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The curse of low expectations[1]

Remedial education and perceived returns to education of Roma people

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Abstract

We examine how a remedial education programme for primary school-age children affects parental expectations about their children's future. Using original survey data we collected in Serbia, we investigate whether expectations on labour market prospects and educational attainment change as a consequence of exposure to the Roma Teaching Assistant programme. Our results show that parents of pupils in treated schools expect higher returns to education for their children and are more likely to expect them to achieve a secondary level of education. We also investigate the possible mechanisms in place due to the characteristics of the programme: remedial education and role model.

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1. Introduction

Expectations for the future consistently affect choices made in the present. The social environment where one lives plays a role in shaping expectations. The aim of this paper is to examine the impact on parental expectations of a remedial education programme for primary school-age children targeting the marginalized Roma minority group. We study expectations on returns to education and educational achievement as they affect future-oriented behaviours and we are interested in investment in education. Roma people usually attain very low education: primary school attendance rates are in the range of 40 to 60 percent in most countries (Brueggemann, 2012). Their upper secondary school completion rates are even lower: only 1 percent of Roma adults aged 25–64 in Portugal, where the rate is lowest, to 23 percent in Poland, where the rate is highest, have completed compulsory upper secondary education. There is a clear low investment in education among them, which can be due to financial constraints and the existence of barriers of access to education.[2] However, it is reasonable to assume that Roma people do not invest in education because they may not expect schooling to give them enough future opportunities. They might perceive that they face a 'job ceiling'. In the formal job market, there is often discrimination against minority groups and they might not find a job even with a high educational level attained. Conversely, the informal job market – where they mainly work – does not often require any level of education: Roma people are primarily involved in casual and seasonal jobs, performed without a written contract (see Section 2.1). The cost of investing in education would be perceived as too high compared to the discounted stream of expected future benefits. Therefore, there is no incentive to invest. If we believe that educational and career expectations are important factors determining one's future educational attainment, then a policy intervention targeting a minority and being able to change expectations can trigger higher educational attainment.

We first investigate whether expectations on labour market prospects and educational attainment change as a consequence of exposure to the Roma Teaching Assistant programme (RTA). This is a remedial education programme introduced in Serbia in 2009 and it consists of assigning to each school one Roma Teaching Assistant who works with Roma children. We focus on parental expectations because we argue that at such a young age (6–15 years) parents' beliefs are more relevant for a child's

[2] Roma people often lack the required ID and face financial constraints. On average, costs associated with schooling (books and other school material) in Serbia correspond to almost 2 percent of yearly household income (LSMS, 2003). In our sample of Roma people, these costs correspond to almost 6 percent of yearly household income. Moreover, some children face difficulties at school due to language barriers. In the Multiple Indicator Cluster Survey conducted by UNICEF in 2006, only 10 percent of Roma declared Serbian to be their mother tongue.

educational attainment and more reliable for expected returns to education than a child's expectations. Next, we investigate the potential channels for these effects: remedial education and the role model mechanism. Parents have higher expectations for their children because they perform better now thanks to the Roma Teaching Assistant. Moreover, in the RTA programme, all the assistants are Roma and from the same social background as the pupils they help. In order to be assistants, they needed to invest in education. Their successful experience can be shared with students and their parents who will be motivated to believe that their children can achieve analogous results, thanks to the investment in education. We find that parents whose children participate in the programme expect higher returns to education for their kids. They are also more likely to expect them to achieve a secondary level of education.

For the purpose of our analysis, we have conducted an extensive survey with 300 Roma households in the capital of Serbia, Belgrade. In Fall 2010, we interviewed both parents and their children attending 9 schools in 13 different settlements of the city. The pupils interviewed were randomly selected among students attending the schools involved in the programme. The RTA programme began in 2009 and we look at its impact a year after its implementation. The programme was introduced gradually: some schools received their teaching assistant before others. Parents and children who attend schools with the teaching assistant in Fall 2009 are our treated group. Parents and children who attend schools that received the teaching assistant at a later point in time (Fall 2010) are our control group. The allocation of Roma teaching assistants was not designed as a randomized experiment: the programme was designed in such a way that schools and assistants had to apply to be part of it. Nonetheless, the phasing in of the programme and its selection characteristics mitigate concerns regarding the endogeneity of the selection process. Moreover, the preliminary analysis suggests that before the introduction of the programme, the schools with treated and control children were similar in terms of observable characteristics that could affect expectations. We also explore a second definition of treatment because there is only one assistant per school and not every Roma child in the treated school is helped by the assistant. A household is treated here if at least one child is in a treated school and there is evidence from the survey that the assistant has worked with the child. The assistant chooses the pupils to work with: they are not a randomly selected subset of kids in treated schools. Therefore, being in a treated school only captures the intention-to-treat (ITT) effect and can be used as in a instrumental variable for being helped by the assistant. The local average treatment effect (LATE) we estimate is the effect of treatment on the treated. In order to check the robustness of our results we also employ a propensity score matching method. The analysis and results are reported in Section B.3. of the online Appendix.

Knowing who is actually helped by the assistant also allows us to better investigate the possible mechanisms leading to the changes in perceived returns we observe: the remedial education channel and the role model mechanism. Parents are likely to expect their children to go to school more because they now perform better

thanks to the assistant. Remedial education is effective. However, once we select the students performing badly among both the treated and control schools, we still find higher expected returns to education for pupils helped by the assistant compared to those not helped. The presence of a person from the same social background sharing a successful story may affect parents' expectations about their children's future. However, the lack of variation in whether the assistant in the RTA programme is Roma or not makes it more difficult to draw strong conclusions in this respect.

Our paper is in line with the contributions on subjective expectations and information gap between perceived and actual returns to schooling.[3] Standard economic theory suggests that, in the presence of perfect information, individuals choose their level of education by equating the marginal benefits of education to its marginal costs. Underinvestment in education can be due to credit constraints, high discount rates or low school quality.[4] However, several works emphasized the importance of subjective expectations (Jensen, 2010; Kaufmann and Attanasio, 2009; Manski, 1993; Nguyen, 2008). The returns perceived by individuals affect schooling decisions. Yet, perceptions may be inaccurate, due to limited or imperfect information. The paper is also related to the literature on role models for minorities or disadvantaged people. In the nineties, a series of researchers and policymakers advocated for an increased hiring of minority teachers in the United States (Graham, 1987; Ladson-Billings, 1994), where the Black–White mark gap has been intensively investigated. In fact, the relevance of having a teacher with the same background has been found to be significant in improving the achievement gap for minorities (Dee, 2004). Our paper, together with its companion paper (Battaglia and Lebedinski, 2015), adds evidence on the short-term effects of remedial education programmes targeting minority groups. It provides replicable examples in contexts where minorities suffer low attainment rates and social exclusion, suggesting the role of targeted programmes can be instrumental in increasing their educational attainment. For Roma people, for instance, this is the case in many other European countries and so far there are few attempts to investigate how to improve their life circumstances, in general, and of children, in particular. Furthermore, we contribute to the existing literature by providing primary data in a context where data are scarce.

The rest of the paper is organized as follows. Section 2 provides information on the institutional setting and the Roma Teaching Assistant programme. The characteristics of the programme are crucial to understand the possible mechanisms at play. Section 3 describes the way the survey was designed and the data collected in order to carry out our analysis and it provides some descriptive statistics. Section 4 presents the estimation strategy and results. Section 5 discusses our findings and suggests possible general implications of the current research.

[3] The literature suggests that this gap can also be filled by providing additional information through statistics (Brueggemann, 2012). These tools turn out to be mostly cost–effective solutions rather than incentives, like cash transfers or private school vouchers.

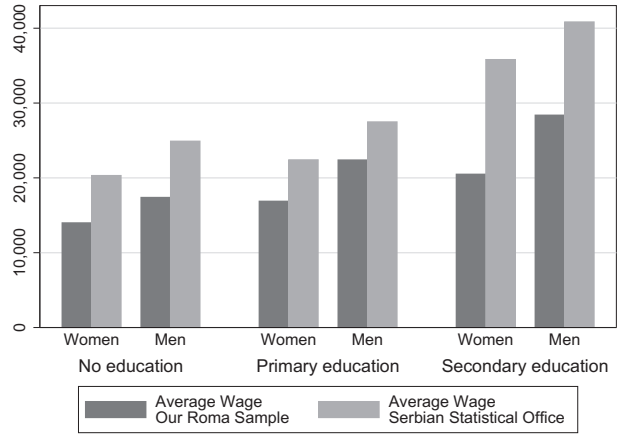
[4] See Glewwe and Kremer (2006) for an extensive summary on education in developing countries.

2. Institutional setting and the Roma Teaching Assistant programme

2.1 Education and the labour market

There are many reasons to believe that Roma people may underestimate the need to invest in education. (For an extensive overview of the situtation of Roma see online Appendix section B.1.) First, a large percentage of Roma live in segregated settlements. Since they are isolated from the mainstream society, they do not often have different models to which they can relate in their immediate neighbourhood (Akerlof and Kranton, 2000; Wilson, 1987). This is confirmed by the 2002 Census data; 83 percent of people who declared to be Roma live in census tracts where at least 7 percent of the entire population is Roma (vs. a country average of 1.44 percent). Second, it is extremely rare that Roma people perform jobs for which high levels of education are required. In Serbia, for instance, there are usually no teachers of Roma origin working in schools and it is rare to find them working in public offices.[5] Third, there is evidence that the mean earnings of Roma workers are lower than those of non-Roma workers, especially for higher levels of education. Figure 1 reports average wages for the city of Belgrade for Roma and non-Roma. Data for Roma and non-Roma come from different sources: the Serbian Statistical Office provides earnings statistics for the whole population and does not distinguish

Figure 1. Comparison of wages by level of education (in dinars) for Roma and non-Roma (Belgrade)



[5] In our sample only 7 percent of women and 6 percent of men of working age perform jobs under a full-time contract in the formal sector and none of them in the public sector.

between different ethnic groups. These data are collected for jobs in the formal sector that are not usually performed by Roma people and correspond in the figure to those of the non-Roma group. Data for Roma are calculated from our sample.[6] This figure intends to simply provide a picture of the context and does not attempt to be indicative of the exact amounts. The gap in real wages between the two groups is unambiguous for every level of education and each gender.

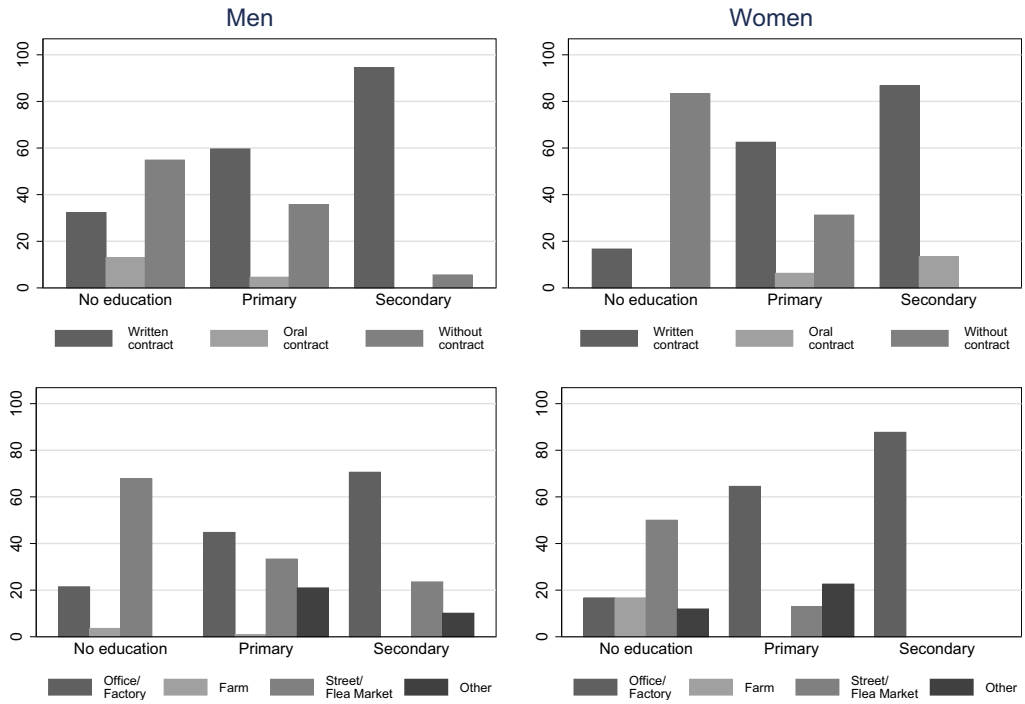
Nonetheless, even if there is evidence of a gap between Roma and non-Roma, among Roma themselves, there are large differences in average earnings across different educational levels. In our sample, for instance, average wages with secondary education are 27 percent higher than with primary education for men and 21 percent for women, and average wages with primary education are 29 percent higher than with uncompleted primary for men and 21 percent for women.[7] Moreover, the higher the educational level attained, the better are the job market prospects, both in terms of type of contract and place where the job is performed. The data from the Living Standards Measurement Study (LSMS, 2003) reported in Figure 2 suggest that these differences are substantial for Roma living in Serbia. The top panel presents their types of contract by the educational level. As can be seen, 90 percent of men and 80 percent of women with a secondary education level have got a written contract, while almost none works without a contract. Conversely, among those with only primary schooling, almost 30 percent do not have a contract. The bottom panel reports the places where jobs are performed. The percentage of those who work in the street or in flea markets reduces drastically with the level of education. This is even more evident for women. The opposite pattern is observed when the workplace is an office or factory.

Therefore, conditional on the fact that Roma people's earnings are lower than those of non-Roma, there is still room for improvement based on education among Roma. The more one studies, the higher the potential wages and the better the potential job conditions. However, it is crucial to understand whether parents are aware of the actual returns to schooling. If Roma people underestimate the outcomes of investing in education, a policy intervention can be successful in increasing their expectations. Because we could not conduct a baseline survey before the programme was implemented, we need to look at data of parents not affected by the programme, assuming they are a comparable group to those affected. Figure 3 reports the distributions of expected returns to education for parents whose children attend the schools that received the assistant in the second year of the RTA. Their averages are shown in solid lines. The dashed lines correspond to average wages of people in our sample by education. There are few women who completed primary schooling and especially secondary school. Thus, the results for girls are less informative. Official data do not provide this information.

[6] There are no official data on earnings coming from informal activities, which are mainly performed by Roma people.

[7] For non-Roma the average wages with secondary education are 49 percent higher than with primary education for boys and 60 percent for girls. For non-Roma, we use 2011 data for the city of Belgrade obtained from the Serbian Statistical Office (2010).

Figure 2. Job characteristics by education levels – Roma people (LSMS, 2003) [Colour figure can be viewed at wileyonlinelibrary.com]



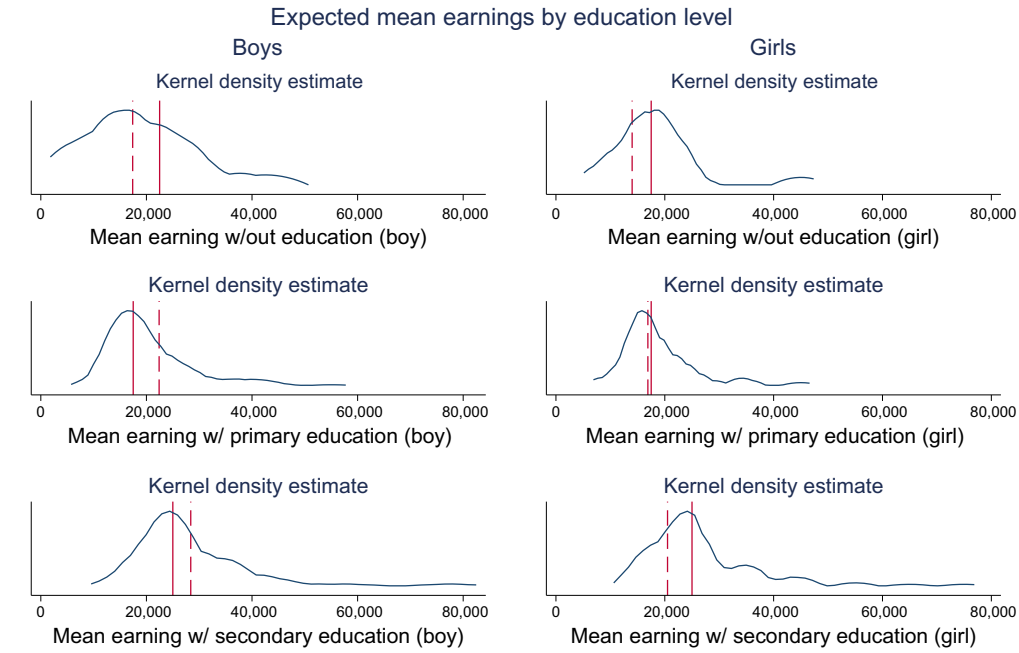
The first panel reports the expected wage distributions, conditional on not having attained any level of education. These distributions are more concentrated to the right of the dashed lines of actual average returns, indicating that parents expect higher returns for their children when no level of education is attained. The second and third panels of the figure report the expected wage distributions, conditional on having a primary and a secondary level of education, respectively. For boys, these distributions tend to be more concentrated to the left of the dashed line of actual average returns. Thus, parents expect for their sons less than what people with these education levels actually earn. There is limited or imperfect information, and this likely fosters low expectations for Roma people.

2.2 *The Roma Teaching Assistant programme*

The Roma Teaching Assistant programme is the main programme in Central and Eastern Europe aimed at improving inclusion of Roma in education.[8] After the

[8] For a more extensive description of the programme see Battaglia and Lebedinski (2015).

Figure 3. Comparison of real and expected returns to education (our sample) [Colour figure can be viewed at wileyonlinelibrary.com]



The vertical dashed (solid) lines mark the estimated (expected) average returns in dinars.

initial pilot phase, the programme attained a wider coverage in the 2009/2010 school year.[9] In Fall 2009, 26 primary schools (*Early Enrollees*) enrolled in the RTA programme. In the following year an additional 77 primary schools (*Late Enrollees*) joined. Each school receives one teaching assistant. On average, the number of Roma per school is 75 (20 percent of total pupils enrolled) and assistants work with one third of them, especially in the lower grades. Although schools are free to allocate the assistants' schedule, their major tasks are helping children during regular classes and organizing extra, after-school classes. One day per week the assistant visits the parents of children who are not attending school and informs other parents about their children's progress.

[9] The Roma Teaching Assistant programme started out as a pilot programme implemented by various NGOs in 2002. A total of 22 schools received an assistant at different points of time from 2002 to 2007. These are not the same schools that had an assistant from 2009 onwards and are excluded from our analysis. In 2007 the Organization for Security and Cooperation in Europe (OSCE) took over the coordination and financing of the programme. Beginning in 2009, the RTA programme had a country coverage and is now coordinated by the Ministry of Education.

The RTA programme was not designed by the Ministry of Education as a randomized experiment: schools and assistants had to apply in order to be part of it. Yet, the phasing in of the programme and its selection characteristics mitigate concerns regarding the endogeneity of the selection process. Schools were selected based on the following two criteria: (1) a percentage of Roma pupils between 5 percent and 40 percent, and (2) preferably the availability of a preschool programme in the school.[10] The requirements for assistants are as follows: (1) secondary school attainment, (2) knowledge of Romani, and (3) preferably experience working with children.[11] It is not explicitly stated that the assistant needs to be Roma: only the knowledge of their language is required. However, all of them are of Roma origin. All the assistants live in the same municipality as the school for which they work. Ideally we would have liked to randomly allocate the schools in the two years and the assistants among schools. Since we could not intervene in the phase of implementation of the programme but only in its evaluation, what we can show is that the phasing in of the programme can be treated as if it were exogenous for the following reasons. First, the selection criteria remained the same in both years and schools and assistants which applied for the RTA programme in the first year and did not get selected could also apply in the second year.[12] Second, schools applying before and after do not differ in terms of the only observable characteristic available to the selection committee: in Belgrade, the percentage of Roma pupils was 14.37 percent in schools that applied for the programme in 2009 and 12.38 percent in the schools that applied in 2010. The difference is not statistically significant ($P = 0.5791$).[13] Third, the schools selected in the first year do not differ from the

[10] Information on the existence of a preschool programme are available only for the 78 schools that applied for the RTA programme in 2009. For the 252 schools applying in 2010 this information was no longer required. In that year, 50 assistants were assigned to kindergartens offering preschool programmes. Schools which were not offering the a programme could have then been close to kindergartens offering one and the Roma pupil would have been helped by an assistant from her entry in the school anyhow. Since 2007, it is compulsory to attend a free preschool programme for at least six months. In 2010, this requirement was extended to nine months. One could argue that this small change in requirements could lead to a selection bias in the two rounds, but some schools without the compulsory programme were also selected in the previous year because it was not a binding requirement and some schools with the preschool programme were selected in the second year. Thus, our data mitigate this concern.

[11] Of the 158 candidates that applied in 2009, 26 were selected. In 2010, of the 329 applicants, 77 got the job (and 50 more became assistants in kindergartens). Among the candidates belonging to the same municipality of the school selected, detailed criteria based on level of education attained, motivation and experience of working with children, were used to evaluate assistants. Thus, first the school is selected, then the assistant.

[12] In Belgrade, the assistants who applied in the first round and did not get selected were not selected in the second round either. Thus, concerns that more qualified assistants are selected first and the worst in the second round are mitigated in our data.

[13] In all Serbia the percentage of Roma pupils is 13.99 percent in schools which applied in 2009 and 13.08 percent in schools which applied in 2010. The difference is not statistically significant ($P = 0.4581$). For schools that applied and did not get selected, this is the only information available, together with their size. In Belgrade, the schools applying in 2009 had, on average, 780 pupils, while those applying in 2010 had 657 pupils. The difference is not statistically significant ($P = 0.5226$).

schools that were selected later in terms of their observable characteristics. The same holds for the assistants. Table 1 reports the school characteristics for the schools in our sample before implementing the RTA programme.

We collected data from nine schools in Belgrade, five of which received an assistant in 2009 and four of which received one in 2010.[14] *Early* and *Late Enrollees* schools are similar in terms of their observable characteristics.[15] Before the introduction of the programme, *Early Enrollees* and *Late Enrollees* schools had a similar number of Roma per class, 4.75 and 5.75, respectively, a comparable class size (20.60 and 22.75 students, respectively) and a similar percentage of Roma per school (23 percent and 24 percent, respectively). The sex composition among Roma is the same: in *Early Enrollees* schools 53 percent of students are female and in *Late Enrollees* schools 47 percent are female. As regards place of birth, 80 percent of Roma in *Early Enrollees* schools are born in Belgrade, and 76 percent in *Late Enrollees* schools. These characteristics are also comparable in the previous year, with no statistically significant difference in any of the observable characteristics. The descriptive statistics are reported in Table A1 of the Appendix. The characteristics of the assistants in the two types of school are also comparable. Almost all of them are female and have experience in NGOs. In *Early Enrollees* schools, 40 percent of the assistants got a university degree; in *Late Enrollees* schools 33 percent.[16] Furthermore, school quality is not different between *Early* and *Late Enrollees* schools. Average marks, absences and dropout rates of previous years in *Early Enrollees* schools do not suggest they are better schools (Battaglia and Lebedinski, 2015).[17]

One concern is that motivation may differ between schools applying before and schools applying later.[18] If these motivations are related to differences in principle or school quality it might cause a selection bias problem. Our estimates can be overestimated: parents' expectations can be correlated with the quality of the school. If children are going to better schools, parents may reasonably expect better educational attainment and better labour market prospects for them, unconditional to the

[14] Six schools in Belgrade were selected to participate in the RTA programme in the first year of its implementation. One school did not provide us the list of students so it is excluded from our sample. Nine schools were selected in 2010. We obtained the list of students and detailed administrative data from a subsample of four schools. For the remaining five schools we only know the percentage of Roma per school. Their percentages are comparable to those in our subsample.

[15] The same holds for the whole sample of schools involved in the programme in Serbia. In the RTA programme, the schools selected in the first year are not different from the schools selected later in terms of their observable characteristics. The same holds for the assistants (Battaglia and Lebedinski, 2015).

[16] Among assistants in *Late Enrollees* schools there is one missing value for the information on the maximum level of education. This explains why the categories *secondary school* and *university* do not sum to 1.

[17] Table A2 of the Appendix reports the marks, absences and dropout rates of pupils in both *Early* and *Late Enrollees* schools in Belgrade for the year of the introduction of the programme and the previous year.

[18] In both years, the programme was advertised in the *Politika* and *Prosvetni Pregled* newspapers. The latter newspaper is for people working in the education sector. In 2010/2011 schools' directorates – one directorate may be responsible for more than a municipality – were in charge of sending applications directly to the schools.

Table 1. Characteristics of the schools and assistants (Belgrade) before the introduction of the RTA programme 2009

	All	Early Enrollees	Late Enrollees	Difference (1 − 2)	*P*
	(1)	(2)	(3)	(4)	(5)
Characteristics of the schools					
Class size	21.55	20.60	22.75	−2.15	[0.505]
No. of Roma per class	4.67	3.80	5.75	−1.95	[0.566]
No. of Roma per class (if at least one Roma)	5.25	4.75	5.75	−1.00	[0.764]
No. of Roma per school (%)	0.23	0.23	0.24	−0.01	[0.952]
Female					
Roma	0.49	0.53	0.47	0.06	[0.209]
Non-Roma	0.47	0.48	0.47	0.01	[0.544]
Born in the same town					
Roma	0.77	0.79	0.75	0.04	[0.358]
Non-Roma	0.90	0.90	0.90	0.00	[0.722]
Number of schools	9	5[a]	4		
Number of Roma pupils	581	231	350		
Number of Non-Roma pupils	2,133	927	1,206		
Characteristics of the assistants					
Female	0.875	0.8	1	−0.2	[0.374]
Maximum level of education					
Secondary school	0.5	0.6	0.33	0.27	[0.543]
University	0.375	0.4	0.33	−0.07	[0.877]
Experience with Roma	0.75	1	0.33	0.67	[0.183]
Experience in NGO	1	1	1	0	[.]
Number of assistants	8	5	3[b]		

Notes: [a] *Early Enrollees* schools are 6. One school did not provide us the list of students so it is excluded from our sample.
[b] We could not get information about one assistant in *Late Enrollees* schools.

programme. However, this concern can be mitigated since school quality is not different between *Early Enrollees* and *Late Enrollees*. Moreover, principal quality should be reflected in assistant quality in order to affect parents' expectations but the assistants are not chosen by the principal.

Therefore, we know first that selection criteria remained the same in both years and the selection committee rated schools in the same way. Second, schools could

apply in both years and those applying before and after do not differ in terms of observable characteristics. Third, the schools selected in both years do not differ in terms of observable characteristics. Fourth, the quality of the schools selected in both years is not different. Based on these facts, we argue that the phasing in of the programme can be treated as if it were exogenous.

3. Data and descriptive statistics

We use first-hand collected data obtained through a survey conducted with 300 Roma households in five municipalities of Belgrade.[19] The survey took place in Fall 2010, one year after the implementation of the programme in *Early Enrollees* and before *Late Enrollees* schools received the assistant. In 2010 schools received the assistant in November/December.

The households in our sample have children who were enrolled in both types of primary school. The pupils interviewed were randomly selected among the students attending the schools.[20] We know that assistants work mainly with pupils in the lower four grades and we are interested in the effect of the RTA programme on this subgroup of children.[21] Our sample is constructed in such a way that all households have at least one child in the lower four grades of primary school in the school year 2009/2010.[22]

Figure 4 displays a map of Belgrade with the 13 neighbourhoods located in the five municipalities where the survey was conducted. In settlements 1 to 5, the assistants began to work in 2009/2010 and they correspond to the settlements with children from *Early Enrollees* schools. Settlements 6 to 13 had assistants starting from 2010/2011 (*Late Enrollees*). As shown, *Early* and *Late Enrollees* are located in different neighbourhoods of Belgrade, so concerns regarding potential spillovers across schools are not relevant in our context.[23] Potential spillovers between students who are helped or not by the assistant within *Early Enrollees* schools cannot be excluded. Parents who live in the same area are also likely to interact.[24] Nevertheless, if there

[19] The five municipalities are Voždovac, Zvezdara, Zemun, Palilula and Čukarica.

[20] The response rate was 93.46 percent: 321 households were contacted and 300 answered. Households were not compensated for their participation.

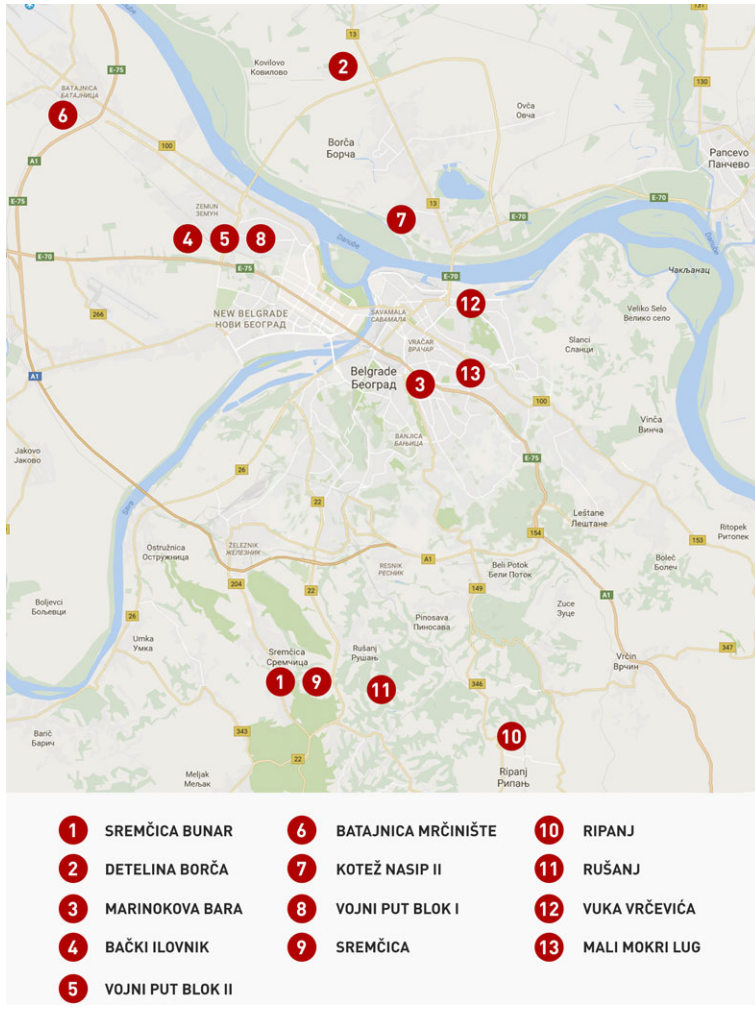
[21] In Serbia, primary school is 8 years in duration. In the first four grades pupils get one teacher who teaches all compulsory subjects except English, while in the upper four years pupils get one teacher per subject. School is compulsory until the age of 15. Children enroll in primary school if they are aged at least 6.5 years at the start of the school year in September.

[22] Our sample includes households which *enrolled* their children in 2009/2010. Thus, students who dropout are included in the sample.

[23] Only one suburban area has an *Early* and a *Late Enroollees* school, which are located on opposite sides of the village.

[24] In 70 percent of cases the pupils live in different areas of the neighbourhood where the school is located and among those living on the same street, pupils not helped by the assistant are older and enrolled in higher grades. They might be less likely to interact, both inside and outside the school, with pupils who are helped.

Figure 4. Neighbourhoods of the survey [Colour figure can be viewed at wileyonlinelibrary.com]



are spillover effects from children helped by the assistants to children not helped by them, the impacts we observe would be the lower bounds of the actual impact. We would be underestimating the effects of the programme.

3.1 First definition: Early Enrollees vs. Late Enrollees

Our sample is divided into two groups. The first group consists of 122 households with children enrolled in the five schools which received a Roma teaching assistant

in 2009/2010. These households are randomly selected among households with at least one child in a *Early Enrollees* school and correspond to the treated group. The 178 remaining households were randomly selected from students attending the four schools which implemented the RTA programme in 2010/2011 and they are our control group. The number of households selected from each settlement is proportional to the size of the settlement. We consider the whole household to be treated if at least one child goes to a school with an assistant in the first year of the implementation of the programme.

Table 2 reports the predetermined characteristics of treated and control groups. The treated and control groups are comparable in terms of observable characteristics. Their differences in means are not statistically significant in almost all cases. Wealth, monthly income, educational attainments and household composition do not differ between the groups.[25] Households are equally located in rural and urban areas,[26] and in only Roma neighbourhoods.[27] A total of 32 percent of households in the control group and 31 percent in the treated group have at least one member working in the informal sector. The only statistically significant difference is found in terms of religion: there are significantly more Muslims among non-treated households (80 percent vs. 57 percent). Therefore, it would be worthwhile investigating whether the programme impacts differently depending on the religion (see section A.4 of the Appendix on heterogeneous effects).[28]

Unfortunately, our study has a limited power and we are aware that significant true differences might go undetected due to it.[29] However, if we were concerned that a possible imbalance between the two groups may affect the results, it is unlikely that it would lead to their overestimation. The characteristics of the households whose children are enrolled in the *Early Enrollees* schools are associated, if any, with higher labour market prospects and educational attainment. The small, non-significant differences go in a direction such that one would expect them to make our estimates lower bounds of the true effects.

[25] Birth order among siblings is significantly higher among treated households. Nonetheless, we do not believe this to be problematic given that household composition does not differ between the two groups.

[26] We define urban area as a local community with more than 35,000 inhabitants, in line with the definition of the Municipality of the City of Belgrade that distinguishes between urban and suburban areas in its own territory.

[27] We asked households whether there were only Roma or both Roma and non-Roma in their community/neighbourhood (200 meters around their house). Therefore, the neighbourhoods do not correspond exactly to the 13 settlements where the survey was conducted.

[28] Overall, the characteristics of our sample are in line with official data (LSMS, 2003). Surprisingly, there are few households where both parents have not completed primary school (7 percent) and in a relatively large share of households at least one parent has completed secondary school (19 percent). However, this might be driven simply by the fact that LSMS data are only collected in segregated settlements that are likely to be poorer.

[29] With nine clusters and 75 students per school, the minimum detectable effect in our case is 0.35 of a standard deviation (Spybrook *et al.*, 2011).

Table 2. ***Early Enrollees*** **vs.** ***Late Enrollees***
Means of control variables in treated and control households

	All	Treatment	Control	Difference
Variables at the household level				
Wealth[a]	0.08	−0.14	0.22	−0.36
				(0.39)
Monthly total income (in dinars)[b]	28,949.47	28,224.39	29,453.33	−1,228.94
				(2,574.97)
Informal (=1)[c]	0.32	0.31	0.32	−0.01
				(0.05)
Urban (=1)	0.51	0.47	0.53	−0.06
				(0.06)
Only Roma in neighbourhood (=1)[d]	0.21	0.28	0.16	0.12
				(0.07)
No schooling/unfinished primary school (=1)[e]	0.07	0.07	0.07	0.00
				(0.03)
Finished primary school (=1)[e]	0.74	0.69	0.76	−0.07
				(0.05)
Finished secondary school (=1)[e]	0.19	0.23	0.16	0.07
				(0.05)
Muslim (=1)	0.71	0.57	0.80	−0.23**
				(0.10)
Number of children under 5 years	0.72	0.75	0.70	0.05
				(0.10)
Number of female children between 6 and 15 years	1.65	1.73	1.59	0.14
				(0.13)
Number of male children between 6 and 15 years	1.75	1.80	1.80	0.10
				(0.12)
Number of adults	2.44	2.46	2.44	0.02
				(0.12)
Max no. observations	300	122	178	
Variables at the individual level				
Children characteristics				
Male (=1)	0.52	0.50	0.54	−0.04
				(0.04)
Age of child	9.89	10.11	9.74	0.37
				(0.28)

Table 2. (Continued)

	All	Treatment	Control	Difference
Rank among siblings	2.20	2.33	2.11	0.22**
				(0.10)
Mark in Mathematics[f]	2.77	2.86	2.70	0.16
				(0.11)
Mark in Serbian[f]	2.85	2.94	2.79	0.15
				(0.11)
Max no. observations	673	280	393	

***Notes*:** Standard errors in parentheses: significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.
[a]The wealth index was calculated with principal component analysis. The index ranges between −5.55 and 3.69.
[b]28,950 dinars corresponds to 279 Euro (1 RSD = 0.009626 Euro, November 2011).
[c] = 1 if at least one household member works in the informal sector.
[d] = 1 if the respondent declared that the household lives in an exclusively Roma neighbourhood.
[e]It refers to the highest level of education obtained by parents.
[f]We use demeaned mark in Mathematics and Serbian. The mark is demeaned from the average school mark.

In addition to the comparability between treatment and control group, identification requires the absence of selective sorting into treatment. Schools and assistants were informed in late June 2009 if they had been accepted into the programme. Neither the Ministry of Education nor the schools disseminated the information about the RTA programme among the parents. The programme was not publicized on TV or radio. By the end of June when the Ministry decided who would participate in the programme, parents whose children were going to enter the first grade in September of that year had already enrolled them in school. Our data also confirm that *Early Enrollees* did not attract more Roma students than *Late Enrollees* in the first year of the programme.[30] We can conclude that there is no selection of children into schools.

3.2 Second definition: Directly helped vs. not directly helped by the assistant in Early Enrollees schools

Our definition of treated and control group assumes that everyone in a school with an assistant is aware of her presence. Roma people usually live in communities where they know each other and regularly interact. They are even in contact with people living elsewhere belonging to the same community. Family and community

[30] Roma pupils attending *Early Enrollees* schools in the pre-treatment year – 2008/2009 – corresponded to 2.4 percent of all Roma enrolled in these schools. In *Late Enrollees* they accounted for 2.1 percent. In 2009/2010 these percentages were 1.6 percent and 1.3 percent, respectively. The number of Roma pupils enrolling in school for the first time reduced from one year to the other but it did it proportionally in both types of schools.

ties are strong. Moreover, all the assistants live and work in the same municipality. It is unlikely that households do not know that there is a Roma assistant in the school their children are enrolled in. However, there is only one assistant per school and not every Roma child in the treated school is helped by her. One can argue then that only parents of children directly interacting with the assistant are the actual treated group. We can therefore explore another definition of being treated beside the main one. A household is treated if at least one child is in an *Early Enrollee* school and there is evidence from the survey that the assistant has worked with her. The school cannot keep track of the names of the children with whom the assistant interacts but we obtain this information from the parents. In this case we are certain that the treated households are aware of the presence of the assistant. A household is treated if either (1) parents state that there is someone in the school who helps the child with her homework or she is having additional classes at school, or (2) there is someone from the school who comes to her place or calls her because of the child. In these cases we know from the parents the name of the person who is helping their child and we can match it with the name of the assistant.[31]

Table 3 reports the characteristics of households with treated and not treated children in *Early Enrollees* schools.[32]

The differences in means between those helped by the assistant (*treated*) and those not helped (*untreated*) are often not statistically significant. Nonetheless, children helped by the assistant mainly live in urban areas and have at least one family member working in the informal sector and less educated parents. There are also more Muslims among them. Moreover, the assistant works mainly with younger children, as also suggested by the guidelines of the programme.

3.3 Outcome variables

We use three different sets of questions to understand whether the programme is effective in changing parents' expectations about their children's future opportunities. (See section B.2. of the online Appendix to understand how our variables are derived from the Likert scale.) We focus on parents' expectations because we believe that at such a young age (6–15 years) they are more relevant for children's educational attainment and more reliable for expected returns to education than children's expectations. However we ask pupils about the highest expected level of education they expect to achieve (see section B.4. of the online Appendix).

[31] We decided not to explicitly ask the parents whether their child's school was in the RTA programme because it was not clear to us whether the parents are aware of the name of the programme and how they perceive the teaching assistant, for instance as assistant or teacher.

[32] Treated children account for 50 percent of pupils in *Early Enrollees* schools. This percentage is slightly higher than has been reported before – assistants work with one-third of Roma students – because in the survey, we selected households with at least one child in the lower four grades of primary school (not in all eight grades). The rationale is that we know that assistants work mainly, if not exclusively, with them.

Table 3. ***Early Enrollees***
Means of control variables for treated and untreated households

	Treated	Untreated	Difference
Variables at the household level			
Wealth[a]	−0.38	0.13	−0.51
			(0.46)
Total income (in dinars)[b]	27,905	29,052	−1,147
			(3,270)
Informal (=1)[c]	0.39	0.25	0.14*
			(0.081)
Urban (=1)	0.66	0.29	0.37***
			(0.07)
Only Roma in neighbourhood (=1)[d]	0.35	0.21	0.14
			(0.09)
No schooling/unfinished primary school (=1)[e]	0.11	0.03	0.08
			(0.06)
Finished primary school (=1)[e]	0.63	0.75	−0.12*
			(0.07)
Finished secondary school (=1)[e]	0.25	0.21	0.03
			(0.07)
Muslim (=1)	0.68	0.47	0.21**
			(0.09)
Number of children under 5 years	0.87	0.66	0.21
			(0.16)
Number of female children between 6 and 18 years	1.82	1.67	0.15
			(0.16)
Number of male children between 6 and 18 years	1.86	1.75	0.11
			(0.18)
Number of adults	2.53	2.38	0.15
			(0.17)
Max no. observations	65	56	
Variables at the individual level			
Children characteristics			
Male (=1)	0.5	0.5	0
			(0.05)
Age of child	9.73	10.44	−0.69**
			(0.31)

Table 3. (Continued)

	Treated	Untreated	Difference
Rank among siblings	2.34	2.33	0.01
			(0.17)
Mark in Mathematics[f]	2.91	2.81	0.10
			(0.14)
Mark in Serbian[f]	3.03	2.87	0.16
			(0.14)
Max no. observations	148	130	

***Notes*:** Standard errors in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.
[a]The wealth index was calculated with principal component analysis. The index ranges between −5.55 and 3.69.
[b]28,950 dinars corresponds to 279 Euro (1 RSD = 0.009626 Euro, November 2011).
[c] = 1 if at least one household member works in the informal sector.
[d] = 1 if the respondent declared that the household lives in an exclusively Roma neighbourhood.
[e]It refers to the highest level of education obtained by a household member.
[f]We use demeaned mark in Mathematics and Serbian. The mark is demeaned from the average school mark.

The first and second set of questions relates to expected returns to education. Although either the mother or father (or caretaker) are asked these questions, the mother is the main interviewee in 92 percent of cases. They are asked about the oldest boy and the oldest girl in the household.[33] The first set of questions considers expectations about the likelihood of getting a job given a certain level of education attained. The second set of questions elicits minimum and maximum amounts parents expect that their children will earn once employed. We obtained the minimum and maximum earnings and we use their (log) average as our measure of expected earnings. The third relevant outcome is the highest expected educational level of the child. The question is asked for each child between 6 and 15 years old, so that all the oldest boys and oldest girls are included and possibly their younger siblings as well.[34,35] Summary statistics for

[33] In the pilot survey, we asked the questions for each child but we realized that there was no variation in the responses between the children of the same sex. As a result, we decided to ask these questions only with regard to the oldest male and for the oldest female child. In only 6 percent of cases, the oldest child is older than 15 and thus not enrolled in a compulsory school. In this case, respondents were asked to respond to these questions for the second oldest child.
[34] The median number of children aged 6–15 per household is 2. There are many missing values for this outcome of interest. This explains why our sample is as large as with the other outcomes.
[35] For the sake of consistency, we also estimate the impact with a reduced sample, corresponding only to the case that *(name)* is the oldest boy or the oldest girl in the household. The results are not reported, but they are discussed in the footnotes.

the outcome variables in our sample are reported in Table 4 and suggest a possible positive impact of the programme on both expected salary and level of education attained.

Table 4. ***Early Enrollees*** **vs.** ***Late Enrollees***
Means of outcome variables in treated and control households

	All	Treatment	Control	Difference
Variables at the household level				
Probability of finding a job: Boys				
With primary school (=1)[a]	0.42	0.35	0.48	−0.13**
				(0.06)
With secondary school (=1)[a]	0.82	0.82	0.82	0.00
				(0.05)
Probability of finding a job: Girls				
With primary school (=1)[a]	0.35	0.31	0.39	−0.08
				(0.06)
With secondary school (=1)[a]	0.79	0.74	0.82	−0.07
				(0.05)
Max no. observations	296	120	176	
Expected mean log earning: Boys				
With primary school	9.91[b]	9.97	9.87	0.10
				(0.06)
With secondary school	10.21[c]	10.24	10.18	0.06*
				(0.19)
Expected mean log earning: Girls				
With primary school	9.82[d]	9.90	9.78	0.12*
				(0.07)
With secondary school	10.14[e]	10.18	10.11	0.07*
				(0.04)
Max no. observations	241	97	144	
Variables at the individual level				
Expected to finish: Boys				
Secondary school (=1)	0.61	0.67	0.57	0.10*
				(0.06)
Expected to finish: Girls				
Secondary school (=1)	0.63	0.67	0.60	0.07
				(0.06)
Max no. observations	299	120	179	

Table 4. (Continued)

***Notes*:** Standard errors in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.
[a]Respondent expects the child to find a job given a certain level of education achieved.
[b]The corresponding average earning is 21,709 dinars (208 Euro, November 2011). For treated households is 22,985 dinars (221 Euro, November 2011); for control households is 21,075 dinars (202 Euro, November 2011).
[c]The corresponding average earning is 28,654 dinars (276 Euro, November 2011). For treated households is 29,398 dinars (283 Euro, November 2011); for control households is 28,141 dinars (271 Euro, November 2011).
[d]The corresponding average earning is 19,432 dinars (187 Euro, November 2011). For treated households is 20,915 dinars (201 Euro, November 2011); for control households is 18,682 dinars (180 Euro, November 2011).
[e]The corresponding average earning is 26,923 dinars (259 Euro, November 2011). For treated households is 27,529 dinars (265 Euro, November 2011); for control households is 26,527 dinars (255 Euro, November 2011).

4. Estimation strategy and results

4.1 First definition: Early Enrollees vs. Late Enrollees

We estimate the impacts of the RTA programme on expected returns to education with the following specification:

$$Y_j = \alpha_0 + \alpha_1 treatment_j + \alpha_2 X_j' + \varepsilon_j \quad (1)$$

where Y_j corresponds to the outcomes of interest for the household j: likelihood of finding a job with primary school as the highest level of education attained, likelihood of finding a job with secondary school as the highest level of education attained, (log) mean amount of earnings per month with primary education and (log) mean amount of earnings per month with secondary education. $treatment_j$ equals 1 if there is at least one child in the household who goes to an *Early Enrollees* school. X_j' includes household wealth *per capita*, if a member of the family works in the informal sector, if the household lives in a urban area and in a Roma neighbourhood, if the household is Muslim and the maximum educational level of parents and household composition characteristics. For the outcome 'secondary school as the highest expected level of education', we have information for each child in the household between 6 and 15 years old. We introduce a second specification where the dependent variable is at the child level:

$$Y_{ij} = \beta_0 + \beta_1 treatment_j + \beta_2 X_{ij}' + v_{ij} \quad (2)$$

where $treatment_j$ is defined as above. X_{ij}' also includes age and age squared of the child, the child's gender, birth order among siblings, and – in order to control for each child's ability – demeaned mark in Mathematics and Serbian of the previous school year.[36] Robust standard errors are clustered at the school level and corrected

[36] The marks are demeaned from the average school marks (among Roma). For children in their first grade, the average school marks are used.

with Moulton confidence intervals in case of linear regressions (Imbens and Kolesár, 2016). Regressions are estimated separately for boys and girls because we are interested in the effects for each gender.[37] We also report the results with the pooled sample in Table A3 of the Appendix.

The results for the probability of finding a job, expected earnings and highest expected educational level are reported in Table 5. For purposes of consistency, all the estimates are OLS, but probit estimates for the two dummy outcomes confirm the results. We need to bear in mind that the way in which the programme is designed constrains us to nine clusters – the schools involved in the programme – and 75 pupils treated per school, who are equally distributed between boys and girls. The intra-class correlation with our outcomes is equal to 0.05 for the sample of boys and 0.10 for the sample of girls. With a power of 0.8, the minimum detectable effect is 0.5 of a standard deviation (McConnell and Vera-Hernandez, 2015; Spybrook *et al.*, 2011). Nonetheless, we obtain statistically significant results with the limited power of 0.4.

Columns (1)–(4) show estimates for boys, while columns (5)–(8) refer to girls. The coefficients for the expected probabilities of finding a job with primary and secondary school as the highest level of education are reported in the top panel of Table 5. The results are not statistically significant in all specifications. We retain the null hypothesis that the population mean of the outcome variable in the treatment group will be the same as in the control group for the probability of finding a job. However, due to the limited power of our study, we may have committed a Type II error, that is, the probability of finding no intervention effect when one actually exists, making these results virtually impossible to interpret. We cannot claim the programme to be ineffective on job opportunities if there is low power, since low power means that we have little chance of finding an effect on the mean of the outcome variable when there is a true effect.

The middle part of the table shows the results for the expected (log) mean earnings per month. Parents in treated households expect higher future wages for both boys and girls, although the results are statistically significant only at 10 percent. Conditional on having attained a secondary educational level, being in a treated household increases the expected monthly earnings by almost 9.4 percent for boys and 10.07 percent for girls, on average (0.286 and 0.291 of a standard deviation, respectively).[38] This increase corresponds to almost 26 euros (roughly 7 euros more per week) with respect to an average expected earning in households not involved in the programme of 271 euros for boys and 255 euros for girls. Thus, although

[37] It is worth investigating whether the gender of the assistant may affect boys and girls differently for our outcomes of interest. The results do not suggest that expectations change depending on the sex of the assistant. This seems to matter only for the likelihood of getting a secondary education level for girls. Their parents expect them to achieve a higher level of education when the assistant is female than when he is male. However, the caveat here is that among assistants only one is male.

[38] The regression coefficients can be interpreted as semi-elasticities. 0.090 corresponds to $100 \times (e^{0.090} - 1)$; 0.096 corresponds to $100 \times (e^{0.096} - 1)$. We estimate the effects also with minimum and maximum earnings. The results are similar.

Table 5. ***Early Enrollees*** **vs.** ***Late Enrollees***
All outcomes by education level and by gender

Max. level of education	Boys				Girls			
	Primary school		Secondary school		Primary school		Secondary school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Probability of finding a job with primary/secondary school								
Treatment	−0.109	−0.069	0.010	0.016	−0.067	−0.111	−0.060	−0.032
	(0.077)	(0.078)	(0.058)	(0.065)	(0.078)	(0.074)	(0.054)	(0.055)
Controls[a]	No	Yes	No	Yes	No	Yes	No	Yes
No. observations	300	279	300	279	296	271	294	270
R^2	0.012	0.138	0.000	0.071	0.005	0.118	0.005	0.122
Expected log earnings with primary/secondary school								
Treatment	0.128	0.108*	0.077	0.090*	0.149*	0.123*	0.075	0.096*
	(0.078)	(0.072)	(0.056)	(0.048)	(0.078)	(0.071)	(0.064)	(0.055)
Controls[a]	No	Yes	No	Yes	No	Yes	No	Yes
No. observations	129	120	247	226	105	98	233	218
R^2	0.031	0.201	0.017	0.122	0.050	0.241	0.015	0.147
Secondary school as the highest expected level of education								
Treatment			0.097	0.120*			0.067	0.003
			(0.097)	(0.069)			(0.139)	(0.172)
Controls[b]			No	Yes			No	Yes
No. observations			299	232			275	221
R^2			0.009	0.346			0.005	0.230

***Notes*:** Robust standard errors corrected for clustering at the school level with Moulton confidence intervals in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

[a]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults.

[b]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

treated parents may not expect higher employment perspectives for their children, they do expect higher wages once they obtain a job. This suggests that they expect them to get better jobs.[39] Moreover, being in a treated household increases the expected monthly earnings by almost 11 percent for boys and 13 percent for girls, on average, conditional on having attained a primary educational level (0.312 and 0.123 of a standard deviation, respectively).

The regression results for secondary education as the highest expected level of education are reported in the bottom part of the table. We find that parents in treated households are more likely to expect their children to finish secondary school. The impact is statistically significant only for boys. On average, parents of pupils in *Early Enrollees* schools are 12 percentage points more likely to expect their sons to finish secondary school (0.243 of a standard deviation).[40]

4.2 *Second definition: Directly helped vs. not directly helped by the assistant in Early Enrollees schools*

There is only one assistant per school and not every Roma child in the treated school is helped by her. One can argue that only parents of children directly interacting with the assistant can be affected by her presence in the school. Our second definition considers a household to be treated only if at least one child attends an *Early Enrollees* school and the assistant has worked with her.

The assistant chooses the pupils she works with: treated children are not randomly selected. The children receiving the treatment are a selected subset of *Early Enrollees* school children. A simple comparison between those actually helped and the control group (those not helped in *Early Enrollees* schools and children in *Late Enrollees* schools) would be misleading. To address this problem, we use an instrumental variable strategy. By assumption, here being in a Early Enrollees school is only capturing the intention-to-treat (ITT) effect, because only some children in treated schools work with the assistant and are therefore actually treated. Instead, we use the assignment of treatment (being in an Early Enrollee school) as an instrumental variable for treatment received (being helped by the assistant) to estimate the effect of treatment on the treated.

$$Y_{(i)j} = \theta_0 + \theta_1 assistant_j + \theta_2 X'_{(i)j} + \varepsilon_{(i)j} \tag{3}$$

where $Y_{(i)j}$ corresponds to the outcomes of interest of individual i in household j: likelihood of finding a job with primary school as the highest level of education attained, likelihood of finding a job with secondary school as the highest level of

[39] The minimum monthly wage in Serbia in 2010 was almost 200 euros (21,645 dinars; 1 RSD = 0.009626 euros as of November 2011). The average wage was around 330 euros (34,422 dinars) and in Belgrade it was around 400 euros, corresponding to 42,421 dinars (Serbian Statistical Office, 2010).

[40] If we consider only the oldest boy and the oldest girl in the household we obtain similar results. The magnitude of the coefficients is even larger. The coefficient of treatment for boys is statistically significant at 10 percent.

education attained, (log) mean amount of earnings per month with primary education and (log) mean amount of earnings per month with secondary education and secondary school as the highest expected level of education. $assistant_j$ is equal to 1 when there is at least one child in the household who is directly helped by the assistant, and 0 otherwise. Given the problem of selection bias, we know that the error term $\varepsilon_{(i)j}$ is composed here of two parts:

$$\epsilon_{(i)j} = \eta_{(i)j} + u_{(i)j} \tag{4}$$

where $\eta_{(i)j}$ is an unobservable individual term and $u_{(i)j}$ is a random term. $assistant_j$ depends on some factors captured by $\eta_{(i)j}$. We therefore model $assistant_j$ in a reduced form framework as follows:

$$assistant_j = \gamma_0 + \gamma_1 treatment_j + \gamma_2 X'_{(i)j} + \eta_{(i)j} + v_{(i)j} \tag{5}$$

where $treatment_j$ is equal to 1 if there is at least one child in the household enrolled in a *Early Enrollees* school. Being enrolled in a *Early Enrollees* school is correlated with the fact of being helped by the assistant but uncorrelated with any unobservable attributes that affect the outcomes of interest. The instrument is as good as randomly assigned. It also satisfies the exclusion restriction by assumption: only parents of children directly interacting with the assistant are aware of her presence in the school. The instrument operates only through the fact of being helped by the assistant ($Y_i(d, 0) = Y_i(d, 1)$ for $d = 0, 1$). The monotonicity assumption needed for heterogeneous IV models holds: while the instrument may have no effect on some people, all those who are affected are affected in the same way. θ_1 in Equation (3) captures the local average treatment effect (LATE), which in this case is the effect of treatment on the treated. There are no *always-takers* in this case that is children who would receive the treatment independently of the school that they attend. Children who are helped by the assistant are only in treated schools (*Early Enrollees* schools). The treated population consists only of *compliers*: children who are assigned to treatment are also those who actually do receive the treatment.

Results for the probability of finding a job, expected earnings and highest expected educational level are reported in Table 6.[41] Columns (1) to (2) show the estimates for boys, while columns (3) to (4) show the estimates for girls.

[41] The use of IV to solve selection bias problems is illustrated in Table A4 in the Appendix. Columns (1) and (2) report OLS results. These estimates are misleading because they compare pupils according to the actual treatment received: those helped by the assistant vs. those not helped in the same *Early Enrollees* schools and children in control schools. Columns (3) and (4) compare pupils according to whether they are potentially treated: being in a *Early Enrollees* or *Late Enrollees* school. This is the intention-to-treat (ITT) effect. Since *treatment* was as good as randomly assigned, ITT tells us the causal effect of being in a *Early Enrollees* school. It builds in the fact that some pupils in treated schools are not treated. For this reason, it is smaller than the average casual effect on those actually treated. It clearly corresponds to our main specification where we assume instead that everyone in a *Early Enrollees* school is treated. Columns (5) and (6) measure the effect of treatment on the treated. They do not consistently differ from OLS estimates because treatment and control groups are not so different in observable characteristics. We know that there is a problem of selection bias because the assistant chooses the pupils to work with. Still, her choice seems to be close to a random choice. The selection bias in this case is negative: those who are helped by the assistant tend to be the worst students.

Table 6. Helped by the assistant
All outcomes by education level and by gender

Max. level of education	Boys		Girls	
	Primary school (1)	Secondary School (2)	Primary school (3)	Secondary school (4)
Probability of finding a job with primary/secondary school				
Assistant	−0.135	0.032	−0.224	−0.061
	(0.149)	(0.111)	(0.152)	(0.103)
Controls[a]	Yes	Yes	Yes	Yes
No. observations	276	276	268	267
R^2	0.143	0.070	0.113	0.120
Expected log earnings with primary/secondary school				
Assistant	0.285*	0.190*	0.284	0.194*
	(0.165)	(0.109)	(0.189)	(0.116)
Controls[a]	Yes	Yes	Yes	Yes
No. observations	119	224	98	216
R^2	0.162	0.047	0.217	0.094
Secondary school as the highest expected level of education				
Assistant		0.260*		0.007
		(0.136)		(0.176)
Controls[b]		Yes		Yes
No. observations		232		221
R^2		0.340		0.231
First stage – Being helped by the assistant				
Treatment		0.472***		0.465***
		(0.058)		(0.073)
Controls[c]		Yes		Yes
No. observations		232		221
R^2		0.464		0.446
F-statistic on treatment		84.14		47.21

***Notes*:** Robust standard errors corrected for clustering at the school level in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.

[a]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults.

[b]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

[c]The coefficients are estimated both with the controls used with the first two outcomes and with the third one. The estimates reported are obtained by using the outcome 'secondary school as the highest expected level of education'. Therefore, here control variables include wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

The coefficients for the expected probabilities of finding a job with the primary and secondary school as the highest level of education are reported in the top panel. They are similar to those obtained in the main specification. As before, results are not statistically significant in all specifications and, due to low power, we cannot claim the programme to be ineffective on job opportunities. The second part of the table shows the results for the expected (log) mean earnings per month. Parents in treated households expect higher future wages for both boys and girls, as in the main specification. The impacts are reasonably higher: in this case, we are sure parents know the assistant and their children are actually helped by her. Conditional on having attained a secondary education level, being in a treated household increases the expected monthly earnings by almost 21 percent for both boys and girls, on average.[42] This increase corresponds to roughly 55 euros. As before, for boys this is the case also conditional on having attained a primary educational level: being in a treated household increases the expected monthly earnings by almost 33 percent,[43] on average, corresponding to 89 euros. The regression results for secondary education as the highest expected level of education are reported in the third part of the table. We find that parents whose children are helped by the assistant are more likely to expect their children to finish secondary school. As in the main specification, the impact is statistically significant only for boys. On average, parents of pupils in *Early Enrollees* schools are 26 percentage points more likely to expect their sons to finish secondary school.[44] The bottom part of Table 6 reports the results for the first-stage. The coefficients of being in an *Early Enrolless* school are positive, as expected, and highly statistically significant. The first-stage results for the instrumental variable estimation show that F-statistics on the incidence of treatment are clearly above 10.[45]

4.2.1 Remedial education programme and role model

Knowing who is actually helped by the assistant allows us to better understand the possible mechanisms behind the changes in expectations we observe. The effect of the programme passes through the remedial education channel: parents expect their children to go to school more because they now perform better thanks to the assistant. Moreover, in the Roma Teaching Assistant programme all the assistants are Roma and from the same social background as the pupils they help. In order to obtain the job they needed to have invested in education in the first place. They can therefore act as role models for the children with whom they work. The presence of a person sharing her successful story can affect children's and their parents' expectations about their future. Their accomplishment can shape parents' and children's beliefs about what they can achieve and in turn can affect educational choices.

[42] They correspond to 0.627 and 0.608 of a standard deviation, respectively.
[43] It corresponds to 0.821 of a standard deviation.
[44] It corresponds to 0.531 of a standard deviation.
[45] *F*-statistics on the incidence of treatment are reported in the bottom line of Table 6. The Cragg–Donald Wald *F*-test of weak instruments is equal to 116.297.

We first consider the remedial education channel. In our survey we ran quick tests in the subjects of Mathematics and Serbian. We define *Maths score* equal to 1 when the child is able to correctly answer both questions – 'Please tell me how much is 5 + 4?' and 'Please tell me how much is 23 + 12?' and 0 otherwise. We define *Serbian score* as equal to 1 when the child is able to read and write. A child is able to read when she knows how to correctly read the sentence written on a card 'Could you please read me the letters, the word and the sentence on this card?'; *Able to read* takes the value of 0 when she does not know letters, recognizes only letters or is able to read the words but cannot read the whole sentence. A child is able to write when she knows how to correctly write a proposed sentence 'Please write the following sentence'; *Able to write* takes the value of 0 when she does not know how to write at all or she writes the sentence with mistakes. These abilities are supposed to be acquired in the first year of primary school. Hence, we do expect children of any grade to be able to answer them. Results for the LATE are reported in Table 7.

Pupils who are helped by the assistant perform better than their classmates and pupils in *Late Enrollees* schools in both test scores, although the impacts are statistically significant only for Serbian. On average, boys get 0.56 of a standard deviation more; girls get 0.66 of a standard deviation more.[46] Thanks to the assistant, treated kids learn more and these impacts are large.

In order to disentangle the two possible mechanisms we have in mind – remedial education and role model – we select only those pupils who did not answer the Serbian and the Maths tests correctly.[47] If among the worst performers we find that expectations have increased for those helped by the assistant, we have evidence that the effect of the programme does not occur only through the remedial education channel. They have been helped by the assistant but they are not learning more at school. Still, the parents believe that their children's returns to education will be higher and that they will attain a secondary level of education. We are aware that parents may misperceive their children abilities and overestimate them (Dizon-Ross, 2013). We are assuming that this happens in the same way for treated and untreated households. If, however, we believe that misperception takes place differently between the two types of households, it is likely that our estimates are underestimating the effect. Parents with children helped by the assistant can be more informed of their actual abilities and less likely to overestimate them than parents who do not interact with the assistant. The results are reported in Table 8.

[46] We also studied the impact of the programme on schooling outcomes in its first year of implementation in a companion study Battaglia and Lebedinski (2015). In that study, we found that the programme had a positive effect. There is evidence that children involved in the RTA programme attended school more regularly and that, on average, marks improved and dropout rates reduced for children in their first grade. Higher and more systematic impacts are obtained in schools with a lower number of Roma, especially if female.

[47] To check the robustness of our findings, we estimate the effects separately by selecting those who did not answer the Serbian test correctly and those who did not answer the Maths test correctly. The results do not change.

Table 7. Helped by the assistant
Test scores by gender

	Maths score		Serbian score		Able to read		Able to write	
	Boys (1)	Girls (2)	Boys (1)	Girls (2)	Boys (1)	Girls (2)	Boys (1)	Girls (2)
Assistant	0.133	0.085	0.280*	0.304*	0.187	0.107	0.170	0.100
	(0.149)	(0.158)	(0.154)	(0.159)	(0.147)	(0.161)	(0.138)	(0.124)
Controls[a]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. observations	189	153	185	153	189	155	184	154
R^2	0.210	0.186	0.172	0.046	0.136	0.106	0.228	0.131

***Notes*:** Robust standard errors corrected for clustering at the school level in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.
[a]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

The previous results are confirmed, although caution should be taken in the interpretation of the coefficients given the even smaller sample size. The results suggest a positive trend in expectations for the probability of finding a job, although they are still not statistically significant. Conditional on having attained a certain educational level, being helped by the assistant may increase the expected monthly earnings, although the results are not significantly different from zero. Due to the limited power of our study, we cannot interpret them as the programme was ineffective on these outcomes. The coefficients for the highest expected level of education are higher than in the previous case when the whole sample is considered. On average, parents of pupils directly interacting with the assistant are 39.2 percentage points more likely to expect their sons to finish secondary school.[48] Even if their children – who are helped by the assistant – are not learning at school, parents still believe they will go to school more and have higher returns to education, once the children have attained a secondary level of education. There is evidence that the programme changes parents' expectations not only through the remedial education channel. The presence of a person from the same social background sharing her successful story may affect parents' expectations about their children's future. Yet, the unfortunate absence of non-Roma assistants does not allow us to state the effective relevance of the role model. In the RTA programme, all the assistants are Roma and, given that there is no variation in whether the assistant is Roma or not, it is hard to draw strong conclusions.

[48] It corresponds to 0.927 of a standard deviation.

Table 8. Helped by the assistant. Worst performers
All outcomes by education level and by gender

Max. level of education	Boys		Girls	
	Primary school (1)	Secondary School (2)	Primary school (3)	Secondary school (4)
Probability of finding a job with primary/secondary school				
Assistant	−0.259	−0.025	−0.156	0.026
	(0.177)	(0.151)	(0.215)	(0.138)
Controls[a]	Yes	Yes	Yes	Yes
No. observations	151	151	145	145
R^2	0.164	0.158	0.074	0.234
Expected log earnings with primary/secondary school				
Assistant	0.580	0.146	0.321	0.084
	(0.438)	(0.160)	(0.270)	(0.155)
Controls[a]	Yes	Yes	Yes	Yes
No. observations	77	121	60	118
R^2	0.303	0.106	0.249	0.107
Secondary school as the highest expected level of education				
Assistant		0.392**		0.022
		(0.180)		(0.233)
Controls[b]		Yes		Yes
No. observations		128		117
R^2		0.372		0.269

***Notes*:** Robust standard errors corrected for clustering at the school level in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.
[a]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults.
[b]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

4.3 Dropout rates at the end of primary school

It is not a foregone result that the effects we observe on parents' expectations necessarily translate to more years of schooling. Data on persistence in school can help us to shed light on the actual effects of the RTA programme. Unfortunately, both types

of schools – *Early* and *Late Enrollees* – are treated starting from 2010 and it is impossible to detect the long-term effects on dropout rates in the absence of a control group. Using administrative data from the final examination at the end of primary school obtained from the Ministry of Education (Serbian Ministry of Education, 2016), we can track the children of our survey from *Early* and *Late Enrollees* schools, observe whether they finish primary school and provide descriptive statistics on the share of pupils who finished primary school according to the number of years exposed to the treatment. We can repeat the same exercise with children who were directly helped by the assistant in *Early Enrollees* schools.[49]

Table 9 shows the share of pupils from our survey who finished primary school according to the number of years of treatment, both for all pupils (columns (1) and (2)) and for those pupils who were helped by the assistant (columns (3) and (4)). The maximum number of years of treatment is seven because the programme was introduced in the 2009/2010 school year and the available final examination dataset ends in September 2016. The minimum number of years of treatment is five because the assistants worked primarily with the lower four grades and started to work in 2009/2010: in 2009/2010 the oldest treated pupils were in the fourth grade and they finished primary school at the earliest in 2013/2014.

We can observe that, in both samples, the share of pupils finishing primary school is higher if pupils were treated for seven years than if they were treated for fewer years. Moreover, note also that the assistants worked mainly with low performing students and even these students were as likely to finish primary school as other students. We cannot claim there is a causality here: we cannot conclude that the effects on parents' expectations necessarily translated to more years of schooling.

Table 9. Share of pupils in our survey who finished primary school by number of years of treatment

	All pupils *Early* and *Late Enrollees*		**Pupils helped by the assistant *Early Enrollees***	
	Percentage	**No. observations**	**Percentage**	**No. observations**
	(1)	**(2)**	**(3)**	**(4)**
Seven years of treatment	37.78	45	40.00	20
Six years of treatment	28.04	107	25.93	27
Five years of treatment	31.51	73	25.00	16

***Source*:** Data from the final examination at the end of primary school (Serbian Ministry of Education).

[49] It is too early to get any information on secondary education: pupils involved in the programme have not yet reached the age to finish secondary school. They were at most in the fourth grade in 2010 and therefore they are in the first grade of secondary education in 2016.

Nonetheless, the information available suggests that more years of exposure to the RTA programme are associated with a higher probability of at least finishing primary school. This is extremely relevant in a context where even primary completion rates are usually low.

5. Conclusion

We exploit the gradual implementation of the RTA programme to identify its impact on expectations. Our data, collected one year after the first implementation, suggest that parents of children involved in the programme expect higher returns to education for their children. They may not be more likely to expect them to find a job, but once employed they are expected to earn higher wages. This suggests that they might expect better jobs for them. On average, being in a treated household increases the expected monthly earnings by almost 9.4 percent for boys and 10.07 percent for girls. Moreover, on average, parents of pupils in treated schools are 12.3 percentage points more likely to expect their sons to finish secondary education than parents of pupils in control schools. However, there is only one assistant per school and not every Roma child in the treated school is helped by her. One can argue that only parents of children directly interacting with the assistant are the actual treated group. Our second definition of treatment considers a household to be treated only if at least one child is in a treated school and there is evidence from the survey that the assistant has worked with her. We obtain results similar to the main specification. The impacts are reasonably higher, although the results are always weakly statistically significant. The effect of the programme occurs through the remedial education channel, especially for pupils we know are helped by the assistant. Parents expect their children to go to school more because they now perform better thanks to the assistant. From our survey, we know that those helped by the assistant do better in test scores. However, if we select the students performing badly, we still find higher expected returns to education for pupils helped by the assistant. In addition to the remedial education mechanism, the presence of a person of the same social background who is successful may motivate parents to believe their children can succeed. We cannot conclude that the effects on parents' expectations necessarily translate to more years of schooling. Nonetheless, administrative data from the final examination at the end of primary school obtained from the Ministry of Education suggests that more years of exposure to the RTA programme are associated with a higher probability of at least finishing primary school.

The Roma Teaching Assistant programme raises the expectations of the targeted minority. Roma people may reasonably underestimate the need to invest in education: they often perceive low benefits of going to school compared to the respective costs and underinvest in education. The success of the remedial education mechanism, together with the provision of a role model, can be effective in increasing households' current investment in education. Moreover, remedial education

programmes that target minorities through the hiring of minority teachers can help create role models by providing previously unexpected opportunities to a group. The unfortunate absence of non-Roma assistants does not allow us to state the effective relevance of the role model. Yet, this study suggests replicable examples in contexts where minorities suffer low attainment rates and social exclusion. One year of a remedial education programme may not be enough to break the curse of low expectations, but encouraging results are found in this direction. Investigating the effects of such programmes in the long-run is a central question for future research.

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Appendix A

A.1 School characteristics

Table A1. Characteristics of the schools and assistants (Belgrade) the year before the introduction of the RTA program – 2008

	All	Early Enrollees	Late Enrollees	Difference (1 − 2)	*P*
	(1)	(2)	(3)	(4)	(5)
Characteristics of the schools					
Class size	22.22	20.00	25.00	−5.00	[0.147]
No. of Roma per class	4.67	3.60	6.00	−2.40	[0.493]
No. of Roma per class (if at least a Roma)	4.67	3.60	6.00	−2.40	[0.493]
No. of Roma per school (%)	0.22	0.22	0.21	0.01	[0.968]
Female					
Roma	0.48	0.52	0.45	0.07	[0.109]
Non-Roma	0.48	0.50	0.47	0.03	[0.289]
Born in the same town					
Roma	0.77	0.79	0.76	0.03	[0.353]
Non-Roma	0.89	0.90	0.88	0.02	[0.278]
Number of schools	9	5[a]	4		
Number of Roma pupils	567	238	329		
Number of Non-Roma pupils	2,199	975	1,224		

Notes:[a]*Early Enrollees* schools are 6. One school did not provide us the list of students so it is excluded from our sample.

A.2 First definition: Early Enrollees vs. Late Enrollees

Table A2. Marks, absences and dropouts in previous years

	All	Early Enrollees	Late Enrollees	Difference (1 − 2)	*P*
	(1)	(2)	(3)	(4)	(5)
Year of the introduction of the RTA program – 2009					
Marks in Mathematics	3.85	3.90	3.81	0.09	[0.495]
Marks in Serbian	3.99	4.05	3.95	0.10	[0.369]
Absences	58.49	60.02	57.34	2.68	[0.673]
Dropout	0.00	0.00	0.00	0.00	[0.577]
Number of schools	9	5[a]	4		
No. observations	2,693	1,153	1,540		
Previous year – 2008					
Marks in Mathematics	3.80	3.82	3.78	0.45	[0.705]
Marks in Serbian	3.94	3.95	3.94	0.01	[0.956]
Absences	60.88	68.72	55.05	13.67	[0.143]
Dropout	0.00	0.01	0.00	0.01	[0.560]
Number of schools	9	5[a]	4		
No. observations	2,638	1,123	1,515		

Notes: [a]*Early Enrollees* schools are 6. One school did not provide us the list of students so it is excluded from our sample.

A.3 Second definition: Directly helped vs. not directly helped by the assistant in Early Enrollees schools

Table A3. *Early Enrollees* vs. *Late Enrollees*
Pooled sample: all outcomes by education level

Max. level of education	Primary school (1)	Secondary school (2)
Probability of finding a job with primary/secondary school		
Treatment	−0.112	−0.046
	(0.094)	(0.111)
Treatment × male	0.037	0.050
	(0.107)	(0.114)
Controls[a]	Yes	Yes
Total Effect	−0.074	0.003
	(0.111)	(0.143)
No. observations	534	533
R^2	0.135	0.095
Expected log earnings with primary/secondary school		
Treatment	0.109	0.096
	(0.107)	(0.119)
Treatment × male	−0.029	−0.011
	(0.123)	(0.114)
Controls[a]	Yes	Yes
Total effect	0.079	0.085
	(0.525)	(0.308)
No. observations	209	431
R^2	0.223	0.139
Secondary school as the highest expected level of education		
Treatment		−0.001
		(0.111)
Treatment × male		0.123
		(0.115)
Controls[b]		Yes
Total effect		0.122*
		(0.074)

Table A3. (Continued)

Max. level of education	Primary school (1)	Secondary school (2)
No. observations		454
R^2		0.286

***Notes*:** Robust standard errors corrected for clustering at the school level with Moulton confidence intervals in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.
[a]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults.
[b]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

A.4 Heterogeneous effects

A.4.1 Muslim households vs. Non-Muslim households

Our main specification (1 and 2) is suggestive of the fact that there could be a differential effect of the programme on Muslim households: parents from Muslim households expect higher earnings conditional on finishing secondary school and they expect their children to attain a lower level of education for both genders when compared to non-Muslim households.[50] Moreover, the descriptive statistics (see Table 2) show that the treated and control groups differ in terms of the number of Muslim households: there are significantly more Muslim families among households with children enrolled in *Late Enrollees* schools. We think it would be worthwhile investigating whether the programme affects Muslims differently.

We proceed with the following specification (A1) which includes the interaction of being in a Muslim household and in a treated household:[51]

$$Y_{(i)j} = \delta_0 + \delta_1 treatment_j + \delta_2 muslim_j + \delta_3 treatment_j * muslim_j + \delta_4 X'_{(i)j} + \epsilon_j \quad \text{(A1)}$$

The outcomes, $Y_{(i)j}$, are the same as in previous estimations. The coefficient δ_1 captures the effect of treatment on non-Muslim households. The coefficient δ_2 captures the difference between Muslims and non-Muslims among the *Late Enrollees*, and δ_3 is the differential impact of interest. Our results are reported in Table A5.

[50] Results are not reported because we decided not to show the coefficients of control variables in any specification, but they are available upon request.
[51] In this specification $X_{(i)j}$ does not include if the household is Muslim.

Table A4. *Early Enrollees* vs. *Late Enrollees*
OLS and IV estimates: all outcomes by gender

Gender	OLS		ITT		IV	
	Boys	Girls	Boys	Girls	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
Probability of finding a job with primary/secondary school						
Primary school	−0.129*	−0.135*	−0.069	−0.116	−0.135	−0.224
	(0.076)	(0.081)	(0.078)	(0.065)	(0.149)	(0.152)
Secondary school	0.003	−0.031	0.016	−0.032	0.032	−0.061
	(0.084)	(0.076)	(0.065)	(0.055)	(0.111)	(0.103)
Controls[a]	Yes	Yes	Yes	Yes	Yes	Yes
Expected log earnings with primary/secondary school						
Primary school	0.097	0.148	0.108*	0.123*	0.285*	0.284
	(0.100)	(0.095)	(0.072)	(0.071)	(0.165)	(0.189)
Secondary school	−0.005	0.041	0.090*	0.096*	0.190*	0.194*
	(0.057)	(0.059)	(0.048)	(0.055)	(0.109)	(0.116)
Controls[a]	Yes	Yes	Yes	Yes	Yes	Yes
Secondary school as the highest expected level of education						
Secondary school	0.152*	0.138	0.120*	0.003	0.260*	0.007
	(0.082)	(0.148)	(0.069)	(0.172)	(0.136)	(0.176)
Controls[b]	Yes	Yes	Yes	Yes	Yes	Yes

***Notes*:** Robust standard errors corrected for clustering at the school level with Moulton confidence intervals in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent. The maximum number of observations with primary school is 276 for boys and 268 for girls; with secondary school is 276 for boys and 267 for girls.

[a]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults.

[b]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5 years, number of female children between 6 and 15 years, number of male children between 6 and 15 years and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

The estimates reported in Table A5 suggest that the programme does not impact Muslims and non-Muslims differently in terms of job market prospects and expected wages. Nonetheless, non-Muslim households who attend *Early Enrollees* schools are, on average, 21.4 percentage points more likely to expect their sons to finish secondary education compared to non-Muslim households in control schools. We do not know the religion of the assistants in order to investigate further.

Table A5. *Early Enrollees* vs. *Late Enrollees*
Heterogeneous effects: all outcomes for secondary school by gender

Max. level of education	Boys	Girls
	Secondary school	
	(1)	(2)
Probability of finding a job with secondary school		
Treatment	−0.026	−0.038
	(0.111)	(0.097)
Muslim	−0.010	0.020
	(0.091)	(0.085)
Treatment × muslim	0.064	0.009
	(0.134)	(0.118)
Controls[a]	Yes	Yes
No. observations	279	270
R^2	0.144	0.135
Expected log earnings with secondary school		
Treatment	0.009	−0.007
	(0.082)	(0.095)
Muslim	0.077	0.067
	(0.069)	(0.081)
Treatment × muslim	0.125	0.157
	(0.100)	(0.113)
Controls[a]	Yes	Yes
No. observations	226	218
R^2	0.131	0.158
Secondary school as the highest expected level of education		
Treatment	0.214**	0.175
	(0.116)	(0.311)
Muslim	−0.131	−0.097
	(0.100)	(0.239)
Treatment × muslim	−0.149	−0.244
	(0.145)	(0.336)
Controls[b]	Yes	Yes
No. observations	233	221
R^2	0.350	0.236

***Notes*:** Robust standard errors corrected for clustering at the school level with Moulton confidence intervals in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.
[a]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), number of children under 5, number of female children between 6 and 15, number of male children between 6 and 15 and number of adults.
[b]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), number of children under 5, number of female children between 6 and 15, number of male children between 6 and 15 and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

A.4.2 Young vs. old kids

Parents of younger children aged 6–10 years may respond differently to the programme than parents of older children aged 11–15 years. There are two reasons to expect this to be the case. First, assistants were explicitly asked to work more with younger children. Second, we know that the gap in knowledge between Roma and non-Roma pupils is already present when children enroll in primary school and that it increases over time. Under such circumstances, it might be easier to influence parents' expectations for younger children than for the older ones.

We estimate the following regression by gender. We have individual outcomes only for the expected educational level and we only estimate this outcome.

$$Y_{ij} = \vartheta_0 + \vartheta_1 treatment_j + \vartheta_2 young_{ij} + \vartheta_3 treatment_j \times young_{ij} + \vartheta_4 X'_{ij} + \tau_{ij} \qquad \text{(A2)}$$

where *young* is equal to 1 if the child is aged 6–10 years and equal to 0 if she is aged 11–15 years. The

results are shown in Table A6.

Our coefficients are not statistically significant when we compare boys in *Early Enrollees* and *Late Enrollees* schools, although the magnitude and direction are still suggestive of the effect. We find that there is little difference between younger and

Table A6. *Early Enrollees* vs. *Late Enrollees*
Heterogeneous effects by gender (Young vs. Old)

	Boys (1)	Girls (2)
Secondary school as the highest expected level of education		
Treatment	0.091	−0.159
	(0.102)	(0.166)
Young	0.071	−0.378**
	(0.106)	(0.157)
Treatment × young	0.055	0.279*
	(0.137)	(0.167)
Controls[a]	Yes	Yes
No. observations	233	221
R^2	0.348	0.264

***Notes*:** Robust standard errors corrected for clustering at the school level with Moulton confidence intervals in parentheses: *significant at 10 percent, **significant at 5 percent, ***significant at 1 percent.
[a]Control variables included are wealth, informal (=1), urban (=1), only Roma in neighbourhood, finished primary school (=1), finished secondary school (=1), muslim (=1), number of children under 5, number of female children between 6 and 15, number of male children between 6 and 15 and number of adults, age of child, age of child squared, rank among siblings, demeaned mark in Mathematics and demeaned mark in Serbian.

older boys. The programme affects the probability to finish secondary school similarly for both groups, although the effect is slightly higher for younger children. We find a different effect for girls. Young girls in *Early Enrollees* schools are on average 27.9 percentage points more likely to be expected to finish secondary school than older female schoolmates.

Supporting Information

Additional Supporting Information may be found in the online version of this article:

Appendix B.1: Official Data on Roma.
Appendix B.2: Likert scale
Appendix B.3: First definition: Early Enrollees versus Late Enrollees
Appendix B.4: Pupils' expectations