between Roma and Non-Roma arise already at an early age and increase over time. In Serbia, for instance, Baucal (2006) finds that in the third grade of primary education, Roma pupils lag behind their Non-Roma peers already 2.2-2.6 of school years in terms of cognitive skills. For Hungary, Kertesi and Kézdi (2011) report a raw gap of one standard deviation for reading and mathematics in the eight grade between Roma and Non-Roma. The gap decreases when accounting for health, family background, and school and class fixed effects, indicating that a large part of it can be explained by environmental factors. However, Bhabha et al. (2018) also document that discrimination in education can be an additional obstacle that Roma children face.

The RTA Program was first introduced in Serbian schools in 2009 and since then has covered 156 schools in the entire country. The program consists of assigning one person, the so-called Roma teaching assistant, to each school participating. The duties and responsibilities of the assistants have been included and defined in the main law regulating education in Serbia since 2009.[6] According to the law, assistants have three broad duties. First, they provide additional help and support to pupils in accordance with their needs, especially in the first four grades. This implies that if the assistant cannot reach all students, the youngest and, among them, the most disadvantaged Roma are those more likely to be helped. Second, they assist teachers, educators and other school employees in their work with students who need additional educational support. Third, they cooperate with parents (or guardians), the community, institutions and other relevant organizations and by doing so they build a bridge between the school and the Roma community. While the specific duties of an assistant can vary depending on the needs of the school, a report on the practical implementation of the program in Serbian schools suggests that the main task of the assistants is to provide learning support to pupils during and after class (Milivojević, 2015). In the first few months on the job, some assistants work primarily in the field with parents to get to know the community and the circumstances of the children and turn after the introductory phase to standard tasks of learning support. Work with the community remains an important part of the duties of the assistants especially, because dropouts and numerous absences are challenging obstacles to Roma pupils' education.

In 2009, the MoESTD developed a rulebook which defines a one-year study program that each assistant needs to attend to get a license as RTA. The assistants enroll in the program only

[6]Law on Fundamentals of Education System.

after having been selected and hired by the school. The study program for RTAs aims to train pedagogical assistants for the various aspects of their work such as preparation and realization of teaching activities, development and safety of children, work with family, cooperation with school pedagogues and school psychologists, organizations and institutions. Since the official beginning of the program, all RTAs are hired on an annual basis and their contracts are extended each year. Their salaries are fully funded by the MoESTD.

In order to get a Roma teaching assistant, schools had to apply. There were initially two rounds of applications: the first round in 2009 and the second round in 2010. In 2009, 78 schools applied for an assistant and, out of these, 26 schools were selected. The program was expanded in 2010 to another 77 schools out of 190 applicants. The main criterion for getting admitted to the program was having a share of Roma between 5% and 40%. Schools applying in the first round were also required to offer a preschool program. This condition was relaxed in the second round because in 2010 Roma teaching assistants were also assigned to preschool institutions. Starting from 2011, once a school has selected a candidate, it needs to apply for funding from MoESTD through its regional office, and it is not clear how MoESTD approves or rejects applications from schools. Presumably the regional office makes a recommendation whether a school needs an assistant and the MoESTD approves this decision. Since then, an additional 53 schools joined the program. Similar to the schools, Roma teaching assistants had to apply and the following requirements were defined for the candidates: knowledge of the Romani language − the mother tongue of the Roma people, secondary school diploma and experience in working with children. Although it was not required for the teaching assistant to be Roma, the fact that they were expected to speak Romani implies that almost all of them belong to the Roma minority. This adds a relevant feature to the program. The teaching assistants can act as an important reference point for the community and can be seen as a role model for their students: in order to be assistants, they need to have invested in education in the first place and, thanks to such investment, they have obtained a good full-time job in the formal sector. By sharing their successful experience with students, they can motivate them to believe that they can achieve analogous results. Since the program targets a specific minority, one can be worried that it could potentially stigmatize Roma children and therefore have a negative effect on them. We take into account this possibility while discussing the results, but it is reasonable to assume that all schools which applied in the program are aware and arguably acknowledge the difficulties that Roma children face at school and are expected to promote diversity and reduce the potential stigma. This is also why the assistants are referred in the school to as *pedagogical assistants*, with no reference to the target group.

The number of applicants and schools selected in each round are reported in the timeline in Figure 1. This study uses only applicants from the first two rounds, in 2009 and 2010, because in these rounds the selection criteria were universal and clearly pre-defined. The round 3 applicants and schools are not considered in the evaluation since the selection of schools is based on proactive applications and we do not know the criteria which apply.

[Insert Figure 1: Timeline]

Unfortunately, we only have information on the percentage of Roma in the entire school at application, and since we have data available only for the last grade of primary school, we cannot know the proportion of Roma enrolled at school in each year. This implies that formally we cannot test whether there is some selection of children into schools due to the program. Parents who decide to get their child enrolled in participating schools may differ from those who enrolled their child in this same school before the program, and from parents who send their child to not participating schools. Once the intervention is known in the community, Roma parents who care more about education may choose participating schools instead of another school in the neighborhood. Non-Roma parents may be either happy that a teacher assistant take care of Roma children so that their child will benefit from a better learning environment and thus send her to a participating school, or may dislike the pro-Roma program, fear that many Roma children will get enrolled as a response, and decide to get their child enrolled in a different school. Nonetheless, concerns about changes in school composition are mitigated in this context. By law in Serbia, children are expected to go to the closest primary school to their home. In fact, each primary school has a catchment area and children from the catchment area are given preference when enrolling. While it is possible to enrol a child in a school in a catchment area where one does not belong, it happens only in rare cases and only if there are places available in the school after all children from the catchment area enrolled. The median aerial distance between two primary schools in Serbia is 2.37 kilometers, suggesting that enrolling a child in a school outside of the own catchment area is also costly in terms of time. Furthermore, data for the initial two years of the RTA (2009 and 2010) suggest that the share of Roma enrolled in the first grade remained unchanged once the program was introduced. For this period we find no evidence of selection into treated schools.[7]

3 Dataset and descriptive statistics

3.1 Description of the dataset

We use two sources of data: (i) the MoESTD primary school final examination dataset and (ii) the MoESTD list of schools that applied for the RTA Program in 2009 and 2010.

The MoESTD final examination dataset includes the whole population of pupils attending the eighth and therefore the last grade of primary school in a given year. The final examination is a standardized test that all pupils who are finishing eight years of primary school take at the end of the last year of school. The pupils can formally finish primary school only if they sit the test. The test consists of three parts: one in Math, one in Language (Serbian) and one in a mix of different subjects (geography, chemistry, physics, history and biology).[8] The test is nationwide and is scheduled on the same three days. On each of the three days pupils get examined in one of the three parts (Math, Language and Combined). Pupils get assigned to a secondary school depending on their total score at the final examination, their average marks from the 6th to the 8th grade and their expressed preferences regarding which school they want to enroll. Students reveal their preferences only after being informed about their final examination test score and they can list up to 20 schools. In 2011 there was a policy change as to who is required to sit the final exam. Until 2011, only pupils who wanted to enrol in a four-year secondary school track (either technical or general) were required to sit the final exam. Conversely, pupils who wanted to enrol in a three-year technical track were not asked to take the final exam. The policy change introduced in 2011 meant that one had to sit the final exam in order to receive a primary school diploma. There is no requirement in terms of achievement at the test, everyone who is present at the test receives the primary school diploma but the presence is required and everyone's test scores are recorded.

Our dataset includes the test results for the years 2008 to 2018 and it contains demographic information on pupils, their marks in all subjects from 6th to 8th grade, their test scores for each of the three tests and information on their secondary school enrollments. It also reports whether

[7]The number of Roma pupils enrolling at school for the first time after the program remained the same as in the pretreatment year in both participating and not participating schools (Battaglia and Lebedinski, 2015). In the pretreatment year, first grade Roma pupils in schools joining the program in 2009 corresponded to 29% of all Roma enrolled in these schools. In schools joining later, they were 26%. In the first year of the program, that is 2009, these percentages were 29% for participating schools and 28% for not participating schools.

[8]The last part was only introduced in 2014.

a pupil benefited from the affirmative action policy introduced in 2009. This policy allows a student to get additional points for admission to secondary school based on her ethnicity and since it targets Roma pupils, the dataset implicitly contains information on the Roma ethnicity of students.[9] The MoESTD does not have digitized datasets of final examinations before 2008.

The final examination dataset is complemented with a list of schools that applied for the RTA Program in 2009 and 2010. The school application list contains information on school size and share of Roma in school and reports whether the school received the program and at which point in time. From the original list of schools that applied and were selected in 2009 and 2010, the following schools were excluded from our sample: (1) schools with less than the minimum number of Roma at the time of application required to participate (5%),[10] (2) schools that received an assistant initially but did not have an assistant anymore in 2015 and later,[11] (3) primary schools for functional education, (4) schools without any Roma in 8th grade in the pre-treatment or treatment years,[12] and (5) schools which were assigned a zero weight in the balancing procedure.[13] As a result, schools are defined as treated if they applied either in 2009 or in 2010, got selected and still had an assistant at the time of follow-up and in the following years, and if they had a share of Roma of at least 5% at the time of application and at least one Roma in the eight grade in both pretreatment and treatments years. Schools are defined as control if they applied in any of the two years and never got selected, and if they had a share of Roma of at least 5% at the time of application and at least one Roma in the eight grade in both pretreatment and treatments years. In our final sample there are 64 treated schools and 39 control schools, located all around Serbia.

3.2 Descriptive statistics and balancing weights

The empirical strategy exploits the fact that, while all schools applied, some schools were admitted to the program while others were not. No admitted schools provide the so-called comparison group. Since all schools in our sample applied to be part of the RTA, we can expect they are

[9]Although the affirmative action was introduced in 2009, data on whether someone benefited from affirmative action was recorded in the dataset only since 2016. Prior to 2016 pupils additional points were not recorded in the data base instead pupils had a document confirming their status and the right to obtain additional points.

[10]There are no schools with more than the maximum number of students required to be selected, 40%.

[11]Recall that we have information on assistants only for 2015.

[12]Schools without Roma in the 8th grade in the pre-treatment or treatment years are omitted because the difference-in-difference methodology requires that each school has Roma both in the pre-treatment and treatment period.

[13]We lose in total 9 schools that would have been considered as treated and 9 schools that would have been considered as control, as these schools get assigned a 0 weight.

all equally motivated to participate and that concerns about differences in unobservables can be mitigated. Nonetheless, we have not been informed by the MoESTD why certain schools are selected and others not. At first sight, the differences in observable characteristics do not suggest obvious criteria: for instance, the difference in the percentage of Roma at application between selected and not selected schools is statistically indistinguishable, and they are equally distributed in all country districts (see more below in Figure 2 of Section 4). However, the treated and comparison schools could still differ in their pretreatment characteristics and outcomes. For this reason, we first show their differences for both Roma and Non-Roma in terms of percentage of Roma at application and outcome variables in the year prior to the introduction of the program and then, to alleviate the problem, we pre-process the data with the entropy balancing method (Hainmueller, 2012).

Our outcomes of interest are marks in Language and Math, the probability of not sitting the final examination, and standardized test scores in Language and Math. Marks range from 1 (worst) to 5 (best). Mark 1 in a given subject is considered being insufficient, that is a non-passing grade, while mark 2 or higher are passing grades.[14] We also consider secondary school enrollment and school track. In Serbia, only primary school is compulsory and we examine whether a child continues studying after finishing the compulsory part of the education. Secondary school enrollment is a dummy equal to 1 if the student enrols in secondary education, conditional on having completed primary education and therefore having done the test. School track choice is a dummy equal to 1 if she enrolls in a four-year track (either technical or general) and 0 if in a less demanding three-years technical track.

Panels A and C of Table 1 show that, overall, the means of school characteristics and outcome variables in treated and control schools before the program are fairly balanced for Roma pupils. The only statistically significant difference is observed for the enrollment in secondary education that is higher in control schools. Among Non-Roma we find more differences in the pretreatment year, namely in terms of the probability of not sitting the final examination and enrollment in secondary education. Both are lower in treated schools: there are fewer students who did not complete primary education, but, among those completing, there are fewer

[14]Students at the eight grade who receive an insufficient mark in a subject at the end of the academic year (May) are expected to take a make-up exam for that subject in June. For those in previous grades, the make-up exams are only in August. Standardized tests take place at the end of June. This implies that in our sample we observe few students who received 1 in either Language or Math but could sit the final tests. These students passed the make-up exams. The maximum number of points at tests in Language and Math can vary by year, but we have standardized test scores to alleviate this problem and make them comparable between years.

who continue studying. To alleviate the problem of differences in pre-treatment characteristics between treated and control groups, we pre-process the data with the entropy balancing method (Hainmueller, 2012). The entropy balancing method is grounded in the idea to reweigh each observation from the comparison group so that the reweighted data satisfy a set of specified moment conditions. In our case, we specify that the first two moments (mean and variance) of pretreatment characteristics and outcome variables of the treatment group should match these two moments of the comparison group.[15] Since in our case treatment is assigned at the level of the school, also balancing weights are estimated at the school level. After pre-processing the data with entropy balancing, there are no statistically significant differences between Roma in treatment and control schools in the pretreatment year (Table 1, panel B). For Non-Roma, even after balancing, treated schools have marginally fewer students enrolled in secondary education, but overall the differences in outcomes are not statistically significant (Table 1, panel D).

[Insert Table 1: Balancing of school characteristics of treated and control schools at application in 2008 - Roma and Non-Roma]

Once we establish that the dataset is balanced in the pretreatment year, we report the predetermined characteristics and outcomes in the pretreatment and treatment years for both Roma and Non-Roma (Tables 2 and 3, respectively). In addition to the characteristics discussed before, the table reports the following covariates we use in the analysis: gender, whether the pupil was born in a different district than the one where the school is located,[16] age at test, and class size.

[Insert Tables 2 and 3: Means of covariates and outcomes in pre-treatment and treatment years - Roma and Non-Roma]

Table 2 shows that among Roma, there are no differences in neither pretreatment characteristics nor outcomes between treated and comparison schools, except for class size that is significantly higher in treated schools. On average, children in the eight grade are 15 years old and roughly 17% of them were born in a different district than the one of the school. Their marks are relatively low, less than 3 on a scale 1 to 5, and approximately 35% of them are enrolled in the last year of primary school but do not sit the final exam (*dropout*). Once they take

[15] All variables are reported in Table 1.

[16] The district corresponds to an area smaller than a region and bigger than a municipality. In Serbia (excluding Kosovo), there are 4 regions, 25 districts and 145 municipalities.

the exam, they continue with secondary education with a high certainty. As for the Non-Roma pupils, Table 3 shows that on average in both treated and comparison schools children in the eight grade are slightly younger than Roma and more likely to be born in the same district of the school. On average, they also have better marks than Roma and a lower probability of not sitting the final examination (around 10%). Among those completing primary education, almost everyone enrolls in secondary school, mainly in more demanding tracks. While in the treatment years there are no differences in outcomes for Roma between treated and comparison schools, we do find that Non-Roma pupils in treated schools score higher on standardised test scores in Language and Math and are more likely to enroll in 4-year tracks than pupils from comparison schools.[17]

Higher participation in the final examination together with a consequent lower enrollment in secondary school from pretreatment year to treatment years are explained by a regulatory change introduced in 2011. Note that this pattern is observed for both Roma and Non-Roma, in treated and control schools. Prior to 2011, pupils with passing grades who wanted to enroll in three-year tracks in secondary schools were not required to sit a final examination and would get a primary school diploma independently of attending the final exam. Since 2011, only pupils who sit the final exam, regardless of the number of points at the exam, receive a primary school diploma. As a result, also pupils who wanted to enroll in three-year tracks were required to sit the final exam. Due to this change in requirements, the number of pupils who sit the final exam increased largely in treatment years (evident from the fall in the outcome variable *Dropout (=1)*), but the share of pupils enrolled in secondary school, conditional on sitting the final exam, decreased (evident from the outcome variable *Enrolled in secondary education (=1)*). The reform incentivized more pupils to sit the exam in order to get a primary school diploma, but now, conditional on sitting the exam, a larger share of them do not enroll in secondary education. The unconditional values show that there are no differences in the percentage of students in the eight grade of compulsory education that keep studying, before and after the regulatory change. Among eight grade Roma, there were 76% of students before and 74% after who went to secondary education, of which 52% chose a four-year track. Among Non-Roma, about 94% of eight grade students went to secondary education both before and after, choosing

[17]In Figures A.1 and A.2 of the Appendix we provide a graphical representation of the raw trends of the outcome variables for Roma and Non-Roma. Overall, the graphs confirm what discussed here. For Roma, the average outcomes generally follow a similar trend, although there are some outlier years. For Non-Roma we observe some divergence for standardised test scores and enrollment in 4-year secondary school tracks in the treatment years.

mainly more demanding tracks (78% before and 84% after).

4 Empirical strategy

We estimate the impact of the RTA Program using a difference-in-difference methodology:

$$\begin{aligned} outcome_{ist} &= \beta_0 + \beta_1 treated_{st} + \beta_2 post_{st} + \beta_3 treated_{st} * post_{st} + \\ &\quad + \rho_1 X_{ist} + \mu_s + \gamma_t + \epsilon_{ist} \end{aligned} \tag{1}$$

where $outcome_{ist}$ stands for the outcomes of pupil i in school s at time t, namely eighth grade marks in Language (Serbian) and Math, whether the child does not sit the final examination and thus does not finish primary school (*Dropouts*) and standardized test scores in Language (Serbian) and Math. We also investigate secondary school choices. More precisely, we are interested in the effect of the program on (i) the probability of enrolling in the secondary school and on (ii) choosing a four-year secondary school track (either general or technical track) versus a three-year technical track, conditional on enrolling. Graduating in a four-year secondary school track is a requirement for entering higher education. $treated_{st}$ is a dummy capturing whether the school received the program, $post_{st}$ is equal to 1 for the years 2014 to 2018 and 0 for the year 2008, and our coefficient of interest for the overall impact of the program is β_3. Year 2008 is used as baseline and we examine the impact for 2014 and the subsequent years because the generation finishing school in 2014 was the first to be treated. When the program started, assistants mainly worked with pupils in the first four grades. Consequently, students enrolled in the fourth grade when the RTA was first implemented, in 2009, should be in the eight grade in 2014. This is the first treated generation. We run the main analysis for Roma and Non-Roma students separately. Since the program is targeting only the former, it is plausible to expect effects mainly on this subgroup of the entire population.[18] The set of exogenous individual characteristics is expressed by X_{ist}. The control variables, reported in Tables 2 and 3, include: a dummy for being female, a dummy for being a pupil born in a different district from the one where the school is located, age at test, and class size and class size squared. μ_r corresponds to

[18] We also investigate the impacts for the full sample of students by interacting *treated* and $roma_{is}$. To capture the condition of being a pupil of Roma ethnicity and being in a treated school s, we estimate the effects as follows: $outcome_{ist} = \beta_0 + \beta_1 treated_{st} + \beta_2 post_{st} + \beta_3 treated_{st} * post_{st} + \delta_1 roma_{is} + \delta_2 treated_{st} * roma_{is} + \delta_3 post_{st} * roma_{is} + \delta_4 treated_{st} * post_{st} * roma_{is} + \rho_1 X_{ist} + \mu_r + \gamma_t + \epsilon_{ist}$. The specification establishes whether there are differential effects between treated Roma and Non-Roma and thus δ_4 is our main coefficient of interest. We also estimate the RTA impacts on the full sample with school fixed effects. The results are reported in the Appendix.

region fixed effects and γ_t to survey-year fixed effects. Robust standard errors are clustered at the school level.

To capture the heterogeneity due to the intensity of the program, we run the previous specification by quartiles of percentage of Roma at school at application. Recall that there is only one assistant per school. If there are fewer Roma students in a school, they have a higher probability of being treated. At the same time, the more Roma students in a school, the higher can be the spillover effects.

Our data face two important limitations: first, we observe pupils only at the end of primary school, and we cannot follow them over the years. In the period that we study, schools kept their records on educational attainments of pupils exclusively on paper and this data is not available in digital format. Consequently, the MoESTD does not have individual level records of primary school pupils. Second, we cannot formally test the parallel trend assumption.

As a consequence of the first limitation, in the absence of panel data, we are not able to control for differential attrition between control and treatment schools. We cannot exclude the possibility that more fragile Roma are, as a result of the program, more likely to stay in treatment schools than in control schools until the end of compulsory schooling. However, if this were the case, then we would underestimate the impacts of the program and our estimates should be interpreted as lower bounds.

Second, the difference-in-difference approach relies on the parallel trends assumption: in the absence of the program, treatment and control schools would have had a parallel trend in the outcomes of interest. There is available information only for one pre-treatment year, that is 2008. This makes it difficult to formally test the assumption. We propose two complementary exercises to provide confidence in the hypothesis. As shown in the previous section, we first use the entropy balancing method. This allows to reduce possible differences in baseline levels, and not just in trends, of the treated and control groups. Ryan et al. (2018) illustrate, via a Monte Carlo simulation, that matched difference-in-difference does well at dealing with non-parallel trends in a context of health policy interventions. Moreover, in the pre-treatment period there were no other policies that affected differently treated and control schools and that might be a threat for the parallel trends assumption. Second, we can run a placebo test by comparing treated and control schools in the first three treated years versus treated and control schools before the introduction of the program. We include in the placebo test years from 2009 to 2011, that is the years immediately after the introduction of the program, because when the program started, assistants were working with lower grades and we can assume that students in grades higher than four were not treated. Students who are in the eighth grade from 2009 to 2011 were unlikely targeted by the assistants: when the program started they were already in the sixth, seventh or eighth grade of primary education. Significant coefficients in placebo regressions would invalidate this estimation strategy and would question the adequacy of our comparison group.

We now turn to a discussion of the Serbian context and whether other educational reforms can be relevant for the robustness of the difference-in-difference methodology. In the period considered in this study, compulsory preschool education was extended to 9 months. This change happened at the same time in all schools and there is no reason to believe that treated and control school were affected differentially.[19] We are therefore confident that this policy could not invalidate our methodology. Additionally, there was a regulatory change in the period of this study. From 2011 onwards, sitting the final exam became a requirement for attaining a primary school diploma. This regulatory change was implemented in the whole country in the same year, and it cannot undermine our findings. We trust that treated and control schools were affected by these two reforms and any other policy change in the same way for two main reasons. First, Serbia is a highly centralized country and educational policy is governed and commonly implemented at once in the whole country by the MoESTD. Second, as shown in Figure 2, treated and control schools are equally distributed in the different 25 districts and they are not clustered in certain areas. It is not possible that some schools were affected by some reforms while others were not. It is therefore highly unlikely that there is any policy unrelated to the RTA program, which targeted only treated schools and could lead us to confound the results.

[Insert Figure 2: Geographical distribution of treated and control schools]

We are finally interested in the characteristics of the assistant and whether they affect differently their pupils. We look at gender, whether she has a university degree or not, and to her working experience (longer or shorter than the median years of tenure). We concentrate on Roma children.

[19] A six months program of compulsory preschool education was introduced in 2006/2007 and the program was extended to 9 months in 2009/2010. The generation entering primary school in 2007 was the first to attend 6 months of preschool, while the generation starting primary school in 2010 was the first to attend 9 months of preschool program in 2009/2010.

5 Results

5.1 Impact of RTA on Roma Children

Since the program was targeting Roma children, we first analyze the RTA impacts on them. Columns (1) and (2) of Table 4 report the program effects on the marks in Language and Math for children enrolled in the eight grade, column (3) on the probability of not taking the standardized exam, columns (4) and (5) on the marks in Language and Math among those students who take the standardized test and therefore finish primary school and columns (6) and (7) on the standardized test scores in Language and in Math. Bear in mind that not all students enrolled in the last academic year take the exam to finish primary school. Almost 17% of Roma children attending the eighth grade do not sit the final examination and have no test scores, as the reduction in the number of observations from columns (3) to (4) suggests. Columns (8) and (9) report the probability of enrolling in secondary school and, conditional on continuing the studies, of choosing a longer track. In Panel B we report the coefficients of the placebo regressions. All regressions control for students' characteristics and year and region fixed effects.

[Insert Table 4: Roma children]

Overall, the RTA effects are not statistically significant: the remedial program does not seem to be effective in improving Roma pupils' schooling attainments in the medium-run, nor in terms of marks neither of standardized test scores.[20] Roma in treated schools are also no more likely to keep studying than their counterparts in control schools due to the program. Nonetheless, if they go to secondary school, they are almost 17 percentage points more likely to choose a four-year secondary school track (either general or technical) versus a three-year technical track. In control schools, 54.5% of Roma students enrolled in secondary education choose a four-year secondary school track. In treated schools, thanks to the RTA, almost 71% chose a longer track.[21] The coefficients of the placebo regressions reported in Panel B are all

[20]The positive and statistically significant coefficients in Table 4 for *female* suggests that we might observe different impacts depending on students' gender: girls perform better in any outcome of interest. We therefore look at the RTA's effects on boys and girls: Table A.1 in the Appendix report them separately, by row. The reduction in the marks does not mask heterogeneity by gender: there are not statistically significant results. The effects on more demanding tracks are observed for both genders.

[21]The values unconditional of having sit at the final examination suggest the same pattern. They are not reported, but they are available upon request. Roma who reach the eight grade of primary education in treated schools are not more likely to keep studying but they are 17 percentage points more likely to choose a more demanding track. In control schools, 76% Roma students enrolled in secondary education. Conditional on enrolling, 52% choose a four-year secondary school track in control schools, while in treated schools this share is

not statistically significant, therefore validating the comparability of our control group.

5.1.1 Subjective versus objective grading

We can look more closely at the marks distribution to investigate if there are some program effects. Figure 3 reports the school mark distribution of Roma pupils in both Language and Math, before and after the introduction of the program, in treated (black) and control (gray) schools. In both subjects, after the implementation of the program, we observe a polarization of marks around 2, the sufficient grade to pass the course. It comes at the cost of less medium marks (3 and 4). This is especially the case in treated schools, where however the number of children who get an insufficient grade also increases. Interestingly, if we look at standardized test scores by mark received in the subject (Figure 4), we observe that Roma in treated schools perform better than their counterparts in control schools, and better than before the program. Such shift to the right in the score distribution is bigger for those children graded the lowest passing marks, 2 and 3. The impacts are stronger for Math than Language, but overall they seem to suggest that lower marks in treated schools than in control are not followed by worse performances in the final exam.

[Insert Figures 3 and 4: Marks and Standardized test scores of Roma children]

This, at first, puzzling evidence could suggest that in treated schools teachers mark more rigorously Roma pupils due to the program. In these schools, students who receive a pass grade are better prepared in the subjects examined, and students at the margin of passing a course are less likely to be over-graded. We test our hypothesis by estimating the following regression:

$$\begin{aligned} subjective_{ist} &= \beta_0 + \beta_1 treated_{st} + \beta_2 post_{st} + \beta_3 treated_{st} * post_{st} + \\ &\quad + \delta_1 objective_{ist} + \rho_1 X_{ist} + \mu_r + \gamma_t + \epsilon_{ist} \end{aligned} \tag{2}$$

where $subjective_{ist}$ stands for the marks in Language and Math of pupil i in school s at time t and $objective_{ist}$ for the corresponding test scores. We consider marks to be subjective evaluations because they possibly incorporate teachers' discriminative behavior as they are non-blind assessments. On the contrary, standardized tests are assessed by external teachers and

almost 70%.

are not expected to suffer from any bias. The remaining variables are as in specification (1). If the coefficient β_3 for the treatment effects is still significant once we control for the standardized test score in the same subject ($objective_{ist}$), then there is likely some discriminating behavior at play. There are two main reasons why teachers' assessment could differ from objective test scores. First, teachers' assessments take into account non-cognitive skills such as behaviour, which is not accounted for by standardized test scores (Cornwell et al., 2013). Second, in certain contexts teachers grade differently ethnic minorities: depending on their expectations, they can either over-grade or under-grade them (Burgess and Greaves, 2013). There is much anecdotal evidence suggesting that teachers over-grade Roma pupils due to lower expectations and that they see keeping them at school as the main challenge and goal. The presence of differential grading in Serbia is empirically confirmed by a study which shows that in the third grade of primary school Roma pupils take a lower test score than Non-Roma pupils conditional on teacher marks (Baucal, 2006). The same is confirmed in our data.

The results are presented in Table 5. A positive (negative) coefficient of β_3 would suggest that teachers in treated schools over-grade (down-grade) Roma students due to the program. Put differently, it compares pupils' marks in treated and control schools, conditional on having the same test score: if teachers' evaluations were to be fully objective, there should not be differences in the way students are graded in the two types of schools.

[Insert Table 5: Roma children: Marks and standardized test scores]

Our results suggest that, holding test scores constant in the two types of schools, Roma students in treated schools are graded lower due to the program than their counterparts in control schools (columns (1) and (2)). Although the coefficients are not statistically significant, their sign can indicate an adjustment of the marks to their true value. We speculate that the RTA program affected teachers in such a way that they over-grade fewer Roma students at the margin. Once the program is implemented, there is someone taking care of them and teachers may now raise their expectations and demand the same learning achievements as from Non-Roma pupils. The previous positive discrimination is now replaced by a more impartial behavior.[22]

[22] The last two columns of the Table A.1 in the Appendix report the results of our test in specification (2) by gender. They show that, among boys with the same standardized test score in Math, those in treated schools are marked 0.279 points less in the subject than those in control schools: 2.1 instead then 2.4. Students at the margin of passing a course are less likely to be over-graded due to the program. Plausible positive effects on cognitive skills for some subgroups of students are however not big enough to provide overall statistically significant impacts on the standardized test scores.

5.1.2 Intensity of the program

The percentage of Roma in school at application is a crucial pre-determined characteristic to understand the program impact. A previous study of the short-term impacts has shown that children in schools with fewer Roma benefit more from the program (Battaglia and Lebedinski, 2015). There is only one assistant per school and the higher is the number of Roma pupils she needs to interact with, the lower will be the intensity of the program. If the assistant cannot reach all Roma students, she is instructed to focus on the first four grades, and among them, the most disadvantaged Roma are those more likely to be helped. In Table 6 we report the impact of the RTA on the marks in Language and Math (columns (1) and (2)), on the probability of not taking the standardized exam (column (3)) and on the standardized test scores in Language and Math (columns (4) and (5)). Columns (6) and (7) report the effects on the probability of enrolling in secondary school and, conditional on continuing the studies, of choosing a longer track. The table documents the effects for the schools in the first quartile of the percentage of Roma in school at application (lower than 7.1% of Roma students). All the other quartiles are reported in Table A.3 in the Appendix: the second quartile includes schools with 7.1% to 11% of Roma at application, the third quartile schools with 11% to 14%, and the last quartile schools with more than 14% of Roma at application.

[Insert Table 6: First quartile of the percentage of Roma at application]

Among schools with a low percentage of Roma at application, we find lower marks and higher test scores in Math for treated students, in line with the story that Roma students are unlikely to be over-graded, but they learn more and perform better once properly tested. In treated schools, Roma students receive 0.348 less of a point in Math and 0.405 of a standard deviation more in the corresponding test than their counterparts in control schools. The worst students are consistently also more likely not to sit the final exam. The impacts on secondary schools outcomes remain positive, although less precisely estimated.[23]

Columns (3) and (4) of Table 5 report the impact of the RTA on the marks in Language and Math, conditional on the result in the corresponding standardized test scores. It shows the effects for Roma students in schools in the first quartile of the percentage of Roma at

[23] In schools with a high percentage of Roma at application, effects on standardized test scores smooth away: their coefficients are not anymore statistically significant. The higher is the number of Roma, the lower is the intensity of the program, since there is only one assistant per school, and the fewer its impacts. The results are in Table A.3 in the Appendix.

application. In schools with few Roma, among students with the same standardized test score in Math, those in treated schools are marked 0.610 points less in the subject than those in control schools (column (4)).[24] Students at the margin of passing a course are less likely to be over-graded due to the program, especially when the program is more intensively implemented.[25]

We acknowledge that schools with few or a lot of Roma students could differ in other ways that can also interact with the program and impact its success. These differences, however, are not invalidating our findings that the program has more success in schools with a smaller share of Roma.

5.1.3 Discussion of the mechanism of the program

We observe that overall the remedial program does not have transformative effects on Roma pupils schooling attainments in the medium-run, nor in terms of marks neither of standardized test scores, except for students in schools with a low percentage of Roma at application. Roma in treated schools are also no more likely to keep studying than their counterparts in control schools because of the program. Nonetheless, if they go to secondary school, they are more likely to choose a more demanding track.

This result is particularly important if we consider that choosing a four-year track allows to enter university, and that reaching this level of education is extremely rare for this minority. The remedial activities have not shown to change Roma students' average attainments, and the gap with Non-Roma pupils remains big. Nonetheless, we observe some unexpected behavior in grading and an improvement at the margin. Roma students get lower marks but higher test scores in Math in schools where the program is more intensively implemented. They are unlikely to be over-graded, but they learn more and perform better once externally tested. We speculate the RTA program affected teachers in such a way that they over-grade fewer Roma students at the margin of passing the course. With the program, there is someone taking care of them, and teachers may now raise their expectations and demand the same learning achievements as from Non-Roma pupils. We cannot exclude that lower marks result from higher stigmatization of Roma pupils in treated schools. Since the program targets a specific minority, one can be worried that it might stigmatize them with consequent negative effects. Yet, such explanations

[24]Recall that students at the eight grade who receive an insufficient mark in a subject at the end of the academic year (May) are expected to take a make-up exam for that subject in June. The coefficient for treatment in column (4) suggests that on average students receiving the same test score, if in treated schools, were receiving 1.7 points (the mean in the control group is 2.313), that is, they were expected to take a make-up exam and passed it.

[25]All the other quartiles are reported in Table A.2.

would not explain positive effects on standardized test score nor on the likelihood of choosing a more demanding track in secondary education.

Such more impartial behavior on the teacher side, if there, is however not enough to explain our overall results. Roma people usually attain very low education and their upper secondary school completion rates are much lower than Non-Roma. There is a clear low investment in education among them because of financial constraints, barriers of access to education, or low expectations for schooling to give them enough future opportunities. Having someone from their community who had to invest in education in the first place and, thanks to such an investment, has got a good full-time job in the formal sector may encourage them to follow a similar path. The teaching assistant is an important reference point for the community and can act as a role model. By sharing her successful experience with students, she can motivate them to believe that they can achieve analogous results. Graduating in a four-year secondary school track can be the first step since it is a requirement for entering higher education.

5.2 Spillover effects: Impact of RTA on Non-Roma Children

We then turn to Non-Roma children. The assistants do not work with them, and none of these students are directly treated by the RTA. Nonetheless, Roma and Non-Roma children are attending the same classes, interacting with each other in the school, and we can think of several channels for spillover effects in this setting. First, the program changes the quality of the peers of Non-Roma pupils and this could have direct effects. Then, the program may also free up teachers' time so they can work more with the low-performing Non-Roma students and it can affect teachers' behavior, e.g. evaluations of both Roma and Non-Roma pupils in their classes.

Table 7 reports the impacts for the entire sample of Non-Roma (Panel A), placebo effects for the eight graders not affected by the policy (Panel B), and the effects looking at the first quartile of the percentage of Roma in school at application (Panels C).[26] The percentage of Roma at application could be as relevant for Non-Roma as for Roma children: the more Roma pupils at school, the weaker the impact of the program, the smaller the spillover effects for Non-Roma children and/or the time allocated to untreated low-performing students. We report the impact of the RTA on the marks in Language and Math (columns (1) and (2)), on the probability that students do not take the standardized exam (column (3)), on the standardized test scores in

[26] All the other quartiles are reported in Table A.5 in the Appendix.

Language and Math (columns (4) and (5)), and on the probability of enrolling in secondary school and, conditional on continuing the studies, of choosing a longer track (columns (6) and (7)).

[Insert Table 7: Non-Roma children]

Overall, the results are not statistically significant, except for the test in Language. Non-Roma children in treated schools perform 0.156 of a standard deviation more in the Language standardized test. They are neither more likely to enroll in secondary education nor more likely to choose a four-year secondary school track than their counterparts in control schools. Yet, this is not particularly surprising since, on average, in control schools, almost 95% of Non-Roma continue to study in secondary education and 82% of them choose a four-year secondary school track.[27] The coefficients of the placebo regressions are not statistically significant, therefore validating the comparability of our control group.[28] Largely, the program seems not to have had big significant impacts on Non-Roma students, although there are some positive spillovers.

The results on marks and standardized tests are confirmed if we deepen the analysis and look at which point of the school mark distribution we can observe the biggest RTA effects. As shown in Figure 5, there are no relevant differences in neither subject between Non-Roma students in treated and control schools, before and after the introduction of the program. We observe some improvements in treated schools in Language, since we find a small shift from grade 2 to grade 3, but not much differences in standardized tests (Figure 6).

[Insert Figures 5 and 6: Marks and Standardized test scores of Non-Roma children]

As before, we test whether there is some evidence of over-grading induced by the program. Table A.4 in the Appendix shows that teachers are more consistent and hence objective when evaluating their Non-Roma students. Coefficients for treatment in columns (1) and (2) are modest and much smaller than in the case of Roma and are not statistically significant.[29]

[27]The values unconditional of having sit at the final examination suggest the same pattern. They are not reported, but they are available upon request. There are not statistically significant differences. In control schools, on average almost 94% of students go to secondary school and 80% of them choose a four-year track. These data are totally comparable with the whole population of students in Serbia in the period 2014 to 2018. Among pupils enrolled in the 8th grade of primary school, 98.47% sit the final exam. Similarly, among those enrolled in the 8th grade, 92.89% continue to secondary school and 75.91% choose four-year tracks (authors' calculation from MoESTD)).

[28]There is one exception for the probability of choosing a more demanding track.

[29]Table A.6 in the Appendix reports the effects for Non-Roma boys and girls separately, by row. The effects of RTA are not heterogeneous by gender, except for the probability of choosing a more demanding track. The last two columns of the table report the results of our test in specification (2). All coefficients are small and not statistically significant. We find that Non-Roma girls are more likely to choose a four-year track.

If we turn to the intensity of the program, we observe that being in a school with a low percentage of Roma students at application has a positive effect also for Non-Roma pupils, on both marks and standardized test scores. As reported in Panel C in Table 7, students in treated schools have on average 0.176 of a point more in Language than their counterparts in control schools, and 0.310 and 0.433 of a standard deviation more in Language and Math test score, respectively. They are also 8.6 percentage points more likely to choose a four-year track in schools with few Roma, reaching the 90% of students in treated schools.[30]

Overall, we can conclude that in the medium-term there are significant effects of the program also on the not targeted group especially in those schools where its intensity is higher, that is, where there are fewer Roma students per assistant. It can either be the result of positive peer learning spillover effects of the program on Non-Roma students or of more time now devoted by teachers to those among them who under perform.

5.3 Characteristics of the assistants

We are finally interested in understanding if the characteristics of the assistant can be important for our outcomes of interest. We would like to see which traits of the assistant correlate with better educational achievements of the pupils. We know the gender of the assistant, whether she has a university degree and her work experience in the school, in the year 2015, for 64 treated schools. Slightly more than half of the assistants are female (52%), almost 23% of them received a university degree and they have on average 4.5 years of experience in the school where they are currently working (median 4.35 years).

Table 8 reports the coefficients for the different outcomes of interest: marks in Language (columns (1) to (6)), in Math (columns (7) to (12)), probability of not sitting the final exam (columns (13) to (18)), standardized tests scores in Language (columns (19) to (24)) and Math (columns (25) to (30)), probability of enrolling in secondary school (columns (31) to (36)) and of choosing a longer track (columns (37) to (42)). Note that these findings reflect what we have already shown previously in Table 4 with respect to these outcomes. However, here we would like to examine whether any characteristic of the assistant is correlated with the outcomes and hence whether some types of assistants are more successful.

[Insert Table 8: Characteristics of the assistant]

[30]Moreover, in schools in the first quartile there is less evidence of discriminating behaviors (columns (3) to (4) of Table A.4 in the Appendix). The coefficient is half in magnitude than the one in the case of Roma and is statistically significant only at 10%.

Overall, teachers' characteristics seem to be weakly relevant, except for secondary school decisions. The gender of the assistant is on average not important, aside from standardized test scores in Language: having a female assistant increases test scores in Language in treated schools by 0.2 of a standard deviation, compared to control schools. The probability of studying in a four-year secondary education track is not different in schools with a female assistant compared to schools with a male assistant: the p-value of the difference between the coefficients in columns (37) and (38) is 0.1457. In both cases, students in treated schools are more likely to choose more difficult tracks, compared to students in control schools. Conversely, having an assistant with a university degree increases the probability of going to secondary school by 5 percentage points and of choosing a longer track by 19 percentage points[31], compared to not having received the treatment. Finally, years spent in the same school are important, especially for secondary school decisions. If the assistant has more years than the median, students are 5.9 percentage points more likely to go to secondary school, although less experienced and possibly younger assitants matter more for the choice of the track. The results hence suggest that pupils, whose assistants are more educated and spent more time in the same school are more likely to keep studying. Such evidence could again point at the importance of a role model: having someone from your own community who, thanks to education, has succeeded, encourages you to follow a similar pattern. Graduating in a four-year secondary school track can be the first step since it is a requirement for entering higher education.[32]

6 Conclusions

The Roma Teaching Assistant program is one of the main program in Eastern Europe targeting the Roma population and aiming at increasing their educational attainment. We studied its impacts on children 5 to 8 years after its first implementation. Our study complements the results obtained in the short-term by Battaglia and Lebedinski (2015) which have shown that the program improved school attendance and that younger children benefited more from it.

[31] Having an assistant who holds a university degree matters for the decision to study in a longer secondary school track. The p-value of the difference between the coefficients in columns (39) and (40) is 0.0000.

[32] If we concentrate our attention on the assistant's gender, we can further investigate its interaction with the student's gender. As reported in Table A.7 in the Appendix, girls' test scores in Language increase by 0.395 of a standard deviation when their assistant is a woman, while boys' test scores in Math increases by 0.352 of a standard deviation when their assistant is a man. There is a same-gender effect already observed in the literature. Having a female assistant also increases the probability of going to secondary school and of choosing a longer track for girls. For boys, the gender of the assistant is less relevant for these outcomes. These results provide suggestive evidence on the existence of a role model mechanism, especially for girls. They identify with female assistants.

Overall, the RTA does not have strong statistically significant effects on educational outcomes of treated students in the medium-term: marks and standardized test scores in Language and Math do not improve as a consequence of the program. However, a group of pupils does reap the benefits of the program and these are students in schools with a low percentage of Roma at application. In these schools, Roma students receive 0.405 of a standard deviation higher test score in Math than their counterparts in control schools, and Non-Roma students perform between 0.31 and 0.433 of a standard deviation better in standardized test scores compared to their counterparts. Disappointing average effects are also compensated by reassuring impacts on the probability of choosing more demanding tracks in secondary school. Roma students in treated schools were 16.8 percentage points more likely to choose four-year tracks than their counterparts in control schools.

Roma ethnic minority has extremely low educational attainment and high poverty levels. A necessary condition to ensure a better life of the young generation of Roma is to provide them with an education that can help them rise the economic ladder. Remedial education programs as the RTA can help disadvantaged students escape the vicious cycle of poverty and attain better jobs than the generation of their parents. Since the assistant belongs to the same minority, she can act as an important reference for her community and as a role model for her students. Investing in education to achieve better future outcomes can appear now feasible and relevant. Our results suggest that, in the medium-term, more Roma students enrol in higher demanding tracks thanks to such program and that there can be some marginal improvements in learning, depending on the intensity of the program. Positive impacts are clearly observed when intensity is higher, both for the targeted group and, thanks to spillover effects, for their schoolmates. We also observe that pupils with assistants holding a university degree or with higher years of experience in the school were more likely to keep studying and to choose more demanding tracks. Nonetheless, the overall effects of RTA are quite modest and limited to schools with a small percentage of Roma, and the program is not achieving its aim of raising educational attainments of all Roma pupils. Our results show that this program is insufficient to bridge the gap between Roma and Non-Roma pupils and that additional measures are required to achieve the goal of integration for this disadvantaged minority.

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7 Tables

Table 1: Balancing of school characteristics of treated and control schools at application in 2008 - Roma and Non-Roma

	Treated		Control		Diff.	P-value
	(1)	(2)	(3)	(4)	(5)[(1)-(3)]	(6)
	Mean	Std. dev	Mean	Std. dev.		
Panel A: Before balancing - Roma						
Percentage of Roma in school at appl. (2008 or 2009)	0.169	0.136	0.175	0.215	-0.006	0.921
Language grade in 8th year	2.887	1.078	2.740	1.017	0.147	0.210
Mathematics grade in 8th year	2.511	0.918	2.423	0.865	0.087	0.356
Dropout in 8th grade (=1)	0.372	0.484	0.459	0.500	-0.087	0.223
Standardised testscore Language	-0.844	0.922	-0.874	0.915	0.030	0.843
Standardised testscore Mathematics	-0.761	0.944	-0.846	0.807	0.085	0.451
Enrolled in secondary school (=1)	0.963	0.189	0.991	0.097	-0.027*	0.063
Enrolled in 4y sec. school (=1)	0.680	0.467	0.705	0.458	-0.025	0.658
Observations	734		196			
Number of schools	73		48			
Panel B: After balancing - Roma						
Percentage of Roma in school at appl. (2008 or 2009)	0.167	0.133	0.125	0.094	0.042	0.145
Language grade in 8th year	2.914	1.078	2.818	1.105	0.096	0.485
Mathematics grade in 8th year	2.530	0.922	2.505	0.951	0.025	0.859
Dropout in 8th grade (=1)	0.353	0.478	0.345	0.477	0.009	0.882
Standardised testscore Language	-0.844	0.922	-0.747	0.904	-0.097	0.643
Standardised testscore Mathematics	-0.761	0.944	-0.778	0.754	0.017	0.883
Enrolled in secondary school (=1)	0.963	0.189	0.988	0.109	-0.025	0.131
Enrolled in 4y sec. school (=1)	0.680	0.467	0.744	0.438	-0.064	0.250
Observations	713		164			
Number of schools	64		39			
Panel C: Before balancing - Non-Roma						
Percentage of Roma in school at appl. (2008 or 2009)	0.130	0.083	0.112	0.071	0.018	0.134
Language grade in 8th year	3.723	1.176	3.674	1.173	0.049	0.436
Mathematics grade in 8th year	3.377	1.219	3.345	1.226	0.032	0.620
Dropout in 8th grade (=1)	0.125	0.331	0.178	0.382	-0.052**	0.034
Standardised testscore Language	-0.093	0.996	-0.156	1.002	0.063	0.474
Standardised testscore Mathematics	-0.033	1.015	-0.154	0.975	0.120	0.241
Enrolled in secondary school (=1)	0.984	0.126	0.990	0.100	-0.006*	0.070
Enrolled in 4y sec. school (=1)	0.872	0.334	0.870	0.336	0.002	0.894
Observations	6345		2506			
Number of schools	73		48			
Panel D: After balancing - Non-Roma						
Percentage of Roma in school at appl. (2008 or 2009)	0.128	0.082	0.109	0.057	0.019	0.148
Language grade in 8th year	3.738	1.177	3.672	1.174	0.066	0.412
Mathematics grade in 8th year	3.379	1.221	3.403	1.225	-0.024	0.789
Dropout in 8th grade (=1)	0.121	0.327	0.153	0.360	-0.032	0.221
Standardised testscore Language	-0.092	0.998	-0.115	0.999	0.023	0.829
Standardised testscore Mathematics	-0.041	1.014	-0.090	0.969	0.049	0.687
Enrolled in secondary school (=1)	0.984	0.127	0.991	0.096	-0.007*	0.061
Enrolled in 4y sec. school (=1)	0.874	0.331	0.844	0.363	0.031	0.198
Observations	5935		2037			
Number of schools	64		39			

Robust standard errors corrected for clustering at the school level are: * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2: Roma - Means of covariates and outcomes in pre-treatment and treatment years

	Pre-treatment year (2008)			Treatment years (2014-2018)		
	Treated Schools (1)	Comparison Schools (2)	Difference (3)[(1)-(2)]	Treated Schools (4)	Comparison Schools (5)	Difference (6)[(4)-(5)]
Characteristics						
Female (=1)	0.452	0.504	-0.053	0.481	0.514	-0.033
			(0.052)			(0.036)
Pupil born in different district (=1)	0.163	0.183	-0.021	0.112	0.147	-0.034*
			(0.067)			(0.020)
Age at test	15.164	15.145	0.019	15.159	15.088	0.071
			(0.052)			(0.059)
Class size	25.749	19.146	6.603***	20.433	17.095	3.338**
			(1.621)			(1.579)
Outcomes						
Language grade in 8th year	2.914	2.818	0.096	2.450	2.530	-0.079
			(0.137)			(0.121)
Mathematics grade in 8th year	2.530	2.505	0.025	2.209	2.240	-0.030
			(0.139)			(0.062)
Dropout in 8th grade (=1)	0.353	0.345	0.009	0.118	0.102	0.015
			(0.059)			(0.034)
Standardised test score Language	-0.844	-0.747	-0.097	-1.186	-1.280	0.094
			(0.209)			(0.117)
Standardised test score Mathematics	-0.761	-0.778	0.017	-1.099	-1.211	0.112
			(0.116)			(0.069)
Enrolled in secondary school (=1)	0.963	0.988	-0.024	0.852	0.861	-0.009
			(0.016)			(0.028)
Enrolled in 4y sec. school (=1)	0.680	0.744	-0.064	0.524	0.490	0.033
			(0.056)			(0.063)
Observations	713	164		2593	665	
Number of schools	64	39		64	39	

Robust standard errors corrected for clustering at the school level are reported in parentheses: * significant at 10%, ** significant at 5%, *** significant at 1%.
[a] Marks range from 1 (worst) to 5 (best). They are categorical.

Table 3: Non-Roma - Means of covariates and outcomes in pre-treatment and treatment years

	Pre-treatment year (2008)			Treatment years (2014-2018)		
	Treated Schools (1)	Comparison Schools (2)	Difference (3)[(1)-(2)]	Treated Schools (4)	Comparison Schools (5)	Difference (6)[(4)-(5)]
Characteristics						
Female (=1)	0.487	0.483	0.004	0.487	0.479	0.008
			(0.021)			(0.008)
Pupil born in different district (=1)	0.131	0.148	-0.017	0.114	0.127	-0.012
			(0.025)			(0.022)
Age at test	15.017	15.031	-0.013	14.900	14.892	.008
			(0.012)			(0.010)
Class size	26.201	19.839	6.362***	22.409	19.308	3.101***
			(1.361)			(0.944)
Outcomes						
Mother tongue grade in 8th year	3.738	3.672	0.066	3.776	3.714	0.061
			(0.080)			(0.059)
Mathematics grade in 8th year	3.379	3.403	-0.024	3.383	3.351	0.032
			(0.088)			(0.066)
Dropout in 8th grade (=1)	0.121	0.153	-0.032	0.009	0.009	0.000
			(0.026)			(0.002)
Standardised test score Language	-0.092	-0.115	0.023	-0.024	-0.201	0.177**
			(0.107)			(0.081)
Standardised test score Mathematics	-0.041	-0.090	0.049	-0.053	-0.244	0.192**
			(0.122)			(0.068)
Enrolled in secondary school (=1)	0.984	0.991	-0.007	0.945	0.945	0.000
			(0.004)			(0.008)
Enrolled in 4y sec. school (=1)	0.874	0.844	0.031	0.861	0.810	0.051***
			(0.024)			(0.016)
Observations	5935	2037		24692	8161	
Number of schools	64	39		64	39	

Robust standard errors corrected for clustering at the school level are reported in parentheses: * significant at 10%, ** significant at 5%, *** significant at 1%.
[a] Marks range from 1 (worst) to 5 (best). They are categorical.

Table 4: Roma children

	All students		Dropout	Students finishing primary		Stand. Test Scores		Secondary School	
	Language	Math		Language	Math	Language	Math	Secondary	Four-year track
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Main results									
treatedpost	-0.114	-0.052	-0.016	-0.116	-0.047	0.299	0.153	0.039	0.168***
	(0.170)	(0.147)	(0.056)	(0.166)	(0.170)	(0.230)	(0.134)	(0.025)	(0.063)
treated	0.100	0.064	0.035	0.098	0.066	-0.248	-0.063	-0.046**	-0.135**
	(0.129)	(0.119)	(0.065)	(0.142)	(0.140)	(0.206)	(0.116)	(0.018)	(0.063)
post	-0.380**	-0.363**	-0.196***	-0.489***	-0.456***	-0.582**	-0.468***	-0.109***	-0.205***
	(0.182)	(0.150)	(0.047)	(0.155)	(0.160)	(0.231)	(0.119)	(0.021)	(0.060)
female	0.383***	0.140***	0.013	0.456***	0.165***	0.273***	-0.015	-0.014	0.122***
	(0.045)	(0.034)	(0.016)	(0.051)	(0.035)	(0.046)	(0.029)	(0.013)	(0.022)
Observations	4135	4135	4135	3443	3443	3443	3443	3443	2995
Mean in control	2.551	2.297	0.162	2.689	2.407	-1.189	-1.092	0.866	0.545
Panel B: Placebo 2009 to 2011 versus 2008									
treatedpost	-0.270	-0.134	0.083	-0.091	-0.017	0.143	0.158	0.048	0.039
	(0.168)	(0.159)	(0.054)	(0.162)	(0.184)	(0.237)	(0.184)	(0.030)	(0.076)
Observations	2611	2611	2613	1570	1570	1570	1570	1114	1072
Mean in control	2.556	2.295	0.410	2.795	2.470	-1.179	-1.079	0.955	0.661

Notes: The table presents the treatment effects on education outcomes for Roma children. All regressions control for an indicator variable for the year of the survey and region fixed effects. "Treated" is a dummy variable equal to 1 if the student was enrolled in a school where the remedial education program was introduced. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table 5: Roma children: Marks and standardized test scores

			First quartile	
	Language	Math	Language	Math
	(1)	(2)	(3)	(4)
Standardised testscore Language	0.477***		0.487***	
	(0.031)		(0.052)	
treatedpost	-0.259	-0.114	-0.207	-0.610***
	(0.170)	(0.161)	(0.247)	(0.188)
treated	0.217	0.094	0.148	0.611***
	(0.173)	(0.144)	(0.336)	(0.183)
post	-0.212	-0.251*	-0.374**	0.018
	(0.164)	(0.146)	(0.164)	(0.120)
Standardised testscore Math		0.437***		0.511***
		(0.031)		(0.049)
Observations	3443	3443	593	593
Mean in control	2.689	2.407	2.725	2.313

Notes: The table presents the treatment effects on education outcomes for Roma children, controlling for the standardized test score. All regressions control for an indicator variable for the year of the survey and region fixed effects. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table 6: Roma children: First quartile of the percentage of Roma at application

				Stand. Test Scores		Secondary School	
	Language	Math	Dropout	Language	Math	Secondary	Four-year track
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
treatedpost	-0.144	-0.348**	0.184*	0.156	0.405**	0.042	0.219
	(0.274)	(0.156)	(0.096)	(0.233)	(0.160)	(0.049)	(0.203)
treated	0.168	0.348**	-0.175	0.010	-0.418***	-0.023	-0.235
	(0.303)	(0.140)	(0.103)	(0.302)	(0.121)	(0.029)	(0.162)
post	-0.522**	-0.253**	-0.291***	-0.446**	-0.598***	-0.090**	-0.217
	(0.234)	(0.103)	(0.099)	(0.204)	(0.104)	(0.041)	(0.202)
female	0.397***	0.102	-0.012	0.245***	-0.052	0.015	0.105**
	(0.066)	(0.065)	(0.028)	(0.082)	(0.063)	(0.016)	(0.042)
Observations	682	682	682	593	593	593	540
Mean in control	2.660	2.266	0.149	-1.247	-1.029	0.906	0.552

Notes: The table presents the treatment effects on education outcomes for Roma children. All regressions control for an indicator variable for the year of the survey and region fixed effects. "Treated" is a dummy variable equal to 1 if the student was enrolled in a school where the remedial education program was introduced. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table 7: Non-Roma children

				Stand. Test Scores		Secondary School	
	Language	Math	Dropout	Language	Math	Secondary	Four-year track
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main results							
treatedpost	-0.002	0.051	0.027	0.156**	0.161	0.005	0.030
	(0.085)	(0.063)	(0.026)	(0.077)	(0.117)	(0.010)	(0.020)
Observations	40824	40824	40825	39453	39453	39453	37557
Mean in control	3.712	3.368	0.042	-0.169	-0.178	0.951	0.826
Panel B: Placebo 2009 to 2011 versus 2008							
treatedpost	-0.072	0.003	0.018	0.045	0.064	0.005	-0.033*
	(0.092)	(0.069)	(0.017)	(0.072)	(0.113)	(0.005)	(0.019)
Observations	31822	31822	31825	28649	28649	21019	20723
Mean in control	3.705	3.382	0.125	-0.149	-0.144	0.989	0.881
Panel C: First quartile of Roma at application							
treatedpost	0.176*	0.029	0.026	0.310**	0.433**	-0.009	0.086***
	(0.095)	(0.121)	(0.047)	(0.145)	(0.182)	(0.019)	(0.030)
Observations	11988	11988	11989	11580	11580	11580	10991
Mean in control	3.712	3.242	0.043	-0.197	-0.124	0.953	0.818

Notes: The table presents the treatment effects on education outcomes for Non-Roma children. All regressions control for an indicator variable for the year of the survey and region fixed effects. "Treated" is a dummy variable equal to 1 if the student was enrolled in a school where the remedial education program was introduced. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table 8: Characteristics of the assistant

	Dropout																		Stand. Test Scores					
	Language						Math												Language					
	Assistant		University		Tenure		Assistant		University		Tenure		Assistant		University		Tenure		Assistant		University		Tenure	
	Male	Female	Without	With	Longer	Shorter	Male	Female	Without	With	Longer	Shorter	Male	Female	Without	With	Longer	Shorter	Male	Female	Without	With	Longer	Shorter
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
treatedpost	-0.100	-0.129	-0.097	-0.101	-0.155	-0.070	-0.032	-0.065	-0.032	-0.078	-0.083	0.005	-0.029	-0.010	-0.043	0.051	-0.037	-0.015	0.009	0.254*	0.105	0.252	0.128	0.155
	(0.173)	(0.174)	(0.167)	(0.204)	(0.185)	(0.163)	(0.146)	(0.160)	(0.146)	(0.185)	(0.150)	(0.153)	(0.069)	(0.057)	(0.060)	(0.062)	(0.061)	(0.063)	(0.157)	(0.147)	(0.141)	(0.170)	(0.148)	(0.160)
treated	0.098	0.077	0.088	0.001	0.121	0.038	0.090	0.031	0.048	0.069	0.098	-0.000	0.066	-0.002	0.073	-0.057	0.081	0.009	0.125	-0.220	-0.023	-0.168	-0.068	-0.065
	(0.134)	(0.148)	(0.129)	(0.179)	(0.151)	(0.132)	(0.121)	(0.133)	(0.120)	(0.159)	(0.128)	(0.128)	(0.081)	(0.062)	(0.070)	(0.065)	(0.077)	(0.068)	(0.130)	(0.143)	(0.127)	(0.156)	(0.145)	(0.140)
post	-0.363*	-0.371**	-0.384**	-0.339*	-0.394**	-0.339*	-0.347**	-0.383**	-0.355**	-0.375**	-0.396**	-0.331**	-0.200***	-0.198***	-0.201***	-0.194***	-0.191***	-0.201***	-0.467***	-0.512***	-0.465***	-0.530***	-0.534***	-0.447***
	(0.192)	(0.180)	(0.185)	(0.183)	(0.191)	(0.178)	(0.154)	(0.156)	(0.152)	(0.158)	(0.153)	(0.154)	(0.049)	(0.046)	(0.047)	(0.049)	(0.049)	(0.047)	(0.120)	(0.120)	(0.119)	(0.122)	(0.121)	(0.119)
Observations	2742	2396	3506	1583	2460	2678	2742	2396	3506	1583	2460	2678	2742	2396	3506	1583	2460	2678	2248	2039	2892	1350	2023	2264
Mean in control	2.551	2.551	2.551	2.551	2.551	2.551	2.297	2.297	2.297	2.297	2.297	2.297	0.162	0.162	0.162	0.162	0.162	0.162	-1.092	-1.092	-1.092	-1.092	-1.092	-1.092
Placebo 2009 to 2011 versus 2008																								
treatedpost	-0.090	-0.235	-0.150	-0.250	-0.228	-0.116	-0.039	-0.100	-0.073	-0.104	-0.136	-0.022	0.050	0.112**	0.053	0.161**	0.055	0.092	0.049	0.315	0.171	0.110	0.076	0.251
	(0.191)	(0.183)	(0.182)	(0.201)	(0.201)	(0.171)	(0.177)	(0.176)	(0.169)	(0.193)	(0.183)	(0.166)	(0.065)	(0.050)	(0.056)	(0.061)	(0.059)	(0.058)	(0.213)	(0.189)	(0.188)	(0.213)	(0.201)	(0.210)
Observations	1681	1512	2222	943	1556	1637	1681	1512	2222	943	1556	1637	1683	1514	2224	945	1558	1639	1006	916	1309	589	924	998
Mean in control	2.556	2.556	2.556	2.556	2.556	2.556	2.295	2.295	2.295	2.295	2.295	2.295	0.410	0.410	0.410	0.410	0.410	0.410	-1.079	-1.079	-1.079	-1.079	-1.079	-1.079

	Stand. Test Scores						Secondary						Four-year Track					
	Math																	
	Assistant		University		Tenure		Assistant		University		Tenure		Assistant		University		Tenure	
	Male	Female	Without	With	Longer	Shorter	Male	Female	Without	With	Longer	Shorter	Male	Female	Without	With	Longer	Shorter
	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)	(41)	(42)
treatedpost	0.290	0.237	0.249	0.303	0.317	0.235	0.045	0.042	0.037	0.058*	0.059*	0.030	0.150**	0.192***	0.153**	0.190**	0.108	0.230***
	(0.237)	(0.237)	(0.231)	(0.255)	(0.238)	(0.240)	(0.036)	(0.026)	(0.030)	(0.030)	(0.033)	(0.028)	(0.074)	(0.066)	(0.067)	(0.071)	(0.070)	(0.068)
treated	-0.238	-0.191	-0.239	-0.170	-0.345	-0.145	-0.081***	-0.025	-0.057***	-0.039*	-0.069**	-0.037**	-0.106	-0.166***	-0.120*	-0.157**	-0.099	-0.166**
	(0.212)	(0.206)	(0.210)	(0.214)	(0.222)	(0.209)	(0.025)	(0.019)	(0.020)	(0.022)	(0.027)	(0.018)	(0.071)	(0.062)	(0.063)	(0.078)	(0.070)	(0.064)
post	-0.545**	-0.672***	-0.571**	-0.656***	-0.649***	-0.572**	-0.093***	-0.113***	-0.102***	-0.100***	-0.102***	-0.105***	-0.200***	-0.217***	-0.198***	-0.224***	-0.200***	-0.214***
	(0.235)	(0.226)	(0.231)	(0.228)	(0.232)	(0.229)	(0.024)	(0.023)	(0.021)	(0.028)	(0.024)	(0.023)	(0.062)	(0.064)	(0.060)	(0.063)	(0.062)	(0.060)
Observations	2248	2039	2892	1350	2023	2264	2248	2039	2892	1350	2023	2264	1925	1807	2494	1196	1764	1968
Mean in control	-1.189	-1.189	-1.189	-1.189	-1.189	-1.189	0.866	0.866	0.866	0.866	0.866	0.866	0.545	0.545	0.545	0.545	0.545	0.545
Placebo 2009 to 2011 versus 2008																		
treatedpost	0.272	0.096	0.191	0.046	0.137	0.196	0.025	0.033	0.043	0.045	0.027	0.037	0.167*	-0.016	0.120	-0.117	0.084	0.014
	(0.287)	(0.273)	(0.266)	(0.278)	(0.280)	(0.281)	(0.039)	(0.034)	(0.030)	(0.039)	(0.034)	(0.036)	(0.095)	(0.083)	(0.080)	(0.077)	(0.085)	(0.091)
Observations	1006	916	1309	589	924	998	709	652	920	425	665	696	672	637	880	413	640	669
Mean in control	-1.179	-1.179	-1.179	-1.179	-1.179	-1.179	0.955	0.955	0.955	0.955	0.955	0.955	0.661	0.661	0.661	0.661	0.661	0.661

Notes: The table presents the treatment effects on education outcomes for Roma children depending on the characteristics of the assistant. All regressions control for an indicator variable for the year of the survey and region fixed effects. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

8 Figures

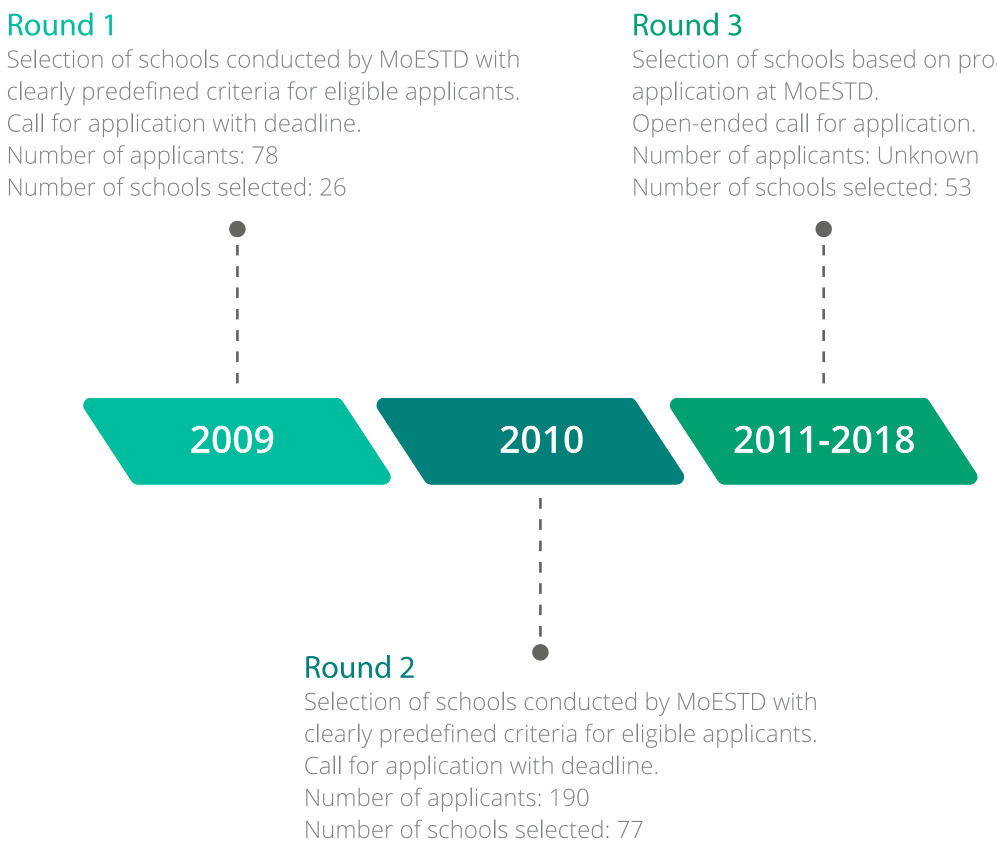

Figure 1: Timeline

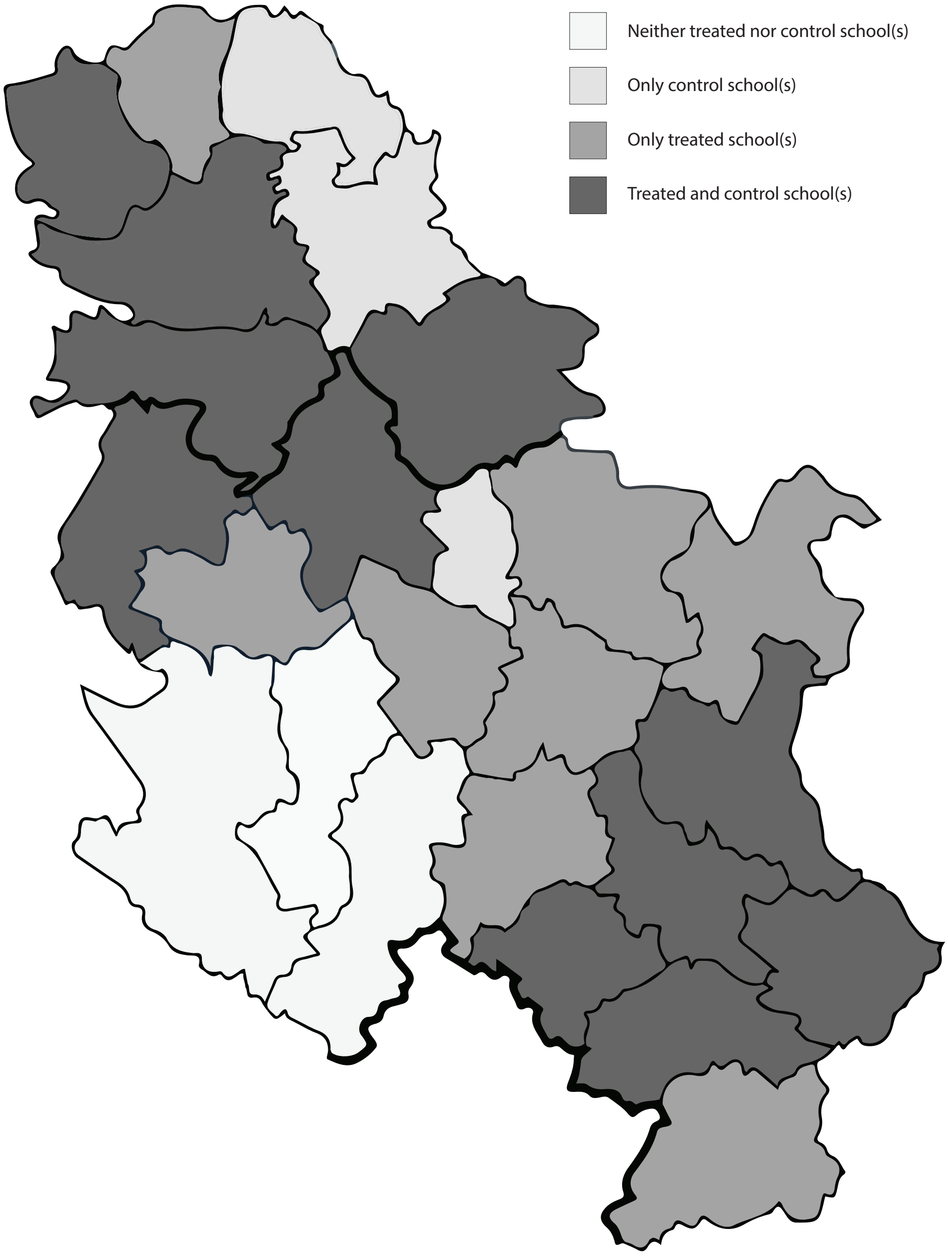

Figure 2: Geographical distribution of treated and control schools (at the district level)

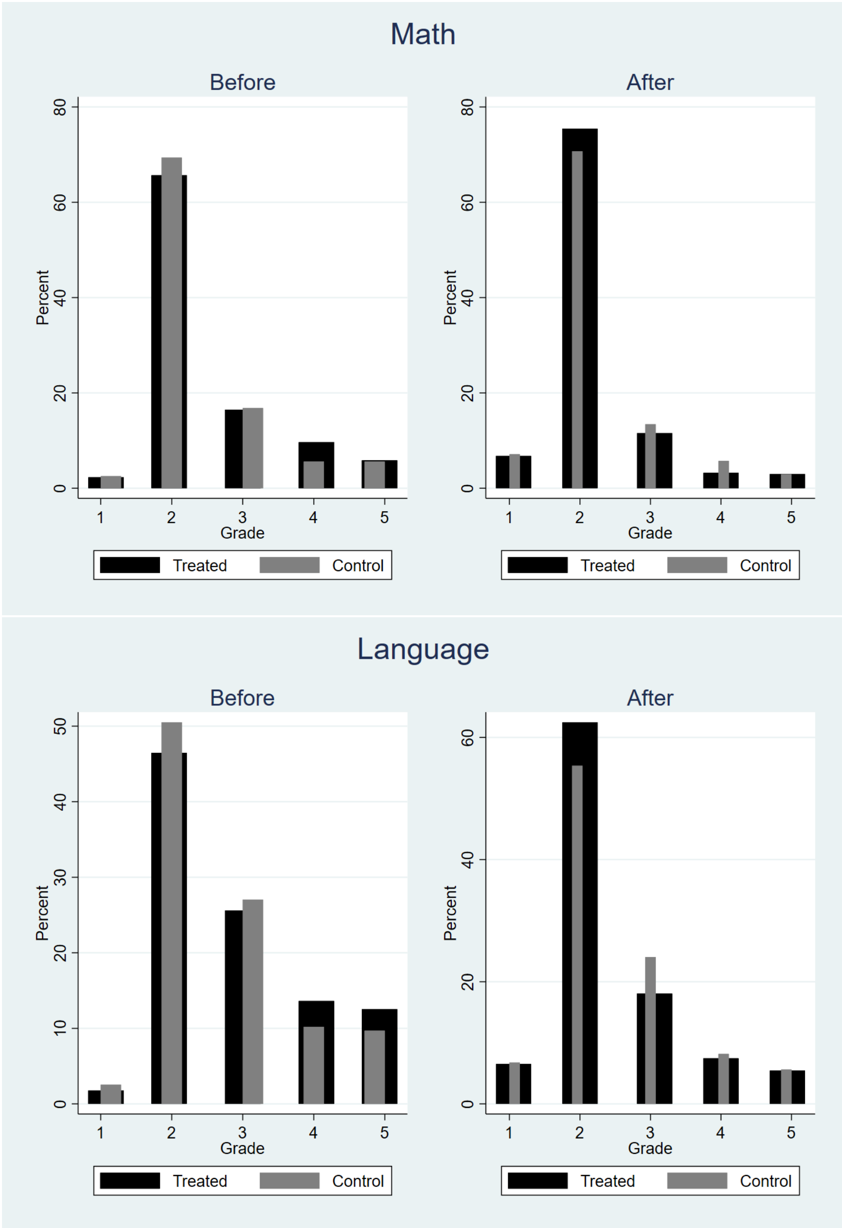

Figure 3: Marks of Roma people

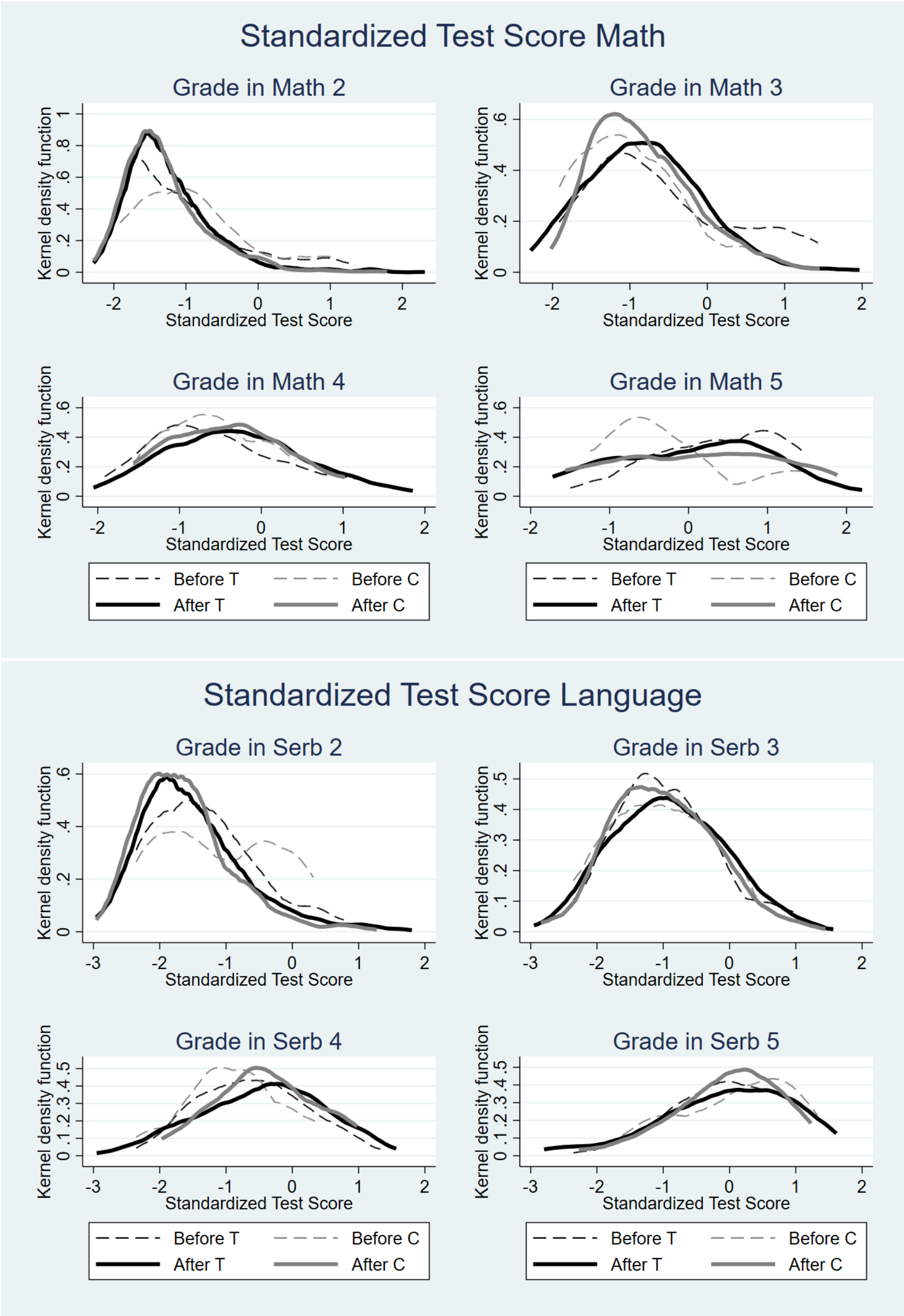

Figure 4: Standardized test score of Roma people

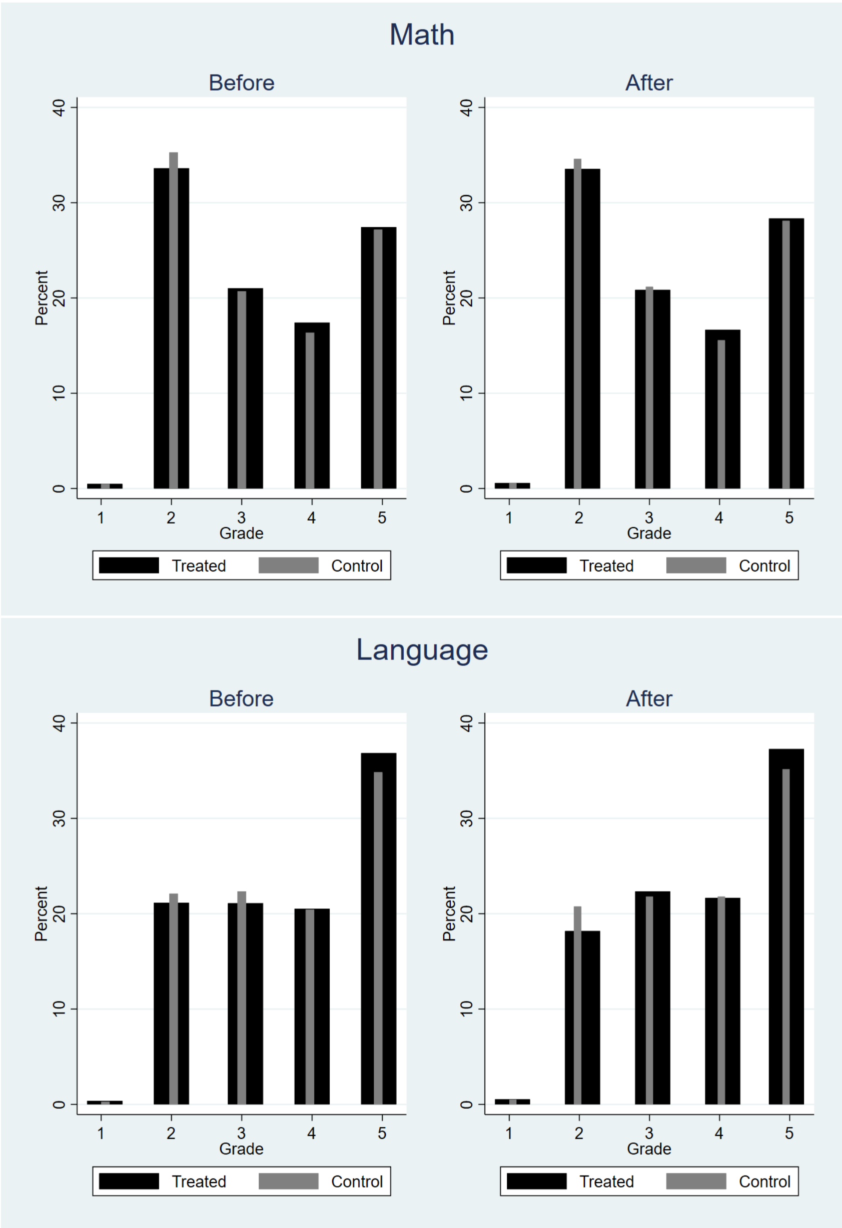

Figure 5: Marks of Non-Roma people

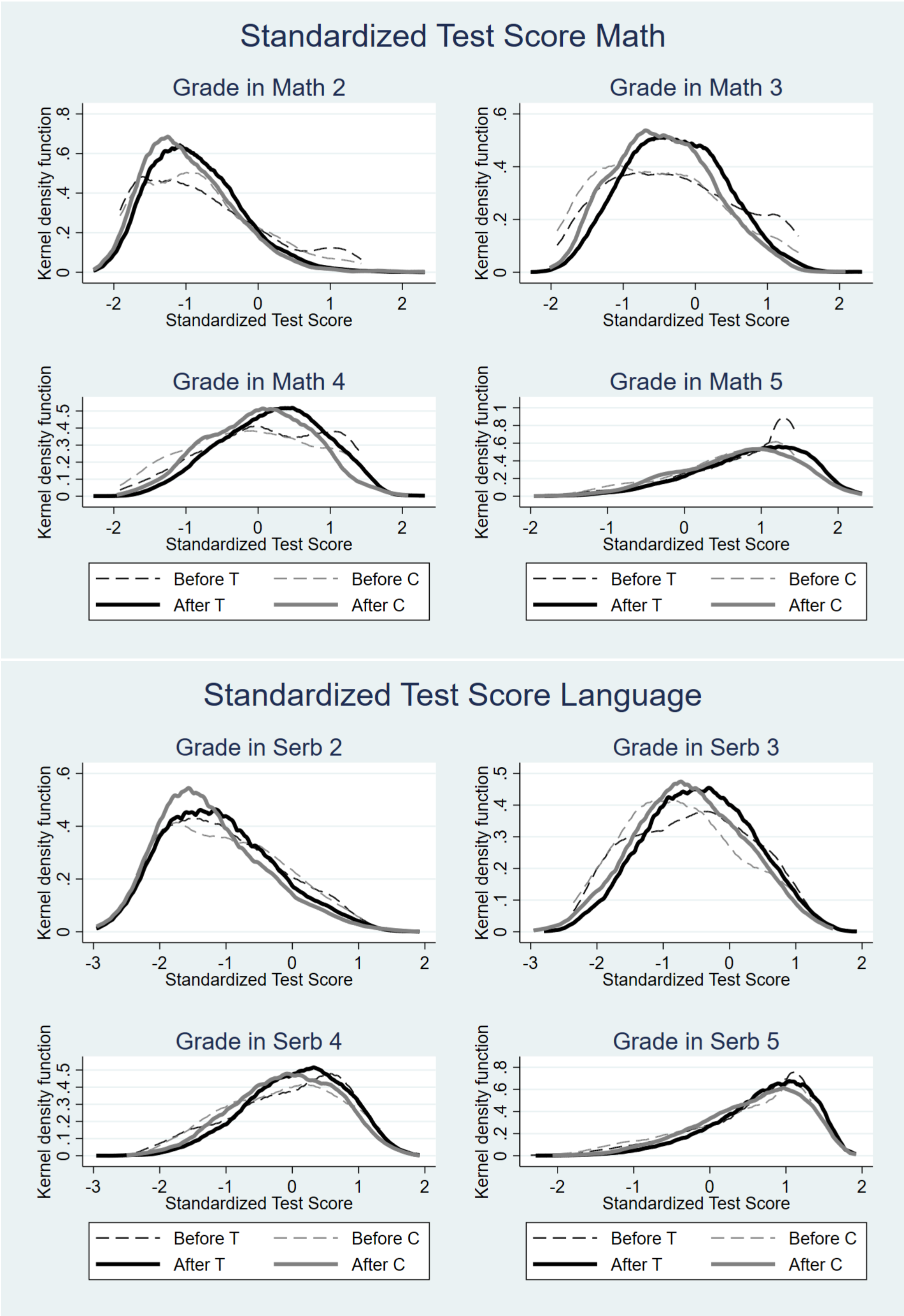

Figure 6: Standardized test score of Roma people

Appendix

A.1 Additional tables

Table A.1: Roma children by gender

	All students			Students finishing primary		Stand. Test Scores				Conditional on St. Test Score	
	Language	Math	Dropout	Language	Math	Language	Math	Secondary	Four-year track	Language	Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
treatedpost (male)	-0.025	-0.133	-0.044	-0.104	-0.213	0.248	0.152	0.020	0.162*	-0.222	-0.279*
	(0.150)	(0.098)	(0.078)	(0.175)	(0.135)	(0.189)	(0.143)	(0.026)	(0.086)	(0.170)	(0.157)
treatedpost (female)	-0.221	0.016	0.019	-0.143	0.112	0.342	0.150	0.058	0.173**	-0.306	0.047
	(0.328)	(0.273)	(0.097)	(0.250)	(0.279)	(0.327)	(0.174)	(0.040)	(0.084)	(0.222)	(0.248)
Observations	4135	4135	4135	3443	3443	3443	3443	3443	2995	3443	3443
Mean in control	2.551	2.297	0.162	2.689	2.407	-1.189	-1.092	0.866	0.545	2.689	2.407

Notes: The table presents the treatment effects on education outcomes for Roma children. All regressions control for an indicator variable for the year of the survey and region fixed effects. The last two column present the treatment effects on education outcomes, controlling for the standardized test score. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table A.2: Roma children: Marks and standardized test scores by quartile

	First quartile		Second quartile		Third quartile		Last quartile	
	Language	Math	Language	Math	Language	Math	Language	Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Standardised testscore Language	0.487***		0.530***		0.456***		0.467***	
	(0.052)		(0.041)		(0.038)		(0.060)	
treatedpost	-0.207	-0.610***	-0.019	0.263	-0.627**	-0.200	-0.131	-0.012
	(0.247)	(0.188)	(0.361)	(0.388)	(0.248)	(0.125)	(0.215)	(0.216)
treated	0.148	0.611***	0.242	-0.326	0.671**	0.122	-0.063	0.026
	(0.336)	(0.183)	(0.286)	(0.347)	(0.274)	(0.129)	(0.186)	(0.190)
post	-0.374**	0.018	-0.416	-0.486	-0.077	-0.412***	-0.108	0.009
	(0.164)	(0.120)	(0.301)	(0.381)	(0.190)	(0.104)	(0.195)	(0.145)
Standardised testscore Math		0.511***		0.406***		0.483***		0.404***
		(0.049)		(0.060)		(0.056)		(0.054)
Observations	593	593	618	618	1017	1017	1215	1215
Mean in control	2.725	2.313	2.590	2.359	2.794	2.558	2.691	2.419

Notes: The table presents the treatment effects on education outcomes for Roma children, controlling for the standardized test score. All regressions control for an indicator variable for the year of the survey and region fixed effects. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table A.3: Roma children: Percentage of Roma at application

				Stand. Test Scores			
	Language	Math	Dropout	Language	Math	Secondary	Four-year track
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: First quartile of Roma at application							
treatedpost	-0.144	-0.348**	0.184*	0.156	0.405**	0.042	0.219
	(0.274)	(0.156)	(0.096)	(0.233)	(0.160)	(0.049)	(0.203)
Observations	682	682	682	593	593	593	540
Mean in control	2.660	2.266	0.149	-1.247	-1.029	0.906	0.552
Panel B: Second quartile of Roma at application							
treatedpost	-0.095	0.056	0.202*	0.400	0.217	0.037	0.209**
	(0.302)	(0.297)	(0.111)	(0.279)	(0.250)	(0.065)	(0.090)
Observations	720	720	720	618	618	618	522
Mean in control	2.509	2.296	0.124	-1.107	-1.042	0.829	0.510
Panel C: Third quartile of Roma at application							
treatedpost	-0.158	0.019	-0.088	0.635	0.066	0.065	0.095
	(0.164)	(0.210)	(0.091)	(0.478)	(0.370)	(0.041)	(0.101)
Observations	1169	1169	1169	1017	1017	1017	904
Mean in control	2.677	2.468	0.113	-1.147	-1.167	0.915	0.589
Panel B: Last quartile of Roma at application							
treatedpost	-0.407*	-0.315**	-0.171*	-0.114	-0.324	-0.004	0.163
	(0.226)	(0.116)	(0.086)	(0.282)	(0.256)	(0.075)	(0.117)
Observations	1564	1564	1564	1215	1215	1215	1029
Mean in control	2.378	2.160	0.277	-1.313	-1.161	0.824	0.536

Notes: The table presents the treatment effects on education outcomes for Roma children. All regressions control for an indicator variable for the year of the survey and region fixed effects. "Treated" is a dummy variable equal to 1 if the student was enrolled in a school where the remedial education program was introduced. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table A.4: Non-Roma children: Marks and standardized test scores by quartile

			First quartile		Second quartile		Third quartile		Last quartile	
	Language	Math	Language	Math	Language	Math	Language	Math	Language	Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Standardised testscore Language	0.700***		0.682***		0.705***		0.728***		0.734***	
	(0.012)		(0.025)		(0.025)		(0.012)		(0.046)	
treatedpost	-0.063	-0.021	0.009	-0.317*	0.014	-0.027	-0.062	0.082	-0.270	0.458**
	(0.098)	(0.115)	(0.154)	(0.180)	(0.176)	(0.229)	(0.187)	(0.276)	(0.184)	(0.189)
treated	0.062	-0.032	-0.085	0.432**	0.230	-0.090	0.203	-0.343	-0.122	-0.438**
	(0.093)	(0.124)	(0.193)	(0.185)	(0.172)	(0.207)	(0.144)	(0.302)	(0.152)	(0.168)
post	-0.104	-0.122	-0.252**	0.074	-0.154	-0.146	-0.070	-0.279	0.170	-0.270*
	(0.083)	(0.096)	(0.105)	(0.114)	(0.102)	(0.175)	(0.181)	(0.246)	(0.155)	(0.151)
Standardised testscore Math		0.845***		0.850***		0.845***		0.861***		0.875***
		(0.011)		(0.025)		(0.020)		(0.018)		(0.030)
Observations	39453	39453	11580	11580	9569	9569	11712	11712	6592	6592
Mean in control	3.768	3.421	3.760	3.285	3.761	3.479	3.762	3.581	3.865	3.408

Notes: The table presents the treatment effects on education outcomes for Non-Roma children, controlling for the standardized test score. All regressions control for an indicator variable for the year of the survey and region fixed effects. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table A.5: Non-Roma children: Percentage of Roma at application

				Stand. Test Scores			
	Language	Math	Dropout	Language	Math	Secondary	Four-year track
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: First quartile of Roma at application							
treatedpost	0.176*	0.029	0.026	0.310**	0.433**	-0.009	0.086***
	(0.095)	(0.121)	(0.047)	(0.145)	(0.182)	(0.019)	(0.030)
Observations	11988	11988	11989	11580	11580	11580	10991
Mean in control	3.712	3.242	0.043	-0.197	-0.124	0.953	0.818
Panel B: Second quartile of Roma at application							
treatedpost	-0.026	-0.014	0.005	-0.013	0.050	0.017	0.023
	(0.105)	(0.118)	(0.049)	(0.115)	(0.214)	(0.023)	(0.027)
Observations	9834	9834	9834	9569	9569	9569	9053
Mean in control	3.716	3.439	0.030	-0.058	-0.113	0.944	0.847
Panel C: Third quartile of Roma at application							
treatedpost	0.057	0.164	0.034	0.249	0.254	-0.011	0.041
	(0.195)	(0.148)	(0.051)	(0.177)	(0.248)	(0.018)	(0.035)
Observations	12138	12138	12138	11712	11712	11712	11190
Mean in control	3.689	3.508	0.052	-0.272	-0.393	0.968	0.810
Panel B: Last quartile of Roma at application							
treatedpost	-0.441*	-0.013	0.092***	-0.108	-0.362*	0.055**	-0.134*
	(0.245)	(0.091)	(0.027)	(0.141)	(0.200)	(0.020)	(0.072)
Observations	6864	6864	6864	6592	6592	6592	6323
Mean in control	3.739	3.292	0.074	-0.355	-0.266	0.938	0.792

Notes: The table presents the treatment effects on education outcomes for Roma children. All regressions control for an indicator variable for the year of the survey and region fixed effects. "Treated" is a dummy variable equal to 1 if the student was enrolled in a school where the remedial education program was introduced. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table A.6: Non-Roma children by gender

	All students			Students finishing primary		Stand. Test Scores				Conditional on St. Score	
	Language	Math	Dropout	Language	Math	Language	Math	Secondary	Four-track	Language	Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
treatedpost (male)	-0.028	0.016	0.045	0.033	0.094	0.131	0.148	0.019	0.019	-0.059	-0.031
	(0.085)	(0.071)	(0.036)	(0.100)	(0.095)	(0.089)	(0.108)	(0.012)	(0.030)	(0.094)	(0.125)
treatedpost (female)	0.026	0.087	0.007	0.056	0.129*	0.173*	0.169	-0.010	0.038**	-0.065	-0.013
	(0.096)	(0.068)	(0.020)	(0.085)	(0.075)	(0.089)	(0.128)	(0.012)	(0.018)	(0.109)	(0.116)
Observations	40824	40824	40825	39453	39453	39453	39453	39453	37557	39453	39453
Mean in control	3.712	3.368	0.042	3.768	3.421	-0.169	-0.178	0.951	0.826	3.768	3.421

Notes: The table presents the treatment effects on education outcomes for Non-Roma children. All regressions control for an indicator variable for the year of the survey and region fixed effects. The last two column present the treatment effects on education outcomes, controlling for the standardized test score. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table A.7: Assistant's and Student's Gender

							Stand. Test Scores							
	Language		Math		Dropout		Language		Math		Secondary		Four-year Track	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
treatedpost (male)	0.050	-0.125	-0.082	-0.193	-0.069	-0.027	0.105	0.128	0.352*	0.029	0.041	0.005	0.172*	0.161*
	(0.165)	(0.160)	(0.104)	(0.127)	(0.095)	(0.079)	(0.179)	(0.176)	(0.196)	(0.220)	(0.036)	(0.029)	(0.098)	(0.088)
treatedpost (female)	-0.286	-0.128	-0.003	0.071	0.023	0.012	-0.096	0.395**	0.210	0.465	0.049	0.081**	0.126	0.227**
	(0.330)	(0.332)	(0.276)	(0.286)	(0.102)	(0.103)	(0.195)	(0.176)	(0.349)	(0.318)	(0.054)	(0.038)	(0.095)	(0.097)
Observations	2742	2396	2742	2396	2742	2396	2248	2039	2248	2039	2248	2039	1925	1807
Mean in control	2.551	2.551	2.297	2.297	0.162	0.162	-1.092	-1.092	-1.189	-1.189	0.866	0.866	0.545	0.545

Notes: The table presents the treatment effects on education outcomes for Roma children by gender depending on the gender of the assistant. All regressions control for an indicator variable for the year of the survey and region fixed effects. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

A.2 Additional figures

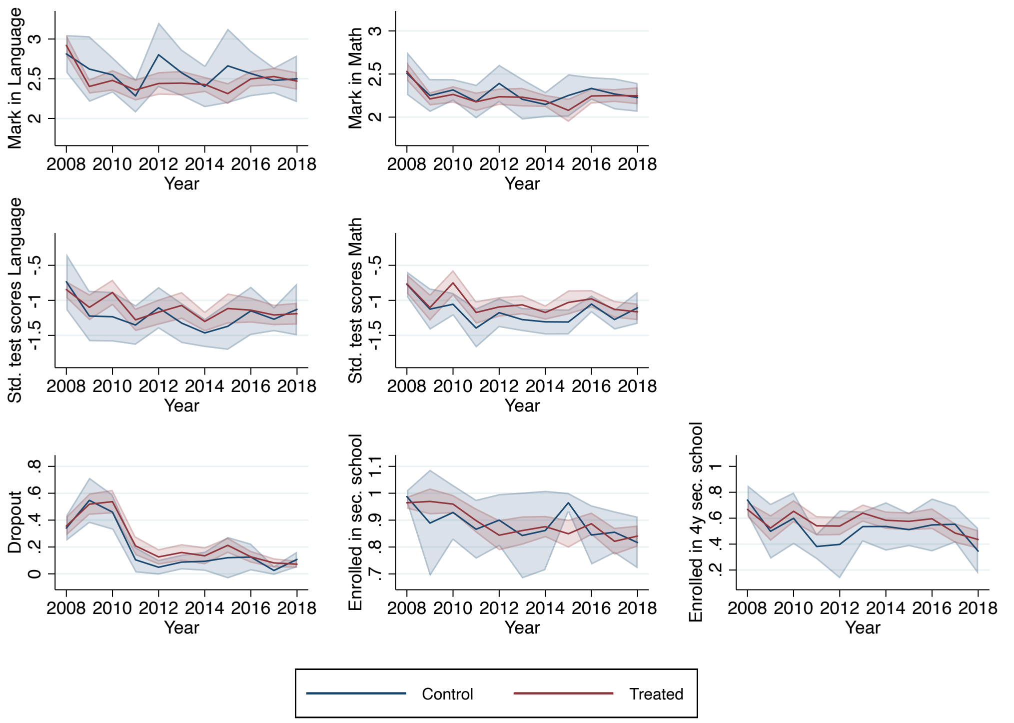

Figure A.1: Roma: Trends of outcome variables

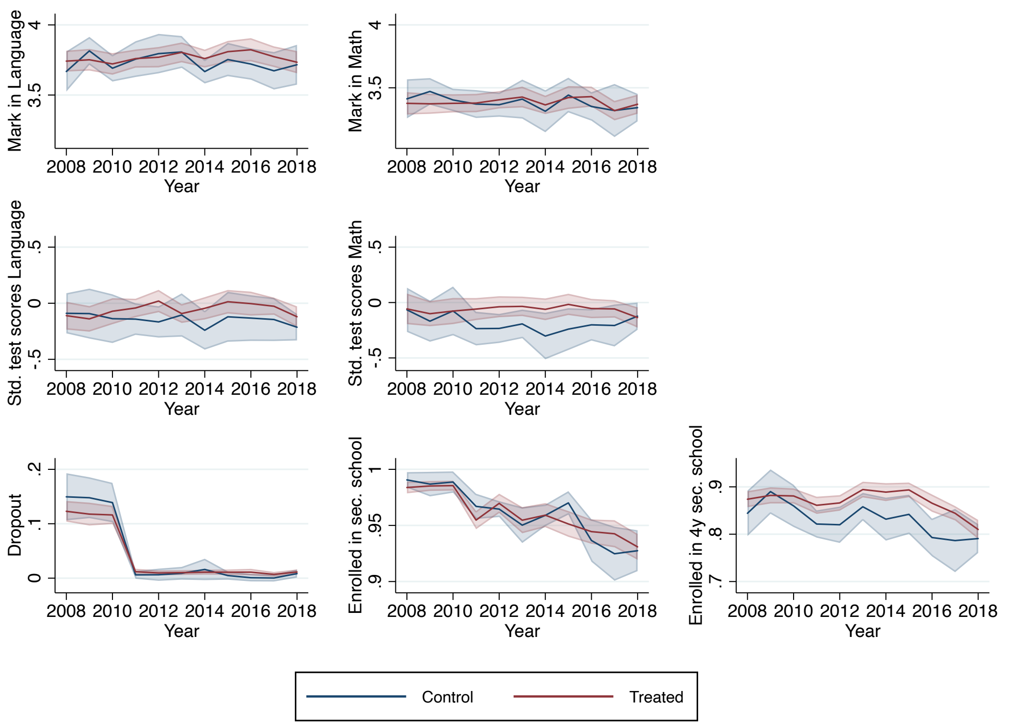

Figure A.2: Non-Roma: Trends of outcome variables

A.3 Overall impact of RTA

Table A.8 reports the RTA impacts for the overall sample and allows to observe differential effects between Roma and Non-Roma pupils. Columns (1) to (4) report the program impacts on the marks in Language, columns (5) to (8) on the marks in Math, columns (9) to (12) on the probability that students do not take the standardized exam and therefore do not finish primary school, columns (13) to (16) on the standardized test scores in Language and columns (17) to (20) on the standardized test in Math. The second part of the table reports secondary school outcomes. More precisely, the first column of each outcome reports the raw impact of the RTA, the second adds the control variables included in X_{is}, the third the interaction with the ethnicity of the child and the last column of each outcome uses survey-year fixed effects and schools fixed effects.

Not surprisingly, the RTA does not have statistically significant effects on any of the outcomes of interest, except for the probability of choosing a more demanding track, as in the main analysis. Panel B reports the coefficients of the placebo regressions, confirming again the comparability of our control group. All these results together seem to suggest modest average effects of the program and hardly indicate important reductions in the achievement gap between Roma and Non-Roma.

Table A.8: All sample

	Language				Math				Dropout				Test Language				Test Math			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
treatedpost	-0.025	-0.028	0.002	-0.015	0.045	0.042	0.051	0.038	0.027	0.027	0.026	0.026	0.144*	0.143*	0.163**	0.125	0.129	0.131	0.164	0.117
	(0.077)	(0.080)	(0.084)	(0.081)	(0.064)	(0.061)	(0.063)	(0.061)	(0.026)	(0.027)	(0.026)	(0.026)	(0.085)	(0.081)	(0.077)	(0.081)	(0.109)	(0.110)	(0.116)	(0.110)
treated	0.055	0.082	0.053		-0.047	-0.023	-0.019		-0.021	-0.023	-0.021		-0.037	-0.018	-0.073		0.003	0.019	-0.050	
	(0.083)	(0.081)	(0.089)		(0.094)	(0.088)	(0.092)		(0.026)	(0.025)	(0.024)		(0.114)	(0.108)	(0.121)		(0.122)	(0.120)	(0.137)	
post	0.025	0.012	0.009		-0.061	-0.073	-0.079		-0.154***	-0.152***	-0.130***		-0.109	-0.094	-0.062		-0.170*	-0.164*	-0.150	
	(0.068)	(0.072)	(0.070)		(0.052)	(0.049)	(0.052)		(0.024)	(0.025)	(0.023)		(0.068)	(0.064)	(0.068)		(0.086)	(0.089)	(0.103)	
female		0.654***	0.647***	0.645***		0.473***	0.467***	0.467***		-0.013***	-0.011***	-0.011***		0.467***	0.462***	0.462***		0.164***	0.160***	0.161***
		(0.017)	(0.017)	(0.017)		(0.019)	(0.019)	(0.019)		(0.002)	(0.002)	(0.002)		(0.018)	(0.018)	(0.017)		(0.011)	(0.011)	(0.011)
roma		-1.197***	-0.841***	-1.132***		-1.104***	-0.894***	-1.054***		0.131***	0.185***	0.107***		-1.073***	-0.626***	-0.969***		-0.973***	-0.674***	-0.880***
		(0.041)	(0.096)	(0.043)		(0.034)	(0.074)	(0.037)		(0.015)	(0.037)	(0.010)		(0.056)	(0.150)	(0.049)		(0.041)	(0.081)	(0.034)
treatedpostroma			-0.132				-0.117				-0.048				-0.015				-0.043	
			(0.140)				(0.123)				(0.047)				(0.200)				(0.108)	
postroma			-0.309**				-0.193*				-0.105***				-0.403**				-0.240***	
			(0.128)				(0.107)				(0.037)				(0.177)				(0.085)	
treatedroma			0.071				0.087				0.037				-0.105				-0.032	
			(0.112)				(0.091)				(0.049)				(0.162)				(0.098)	
Observations	44965	44959	44959	44959	44965	44959	44959	44959	44966	44960	44960	44960	42898	42896	42896	42896	42898	42896	42896	42896
Mean in control	3.624	3.624	3.624	3.624	3.287	3.287	3.287	3.287	0.051	0.051	0.051	0.051	-0.237	-0.237	-0.237	-0.237	-0.239	-0.239	-0.239	-0.239
treatedpost	-0.100	-0.104	-0.077	-0.088	-0.014	-0.017	-0.003	-0.017	0.012	0.013	0.020	0.011	0.073	0.069	0.042	0.042	0.080	0.077	0.063	0.052
	(0.091)	(0.091)	(0.091)	(0.090)	(0.069)	(0.065)	(0.068)	(0.067)	(0.016)	(0.016)	(0.017)	(0.015)	(0.075)	(0.071)	(0.072)	(0.071)	(0.106)	(0.105)	(0.113)	(0.108)
treatedpostroma			-0.185				-0.120				0.038				0.160				0.068	
			(0.150)				(0.140)				(0.053)				(0.200)				(0.118)	
Observations	34436	34433	34433	34433	34436	34433	34433	34433	34441	34438	34438	34438	30221	30219	30219	30219	30221	30219	30219	30219
Mean in control	3.640	3.641	3.641	3.641	3.321	3.321	3.321	3.321	0.141	0.141	0.141	0.141	-0.189	-0.188	-0.188	-0.188	-0.180	-0.181	-0.181	-0.181

Notes: The table presents the treatment effects on education outcomes for the entire sample. All regressions control for an indicator variable for the year of the survey and region fixed effects. "Treated" is a dummy variable equal to 1 if the student was enrolled in a school where the remedial education program was introduced. Columns (1), (5), (9), (13) and (17) report the estimates without controls. Columns (4), (8), (12), (16) and (20) report the estimates with school and year fixed effects. Standard errors are clustered at the school level (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

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	Secondary				Four-year Track			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatedpost	0.008	0.008	0.006	0.008	0.009	0.009	0.029*	0.011
	(0.009)	(0.009)	(0.010)	(0.009)	(0.014)	(0.013)	(0.017)	(0.013)
treated school in 2015	-0.009*	-0.008*	-0.008		0.017	0.020	-0.018	
	(0.005)	(0.005)	(0.005)		(0.036)	(0.036)	(0.038)	
post	-0.052***	-0.052***	-0.038***		-0.025**	-0.023**	-0.013	
	(0.008)	(0.007)	(0.007)		(0.010)	(0.010)	(0.014)	
female		-0.006*	-0.007**	-0.006*		0.086***	0.085***	0.090***
		(0.003)	(0.003)	(0.003)		(0.007)	(0.007)	(0.007)
roma		-0.078***	0.001	-0.074***		-0.157***	-0.101***	-0.127***
		(0.011)	(0.011)	(0.010)		(0.013)	(0.038)	(0.011)
treatedpostroma			0.012				-0.005	
			(0.026)				(0.042)	
postroma			-0.069***				-0.027	
			(0.020)				(0.037)	
treatedroma			-0.020				-0.026	
			(0.015)				(0.041)	
Observations	42898	42896	42896	42896	40554	40552	40552	40552
Mean in control	0.946	0.946	0.946	0.946	0.185	0.185	0.185	0.185
treatedpost	0.008*	0.008*	0.004	0.007	0.016	0.016	-0.013	0.016
	(0.005)	(0.005)	(0.005)	(0.005)	(0.013)	(0.013)	(0.021)	(0.013)
Observations	22135	22133	22133	22133	21797	21795	21795	21795
Mean in control	0.988	0.988	0.988	0.988	0.194	0.195	0.195	0.195