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Going to a Better School: Effects and Behavioral Responses†

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This paper applies a regression discontinuity design to the Romanian secondary school system, generating two findings. First, students who have access to higher achievement schools perform better in a (high stakes) graduation test. Second, the stratification of schools by quality in general, and the opportunity to attend a better school in particular, result in significant behavioral responses: (i) teachers sort in a manner consistent with a preference for higher achieving students; (ii) children who make it into more selective schools realize they are relatively weaker and feel marginalized; (iii) parents reduce effort when their children attend a better school. (*JEL* I21, I28, J13)

Whether students would benefit from attending higher-achievement schools is an important question in education. Clear evidence on this issue is scarce, in large part because students are not randomly allocated to schools. Nevertheless, as discussed below, several papers provide credible estimates of the effect of having access to a better school.

Such estimates do not provide a complete road map for policy, however, as they may reflect but not reveal *behavioral responses* that amplify or reduce the impact of educational quality. For instance, parents might react to their children going to a better school by lowering their own effort. There might also be reactions on the part of students; for example, an individual who makes it into a better school might feel inferior or be stigmatized.[1] Importantly, these responses might change over time, and may thus influence estimates differently depending on when outcome data are collected. Additionally, some of these responses—which we will refer to as *equilibrium effects*—may only emerge once interventions are taken to scale and sustained for a period of time.[2] To illustrate, stratifying students by ability might lead to reactions in the school system itself, e.g., the emergence of norms that assign more qualified

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[1] Partially along these lines, Cullen, Jacob, and Levitt (2006) explore how school choice affects students' attitudes and behaviors.

[2] See, for example, the discussions in Banerjee and Duflo (2008), Acemoglu (2010), and Deaton (2010).

teachers to brighter students. Thus, the very characteristics of an intervention may depend on its reach. The bottom line, as emphasized by Todd and Wolpin (2003), is that knowledge of such behavioral responses is crucial to a full understanding of educational interventions. Yet, there is little evidence on their empirical relevance.

In this context, this paper makes two contributions. First, using administrative data from all of Romania, it provides a rigorous estimate of the impact of going to a better school, where school quality is proxied by peer ability. Second, using data from a survey of parents, teachers, and principals in a subset of Romanian towns, it explores the existence of dynamic behavioral responses and equilibrium effects.

As stated, our starting point is that identifying the effect of access to a better school is challenging. Nonetheless, several analyses have exploited compelling research designs, with Dale and Krueger (2002); Cullen, Jacob, and Levitt (2006); and Hastings, Kane, and Staiger (2009) providing early examples. Several more recent papers rely on regression discontinuity (RD) designs. Specifically, Park et al. (2008), Hoekstra (2009), Saavedra (2009), and Jackson (2010a) find that relative to students who just miss gaining admission to high achievement educational institutions, those who make it have better academic and/or labor market outcomes. In contrast, Clark (2010); Duflo, Dupas, and Kremer (2011); Sekhri and Rubinstein (2010); Abdulkadiroglu, Angrist, and Pathak (2011); and Dobbie and Fryer (2011) find scant evidence of impacts from getting into a higher achievement school or class within a school.

We also apply an RD design to Romania's high school system, exploiting the fact that as they transition into secondary education, Romanian children's ability to choose a high school depends solely on a score which is the average of their performance on a nationwide test and their grade point average. After obtaining their transition score, students submit a list of high school and track (e.g., Mathematics and Social Studies) combinations they wish to enroll in. These tracks are essentially "schools within a school" in that their students take all their classes together, and do not take courses with members of other tracks.

After students have submitted their choices, they are allocated to school/tracks via a nationally centralized process that honors higher scoring students' requests subject to pre-established slot constraints.[3] This gives rise to cutoff scores that determine access into schools/tracks, and we show that there are clear discontinuities in educational quality at these cutoffs. For instance, relative to students who score just below a school cutoff, those who score just above experience, on average, a highly significant increase in the average transition score displayed by their peers. Further, this mechanism generates about 2,000 cutoffs, allowing us to explore the heterogeneity of school effects—whether being able to attend a more selective school, for example, is more valuable to a student whose initial performance is high or low.

We explore the effects of this variation on a "high stakes" outcome: performance on a Baccalaureate exam. Passing this exam is a requirement for application to university, and the grade is used by many institutions as a crucial admission criterion. We find that students do benefit from access to higher ranked schools and tracks within schools. Specifically, relative to individuals who just miss scoring above a

[3] As discussed below, the setting gives students incentives to truthfully reveal their preference rankings.

school cutoff, those who succeed display a statistically significant 0.02 to 0.10 standard deviation advantage in Baccalaureate performance.[4] If scaled by the associated improvements in peer quality, these effects are of a magnitude consistent with some estimates in the literature.[5] They are also often larger and more precisely estimated for cutoffs at higher grade levels.

Having established these results, we turn to describing behavioral responses using a survey we administered in a subset of towns. These data are consistent with teachers sorting in response to the stratification of students: teachers with higher certification standards are more likely to work at better-ranked schools. This sorting persists even within schools as one moves from a weaker to a stronger track, and even within tracks as one moves from a weaker to a stronger class.[6] As a result, although students who score just above a cutoff attend schools that on *average* have more certified teachers, the *marginal* (actual) teachers assigned to them are not observably different from those assigned to students who score just below the cutoff. In short, more qualified teachers are matched with higher achieving students. This seems to be an established norm in Romania, perhaps one that reflects a long term outcome of the interplay between teacher and parental preferences.

In terms of parental effort, a first finding is that children who just score above a cutoff receive *less* homework-related help from their parents. In this sense, Romanian parents may view educational quality and their own effort as substitutes. We also find areas where there seems to be no change in parental choices, again leading to differences in average versus marginal effects. For example, children who make it into more selective schools are exposed to peers whose parents are significantly more involved in their education, yet their own parents show no greater sign of such engagement.

In terms of student responses, we find that children who just score above a cutoff on average perceive themselves as weaker relative to their peers. This is not surprising in a setting in which tracking by ability has been in place a long time and is well understood. Additionally, however, this is associated with greater frequency of negative interactions with peers, providing some evidence that getting into a higher achievement school leads to marginalization.

For the parental and student dimensions, we also find evidence that these responses have a dynamic component. Namely, the RD-estimated feelings of stigmatization and reduction in parental help are strongest earlier in students' high school careers, and diminish over time. This might reflect, for example, students' gradual realization that tracking involves some noise, or parents' realization that their help is necessary even if their child is in a higher achievement setting. Such dynamics imply that the estimated effects of going to a better school might depend, for example, on whether academic outcomes are measured at the ninth or twelfth grade level.

Taken together, these results inform not just the literature on tracking and school effects, but also the research on experimental analyses of educational policy. Specifically, while we do not expect the magnitude or even the direction of the

[4] In contrast, there is no evidence of an effect on the probability of taking the Baccalaureate exam.

[5] For instance, a one standard deviation increase in peer quality is associated with a 0.1–0.2 standard deviation increase in Baccalaureate grade performance.

[6] Stratifying students into classes within tracks (when tracks are large enough) is a common but not universal or codified practice in Romanian high schools.

responses we find to extend to all settings, our results suggest that large scale interventions can result in equilibrium responses by different actors involved in educational markets. These reactions may not be observed or are explicitly held constant in partial equilibrium interventions. In salient examples, the STAR class size experiment (e.g., Krueger 1999) and the tracking experiment in Duflo, Dupas, and Kremer (2011) report on contexts in which one dimension of educational quality was manipulated while teacher quality was held constant by randomly assigning instructors to classrooms. In our data, in contrast, relevant teacher characteristics end up being correlated with educational quality. Similarly, parental effort may not change in a temporary experiment, but might respond once an intervention is sustained and understood.

As stated, such behavioral responses and equilibrium effects may well be setting-specific.[7] To the extent that they exist, however, they may affect the key characteristics and impacts of an educational intervention. Indeed, the presence or absence of similar behavioral responses might partially account for the mixed findings in the growing RD literature on school effects cited above.[8]

The remainder of the paper proceeds as follows. Section I presents a conceptual framework. Section II describes the student allocation mechanism, and Sections III and IV our data and methodology, respectively. Section V presents results, and Section VI concludes.

I. Conceptual Framework

The range of behavioral responses we focus on can be illustrated with a minor addition to the useful framework set out in Todd and Wolpin (2003). The addition reflects that while Todd and Wolpin focus on responses on the part of households, here we also consider reactions on the part of the school sector itself.

Specifically, consider a three period setting in which period $t = 0$ precedes a child entering school, and $t = 1$ and $t = 2$ denote the first and second years of school, respectively. F_t stands for household investments into children's skill acquisition in period t, and μ for a child's innate ability. W denotes family wealth. Finally, let A_t indicate a child's achievement at the *beginning* of period t. For example, A_1 is a child's achievement as she enters school, and reflects only her family's investments in the previous period and her innate ability:

$$A_1 = g_0(F_0, \mu),$$

[7] For example, Duflo, Dupas, and Kremer (2011) point out that in Kenya teacher sorting would result in more effective instructors being matched to weaker children. In contrast, Lankford, Loeb, and Wyckoff (2002) suggest that in the United States low-achieving students are typically matched with the least-skilled teachers.

[8] Our results are also related to research on how families make decisions regarding human capital investments (Becker 1964; Becker 1981; Becker and Tomes 1986). The empirical literature in this area has usually focused on the impact of parental characteristics on child outcomes (e.g., Behrman et al. 1997; Case and Deaton 1999; Brown 2006) without considering parent-school interactions. Das et al. (2013) is a notable exception that studies how parents adjust their educational expenditures in response to anticipated and unanticipated school grants. Additionally, Liu, Mroz, and van der Klaauw (2010) study the interrelationship between school inputs and household migration/employment decisions. A related literature considers private responses to public transfers (e.g., Moffitt 1992; Rosenzweig and Wolpin 1994; Jacoby 2002; and Jensen 2003). Case and Deaton (1998) point out that the impact of transfers might be different in the short and the long run, since it takes time for private behavioral responses to public transfers to have an effect.

where g_t is a period-specific production function.

Children's learning is also enhanced by the school inputs they receive each period, S_1 and S_2. Thus, a child's achievement at the start of the second year of school depends on endowments and the history of family and school inputs:

$$A_2 = g_1(S_1, F_1, F_0, \mu).$$

Todd and Wolpin (2003) make a useful distinction between the amount of school inputs a child would receive if this were entirely up to her family, and the amount she actually receives at school. While families cannot control their children's school inputs, they can influence their level. In the United States, for example, they can do this through residential choice or private schooling; in Romania, they might help their children prepare for transition exams. Let $\overline{S}_t$ denote the amount of inputs households target by such actions. Households choose this level as a function of their wealth and their children's endowment and achievement at the beginning of each period. For example,

$$\overline{S}_1 = \theta(A_1, W, \mu).$$

Schools in turn can choose how to allocate resources to students. For example, a child making clear progress toward reading might receive less attention than a struggling one, or might be "tracked" differently. Schools therefore have decision rules; e.g., they condition the inputs a child receives in period one on her achievement at the beginning of that period and on her endowment:

$$S_1 = \psi(A_1, \mu).$$

With this, the deviation between the level of inputs children actually receive, and the amount their families had targeted for the first period, is $S_1 - \overline{S}_1$. Assume households observe this deviation *before* setting their own home input investment level. For example, for the first schooling period they use a decision rule:

$$F_1 = \phi(A_1, W, \mu, S_1 - \overline{S}_1).$$

In words, a household sets its own investment for the first year of school as a function of its child's achievement at the beginning of the year, endowments, and the deviation between the school-based inputs they would want for their child and those she will actually receive (for instance, the child may not have been admitted to the school they had hoped she would attend).

This setup illustrates the parameters that different types of work identify. For example, a common goal of research is to answer the question: What would be the effect of exogenously changing a first period school input, S_1—say class size—while holding all other inputs constant?

$$\frac{\partial A_2}{\partial (S_1 - \overline{S}_1)} = \frac{\partial A_2}{\partial S_1} = \frac{\partial g_1}{\partial S_1}. \tag{1}$$

This is a question about the properties of the production function.

Todd and Wolpin argue that experiments more typically answer the question: What would be the total effect of an exogenous change in S_1, not holding other inputs constant? They refer to the STAR class size experiment as an illustration, since class size was manipulated exogenously but parents were free to adjust their own effort, for example. Such a "policy effect" is given by

$$(2) \qquad \frac{dA_2}{d(S_1 - \overline{S}_1)} = \frac{dA_2}{dS_1} = \frac{\partial g_1}{\partial S_1} + \frac{\partial g_1}{\partial F_1}\frac{\partial F_1}{\partial (S_1 - \overline{S}_1)}.$$

This is a well-defined and relevant measure, one that in comparison to (1) also contains the indirect (behavioral) effect resulting from changes in parental investments. At the same time, it has some limitations; for example, cost benefit calculations might require ascertaining the relative contributions of school and family inputs. In addition, although in the present framework the behavioral response by parents is instantaneous, in real world situations it might take time for parents to notice and react to changes in school inputs. As a result, the estimated policy effect (2) could vary with the time at which achievement is measured.

Now consider a second school input such that there are two: S_1^x and S_1^y. A randomized experiment might be able to vary one of these, while controlling the level of the other. In that case the resulting impact will still resemble expression (2). This is broadly the way in which we interpret the results in Duflo, Dupas, and Kremer (2011). This study manipulates the peer quality of the classes children have access to, say S_1^x, while at the same time constraining changes to other school inputs. For example, teachers are randomly assigned to high or low achievement classes.

Now suppose the increase in S_1^x originates not in an experiment but from an extensive and sustained policy. Then the school system will have a chance to react to this, and the total effect is

$$(3) \qquad \frac{dA_2}{d(S_1^x - \overline{S}_1^x)} = \frac{dA_2}{dS_1^x} = \frac{\partial g_1}{\partial S_1^x} + \frac{\partial g_1}{\partial S_1^y}\frac{\partial S_1^y}{\partial S_1^x} + \frac{\partial g_1}{\partial F_1}\left(\frac{\partial F_1}{\partial (S_1^x - \overline{S}_1^x)} + \frac{\partial F_1}{\partial (S_1^y - \overline{S}_1^y)}\right),$$

which differs from (2) in also including responses within the school system.

To summarize, Todd and Wolpin (2003) make a useful distinction between production function parameters (1) and policy effects (2). We wish to further emphasize that policy effects might be different in situations where behavioral responses take time to unfold, or where these responses only appear when certain interventions reach a certain scale—(3) versus (2). A further implication is that in the presence of behavioral responses, estimated policy effects are less likely to be externally valid, since indirect (behavioral) effects may vary across settings.

More specifically, aside from attempting to estimate the policy effect of having access to a higher-ranked school, we endeavor to measure the importance of behavioral responses—the terms beyond the first one in the right-hand side of equation (3). For example, we will search for evidence consistent with parents reacting to changes in the school inputs their children experience, an effect captured by $\frac{\partial F_1}{\partial(S_1^x - \bar{S}_1^x)}$.[9] We note it is not our aim to establish the causal impact of any particular one of these mechanisms; this would be difficult given that we have a single instrument for school quality, and as we show below, the evidence suggests multiple mechanisms are operative.

II. The Student Allocation Mechanism

The transition between middle and high school (eighth to ninth grade) in Romania results in an unusually systematic allocation of students to schools. Specifically, every child who completes middle school receives a transition score which equally weights: (i) her performance in a national eighth grade exam covering Language, Math, and History/Geography,[10] and (ii) her gymnasium (grades 5–8) grade point average.

After receiving their transition scores, students are required to submit a list of ranked choices specifying combinations of: (i) a high school, and (ii) one of seven academic tracks: Mathematics, Natural Sciences, Technical Studies, Services, Social Studies, Literature, and Natural Resources and Environmental Protection.[11] These tracks constitute "schools within a school" in that the students in them take all their coursework together and do not take classes with members of other tracks—although they share infrastructure and a principal, meet during breaks, and might share teachers. Not all schools offer all tracks, but all must submit their track-specific capacities in advance, and these are public information.

Students' school/track choices are expressed through an application form submitted (through their gymnasium) to the Ministry of Education in the capital, Bucharest.[12] Using a computerized system, the Ministry then ranks students by their transition score—no other criteria (e.g., sibling preferences or geographic proximity) go into the ranking. The mechanism considers the highest ranked student and assigns her to her most preferred school/track choice. It then moves on to the second and treats her similarly. Eventually, the procedure will reach a student whose first choice is full. If this happens it tries to assign the student to her second choice; if that one is full as well, then to the third, and so on. Only once this student has been assigned to a school does the mechanism move onto the next person.[13] Under this set up students have incentives to truthfully reveal their preference rankings.[14]

[9] Similarly, we will look for evidence consistent with teachers sorting in response to the stratification of students by ability, an effect broadly captured by $\frac{\partial S_1^y}{\partial S_1^x}$.

[10] All tests and grades use a scale ranging from 1 to 10, with a passing grade of 5. Students who score below 5 are not allowed to apply to high school, but can enroll in vocational school.

[11] For the 2001 sample, the administrative data on tracks is not as precise; it combines three of the tracks (Technical Studies, Services, and Natural Resources and Environmental Protection) into one technical track.

[12] During the period we study, schooling in Romania was compulsory until the tenth grade. As a result the entire cohort of students who complete middle school is required to participate in this allocation process.

[13] Some students only request school-track choices with minimum entry scores above their own transition score. These individuals are assigned, in a second round, to schools/tracks that did not fill. Students are warned against this outcome and allowed to submit a list of choices of essentially unlimited length. As a result, for example, in 2007 only 1.1 percent of applicants moved to the second round.

[14] The existing legislation does not allow children to decline their initial assignment, although in rare situations children do manage to switch schools and/or tracks over the years. Such switching does not pose a threat to our "intent-to-treat" research design, which as discussed below, is based on the assigned school/track.

Schools must enroll the children in the admission list returned from the computerized allocation. In cases in which they offer multiple classes of the same track, the system just returns the list of students admitted into the track, without further instructions on how to divide them into classes. We have data on this division for only a subset of schools (as described in Section III); these data and the anecdotal evidence suggest that many schools further stratify classes by transition score.

As shown in the regression results below, the result of this process in most markets is a clear hierarchy of schools by average peer quality, average observable teacher characteristics, and average parental effort. The lack of administrative data on the actual school choices that families submit prevents us from fully exploring what information is used to make these choices. The anecdotal evidence indicates that school quality is the most important determinant of school choice in Romania. That said, we cannot make definite statements, for example, on how parents' rankings of schools weigh aspects like peer quality, parental participation, or teacher value added.[15]

III. Data

We rely on two types of data: (i) administrative information covering the universe of children, and (ii) data from a survey we administered in most towns with two or three high schools.[16]

A. *Administrative Data*

Our administrative data cover the 2001–2003 and 2005–2007 admission cohorts. They provide the name, gymnasium, transition score, and the allocated school/track for all students, but no information on their ranking of school/tracks or their socio-economic characteristics. We focus on two subsamples of these data:

(i) The 2001–2003 cohorts, for which we linked admissions data with information on whether students took the Baccalaureate exam and how they performed (these cohorts took the exam in 2005–2007).[17] A satisfactory Baccalaureate grade is a prerequisite for applying to university, and an excellent one raises the probability of admission to the most prestigious institutions.[18]

[15] See MacLeod and Urquiola (2009) for a framework in which a preference for schools with better reputation might result in tradeoffs between attributes like peer quality and teacher value added.

[16] We use the term town to denote a high school market. The term that appears in the administrative data is locality (*Localitate*, in Romanian). In most cases these units actually correspond to cities/towns. In a few, they denote the largest of a number of small towns or villages—the town which actually contains the high school that might draw from a corresponding catchment area composed of smaller towns or villages.

[17] We merged the admission and Baccalaureate data by student name and county using a fuzzy matching technique to allow for some misspelling of names. Our conclusions are not sensitive to different levels of precision in the matching algorithm, and are also similar if we use only exact matches. Our matched data do not allow us to differentiate between high school dropouts and students who complete high school but do not take the Baccalaureate exam.

[18] The Baccalaureate exam is administered nationally. Students usually take six component exams, with a combination of common subjects (written language, oral language, written foreign language) as well as two track-specific exams and one elective exam. The overall grade is the unweighted average of these scores. The exam is first administered in July. Students who fail are allowed to retake the exam in August (we use the August score for students who took the exam twice). Additionally, students are generally not allowed to take the Baccalaureate exam early.

Table 1—Descriptive Statistics, Administrative Data: 2001–2003 Cohorts

	High school admission cohort								
	2001			2002			2003		
	Mean	SD	*N*	Mean	SD	*N*	Mean	SD	*N*
Panel A. All towns									
Panel A.1. Individual level									
Transition grade	7.68	0.93	107,812	7.87	0.86	110,912	7.96	0.97	115,413
Baccalaureate taken	0.847	0.360	107,812	0.822	0.383	110,912	0.808	0.393	115,413
Baccalaureate grade	8.31	0.93	87,411	8.28	0.95	85,946	8.51	0.88	84,076
Panel A.2. Track level									
Number of ninth grade students	62.6	49.0	1,722	66.6	50.6	1,665	71.5	53.3	1,615
Panel A.3. School level									
Number of ninth grade students	135.3	61.4	797	140.6	63.1	789	144.1	69.2	801
Number of tracks	2.2	1.2	797	2.1	1.2	789	2.0	1.2	801
Panel A.4. Town level									
Number of ninth grade students	804.6	849.6	134	827.7	875.5	134	854.9	919.5	135
Number of schools	5.9	6.0	134	5.9	5.8	134	5.9	5.9	135
Number of tracks	12.9	11.9	134	12.4	11.4	134	12.0	10.9	135
Panel B. Survey towns									
Panel B.1. Individual level									
Transition grade	7.58	0.91	14,951	7.85	0.84	15,257	7.89	0.09	15,641
Baccalaureate taken	0.832	0.374	14,951	0.812	0.390	15,257	0.805	0.396	15,641
Baccalaureate grade	8.37	0.87	11,966	8.31	0.90	11,821	8.63	0.82	11,312
Panel B.2. Track level									
Number of ninth grade students	50.5	37.2	296	53.7	38.8	284	55.1	42.5	284
Panel B.3. School level									
Number of ninth grade students	121.6	54.7	123	124.0	54.3	123	127.2	64.5	123
Number of tracks	2.4	1.3	123	2.3	1.3	123	2.3	1.3	123
Panel B.4. Town level									
Number of ninth grade students	277.4	126.1	55	277.4	126	55	284	140.8	55
Number of schools	2.2	0.4	55	2.2	0.4	55	2.2	0.4	55
Number of tracks	5.2	1.8	55	5.2	1.8	55	5.2	1.6	55

Notes: Panel A describes the universe of Romanian towns with two exceptions (discussed in Section III): (i) towns that make up Bucharest, and (ii) towns that contain a single school. Panel A.1 presents student level statistics, and panels A.2, A.3, and A.4 refer to characteristics at the track, school, and town level, respectively. Panels B.1–B.4 present analogous information for the towns we eventually targeted for surveying. Note that these panels refer to 55 rather than the 59 towns discussed in Section III and described in Table 2. This reflects that the remaining four towns only had one school in 2001–2003, and so are not in our main administrative sample (these four towns did contain two or three schools in 2005, and thus were targeted for the survey). Finally, due to publication-related space constraints, Table 1 does not include maxima and minima; these are in the online Appendix (Table A.1).

These cohorts contain about 334,000 students attending about 800 high schools in 135 towns.

(ii) The 2005–2007 cohorts, for which we have only admissions information and can thus only explore "first stages." These cohorts consist of about 301,000 students from essentially the same schools and towns; it contains the students we surveyed, as described below.

Presenting descriptive statistics, Table 1 thus covers the universe of students admitted to high school during these years, with three exceptions.[19] The first two reflect

[19] Due to publication-related space constraints, Table 1 does not include maxima and minima; these are in the online Appendix (Table A.1).

that, as explained below, we rank schools and set cutoff scores under the assumption that towns are self-contained markets. We therefore omit the capital, Bucharest, which is composed of six towns, the borders of which students can cross with relative ease. We do not find this omission to affect our key conclusions. Second, when our analysis focuses on between-school cutoffs, we omit towns that have only one high school.[20] Third, we drop all students who enroll in the vocational sector; this precludes their access to higher education and hence we do not observe Baccalaureate outcomes for them.[21]

B. *Survey Data*

While the administrative dataset offers substantial sample sizes, it contains only basic information. To explore behavioral responses, we therefore implemented a survey that featured principal, parent, and student questionnaires. The way in which we carried out this survey partially explains our final survey sample, and we therefore begin with a description of its implementation.

The 2005–2007 administrative data provided students' names, but not their addresses or any way of contacting them or their parents. The data also contained almost no information regarding school characteristics. We therefore approached schools and asked their principals/administrators to fill out a school survey, and to provide us with the addresses of the students in the mentioned cohorts (who were still in school). The school survey collected information on the student population, and on school resources and infrastructure. The principals were also asked to provide a subjective ranking of their school relative to other schools in their towns along dimensions like teacher quality, student ability, and parental involvement. Our surveyors also collected administrative data on the experience, education, and certification levels of the teachers responsible for seven subjects: Math, Romanian, History, Geography, Music, Sports, and Computer Science.

During the first half of 2009, we used the list of addresses to directly approach parents and students at home. The survey administered to them had three components. First, we interviewed the family to obtain demographic information on each member of the household, as well as basic household characteristics. Second, we surveyed the primary caregiver to elicit information on each child. Third, we conducted a separate interview with the child from the selected schools. The latter included asking children (for matching purposes) the name of their teacher in the seven subjects for which we collected teacher characteristics at schools.

Two factors led us to restrict our target sample to towns containing two or three schools. First, since we needed information from students on either side of admissions cutoffs, it was imperative that all schools in each town agree to participate, and

[20] Despite these omissions, for simplicity we will describe the sample as covering "all towns" unless we focus only on those towns covered by our specialized survey.

[21] The omission of students who enroll in vocational tracks could be problematic if the probability of enrolling is affected by options in nonvocational schooling. We nonetheless decided to drop these students because it is very unlikely that a large proportion of students would prefer to attend a vocational track over a nonvocational track. We have explored the actual school/track choices as collected ex post in our survey sample. These responses may suffer from ex post rationalization as discussed below, but it is worth noting that less than 1 percent of students who attend a nonvocational track claim that they ranked a vocational track above their assigned track. For further information on vocational education in Romania, see Malamud and Pop-Eleches (2010).

therefore the effort was more likely to encounter problems in larger towns. Second, as shown below the administrative data reveal that the magnitude of the first stages is three to four times larger in smaller towns.

We started with a sample of 57,527 children and 167 schools in the 71 towns with two or three schools. If any school in a given town declined to participate, we abandoned the whole town. In the end, we obtained complete school surveys and student data from 148 schools in 64 towns; the administrators in these schools provided us with 32,307 addresses. We restricted the target sample further to 138 schools in 59 towns, which contained 30,676 children.[22] Due to financial constraints we randomly sampled 19,878 children (about 65 percent of the total) out of this population. From this target sample, we obtained 12,590 parent and child surveys. Our response rate of 63 percent is in line with Gallup Romania's (the firm we contracted with) interview rate for this population. While the resulting sample is not completely representative of the population of these schools, we found no evidence that response rates differed between households whose children had a transition score just above a cutoff, and their counterparts who scored just below. Table 2 presents descriptive statistics from the survey data, using the household and school questionnaires.[23]

IV. Empirical Strategy

Although, in principle, a student can request any high school in the country, we suppose that students restrict their choices to the towns they live in, a reasonable assumption since the applicants are 13–14 year olds likely to be living with their parents. Within each town, we rank schools and school/tracks (in separate exercises) according to their average score, and set the cutoffs equal to their minimum scores.[24] In other words, we set each school's (or school/track's) cutoff equal to the score of the child with the lowest transition score.[25]

This yields a large number of quasi-experiments—1,984 if one considers schools; 6,434 if one considers school/tracks—since each cutoff score makes for a potential RD analysis.[26] In this section we first discuss the conceptual basis for analyzing any

[22] The elimination of five towns reflected that at least one school in each of them, though willing to fill out the school questionnaire, was unable to provide student addresses.

[23] Table A.2 in the online Appendix explores how two sets of towns differ from those for which we eventually got completed surveys: first, the 7 (out of 71) towns where at least one school refused to participate, and second, the 12 (out of 71) towns that either fall into the previous category or else did not complete parental and child surveys. Generally, we did not find significant differences between the towns in our final sample compared to the initial target sample in terms of characteristics including average transition scores, area, population, and income per capita.

[24] We also implemented the exercise ranking schools and tracks by their minimum score, with quite similar results.

[25] Using the minimum admission score is in line with our "intent-to-treat" approach (discussed below), in the sense that only schools that reach capacity will generate meaningful first stages. An alternative approach would have been to set each school's (or school/track's) cutoff equal to the transition score of the child that fills its last slot. We could potentially identify that child since classes are limited to 28 slots (e.g., the track-specific slot availabilities which schools submit prior to the allocation process must be multiples of 28). However, our process for collecting and matching the administrative files (from hundreds of thousands of web pages) creates some measurement error. This limits our ability to determine with certainty if a school reached capacity. Nevertheless, using some approximations, we estimate that excluding the bottom ranked school in each town, the percent of schools that reach capacity ranges (depending on the cohort) between 80 and 90 percent.

[26] The between-school cutoffs are 663, 655, and 666 for the 2001, 2002, and 2003 entry cohorts, respectively; for the between-track cutoffs, the corresponding numbers are 1,956, 1,952, and 2,526.

Table 2—Descriptive Statistics, Survey Data: 2005–2007 Cohorts

	Mean	SD	Observations
Panel A. Socioeconomic characteristics (Household survey)			
Female head of household	0.112	0.316	11,931
Age of household head	46.752	7.145	11,843
Ethnicity of household head			
Romanian	0.938	0.240	11,931
Hungarian	0.050	0.218	11,931
Gypsy	0.003	0.056	11,931
Other	0.008	0.091	11,931
Education of household head			
Primary	0.665	0.472	11,840
Secondary	0.205	0.404	11,840
Tertiary	0.130	0.337	11,840
Female child	0.584	0.493	11,931
Age of child	18.077	0.939	11,866
Panel B. Parental responses (Household survey)			
Parent has volunteered at school in the past 12 months	0.111	0.314	11,868
Parent has paid for tutoring services in the past 12 months	0.237	0.425	11,850
Parent helps child with homework every day or almost every day	0.197	0.398	11,815
Child does homework every day or almost every day	0.752	0.432	11,779
Panel C. Child responses (Household survey)			
Relative rank among peers (1–7, with 7 better ranked)	4.745	1.300	11,798
Index of negative interactions with peers[1]	0.121	0.369	11,838
Child does homework every day or almost every day	0.632	0.482	11,908
Child perceives homework to be easy (1–7, with 7 easiest)	5.450	1.015	9,628
Panel D. Language teacher qualifications (school data matched to children)			
Proportion of teachers with highest state certification	0.608	0.488	11,169
Years of experience	15.801	12.228	11,169
Proportion of teachers who are "novices" (less than 2 years of experience)	0.061	0.238	11,169

Notes: This table summarizes a specialized survey collected in 135 schools located in 59 towns (see Section III for a description of the survey).

[1] Index based on the sum of four indicators for whether, during the past month, children's peers: (i) were mean to them, (ii) hit them, (iii) took their things without asking, or (iv) made them feel marginalized. Each indicator ranges between 0 (did not happen in the past month) and 5 (happened daily).

given one of these experiments, focusing on schools for simplicity. We then describe how we go about summarizing them.

A. *Empirical Setup for a Single Between-School Cutoff*

Consider a town in which i indexes students and $s = 1, \ldots, S$ indexes schools, where the latter have been ordered from the worst to the best in terms of the average transition score observed among their students. Additionally, let $z = 1, \ldots, (S-1)$ index cutoffs, such that, for example $z = 1$ denotes the cutoff between the worst and next-to-worst school in the town, and $z = (S-1)$ indicates the cutoff between the top-ranked school and the next best institution. Let t_i denote student i's transition score, and $\tilde{t}_z$ be the minimum grade required for admission into the higher-ranked of the two schools indexed by z.[27]

[27] We abuse notation as above we used t to denote a time period; henceforth it will stand for the transition score.

In this setup, consider the "reduced form" regression:

$$y_i = \alpha 1\{t_i - \tilde{t}_z \geq 0\} + a(t_i) + \epsilon_i, \tag{4}$$

where y is an outcome, $1\{t_i - \tilde{t}_z \geq 0\}$ is an indicator for whether a students transition score is greater than or equal to the cutoff indexed by z, and $a(t_i)$ is a flexible control function for the transition score. As outcomes, y, we will consider students' Baccalaureate performance as well as a series of behaviors and characteristics related to students, parents, teachers, principals, and schools.

The idea behind the RD design is that if access to a higher-ranked school changes discontinuously at $\tilde{t}_z$, then the causal impact of this access can be identified even if students' transition scores are systematically related to factors that affect outcomes like Baccalaureate grades.[28] Intuitively, suppose the transition score is smoothly related to characteristics that affect achievement, i.e., $a(t)$ is constant in a small enough neighborhood around the cutoff. Under this assumption, students with scores just below $\tilde{t}_z$ will provide an adequate control group for individuals with scores just above, and any difference in their outcomes can be attributed to the fact that they have access to schools of different quality—i.e., α is nonparametrically identified at $\tilde{t}_z$ (Hahn, Todd, and van der Klaauw 2001). More generally, if $a(t)$ is specified correctly, it will capture all dependence of outcomes on the transition score away from the cutoff, and one can use all the data to estimate (4). Below we present such results and also estimates that rely only on observations close to cutoff scores.

As is common in the RD-based literature, we use specifications like (4) to produce "intent to treat" estimates of the effect of having the *opportunity* to attend a higher-ranked school. Our "first stage" results will show that a significant proportion of children who have a chance to go to a better school take this opportunity. This allows us to measure the effect that having this opportunity has on a child's outcome, although not the effect of any specific change in school quality because the children above and below the cutoff attend schools with a range of different qualities.

While we focus on reduced form results, in some cases we also present results from an instrumental variables-type specification:

$$y_i = \gamma E(T_i | t_i) + a(t_i) + e_i \tag{5}$$

$$E(T_i | t_i) = \beta 1\{t_i \geq \tilde{t}_z\} + a(t_i), \tag{6}$$

where (6) is the "first stage" and T_i stands for the average transition score among student i's peers (e.g., the average score among the children at her school). Under assumptions analogous to those discussed above, and if the mean of T conditional on the transition score, $E(T_i | t_i)$, is discontinuous at $\tilde{t}_z$, then (5)–(6) will consistently estimate γ—the effect of having access to a higher-ranked school as measured by peer group quality. We implement (5)–(6) mainly as a descriptive exercise to compare the magnitude of effects across cutoffs. This reflects that γ cannot be given a

[28] This design was proposed by Thistlewaite and Campbell (1960); for an overview see Imbens and Lemieux (2008).

strict instrumental variables interpretation, since as shown below, several aspects of school quality—not just peer composition—change discontinuously at the cutoffs.[29]

B. *Summarizing Information for Many Cutoffs*

The above specifications illustrate how one might exploit one cutoff. As stated, our data contain thousands of cutoffs. In order to summarize these, and for the sake of statistical power, we focus on regressions which pool data across cutoffs, relying on the fact that $t_i - \tilde{t}_z$ measures the distance between each cutoff and the transition score of each student in a town. Specifically, we "stack" the data such that every student in a town serves as an observation for every cutoff, and (when observations are used more than once) run the analyses clustering at the relevant level.[30] Including all observations for every cutoff is relevant in that, for example, the student with the best score in town could successfully request any school. We note, however, that regressions restricted to students in bands close to the cutoffs in fact rarely use student-level observations more than once.

For concreteness, most of our reduced form regressions are specified as follows:

$$y_{iz} = \alpha 1\{t_i - \tilde{t}_z \geq 0\} + \eta(t_i - \tilde{t}_z) + \psi(t_i - \tilde{t}_z) \times 1\{t_i - \tilde{t}_z \geq 0\} + w_z + v_i, \tag{7}$$

that is, a regression of outcomes on a dummy for whether a student's transition score is greater than or equal to the cutoff, along with controls that include: (i) a linear spline in students' grade distance to the cutoff, one which allows the slope to vary on each side of the cutoff, and (ii) a full set of cutoff dummies, w_z.[31]

V. Results

This section first presents results that pool all the between-school and between-track cutoffs. It then turns to describing the heterogeneity in effects observed when discontinuities take place at different points of the transition score distribution. Finally, it closes with exercises that, using our survey data, explore behavioral responses.

A. *The First Stage*

Figure 1, panel A illustrates the basic first stage result in our data, pooling all between-school cutoffs as described in Section IV; this figure summarizes results of specifications analogous to (6). The x-axis describes students' transition scores relative to the cutoffs that allow the opportunity to access a higher-ranked school; the y-axis describes the peer quality students experience, as measured by the mean transition score at their respective school. Panel A plots this mean transition score collapsed into cells containing individuals who are within 0.01 of a transition grade from each other. The right-hand side of panel B plots analogous information, but the

[29] In other words, it is unlikely that a particular channel like peer quality satisfies an exclusion restriction.

[30] To illustrate, in the first year of our data, 2001, the first town in our data, Alba-lulia, has 836 students in seven schools, producing six between-school cutoffs. For that year, this produces a dataset of 5,016 ($=836 \times 6$) observations.

[31] For simplicity, equation (7) does not have a time dimension; in reality our standard specification includes a full set of cutoff/year dummies.

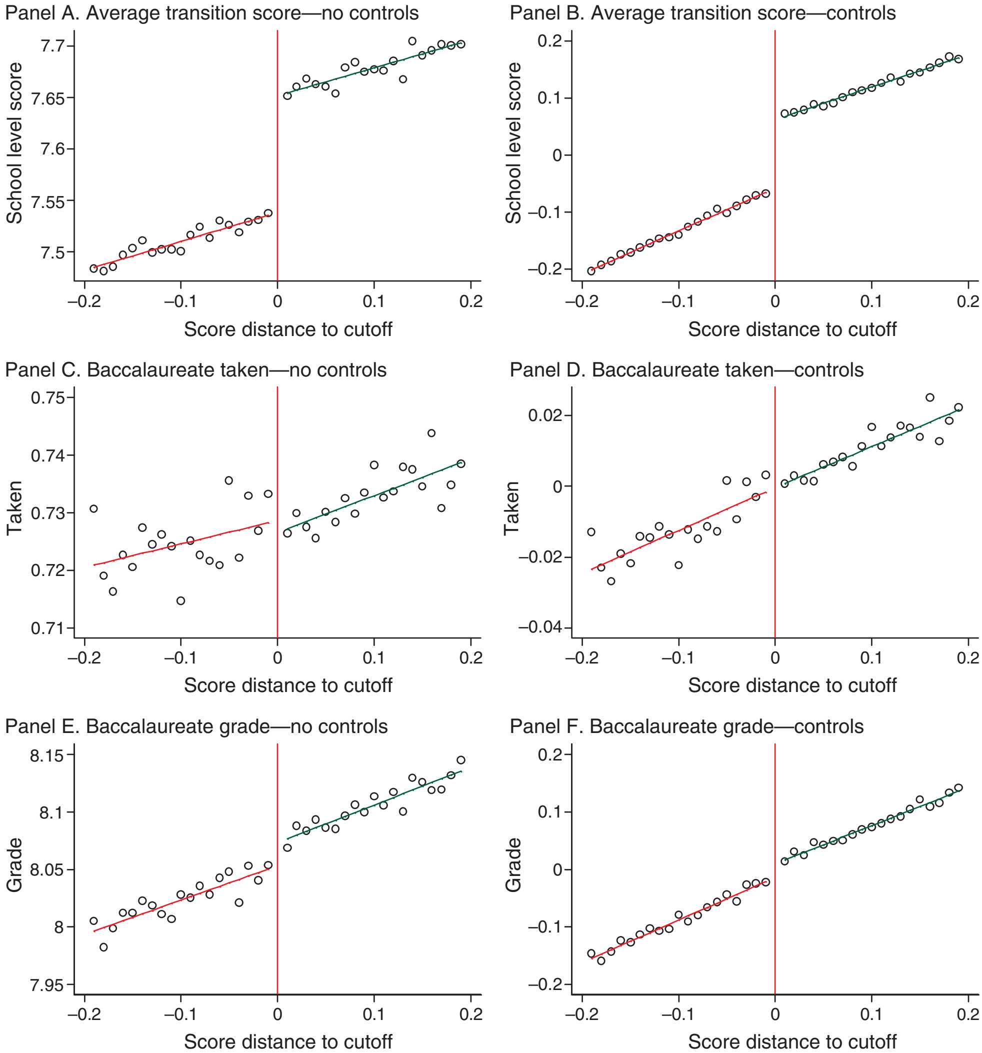

FIGURE 1. BETWEEN-SCHOOL CUTOFFS: ALL TOWNS

Notes: All panels are based on administrative data for the 2001–2003 admission cohorts, and restrict observations to individuals with transition scores within 0.2 points of a cutoff. The left-hand-side panels plot (0.01 point) transition score cell means of the dependent variable. The right-hand-side panels plot analogous means of residuals from a regression of the dependent variable on cutoff fixed effects. In each panel, the solid lines are fitted values of regressions of the dependent variable on a linear trend in the transition score, estimated separately on each side of the cutoff. The dependent variable in panels A and B is the average transition score of the peers students encounter at school; the dependent variable in panels C and D is an indicator for having taken the Baccalaureate test; the dependent variable in panels E and F is the Baccalaureate exam grade.

y-axis is based on residuals from a regression of the mean transition score on cutoff fixed effects.[32] Both panels suggest that the average peer quality students experience increases significantly and discontinuously if their transition score crosses the

[32] Figures 1, 3, and 4 (and A.1–A.6 in the online Appendix) have a similar structure in that the left-hand-side panels use raw data, and the right-hand-side panels use residuals from regressions that control for cutoff fixed effects.

Table 3—First Stages

	Administrative data				Survey data	
	All towns		Survey towns			
	Within 1 point of cutoff (1)	Within IK bound (2)	Within 1 point of cutoff (3)	Within IK bound (4)	Within 1 point of cutoff (5)	Within IK bound (6)
Panel A. School-level average transition grade: 2001–2003 cohorts—between school cutoffs						
1{Transition grade ≥ cutoff}	0.107***	0.115***	0.446***	0.447***		
	(0.001)	(0.001)	(0.007)	(0.007)		
Linear spline	Yes	Yes	Yes	Yes		
R^2	0.790	0.792	0.754	0.754		
Observations	1,857,376	1,160,458	39,363	39,104		
Panel B. Track-level average transition grade: 2001–2003 cohorts—between track cutoffs						
1{Transition grade ≥ cutoff}	0.063***	0.070***	0.188***	0.188***		
	(0.001)	(0.001)	(0.003)	(0.003)		
Linear spline	Yes	Yes	Yes	Yes		
R^2	0.857	0.858	0.792	0.793		
Observations	4,845,812	3,423,493	172,656	166,458		
Panel C. Track-level average transition grade: 2001–2003 cohorts—between school cutoffs						
1{Transition grade ≥ cutoff}	0.073***	0.082***	0.266***	0.274***		
	(0.001)	(0.001)	(0.006)	(0.006)		
Linear spline	Yes	Yes	Yes	Yes		
R^2	0.849	0.852	0.811	0.816		
Observations	1,857,376	1,196,898	39,363	39,633		
Panel D. School-level average transition grade: 2005–2007 cohorts—between school cutoffs						
1{Transition grade ≥ cutoff}	0.107***	0.106***	0.414***	0.438***	0.477***	0.477***
	(0.001)	(0.001)	(0.007)	(0.009)	(0.018)	(0.018)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.808	0.811	0.700	0.691	0.700	0.700
Observations	1,611,388	1,822,434	34,855	22,485	6,559	6,382

Notes: All regressions are clustered at the student level and include cutoff fixed effects. Standard errors are in parentheses. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

threshold that gives them the option of going to a better-ranked school. The vertical distance between the points close to the discontinuity, further, is analogous to the estimate of β in expression (6).

Table 3, panel A presents the regression analog to these results, where columns 1 and 2 refer to all towns. Panel A, column 1 refers to the 2001–2003 cohorts and uses data within one point of the cutoff—about 1.8 million observations from 1,984 cutoffs.[33] It regresses the average transition grade that students experience at school on an indicator for whether their scores are above cutoffs. The specification

[33] Panel D in Table 3 presents similar results for the 2005–2007 cohorts.

includes: (i) a linear spline in students' grade distance to the cutoffs, one which allows the slope to vary on either side of the cutoff, and (ii) cutoff/year dummies—i.e., equation (7) with cutoff/year fixed effects.[34] The results suggest that scoring above a cutoff results in a highly statistically significant jump in peer quality—0.11 points, equivalent to about 0.1 standard deviations in transition test performance.

As stated, column 1 restricts the sample to include only students whose transition scores are within one point of a cutoff (about half of the full sample).[35] This is our preferred specification; it attempts to balance the goal of focusing on observations close to the cutoffs while providing enough data to yield fairly precise estimates. We experimented with several more stringent windows, with similar conclusions.[36] We opt to feature, in column 2, a regression within the bandwidths suggested by the procedure in Imbens and Kalyanaraman (2009)—henceforth, IK—which in our data is generally more restrictive than the one point band used in column 2.[37] All these samples result in similar and highly significant estimates.

Columns 3 and 4 repeat the specifications in columns 1 and 2, focusing only on towns included in our specialized survey (the corresponding graphical evidence is in online Appendix Figure A.1, panels A and B). The observed discontinuities are always statistically significant, and about four times the size of those observed in the full sample.[38]

The "first stages" in Table 3, panel A are those that will be relevant for the Baccalaureate outcomes. They show that the Romanian high school admissions process provides a potentially fruitful setting for an RD analysis of the impact of having access to a better school, at least if school quality is judged by average transition scores.[39] Below we will explore other dimensions along which school characteristics vary at the cutoffs.

Additionally, recall that students apply for school/track combinations, and so the between-track cutoffs also provide candidate first stages. The corresponding regression results are presented in panel B of Table 3. In all cases the coefficient of interest is somewhat smaller (although always statistically significant) than that observed for the between school cutoffs (Figure A.2 in the online Appendix presents the corresponding graphical results).

[34] We note that all our regression results are not qualitatively affected by instead using a linear, quadratic, or cubic specification for $a(t_i)$ in (4), or by excluding the cutoff fixed effects.

[35] Table A.3 in the online Appendix presents the full sample results, omitted here for reasons of space.

[36] For example, a previous version of the paper focused on only the administrative data (which offer substantial sample sizes) featured specifications that for each cutoff used only the two students immediately to the left and right.

[37] Specifically, we follow Lee and Lemieux (2010) and use a simple rectangular kernel. Further, we implemented the bandwidth selection procedure using the Stata ado file labeled rdob.ado, available at http://faculty-gsb.stanford.edu/imbens/documents/rdob.zip (accessed April 13, 2013).

[38] It is possible that the greater magnitude of the "first stage" in the survey towns reflects that these have fewer available cutoffs. For example, if students seek to segregate by ability then (subject to assumptions on the density of scores and the size of schools) towns with fewer schools might have greater discontinuities. On the other hand, if students cared only about proximity to school, or in the extreme chose schools randomly, then the magnitude of the discontinuities might not vary systematically with town size.

[39] The RD approach additionally requires that there be no discrete changes in student characteristics that affect outcomes like Baccalaureate performance. While our administrative data do not contain such variables, our survey data suggest this condition is fulfilled. Specifically, online Appendix Table A.4 shows that a number of mother, child, and household characteristics do not vary discontinuously around the cutoffs (all but one of the 20 estimates are insignificant in the sample within one point of the cutoffs). As an additional test, online Appendix Figure A.7 shows that there is no visible jump in the density around the discontinuity; as expected, the McCrary (2008) test shows no statistically significant break (log difference in height is 0.074 with a standard error of 0.058).

This is consistent with some sorting happening between-tracks within schools, with the implication being that students with access to higher ranked schools experience better peers, but that this effect is more marked if the measure is the average score of their *school*-level rather than their *track*-level peers. Panel C (Table 3) confirms this by exploring how the *track* level average transition grade students experience changes at the cutoffs that determine access to a higher ranked *school*. The observed estimates are still highly significant, but smaller than those observed when peer groups are defined at the school level (panel A).

In order to elaborate on how these first stage results originate, we note that not all students request the highest-ranked school they are eligible for. Specifically, panels A and B in Figure 2 summarize information regarding the cutoffs that separate the best and second-best school in towns with at least three schools. Panel A plots transition score cell means of the percentage of students who attend the highest-ranked school, and not surprisingly this is equal to zero when students scores are to the left of the cutoff these students are not eligible to attend the most selective school. While the proportion of students in the best school jumps discretely once one moves to the right, it does not rise to one; rather, roughly 40 percent of children eligible for enrollment in the best school take advantage of the opportunity. Panel B, which plots the percentage of individuals in the second best school, shows that about 25 percent of those eligible for the best decide to remain in the second-best school, with another 35 percent attending institutions other than the top two.[40]

Multiple factors (e.g., proximity) may account for why not all students request the highest-ranked school they are eligible for. Whichever ones are actually operative, Figure 2 underlines that results generated using the first stages in Table 3 should be interpreted in an "intent to treat" spirit.[41]

B. *Baccalaureate Outcomes*

A first outcome we consider is simply whether students took the Baccalaureate exam. Panels C and D in Figure 1 present the graphical evidence and suggest few if any changes in test-taking rates at the cutoffs. This is confirmed in regressions in panel A of Table 4, where columns 1 and 2 refer to the full sample of towns. The results suggest that having access to a higher-ranked school results in small and statistically insignificant changes in the probability of taking the Baccalaureate exam. The results within bands allow us to rule out differences in test-taking rates of less than a third of a percentage point.[42]

A generally similar conclusion emerges among the towns in our survey sample (Table 4, panel A, columns 3 and 4 and online Appendix Figure A.1, panels C and D) and when we analyze the opportunity to enroll in a better track (Table 4, panel D,

[40] A related note is that all regressions exclude the child whose score was exactly equal to the cutoff, since that student may be selected. This reflects that this student's score dictates the cutoff score and, mechanically, that student attends the better school with probability one, which is empirically not the case with the individuals right above her. This exclusion does not have a qualitative effect on any of our conclusions.

[41] For further reference, panels C and D in Figure 2 show analogous evidence for the cutoffs separating the worst and the next to worst schools in each town; panels E and F plot similar information for towns with only two schools.

[42] The full sample results are in online Appendix Table A.5.

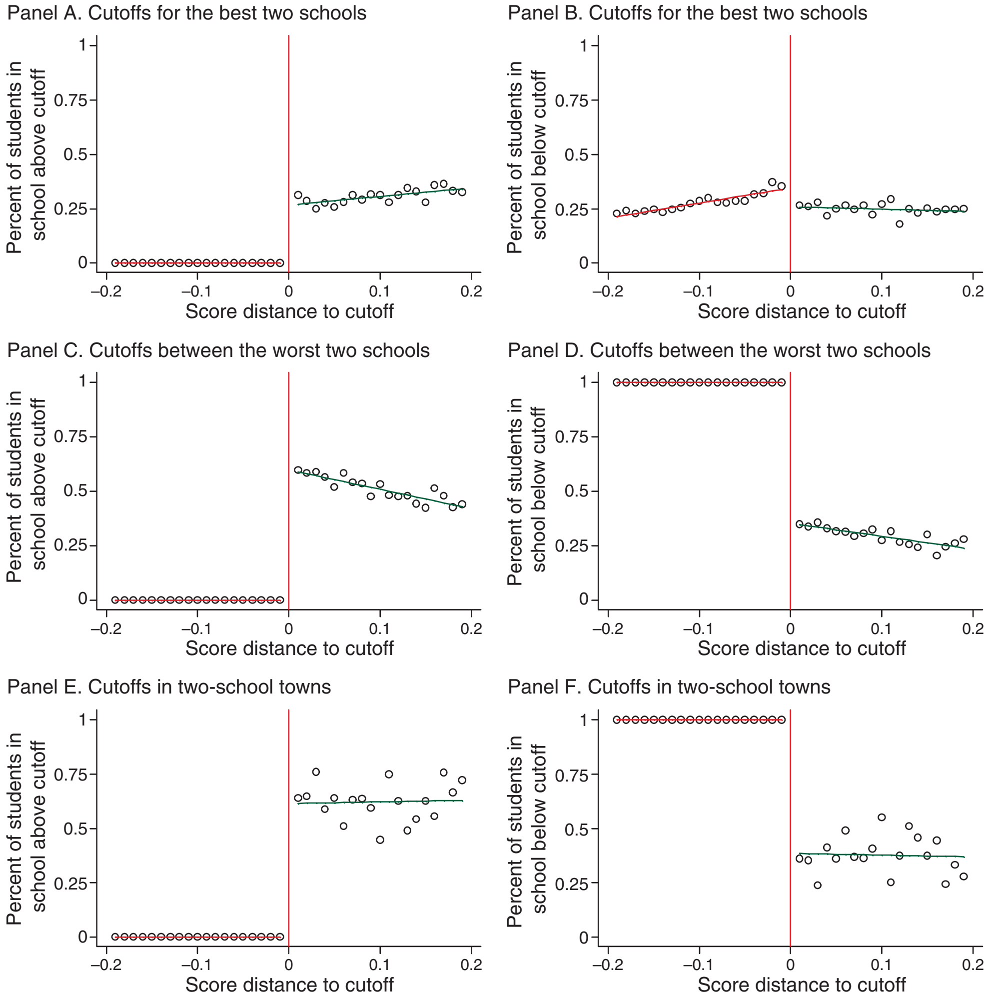

FIGURE 2. TOP AND BOTTOM CUTOFFS IN TOWNS WITH THREE OR MORE SCHOOLS: TWO-SCHOOL TOWNS

Notes: Panels A and B describe cutoffs that determine access to the best school in towns that contain at least three schools. Panels C and D refer to the lowest cutoffs in such towns. Panels E and F describe the cutoffs in two-school towns. All panels are restricted to individuals with a transition score within 0.2 points of a cutoff. The left-hand panels plot (0.01 point) transition cell means of the proportion of students who attend the school above the cutoff; the right-hand-side panels plot the proportion of students who enroll in the school below. The solid lines plot fitted values of residuals from regressions of the dependent variable on a linear trend in the transition score, estimated separately on each side of the cutoff.

and online Appendix Figure A.2, panels C and D).[43] This consistent lack of evidence of selection into test taking makes it easier to interpret effects on Baccalaureate performance.

Turning to this issue, panels E and F in Figure 1 describe grade outcomes at the cutoffs, suggesting a discrete increase in average achievement. The corresponding regression evidence is in panel B of Table 4, which presents statistically significant

[43] In contrast to Table 3, Table 4 no longer has columns 5 and 6. Again, this reflects that for the 2005–2007 cohorts we do not have Baccalaureate outcomes, so these variables are not available for the children we surveyed.

Table 4—Effects on Baccalaureate Taking and Performance

	All towns		Survey towns	
	Within 1 point of cutoff (1)	Within IK bound (2)	Within 1 point of cutoff (3)	Within IK bound (4)
Panel A. Baccalaureate taken dummy: 2001–2003 cohorts—between school cutoffs				
1{Transition grade ≥ cutoff}	0.000	0.001	0.012	0.006
	(0.001)	(0.001)	(0.009)	(0.007)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.054	0.053	0.081	0.082
Observations	1,857,376	2,086,043	39,363	49,100
Panel B. Baccalaureate grade: 2001–2003 cohorts—between school cutoffs				
1{Transition grade ≥ cutoff}	0.018***	0.019***	0.105***	0.099***
	(0.002)	(0.002)	(0.015)	(0.016)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.483	0.488	0.494	0.490
Observations	1,256,038	1,394,577	25,393	24,029
Panel C. Baccalaureate grade: 2001–2003 cohorts—between school cutoffs, IV specification				
Average school transition grade	0.163***	0.177***	0.228***	0.212***
	(0.020)	(0.020)	(0.033)	(0.034)
Linear spline	Yes	Yes	Yes	Yes
Observations	1,256,038	1,394,577	25,393	24,029
Panel D. Baccalaureate taken dummy: 2001–2003 cohorts—between track cutoffs				
1{Transition grade ≥ cutoff}	−0.001	−0.001	0.000	0.000
	(0.001)	(0.001)	(0.004)	(0.004)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.057	0.058	0.084	0.086
Observations	4,845,812	4,567,167	172,656	150,025
Panel E. Baccalaureate grade: 2001–2003 cohorts—between track cutoffs				
1{Transition grade ≥ cutoff}	0.011***	0.011***	0.036***	0.036***
	(0.001)	(0.001)	(0.007)	(0.007)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.490	0.497	0.495	0.493
Observations	3,371,726	3,206,212	117,179	114,663

Notes: All regressions are clustered at the student level and include cutoff fixed effects. Standard errors are in parentheses. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

gains equivalent to about 0.02 to 0.10 standard deviations, depending on whether one looks at the full or the survey sample.

The bottom line is that students who score above cutoffs giving them access to higher ranked schools perform better in the high stakes Baccalaureate exam, and under the assumptions underlying RD designs, this impact can be viewed as causal.[44]

[44] Table A.6 in the online Appendix illustrates that these conclusions are robust to changing the controls included in the regressions. Panel A features no controls at all, and panel B features only cutoff fixed effects. Panel C adds

A similar conclusion emerges when looking at the towns covered in our specialized survey (Table 4, panel B, columns 3 and 4, and online Appendix Figure A.1, panels E and F), and when one considers between-track rather than between school cutoffs (Table 4, panel E, and online Appendix Figure A.2, panels E and F).

The magnitude of the effects on test performance is greatest in the survey towns, which is consistent with the larger first stage estimates observed there. In order to illustrate this, panel C (Table 4) presents specifications in which peer quality is instrumented using the cutoffs, as in equations (5)–(6). Measured this way, the magnitude of the impact of peer quality across samples is relatively stable. We note that we present these IV estimates in a descriptive spirit since, as we return to below, the exclusion restriction is likely not satisfied.

C. *Heterogeneity in Baccalaureate Outcomes*

The results above pool all between school and between track cutoffs. We now explore how the Baccalaureate effects vary according to where the cutoffs are located in the transition score distribution. To provide a visual summary of the results, Figure 3 presents the first stages observed in the top (panels A and B) and bottom terciles (panels C and D) of between-school cutoffs if these were ordered according to the grades at which they happen. It illustrates that on average, students are interested both in accessing the best schools and avoiding the worst—in both cases discrete changes in the probability of attending the higher-ranked school are observed at the cutoff. However, the magnitude of this effect is larger among the higher cutoffs.

Panels E–H present evidence on Baccalaureate performance.[45] Panels E and F suggest that gaining admission to a higher-ranked school when the cutoff in question is in the top third of cutoffs raises testing performance. Panels G and H point to a similar if less precisely estimated effect among the bottom cutoffs.

Table 5 presents regressions for these and other samples. For the sake of space, it uses only observations within one point of the cutoffs, and focuses on between-school cutoffs. To illustrate, panel A refers to the full sample and repeats results presented above. Column 1 presents the first stage (from Table 3, panel A, column 1). Columns 2 and 3 focus on Baccalaureate taking and performance, and column 4 presents an IV specification. Panels B and C refer to the top and bottom tercile of cutoffs, and panels D, E, and F to towns with four or more, three, or two schools, respectively. Column 1 shows that the first stages are larger among the top (relative to the bottom) tercile and in two school towns (relative to larger markets). Column 2 shows that the lack of an effect on test taking persists in all subsamples. In contrast, the estimates surrounding Baccalaureate performance (column 3) again generally suggest a positive impact from having access to a higher ranked school. The magnitude of the effect is again larger wherever the first stages are larger (e.g., top versus bottom tercile, and smaller versus larger markets), although the estimates, especially in the IV specifications, cannot always be statistically distinguished.

Baccalaureate test subject dummies—since we know the subjects that each student took for the different parts of the Baccalaureate exam, we can include subject-specific indicators to control for differences in the composition of the tests students wrote. Finally, panel D adds track fixed effects.

[45] We omit the evidence on test taking because there is again no evidence of an effect along this dimension.

Table 5—Heterogeneity in Baccalaureate Effects
(*All specifications within one point of cutoffs*)

	School level average transition score (1)	Baccalaureate taken (2)	Baccalaureate grade (3)	Baccalaureate grade IV specification (4)
Panel A. Full sample				
1{Grade ≥ cutoff}	0.107***	0.000	0.018***	0.163***
	(0.001)	(0.001)	(0.002)	(0.020)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.790	0.054	0.483	—
Observations	1,857,376	1,857,376	1,256,038	1,256,038
Panel B. Top tercile				
1{Grade ≥ cutoff}	0.158***	0.003	0.048***	0.317***
	(0.002)	(0.002)	(0.003)	(0.022)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.749	0.028	0.448	0.448
Observations	756,141	756,141	579,566	579,566
Panel C. Bottom tercile				
1{Grade ≥ cutoff}	0.099***	−0.008*	−0.005	−0.063
	(0.003)	(0.004)	(0.009)	(0.099)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.459	0.050	0.223	0.222
Observations	392,475	392,475	212,282	212,282
Panel D. Towns with four or more schools				
1{Grade ≥ cutoff}	0.097***	0.000	0.016***	0.162***
	(0.001)	(0.001)	(0.002)	(0.023)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.792	0.053	0.483	—
Observations	1,806,411	1,806,411	1,223,341	1,223,341
Panel E. Towns with three schools				
1{Grade ≥ cutoff}	0.333***	−0.007	0.028*	0.085*
	(0.007)	(0.009)	(0.016)	(0.050)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.745	0.069	0.488	—
Observations	31,149	31,149	19,877	19,877
Panel F. Towns with two schools				
1{Grade ≥ cutoff}	0.697***	0.020	0.179***	0.249***
	(0.010)	(0.013)	(0.023)	(0.032)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.726	0.093	0.475	—
Observations	19,816	19,816	12,820	12,820

Notes: All regressions are clustered at the student level and include cutoff fixed effects. Standard errors are in parentheses. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff. For comparison, panel A replicates the (within one point) specifications in Tables 3 and 4. Panels B and C present analogous specifications for the top and bottom tercile of cutoffs, respectively. Panels D, E, and F present analogous results among towns with four or more schools, towns with three schools, and towns with two schools, respectively.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

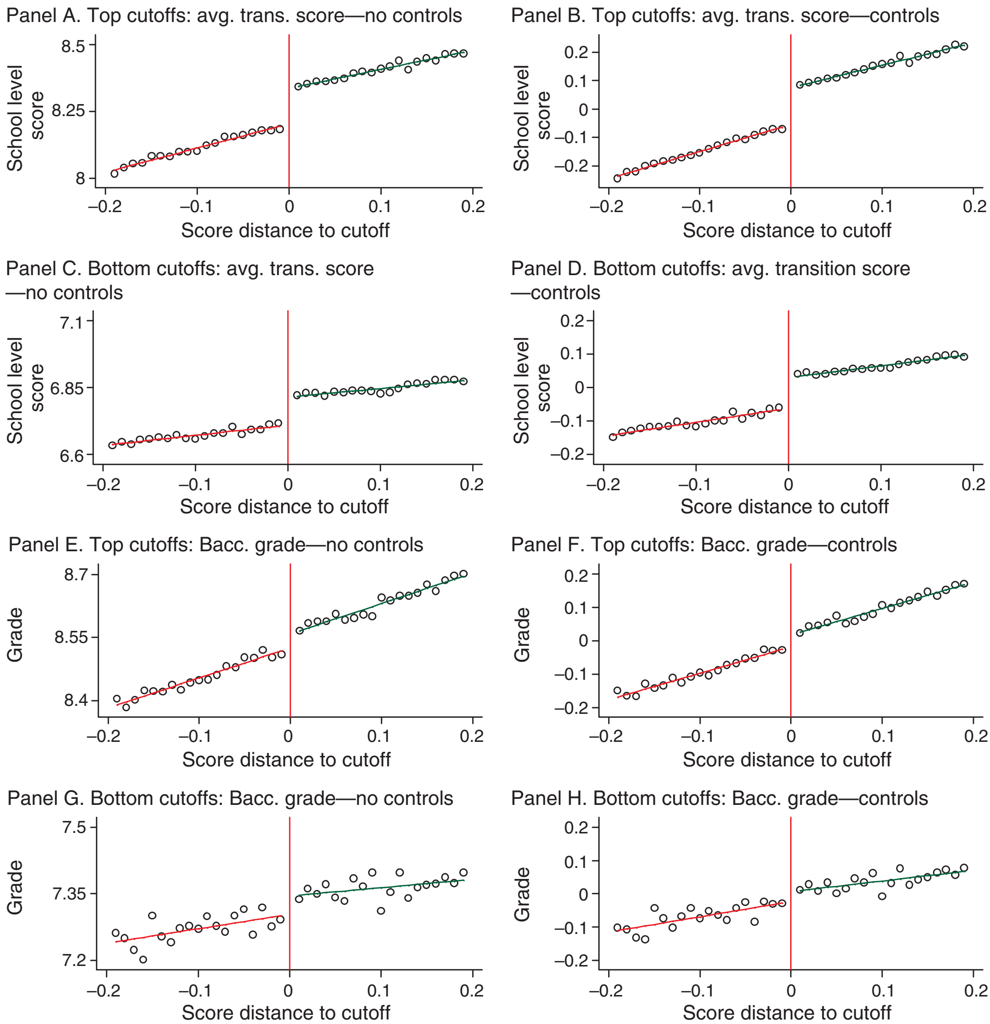

FIGURE 3. TOP AND BOTTOM TERCILES OF BETWEEN-SCHOOL CUTOFFS

Notes: All panels are based on administrative data for the 2001–2003 admission cohorts, and restrict observations to individuals with transition scores within 0.2 points of a cutoff. The left-hand-side panels plot (0.01 point) transition score cell means of the dependent variable. The right-hand-side panels plot analogous means of residuals from a regression of the dependent variable on cutoff fixed effects. In each panel the solid lines are fitted values of regressions of the dependent variable on a linear trend in the transition score, estimated separately on each side of the cutoff. Panels A, B, E, and F refer to the top tercile of between-school cutoffs ordered by the scores at which they take place; panels C, D, G, and H to the bottom tercile. The dependent variable in panels A–D is the average transition score of the peers students encounter at school; the dependent variable in panels E–H is the Baccalaureate exam grade.

The bottom line is that access to a higher-ranked school might be valuable to both high and low-scoring children, but statistical power constrains our ability to further explore such heterogeneity. More generally, school effects are difficult to identify, and sample size issues might account for some of the variation in conclusions observed in the literature.[46]

[46] In addition, online Appendix Table A.7 explores how track choices change around the cutoffs. Gaining access to a higher-ranked school does not on average imply automatically attending a better track, but instead the pattern is

D. *Behavioral Responses and Equilibrium Effects*

Using our survey data, we now investigate whether a major educational intervention, like giving a child access to a higher-ranked school, might lead to behavioral responses. As noted, our analysis does not attempt to isolate to what extent specific responses (e.g., those on the part of parents, teachers, or peers) account for the impact that attending a higher-ranked school has on children's academic achievement.[47]

To present results in this area, we make some notes on the structure of Tables 6–8. All of these estimate specification (7), but the level to which data are aggregated varies across panels. In each table, panel A aggregates outcomes to the school level; panel B aggregates them to the track level, and panel C presents them at the child or parent level. For example, in Table 6 one dependent variable is an indicator for whether Language teachers passed a certification exam. Panel A thus compares the children who scored just above a threshold with those who scored just below, and asks if on average their *schools* have more certified language teachers; panel B asks if the *tracks* they are in are more likely to have certified teachers; panel C asks if their *own* teacher is more likely to be certified.[48] Note that the variables from the principal survey only vary at the school level, so panels B and C are blank for them. As before, for each variable we present two specifications in Tables 6–8, where our preferred one is that restricted to individuals within one transition score point from the cutoff; we usually use these when discussing the results. The corresponding full sample results are in online Appendix Tables A.8–A.10. Finally, here we focus only on the top cutoffs. Among our survey towns, this restriction is relevant only for those with three schools (19 of the 59 towns in the sample); the two school towns of course contain only one cutoff.[49]

In addition, in Figure 4 panels A and B are aggregated to the school level, C and D to the track level, and E and F are at the student/parent level. Since we have fewer observations in the survey data, the cells that we plot are within 0.05 of a transition score from each other.

Teacher Characteristics.—Figure 4 and Table 6 describe the impact that scoring above a school cutoff has on the teacher characteristics that students experience. The first two columns of Table 6 show that students above the cutoff are about 13 percent more likely to attend a school with a principal who claims to have the best teachers in town. The remaining columns describe Language teacher qualifications as provided by their schools based on administrative records.[50]

nonlinear. Scoring above a cutoff means that one is more likely to attend the middle ranked tracks (Social Sciences and Humanities) and less likely to attend the worst (Technical) and best (Math) tracks. These effects are usually larger in markets with fewer schools (panels C and D), where schools offer a more limited range of tracks.

[47] Econometrically this would be hard to do without strong assumptions, given that we have one source of arguably exogenous variation in the treatment (attending a higher-ranked school), and many variables that might account for this effect (e.g., teacher, school, parent, and peer effects).

[48] An individual student's outcome can be different from that observed in his track because some schools feature multiple classes within a track. Our survey asked each student for his or her Language teacher's name, and we used that to match students to teachers and teacher characteristics as supplied by the school based on administrative data.

[49] This reflects that the difference in peer quality between the bottom two schools in three school towns was small.

[50] We focus on Language teachers since all children in all tracks take this subject.

Table 6—Teachers

	Principals perceive their school to be the best in teacher quality		Language teacher has the highest certification standard		Language teacher experience in years		Language teacher is a "novice" (less than two years experience)	
	Within 1 point of cutoff (1)	Within IK bound (2)	Within 1 point of cutoff (3)	Within IK bound (4)	Within 1 point of cutoff (5)	Within IK bound (6)	Within 1 point of cutoff (7)	Within IK bound (8)
Panel A. School level								
1{Trans. grade ≥ cutoff}	0.128***	0.112***	0.095***	0.087***	1.894***	1.790***	−0.037***	−0.033***
	(0.028)	(0.033)	(0.025)	(0.028)	(0.490)	(0.522)	(0.011)	(0.012)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.490	0.470	0.610	0.600	0.600	0.580	0.600	0.590
Observations	6,290	7,380	6,065	6,736	6,065	6,825	6,065	6,128
Panel B. Track level								
1{Trans. grade ≥ cutoff}			0.026	0.037	−0.076	0.118	−0.036***	−0.027
			(0.033)	(0.034)	(0.878)	(0.913)	(0.014)	(0.017)
Linear spline			Yes	Yes	Yes	Yes	Yes	Yes
R^2			0.480	0.450	0.410	0.370	0.450	0.390
Observations			6,065	7,977	6,065	8,978	6,065	8,305
Panel C. Student/parent level								
1{Trans. grade ≥ cutoff}			−0.005	−0.011	−0.625	−1.077**	−0.036***	−0.026**
			(0.021)	(0.021)	(0.539)	(0.496)	(0.011)	(0.010)
Linear spline			Yes	Yes	Yes	Yes	Yes	Yes
R^2			0.370	0.370	0.310	0.300	0.360	0.340
Observations			6,065	5,955	6,065	7,801	6,065	7,242

Notes: All regressions include cutoff fixed effects. Standard errors are in parentheses. The regressions in panel A are clustered at the school-cohort level, the regressions in panel B are clustered at the school-track-cohort level, and the regressions in panel C are clustered at the student level. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff. Panel A presents outcome variables that are aggregated at the school level. Panel B presents outcome variables that are aggregated at the track level. Panel C presents outcome variables that are at the child or parent level (see Section V).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

The dependent variable in columns 3 and 4 is an indicator for whether teachers have attained the highest certification standard—a credential that about 60 percent of teachers in Romania have. Panel A shows that relative to those who just miss, students who score above a school cutoff attend schools where on average Language teachers are about 10 percent more likely to have reached this standard. Panel B shows this effect is reduced to about 3 percent when one looks at the *tracks* students are enrolled in. Panel C shows that the effect essentially disappears once one considers the actual teachers assigned to students—those just above a cutoff are not more likely to have a certified teacher than those just below.

This conclusion is also visible in Figure 4, where panel A shows a sharp discontinuity in the school-level probability of Language teachers having the highest certification standard. Panel B shows significantly smaller discontinuities at the track level, and panel C suggests no discontinuity in terms of the actual teacher students encounter.

In short, in terms of teacher certification differences between schools exist on average, but these disappear when one considers the actual teachers experienced by students at the margin. This is consistent with teachers sorting both across and

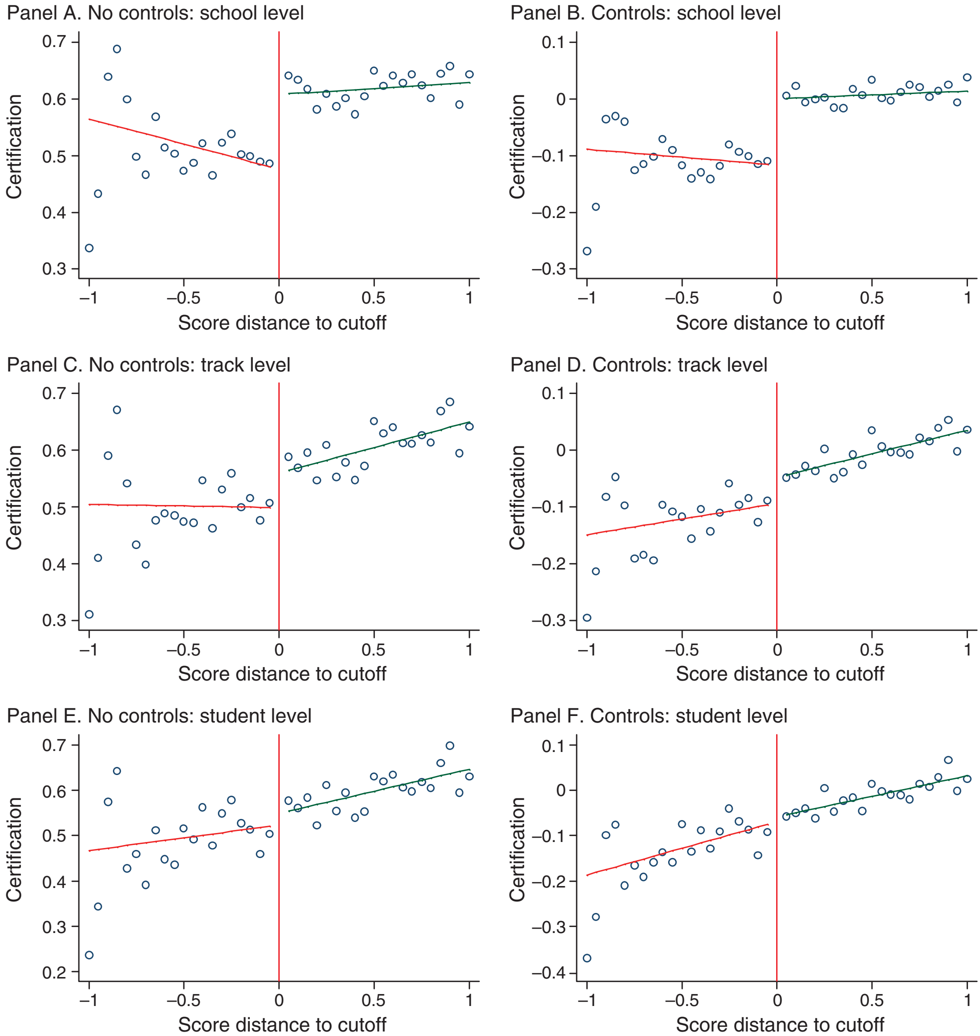

Figure 4. Teacher Certification

Notes: All panels are based on survey data for the 2005–2007 admission cohorts, and restrict observations to individuals with transition scores within one point of a cutoff. The left-hand-side panels plot (0.05 point) transition score cell means of an indicator for whether teachers have attained the maximum certification standard. The right-hand-side panels plot analogous means of residuals from a regression of the dependent variable on cutoff fixed effects. The solid lines are fitted values of regressions of the dependent variable on a linear trend in the transition score, estimated separately on each side of the cutoff. Panels A and B present the outcome variable aggregated to the school level, and panels C and D present it aggregated to the track level. Panels E and F present the outcome variable at the child or parent level.

within schools in a way associated with student stratification. For example, the pattern of results could reflect the highest certification teachers having a preference for—and through seniority gravitating toward—the highest academic ability children. Consistent with this, columns 5 and 6 (Table 6) reveal a similar pattern when teacher quality is measured using years of experience.

We note that differences persist across panels for some of our measures of teacher quality. Columns 7 and 8 show that attending a better school decreases the

Table 7—Parents

	Principals perceive their school to be the best in parental participation		Parents have volunteered in the past year		Parents have paid for tutoring services for child		Parents help child with homework often	
	Within 1 point of cutoff (1)	Within IK bound (2)	Within 1 point of cutoff (3)	Within IK bound (4)	Within 1 point of cutoff (5)	Within IK bound (6)	Within 1 point of cutoff (7)	Within IK bound (8)
Panel A. School level								
1{Trans. grade ≥ cutoff}	0.125***	0.130***	0.012**	0.012**	0.063***	0.064***	0.000	0.000
	(0.044)	(0.042)	(0.005)	(0.005)	(0.009)	(0.010)	(0.007)	(0.007)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.480	0.490	0.710	0.700	0.750	0.750	0.740	0.730
Observations	6,139	5,558	6,522	7,142	6,501	7,255	6,488	9,674
Panel B. Track level								
1{Trans. grade ≥ cutoff}			0.005	0.006	0.009	0.020*	−0.026**	−0.022**
			(0.006)	(0.007)	(0.012)	(0.011)	(0.010)	(0.010)
Linear spline			Yes	Yes	Yes	Yes	Yes	Yes
R^2			0.490	0.470	0.550	0.560	0.500	0.500
Observations			6,522	7,606	6,501	5,363	6,488	7,141
Panel C. Student/parent level								
1{Trans. grade ≥ cutoff}			−0.002	0.007	−0.003	−0.003	−0.043**	−0.033**
			(0.015)	(0.014)	(0.018)	(0.017)	(0.019)	(0.017)
Linear spline			Yes	Yes	Yes	Yes	Yes	Yes
R^2			0.070	0.070	0.130	0.130	0.090	0.090
Observations			6,522	7,905	6,501	6,771	6,488	8,840

Notes: All regressions include cutoff fixed effects. Standard errors are in parentheses. The regressions in panel A are clustered at the school-cohort level, the regressions in panel B are clustered at the school-track-cohort level, and the regressions in panel C are clustered at the student level. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff. Panel A presents outcome variables that are aggregated at the school level. Panel B presents outcome variables that are aggregated at the track level. Panel C presents outcome variables that are at the child or parent level (see Section V).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

probability of having a "novice" teacher (one with two or fewer years of experience) not just on average, but also at the margin.[51]

Parental Effort.—Table 7 and online Appendix Figures A.3 and A.4 describe the impact that access to a higher-ranked school has on measures of parental involvement and effort. We focus on measures of participation at school and on variables intended to capture parents' interactions with their children, such as the willingness to help with homework or pay for tutoring services.[52]

The first four columns of Table 7 indicate that children above cutoffs attend schools where parents are on average more involved at school. This emerges both in principals reports of parental participation (columns 1 and 2, panel A) and in parents' self-reports on volunteering (columns 3 and 4, panel A). However, the impacts at the

[51] On average teachers have about 15 years of experience, with only 6 percent having less than two years.

[52] To illustrate the variation in these dimensions, 11 percent of parents report having volunteered in their child's classroom, school office, or library in the last year. About 24 percent report having paid for private tutoring lessons, a common practice in Romania (secondary education is free in Romania; hence we do not consider tuition expenses as a measure of effort). Roughly 20 percent of parents claim to help with homework on a daily or almost daily basis.

track and individual level become small and statistically insignificant (columns 3 and 4, panels B and C). Online Appendix Figure A.3 confirms that there is a discontinuity in parental volunteering at the school level, but not at the parent level. As was the case with teacher characteristics, this implies differences between the average and marginal parental effort: children who score just above cutoffs have peers whose parents participate more at school, but their *own* parents do not participate more than those of children who score just below. A similar conclusion emerges in columns 5 and 6, where we consider the frequency of parental expenditures on tutoring.

At the same time, there is evidence of parental behavioral responses in other dimensions. Columns 7 and 8 in Table 7 consider the extent to which they help their children with homework. Panel A shows that children who score above cutoffs attend schools where on average children are no more likely to receive parental help on a daily or almost daily basis. This might not be surprising if the need for help declines with academic ability. However, the most striking result is shown in panel C (as well as in panel F of online Appendix Figure A.4): children who score just above cutoffs are *less* likely to get help from their parents. This suggests that at least in our setting, parents might view their own effort and school quality as substitutes.

Interaction with Peers.—Our first stage result is that children who score above cutoffs are on average exposed to peers that have higher average transition scores.[53] This result is confirmed and expanded upon by columns 1 and 2 of Table 8, which measure peer quality using principals' ranking of student quality among schools within their towns.

According to the often cited linear-in-means model, these findings would imply positive peer effects for the children who make it into a higher-ranked school.[54] However, scoring above a cutoff could adversely impact children if their relative ability matters, since this makes them "a small fish in a big pond." Indeed, models which stress relative comparisons suggest negative effects through a reduction in confidence and/or self-esteem.

To explore this possibility, we first investigate whether children who score just above cutoffs actually perceive being lower in their peer ability distribution. Columns 3 and 4 use questions which asked children about their rank within their track. The responses ranged from 1 to 7, with higher numbers indicating a better ability rank. Panels A and B show that children in higher-ranked schools are more likely to feel they are strong relative to their peers; further, as might be expected if individuals have over-optimistic views, the coefficient is positive rather than zero. More interestingly, panel C confirms that in contrast, children who score just above cutoffs rank *themselves* lower than those who score just below—the coefficients are negative and significant in this case. This might not be surprising given that Romania's student allocation system is well understood by students.

Finally, we explore whether such feelings of inferiority are associated with the nature of children's interaction with their peers. We measure this using an index of negative interactions that averages four indicators for whether children report

[53] The first stage for this particular sample is shown in columns 7–9 of panel D, Table 3.

[54] A number of papers investigate the existence and functional form of peer effects (e.g., Hoxby and Weingarth 2006, Lavy, Paserman, and Schlosser 2007, and Jackson 2010b).

Table 8—Peers

	Principals perceive their school to be the best in student quality		Child's perception of his/her rank in his/her track		Child's experience of negative interactions with peers	
	Within 1 point of cutoff (1)	Within IK bound (2)	Within 1 point of cutoff (3)	Within IK bound (4)	Within 1 point of cutoff (5)	Within IK bound (6)
Panel A. School level						
1{Trans. grade ≥ cutoff}	0.336***	0.333***	0.172***	0.159***	0.004	0.005
	(0.045)	(0.046)	(0.028)	(0.032)	(0.008)	(0.008)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.470	0.470	0.690	0.680	0.690	0.690
Observations	6,513	6,737	6,478	8,264	6,500	6,612
Panel B. Track level						
1{Trans. grade ≥ cutoff}			0.090***	0.100***	0.012	0.014
			(0.031)	(0.033)	(0.011)	(0.011)
Linear spline			Yes	Yes	Yes	Yes
R^2			0.520	0.520	0.480	0.490
Observations			6,478	9,666	6,500	8,162
Panel C. Student/parent level						
1{Trans. grade ≥ cutoff}			−0.134***	−0.126***	0.045**	0.036**
			(0.059)	(0.051)	(0.019)	(0.017)
Linear spline			Yes	Yes	Yes	Yes
R^2			0.120	0.130	0.080	0.080
Observations			6,478	9,009	6,500	8,289

Notes: All regressions include cutoff fixed effects. Standard errors are in parentheses. The regressions in panel A are clustered at the school-cohort level, the regressions in panel B are clustered at the school-track-cohort level, and the regressions in panel C are clustered at the student level. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff. Panel A presents outcome variables that are aggregated at the school level. Panel B presents outcome variables that are aggregated at the track level. Panel C presents outcome variables that are at the child or parent level (see Section V).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

that, in the last month, their peers have: (i) been mean to them, (ii) hit them, (iii) taken their things without asking, or (iv) made them feel marginalized. The possible responses for each of these items ranged from zero (did not happen at all in the past month) to five (happened daily); the average of 0.12 across all four indicators suggests that these events are relatively rare. The results in Table 8 (columns 5 and 6) do not reveal average differences at the school level. However, the track and most importantly the individual level provide evidence of more frequent negative interactions for children who score just above cutoffs, a pattern confirmed by the graph in panel F of online Appendix Figure A.5. In short, these results leave open the possibility that getting into a better school might result in feelings of insecurity or marginalization.

Student Effort.—Finally, we explore effort responses on the part of students. Our variables of interest are indicators for whether students did homework daily or almost daily in the month prior to the survey, an assessment of which our survey solicited from both parents and the children themselves. The results are presented in the first four columns of online Appendix Table A.11, where panel A suggests that students in better schools do more homework on average. In this case this effect persists on the margin at least for the parental reports, which suggest a 5 percent increase in the probability of doing homework on a daily or almost daily basis.[55] Finally, columns 5 and 6 show that while on average children at better schools perceive homework to be easier, the coefficient ceases to be statistically significant and changes sign at the margin, suggesting that, perhaps not surprisingly, marginal children encounter more difficulty with homework at higher-ranked schools.[56]

E. *Within Track Analysis*

Thus far, we have focused on the reduced-form effects of having the chance to attend a better school or track within a school. This is a natural first approach given that students apply for high school/track combinations. In this section, we consider the effects of being able to enroll in a better *class* within a given track.

Specifically, the Ministry of Education stipulates that after students are admitted to a particular track, they should be allocated to classes containing at most 28 students.[57] The Ministry does not specify the details of this allocation; this is decided by each school. Our survey data suggest that many schools further stratify children into classes based on their transition scores.[58]

To estimate the effect of having access to a better class (within a track), we focus only on tracks which had slot offerings that were multiples of 28 (i.e., 56, 84, etc.), and which were also filled at the time of the admission process.[59] We ranked the students in these tracks in descending order based on their transition scores, and calculated class level cutoff scores based on the transition score of the 28th (or 56th, or 84th, etc.) student. As above, we stacked the data by keeping, on each side of a particular cutoff, the 28 students within a track with scores closest to the cutoff. Also as above, this analysis should yield intent to treat estimates of scoring above a particular class level cutoff.[60]

Understanding these effects is interesting for two reasons. First, by looking at children in different classes but in the same track, we make comparisons between students who are exposed to the same curriculum. Second, considering classes allows us to analyze behavioral responses in a way that more closely approximates the experimental

[55] The results on children's homework effort are confirmed graphically in online Appendix Figure A.6.

[56] In online Appendix Table A.12 we explore how the main behavioral responses differ if one compares school markets with two versus three schools. Online Appendix Table A.13 similarly analyzes how these vary with whether one looks at the top or bottom half of cutoffs according to the test score levels at which they are located. These exercises do not generally produce evidence of a substantial amount of heterogeneity in the magnitude of behavioral responses.

[57] After being allocated to a particular class, students usually spend the next four years with the same peers.

[58] In anecdotal evidence, our conversations with headmasters confirm that many schools have this policy.

[59] As mentioned, our identification strategy is based on the fact that the majority of schools reach their pre-announced capacity constraint. As noted, however, enrollment in many of the less desirable schools is less than the number of initial slot offerings. In contrast to the administrative data, we have exact information on school and track capacities in this exercise because it was collected in the survey.

[60] Again, while not every school in our sample allocates children to classes based only on the transition score, as long as a fraction of schools do so, we can estimate the effects of being able to attend a better class within a track.

TABLE 9—CLASS EFFECTS (*All specifications within one point of cutoffs*)

	First stage	Teachers			Parents		
	Class level transition score (1)	Language teacher has the highest certification (2)	Language teacher experience (in years) (3)	Language teacher has two or less years experience (4)	Parents have volunteered in the past year (5)	Parents have paid for tutoring services (6)	Parents help child with homework often (7)
Panel A. Class level							
1{Trans. grade ≥ cutoff}	0.127***	0.046**	1.832***	−0.006	0.006	0.026***	0.004
	(0.017)	(0.021)	(0.500)	(0.006)	(0.005)	(0.007)	(0.005)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.890	0.560	0.600	0.530	0.560	0.740	0.590
Observations	5,396	5,179	5,179	5,179	5,362	5,346	5,349
Panel B. Student/parent level							
1{Trans. grade ≥ cutoff}					−0.008	−0.001	−0.012
					(0.012)	(0.016)	(0.015)
Linear spline					Yes	Yes	Yes
R^2					0.120	0.250	0.120
Observations					5,373	5,357	5,360

	Peers		Child		
	Child's perception of his/her rank in track (8)	Child's negative interaction with peers (9)	Child does homework almost every day (child report) (10)	Child does homework almost every day (parent report) (11)	Child perceives homework to be easy (12)
Panel A. Class level (continued)					
1{Trans. grade ≥ cutoff}	0.044**	−0.007	0.021***	0.017**	0.032
	(0.019)	(0.005)	(0.008)	(0.007)	(0.02)
Linear spline	Yes	Yes	Yes	Yes	Yes
R^2	0.620	0.520	0.720	0.700	0.620
Observations	5,342	5,350	5,385	5,355	4,443
Panel B. Student/parent level (continued)					
1{Trans. grade ≥ cutoff}	−0.142***	−0.018	0.003	0.010	−0.032
	(0.047)	(0.013)	(0.017)	(0.015)	(0.042)
Linear spline	Yes	Yes	Yes	Yes	Yes
R^2	0.200	0.100	0.240	0.230	0.190
Observations	5,353	5,361	5,396	5,366	4,453

Notes: All regressions include cutoff fixed effects. Standard errors are in parentheses. The regressions in panel A are clustered at the school-class-cohort level and the regressions in panel B are clustered at the student level. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff. Panel A presents outcome variables that are aggregated at the class level. Panel B presents outcome variables that are at the child or parent level (see Section V).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

setting of Duflo, Dupas, and Kremer (2011), where an RD analysis compares students who are on the margin of being assigned to low or high achievement classes.

Table 9 presents the results at the class (panel A) and child or parent level (panel B). For variables that do not vary within classes, such as class-level peer quality or teacher qualifications, the results in both panels are identical and are therefore presented only once. Column 1 begins by illustrating the "first stage," showing that there is a clear discontinuity in classroom peer quality at the class cutoffs. An increase of 0.13 points in the average transition emerges from a regression using observations within 1 point of the cutoffs. Although the effect is highly significant, its magnitude is about half the size of the track-based estimates, and about one fourth the size of the school-based estimates. This reflects that there is significantly less variability in the transition scores between classes within school/tracks.

Columns 2–4 consider the same teacher characteristics examined in Table 6. The evidence suggests that teacher sorting is also prevalent across classes in a school/track. Students who score above a class cutoff are exposed to teachers who are 5 percent more likely to have the highest certification and have 1.8 more years of experience.[61]

The remaining columns present all the other outcome variables featured in our previous analysis of the survey data (measures of teacher certification, parental participation, and children's interaction with peers) with results that are qualitatively similar to those found above. For example, although the parent of the child who just makes it into a better class is not more likely to pay for tutoring services, this child is more likely to be exposed to peers whose parents buy such services. At the same time, several key coefficients in this table, especially the marginal effects in panel B, are imprecisely estimated, which could be explained both by the smaller sample sizes and the fact that the differences in educational environments (column 1) are less stark than in the school or track level analysis. Nevertheless, the bottom line is that many of the behavioral responses observed above—particularly the sorting of more qualified teachers to better classes—can also be observed across classes within the same track.

F. *Effects across Cohorts*

Finally, we investigate whether the behavioral responses we study have a dynamic component: For example, do students' responses emerge only gradually during their high school years? For the sake of space, Table 10 focuses only on specifications including students within one transition score point of the cutoffs. For reference, panel A repeats previous specifications at the student level. For example, the dependent variable in column 2 is the Language teacher experience measured in years. The coefficient (−0.625) is from Table 6, column 8, panel C, and shows there is no change (at the cutoff) in the experience of the teacher students encounter.

Panel B explores whether these effects vary with time by looking at how they change across the three entry cohorts we surveyed: 2005, 2006, and 2007. This comparison allows us to explore if there are differences according to whether students are in their second, third, or fourth year of high school. This is achieved by including an indicator for whether students' transition scores were above the cutoff, and interacting this dummy with indicators for the 2005 and 2006 cohorts, such that the first coefficient refers to the 2007 (the youngest) cohort.

The results contain some interesting variation. For example, columns 6 and 7 suggest that children's feelings of academic inferiority, and the frequency of their negative interactions with peers, are more marked in earlier stages of high school.

[61] The small and insignificant result on novice teachers (column 4) is not surprising given the results in Table 6, which suggested no difference in this dimension across tracks.

Table 10—Marginal Effects across Cohorts

	First stage	Teachers		Parents		Peers	
	School level transition score (1)	Language teacher experience measured in years (2)	Language teacher has two or fewer years of experience (3)	Indicator for parents having paid for tutoring services (4)	Indicator for parents helping child with homework often (5)	Child's perception of his/her rank in his/her class (6)	Child's experience of negative interactions with peers (7)
Panel A. Student/parent level							
1{Trans. grade ≥ cutoff}	0.477***	−0.625	−0.036***	−0.003	−0.043**	−0.134**	−0.045**
	(0.018)	(0.539)	(0.011)	(0.018)	(0.019)	(0.059)	(0.019)
Bandwidth within 1 point of cutoff	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.700	0.310	0.360	0.130	0.090	0.120	0.080
Observations	6,559	6,065	6,065	6,501	6,488	6,478	6,500
Panel B. Student/parent level by cohort							
1{Trans. grade ≥ cutoff}	0.483***	−0.764	−0.027*	−0.029	−0.061**	−0.165**	−0.055**
	(0.022)	(0.688)	(0.014)	(0.020)	(0.026)	(0.075)	(0.025)
1{Trans. grade ≥ cutoff} × cohort2006	0.043*	0.384	−0.024	0.013	0.018	−0.012	−0.002
	(0.024)	(0.738)	(0.016)	(0.021)	(0.028)	(0.083)	(0.025)
1{Trans. grade ≥ cutoff} × cohort2005	−0.060**	0.047	−0.004	0.065**	0.037	0.103	0.031
	(0.024)	(0.775)	(0.015)	(0.026)	(0.027)	(0.083)	(0.027)
Bandwidth within 1 point of cutoff	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.700	0.310	0.360	0.130	0.090	0.120	0.080
Observations	6,559	6,065	6,065	6,501	6,488	6,478	6,500
p-statistic (1{Trans. grade ≥ cutoff} + 1{Trans. grade ≥ cutoff} × cohort2006 = 0)	0.00	0.58	0.00	0.49	0.08	0.02	0.02
p-statistic (1{Trans. grade ≥ cutoff}+ 1{Trans. grade ≥ cutoff} × cohort2005 = 0)	0.00	0.31	0.01	0.16	0.29	0.42	0.29

Notes: All regressions are clustered at the student level and include cutoff fixed effects. Standard errors are in parentheses. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff. Panel A presents outcome variables that are aggregated at the child or parent level. Panel B presents outcome variables that are at the child or parent level and it also includes interactions for being in the 2005 and 2006 entry cohort with the dummy for whether a student's transition score is greater than or equal to the cutoff (see Section V).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

This might not be surprising to the extent that the transition scores contain noise, and therefore the children ranked lowest upon entry are unlikely to on average turn in the worst performance during the year. As this information is revealed, students' self assessment and their peers perceptions might change, affecting behavior.

Similarly, the reductions in parental help with homework happen early on and are partially reversed by the senior year. Specifically, in column 5 the key coefficient is significant for the youngest cohort (2007), but the joint test for the oldest cohort (2005) is not statistically significant (p-value $= 0.29$). This is consistent, for example, with parents gradually realizing that their child's enrollment in a higher-ranked school might not eliminate her need for support. Consistent with this, column 4 shows that while the parents of children who just qualify for more selective schools are not more likely to invest in tutoring in the second year of high school, by the fourth they are in fact more likely to do so. If tutoring raises academic achievement (e.g., Banerjee et al. 2007) then this might contribute toward finding school effects by the fourth but not the second year. Finally, columns 2 and 3 show less evidence of variation across cohorts in terms of teacher characteristics.

To summarize, we also find evidence that in Romania some of the behavioral responses identified have a dynamic component. This is relevant, for example, because it implies that the estimated effects of experimental or quasi-experimental analyses might depend on whether academic outcomes are measured at the ninth or 12th grade level.

VI. Conclusion

In fields ranging from Labor to Development Economics, great interest surrounds the impact of educational quality on individual outcomes. This impact is difficult to measure mainly because it is hard to find situations in which comparable students enroll in schools of different quality.

In this paper, our first contribution has been to address this obstacle by analyzing Romania's educational system, which allocates students to high schools in one of the most systematic procedures observed around the world. This mechanism yields a large number of RD-based quasi-experiments, enabling us to add to the literature with unusually large sample sizes and an exploration of the heterogeneity in effects at different points of the test score distribution. Our second contribution has been to implement a specialized survey in a subset of towns, using it to explore behavioral responses that arise when a child has the opportunity to attend a higher-ranked school.

Our first reduced form result is that access to a better-ranked school has a positive impact on cognitive outcomes as measured by a high-stakes exam. This has not been a consistent finding in the literature, as some papers—including some which also rely on an RD approach—find little indication that enrolling in a higher-achievement school or class raises learning.

Our second set of results provides evidence of significant behavioral responses and equilibrium effects. Specifically, we find that teachers sort in response to the stratification of students, such that an individual who just makes it into a more selective school is assigned a teacher who is less qualified than the average instructor at the school, and possibly no different than the teacher she would have encountered at the school she just avoided. This teacher sorting may be an equilibrium outcome that has developed over time in this stratified school system.[62] Similarly, while children who gain access to higher ranked schools encounter greater average parental participation, there is little evidence that their *own* parents increase their commitment to education, and in fact there is some indication that they reduce the extent to which they help with homework. Along the same lines, while children who make it into more selective schools are exposed to better peers, they also seem to realize they are weaker and feel marginalized. For the behavioral responses by parents and children, we provide evidence that these effects have a dynamic component—the estimates depend on how many years the child has attended a particular school.

[62] As noted, we are not the first to highlight behavioral responses on the part of teachers. For example, Duflo, Dupas, and Kremer's (2011) experimental setting reveals that in Kenya increased shirking by teachers can counteract the positive impact of class size reduction on learning.

While the magnitude and even the direction of these responses may reflect institutions that are specific to Romania, their existence reflects that educational markets are complex settings in which several agents interact. As a result, the causal mechanisms that link a given school input to outcomes like wages or learning are likely to be complicated. Thus, our results provide support for theoretical work suggesting that educational interventions should be analyzed with reference to their effects on the behavior of agents involved in the educational process, e.g., Das et al. (2013), MacLeod and Urquiola (2009), and Albornoz-Crespo, Berlinski, and Cabrales (2010).

These results also have implications for the experimental evaluation of educational interventions. This research typically relies on partial equilibrium experiments that hold constant factors including responses like those explored here; additionally, it often measures outcomes in the short run. If dynamic behavioral responses are relevant, however, then the very nature (and impact) of a given intervention may change as actors have a chance to respond. Further, some of these responses may only be observed when an intervention is taken to scale and sustained. These possibilities amplify the usual concerns regarding small scale experiments' external validity.

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