Going to a better school: Effects and behavioral responses

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Abstract

This paper: i) estimates the effect that going to a better school has on students' academic achievement, and ii) explores mechanisms and behavioral responses that may account for these effects. For the first task, we exploit almost 2,000 regression discontinuity quasi-experiments observed in the context of Romania's high school educational system. For the second, we use data from a specialized survey of children, parents, teachers and principals that we implemented in 59 Romanian towns. The first finding is that students do benefit from access to higher achieving schools and tracks within schools. A second set of more descriptive results points to behavioral responses from teachers, parents, and children working together to reduce the net benefit that children might derive from going to a better school. While such behavioral responses may well be setting-specific, their existence suggests that educational interventions may have different impacts when implemented in a limited fashion and when fully taken to scale.

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1. Introduction

Whether students benefit from attending higher-achieving schools is an important question in education. For example, part of the rationale underlying *No Child Left Behind* is that a child in a low-achievement institution would be better off transferring to a higher-scoring school. Clear evidence on whether this is the case is scarce, in large part because students are not randomly allocated to schools or classes. Nonetheless, several analyses have exploited compelling research designs to circumvent this problem, with Dale and Krueger (2002) and Cullen, Jacob, and Levitt (2006) providing early examples.

Several more recent papers rely on regression discontinuity (henceforth RD) designs. Specifically, Hoekstra (2009), Jackson (2010), and Saavedra (2009) find that relative to students who just miss gaining admission to high achieving educational institutions, those who do have better academic and/or labor market outcomes. In contrast, Clark (forthcoming), Duflo, Dupas, and Kremer (forthcoming), and Sekhri and Rubinstein (2010), find scant evidence of impacts from getting into a better school or class (within a school), in results that are broadly consistent with Dale and Krueger (2002) and Cullen, Jacob, and Levitt (2006).

All these papers provide valuable arguably causal estimates, but are mostly silent on what mechanisms might account for their findings.[1] This matters because understanding their mixed results might benefit from knowing how the opportunity to be at a better school leads to behavioral responses by the different agents (e.g., children, parents, and teachers) that interact to shape educational outcomes. For one example, the benefit a child derives from going to a better school might be mitigated if her parents react by lowering their own effort. Beyond this, RD-based approaches are often constrained by statistical power and by the necessarily "local" interpretation of their estimates, which may apply, for example, only to elite schools.

This paper addresses these challenges by applying an RD design to Romania's high school system. Its objectives are i) to estimate the effects of getting into a better school or class, and ii) to begin to understand what mechanisms and behavioral responses might mediate these effects.

First, we estimate the effects of attending a better school using Romania's centralized admission process, which generates about 700 potential RD type-cutoffs per year. We pool data from the universe of high schools and from three cohorts of entering students, obtaining about 2,000 cuttoffs

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

and larger sample sizes than have been previously available in this literature. The substantial number of cutoffs further allows 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.

Secondly, we explore mechanisms and behavioral responses using data from a specialized survey that we implemented in a subset of towns. This survey of children, parents, teachers, and principals is motivated by recent theoretical work suggesting that educational interventions should ideally be analyzed with reference to their potential effects on the behavior of agents involved in the educational process, e.g., Albornoz, Berlinski, and Cabrales (2010), Das, Dercon, Habyarimana, and Krishnan (2004), and MacLeod and Urquiola (2009).

More specifically, this paper exploits 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 8^{th} grade test and their grade point average. After obtaining their transition score, students submit a list of high school/track combinations they wish to enroll in, where the tracks are Mathematics, Natural Sciences, Technical Studies, Services, Social Studies, Literature, and Natural Resources and Environmental Protection. 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, although they share inputs like facilities and a principal.

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.[2] This gives rise to cutoff scores that we set equal to the transition score of the child that fills the last slot in a given school/track.

We show that there are clear discontinuities in educational quality at the cutoffs that determine access to schools or tracks. For instance, relative to students who score just below a school cutoff, those who score just above experience, on average, a highly significant 0.2 standard deviation increase in the average transition score displayed by their peers. Our survey reveals other examples of quality improvements. To cite one, while there is no evidence that better infrastructure is available at higher ranked schools, students who just make it into them report making significantly greater

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

use of facilities such as libraries and computer laboratories—more selective schools seem to offer a culture in which such resources are more likely to be utilized.

We use an RD design to 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 actual grade is used by many institutions as an important (or sole) admission criterion.

A first set of results suggests that students do benefit from access to higher ranked schools and tracks within schools. Specifically, relative to individuals who just miss scoring above a cutoff, those who succeed display a statistically significant 0.05 standard deviation advantage in Baccalaureate performance.[3] If scaled by the associated improvements in peer quality, these effects are of a magnitude consistent with some estimates in the literature.[4]

We find these effects are often larger and almost always more precisely estimated for cutoffs that occur at higher grade levels. However, the estimates for higher cutoffs are often statistically indistinguishable from those for cutoffs at lower grade levels. The bottom line is that gaining access to a better school might be valuable to both high and low-scoring children, but statistical power seems to quickly constrain our ability to explore such heterogeneity.

A second more descriptive set of results concerns potential mechanisms and behavioral responses. Overall, it points to behavioral responses from teacher sorting, parental attitudes, and children's relationships with their peers going in the same direction: Toward reducing the net benefit that children might derive from going to a better school. This pattern of results suggests that absent such behavioral responses, our design might yield larger "school effects" than we find.

More specifically, we provide evidence consistent with teachers sorting in response to the stratification of students. Teachers with more experience and higher certification standards are more likely to teach in 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.[5] As a result, although students who score just above a cutoff attend schools that on *average* have teachers with superior observable characteristics, the *marginal* (actual) teachers assigned to them are not observably different from those assigned to students who score just below the cutoff. To

[3] In a finding that facilitates the interpretation of this result, there is no significant impact on test taking.

[4] Specifically, a one standard deviation increase in peer quality is associated with a 0.1-0.2 standard deviation increase in the Baccalaureate grade.

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

the extent that the teacher characteristics we observe do predict teacher quality, this is consistent with sorting undoing a part of the teacher-related gains students experience when they shift to a higher-ranked school.

In terms of parental effort, we again find differences in average vs. marginal effects. For example, children who make it into better schools/tracks/classes are exposed to peers whose parents are significantly more involved in their education—they participate more at school, and are more likely to devote resources to private tutoring (which is common in Romania). To the extent that such involvement affects outcomes, this suggests a specific mechanism through which peer effects may operate. However, we find no evidence of changes in such parental behavior at the cutoffs, and in fact there is some indication that children who just make it into higher achieving schools receive *less* homework-related help from their parents.

Finally, while children who just make it into better schools are certainly exposed to higher-achieving children, they also perceive themselves as weaker relative to these peers, and are more likely to report negative interactions with them. This provides some evidence that getting into a better school is associated with marginalization.

These results inform not just the closely related literatures on tracking and school effects, but also the literature focused on experimental analyses of educational policy. Specifically, while we do not expect the exact nature or even the direction of the behavioral responses we find to extend to all settings, we do believe that our results suggest that large scale interventions will generally result in equilibrium responses by the different actors involved in educational markets.

These responses are often not observed or are explicitly held constant in research focused on partial equilibrium interventions (see Banerjee and Duflo (2008) and Deaton (2010) for discussions). In an influential example, Duflo, Dupas, and Kremer (forthcoming) report on an experiment in which children in randomly selected Kenyan schools were temporarily separated into low and high-achieving classes. They find that average learning was higher in these schools than in those where such tracking did not take place. Further, they implement an RD design that suggests such sorting does not hurt those "left behind" in the weaker classes.[6]

Our results suggest, however, that if such tracking were truly "scaled up" in Kenya, as it has been in Romania, agents would respond in ways that may ultimately result in different conclusions. For one example, while in Duflo, Dupas, and Kremer (2009) teachers were randomly assigned to classes,

[6] See Figlio and Page (2002), Zimmer (2003), and Lefgren (2004) for related quasi-experimental studies.

in our data they seem to exercise a preference for higher achieving classes. Similarly, parental effort may not change in a temporary experiment, but might respond once an intervention is sustained. Likewise, wages may eventually reflect that tracking reveals information on ability, and this may in turn affect student effort (MacLeod and Urquiola, 2009).

As stated, such behavioral responses may well be setting-specific. For example, Duflo, Dupas, and Kremer (2009) are aware that teacher sorting could happen, and argue that in Kenya this would result in more effective teachers being matched to weaker children. In contrast, in the U.S., Lankford, Loeb, and Wyckoff (2002) suggest that low-achieving students are typically matched with the least-skilled teachers. Similarly, while we find evidence that Romanian parents view school quality and their own effort as substitutes, other parents might view them as complements. Again, our point is not that there would be uniformity in responses across all settings, but that such responses may affect the key characteristics and impacts of an educational intervention. Indeed, the presence or absence of similar behavioral responses might account for the mixed findings in the growing RD literature on school effects cited above.

Finally, our work is also related to the literature that studies 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 (1999b), Brown (2006)) without considering parent-school interactions. Das, Dercon, Habyarimana, and Krishnan (2004) is a notable exception that studies how parents adjust their educational expenditures in response to anticipated and unanticipated school grants.[7]

The remainder of the paper proceeds as follows. Section 2 describes the student allocation mechanism, and sections 3 and 4 our data and methodology, respectively. Section 5 presents results, and Section 6 concludes.

2. The student allocation mechanism

The transition between middle and high school (8^{th} to 9^{th} grade) in Romania results in an unusually systematic and transparent allocation of students to schools. Specifically, every child who

[7] A related literature looks at private responses to public transfers (e.g., Moffitt (1992), Rosenzweig and Wolpin (1994), Jacoby (2002), and Jensen (2003)). Case and Deaton (1999a) point out that the impact of public 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.

completes middle school receives a transition score which equally weights: i) her performance in a national 8^{th} grade exam covering Language, Math, and History/Geography, and ii) her gymnasium (grades 5-8) grade point average.[8]

After receiving their transition scores, students submit an essentially unlimited list of ranked choices which specify a combination 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.[9] 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, and some schools offer more than one class per track, with class sizes subject to a cap.

Students' school/track choices are expressed through an application form submitted (through their gymnasium) to the Ministry of Education in the capital, Bucharest. Using a computerized system, the Ministry then allocates individuals into school/tracks, giving priority to higher scoring students and assigning them their most preferred choices until predetermined school/track capacity constraints bind. Schools submit their track-specific capacities to the Ministry in advance, and simply apply the admission lists returned from the capital. Under this set up (in contrast to many school choice schemes), students have incentives to truthfully reveal their preference rankings.

Finally, when a school offers multiple classes of the same track, the system just returns to it 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 detailed in the next section); these data and the anecdotal evidence suggest that many schools further stratify classes by ability.

3. Data

We rely on two types of data: i) administrative information covering essentially the universe of children who make the middle to high school transition, and ii) data from a survey we administered in most towns with two or three high schools.[10]

[8] 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.

[9] 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.

[10] We use the term town to denote high school markets. 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

3.1. **Administrative data.** Our administrative data cover the 2001-2007 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.

For the first three of these cohorts (2001—2003), we linked these data with information on whether students took the Baccalaureate exam and how they performed (these cohorts took the exam in 2005-2007).[11] As stated, a satisfactory Baccalaureate grade is a prerequisite for applying to university, and an excellent one essentially guarantees admission to the most prestigious institutions.

We focus on two subsamples of these administrative data:

(1) The 2001-2003 admissions cohorts, for which we have Baccalaureate outcomes for 334,000 students' attending about 800 high schools in 135 towns.
(2) The 2005-2007 cohorts, for which we have only admissions information and can thus only explore "first stages". This subsample consists of 301,000 students' originating in 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. The first two reflect 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.[12] Finally, 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.[13] After these exclusions, Table 1 presents summary statistics at the individual, track, school, and town level.

3.2. **Survey data.** While the administrative data sets offer substantial sample sizes, they contain only basic information, essentially foreclosing an analysis of what mechanisms may account for

a corresponding catchment area composed of smaller towns or villages. In all cases, these units should approximate self-contained (high school) educational markets.

[11] 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 restrict the analysis to exact matches.

[12] 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.

[13] For analyses of vocational education in Romania, see Malamud and Pop-Eleches (forthcoming).

the effects found below. We therefore carried out a survey collecting more information through 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 brief description of its implementation. The 2005-2007 administrative data described above provided students' names, but not their addresses or any way of contacting them or their parents. The data also contained almost nothing in the way of school characteristics.

We therefore approached schools and asked their principals/administrators to fill a school survey, and to provide us with the addresses of the students in the mentioned cohorts (who were still in school at the time). 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, infrastructure, 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. Each teacher was later matched to the students in the household survey based on who (by name) the students indicated were their teachers in these subjects.

During the first half of 2010, we used the list of addresses to directly approach parents and students at home. The survey we 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 in the family. Third, we conducted a separate interview with the child from the selected school. Both the parental and the child surveys included questions on parent-child relationships, school performance and school experiences, an evaluation of the child's teachers, and a range of questions about child and family well being.

Two factors determined that we restricted 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 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 as large in smaller towns. We therefore started with an initial sample of 57,534 children and 167 schools in the 71 towns with two or three schools. If any school in a given town declined to participate, we simply abandoned the whole town.

In the event, we obtained complete school surveys and student data from 148 schools in 63 towns. The administrators in these schools provided us with 32,307 addresses. We restricted the target sample further to 135 schools in 59 towns, which contained 19,878 children.[14] 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 both information from the household and school questionnaires. We return to a more thorough discussion of its entries in using each section to produce results below.

4. 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 still 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.[15] In other words, we set each school's (or school/track's) cutoff equal to the transition score of the child that fills its last slot, where as stated the number of available slots are announced by schools prior to the admissions process.

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 in our sample makes for a potential RD analysis. In this section we first discuss the conceptual basis for analyzing any given one of these experiments, focusing on schools for simplicity. We then describe how we go about summarizing them.

4.1. **Empirical setup for a single between-school cutoff.** Consider a town in which i indexes students and $s = 1, \ldots, S$ indexes schools, where we assume the latter have been ordered from the

[14] The elimination of four 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.

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

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 a town, and $z = (S-1)$ indicates the cutoff between the top-ranked school and the next best institution. Let T_i stand for the average transition score among student i's peers (i.e., the average score among the children at her school), and let t_i denote the student's own transition score. Finally, let t_z be the minimum grade required for admission into the higher-ranked school of the two schools indexed by z.

In this setup, consider the regression:

$$T_i = \alpha 1\{t_i \geq t_1\} + a(t_i) + u_i \tag{4.1}$$

where $1\{t_i \geq t_1\}$ is an indicator for whether a student's transition score is greater than or equal to the cutoff which determines access into the next-to worst school (cutoff $z = 1$), and $a(t_i)$ is a flexible control function for the transition score. In this case, α estimates by how much students' peer groups improve, on average, when their score is just above rather than just below t_1.

The idea behind RD designs, originally proposed by Thistlewaite and Campbell (1960),[16] is that discontinuities like those measured by α can be used to identify the causal effect of scoring above a cutoff even if students' transition scores are systematically related to factors that affect outcomes like Baccalaureate grades. Intuitively, suppose the transition score is smoothly related to characteristics that affect achievement. Under this assumption, students with scores just below t_1 will provide an adequate control group for individuals with scores just above, and any differences in their outcomes can be attributed to the fact that they experience schools of different quality.

Specifically, one can run a reduced form regression analogous to (4.1) to explain outcomes like Baccalaureate performance, which we denote g_i:

$$g_i = \beta 1\{t_i \geq t_1\} + a(t_i) + v_i \tag{4.2}$$

Again, if in a small enough neighborhood around the cut-off, $a(t)$ is constant, then the effect of achieving access to the next to worst school, β, is non-parametrically identified at t_1 (Hahn, Todd, and VanderKlaauw, 2001). More generally, if $a(t)$ is specified correctly, it will capture all dependence of the Baccalaureate grade on the transition scores away from the cut-off, and one can use all the

[16] For an overview of the RD design, see Imbens and Lemieux (2008).

data to estimate (4.2). Below we will present such results, but also estimates that rely only on observations close to cutoff scores.

Finally, in addition to studying the impacts on Baccalaureate outcomes, we consider how a series of behaviors and/or characteristics on the part of students, parents, teachers, principals, and schools change as one crosses the cutoffs. For example, in a specification like (4.2), we ask if relative to children who score just below t_1, those who who score just above are more likely to have teachers who are certified, parents who help with homework, or access to computer facilities at school. This exercise is ultimately descriptive in nature—it allows us to explore which types of mechanisms/behaviors might be behind the Baccalaureate effects we find, and which would seem to be ruled out. Ultimately it is impossible to assign effects strictly to one mechanism or another.

4.2. **Summarizing information for many cutoffs.** Specifications (4.1) and (4.2) explain how one might exploit one regression discontinuity—that arising from the hypothetical transition from the worst to the next-to worst school in a given town. In fact, our data contain 1,984 such between-school cutoffs, and 6,434 between-track cutoffs.[17] Below, we present information that exploits this wealth of quasi-experiments, exploring, for example, how the impact of scoring above a given cutoff varies with where in the transition test score distribution these cutoffs are located.

However, in order to summarize these data and for the sake of statistical power, we first report regressions in which we pool data across cutoffs. For this, we normalize each cutoff score, z, to zero, and create a variable that measures the distance between each cutoff and the transition score of each student in a town. In some cases we then "stack" the resulting data such that every student in a town serves as an observation for every cutoff, and (since individual level observations are used more than once) run the analyses clustering at the student level.[18] Including all student observations for every cutoff is relevant in that, for example, the student with the best score in town could in principle attend any school she wanted. We note, however, that regressions restricted to students in bands close to the cutoffs in fact rarely use student-level observations more than once.

[17] 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.
[18] To illustrate, in the first year of our data, 2001, the first town in our data, Alba-lulia, has 836 students in 7 schools, producing 6 between-school cutoffs. For that year, this produces a data set of 5,016 (=836*6) observations, with similar calculations for the other two years of data.

5. 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 mechanisms and behavioral responses.

5.1. **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 3. The x-axis describes students' transition scores relative to the cutoffs (normalized to zero) that allow the opportunity to access a better 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 Panel B plots analogous information, but the y-axis is based on residuals from a regression of the mean transition score on a linear trend in students' transition grade and a series of cutoff fixed effects.[19] Both panels suggest that the average peer quality students experience increases significantly and discontinuously if their transition score crosses the threshold that gives them the option of going to a better school. The vertical distance between the points close to the discontinuity, further, is analogous to the estimate of α in expression (4.1).

Table 3, Panel A presents the regression analog to these results, where columns 1-3 refer to all the towns in our sample. Panel A refers to the 2001-2003 admissions cohorts, those for which we have Baccalaureate outcomes. Column 1 uses about 3.6 million observations from 1,984 cutoffs observed across the three cohorts. It regresses the average transition grade that students experience at school on an indicator for whether their scores are above cutoffs. The specification includes: i) a linear spline in students' grade distance to the cutoffs, one which allows the slope to vary on each side of the cutoff, and ii) cutoff dummies analogous to those used in Figure 1, Panel B.[20] The key estimate suggests that scoring above a cutoff results in a highly statistically significant jump in the peer quality students experience—0.09 points, which is equivalent to about 0.1 standard deviations in transition test performance.

[19] Figures 1-9 all have a similar structure in that the left hand side panels use raw data, and the right hand side panels use residuals based on regressions that control for a linear trend in the transition grade and cutoff fixed effects.
[20] We note that these and all the following results are not qualitatively affected by instead using a linear, quadratic, or cubic specification for $a(t_i)$ in (4.1), or by excluding the cutoff fixed effects.

Column 2 restricts the sample to include only students whose transition scores are within 1 point of a cutoff, reducing the number of observations to about half of those in Column 1. This is our preferred specification, as it appears 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.[21] We opt to feature, in Column 3, 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 1 point band used in Column 2.[22] In the event, all these samples result in similar and highly significant estimates of α.

Columns 4-6 repeat the specifications in columns 1-3 using the same administrative data, but focus only on towns included in our specialized survey—most towns with two or three schools, as described in Section 3. The corresponding graphical evidence for the survey towns is in Figure 2, panels A and B. The observed discontinuities are always statistically significant, and about four times the size of those observed in the full sample.

The "first stages" in Table 3, Panel A are those that will be relevant for the Baccalaureate outcomes.[23] They show that the Romanian high school admissions process provides a clear first stage 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.[24]

Further, in applying for high school slots students choose school/track combinations, and so the between-track cutoffs also provide candidate first stages. Figure 3 (panels A and B) present these graphically using the same specifications as panels A and B in figures 1 and 2. The corresponding regression results are presented in panel B of Table 3. In all cases the coefficient of interest is somewhat smaller (although always highly statistically significant) than that observed for the between school cutoffs.

[21] 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.
[22] Specifically, we follow Lee and Lemieux (Forthcoming) and use a simple rectangular kernel. Further, we implemented the bandwidth selection procedure using the Stata ado file labeled rdob.ado available at http://www.economics.harvard.edu/faculty/imbens/software_imbens.
[23] For the sake of space, we omit very similar results for the 2005-2007 cohorts.
[24] Aside from first stage results like those described in Table 3, the RD approach requires that there be no discrete changes in other student characteristics that affect outcomes like Baccalaureate performance. While our administrative information does not contain such variables, our survey data suggest this condition is fulfilled. Specifically, Appendix Table A.1 shows that a number of background characteristics (mother's age, mother's ethnicity, mother's education, child age, and child gender) do not vary discontinuously around the grade cutoff once we consider estimates within 1 point and IK bandwidths (all but two of the nine estimates are also insignificant in the full sample).

This is consistent with some sorting happening between-tracks within schools, with the implication being that students who just make it into a higher ranked school will indeed experience better peers, but that this will be more the case if the measure is the average score of their *school*-level rather than their *track*-level classmates. Panel C in Table 3 confirms this expectation, as it uses the track level average transition grade students experience as a dependent variable, and explores how it changes at the cutoffs that determine access to a higher ranked school. The observed estimates are still highly significant, but as expected are somewhat smaller those observed when peer groups are defined at the school level (panel A).

In order to elaborate on how these first stage results originate, and because it is relevant for later interpretation, we note that while scoring above a cutoff gives students a chance to attend a better school, not all of them avail themselves of the opportunity. Specifically, panels A and B in (Appendix) Figure A.1 summarize information regarding the cutoffs that determine access to fairly selective schools, namely those that separate the best and second-best school (cutoff $z = S - 1$ in the notation of Section 4) in towns that contain at least three schools. Panel A plots transition score cell means of the percentage of students who attend the best 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 in their town. 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.[25]

Multiple factors (e.g. proximity) may account for why not all students take up the chance to go to the best school they are eligible for (an aspect we discuss further below). Whichever ones are actually operative, Figure A.1 underlines that results generated using the first stages in Table 3 should be interpreted in an "intent to treat" spirit.[26]

[25] 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 him or her. This exclusion does not have a qualitative effect on any of our conclusions.

[26] For further reference, panels C and D in Table A.1 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.

5.2. **Baccalaureate outcomes.** A first outcome we consider is simply whether students took the high stakes Baccalaureate exam. Panels C and D in Figure 1 present the graphical evidence for the 2001-2003 cohorts—the ones for which we have Baccalaureate data—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 1-3 refer to the full sample of towns. The coefficient of interest suggests that getting the opportunity to go to a better school resulted in small and (except for the first specification in each sample) 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. In short, the opportunity to enroll in a better school does not seem to affect the likelihood that students take the Baccalaureate test.

A generally similar conclusion emerges among the towns in our survey sample (Figure 2, panels C and D and Table 4, Panel A, columns 4-6) and when we analyze the opportunity to enroll in a better track (Figure 3, panels C and D, and Table 4, panel C).[27] This consistent lack of an effect on 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, particularly in Panel F. The corresponding regression evidence is in Panel B of Table 4, which presents statistically significant gains equivalent to about 0.02 to 0.10 standard deviations, depending on whether one looks at the full or the survey sample.[28]

The bottom line is that students who score above cutoffs giving them access to a better school perform better in the high stakes Baccalaureate exam, and under the assumptions underlying RD designs, this impact can be viewed as causal. A similar conclusion emerges when looking at the towns covered in our specialized survey (Figure 2, panels E and F, and Table 4, Panel B, columns 4-6), and when one considers between-track rather than between school cutoffs (Figure 3, panels E and F, and Table 4, Panel D). 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 (Table 3).

5.3. **Heterogeneity in Baccalaureate outcomes.** The results presented thus far pool all between school and between track cutoffs. We now explore how the Baccalaureate effects vary according to

[27] In contrast to Table 3, Table 4 no longer has columns 7-9. 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.

[28] As stated, if scaled by the peer improvements in Table 3, these estimates are of a magnitude similar to some observed in the literature on peer effects. To further explore this we could run instrumental variable-type specifications where peer quality is instrumented by students' position relative to the cutoff. We refrain from this, however, because as the results below show, many factors other than peer quality change at the cutoffs.

where the cutoffs are located in the transition score distribution. To provide a visual summary of the results, Figure 4 presents evidence on 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. These panels reveal that the discontinuities in average peer quality are of a similar magnitude in both sets of cutoffs. At a first pass level, students seem as interested in attending the best schools as they are in getting out of the worst.

Panels E-H present the graphical evidence on Baccalaureate performance.[29] Specifically, panels E and F suggest that gaining admission to a better 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.

These and other points related to heterogeneity are explored in Table 5. For the sake of space, this table presents only specifications using the optimal IK bandwidths, and focuses on the between-school cutoffs. To illustrate, Panel A refers to the full sample of cutoffs and repeats results presented above. Column 1 presents the first stage (from Table 3, Panel A, Column 3). Within Panel A, column 2 features the track level average transition score as a dependent variable, and columns 3 and 4 a dummy for taking the Baccalaureate exam, and performance on this test, respectively (the latter two repeat the within IK band results in Table 4, Column 3, panels A and B).

Within Table 5, panels B and C refer to the top and bottom tercile of cutoffs. Column 1 show that, as previewed in Figure 4, the first stages are generally similar for the top and bottom terciles. Comparing columns 1 and 2 confirms that as observed in the aggregate sample, when children get the opportunity to enroll in a better school, their track-level peer groups do not improve as much as their school-level peer groups. Turning to the heterogeneity in Baccalaureate effects, Column 3 shows that the lack of an effect on test taking persists in all the subsamples. The coefficients are never statistically significant at the five percent level, and are still generally suggestive of only small impacts on test taking rates.

In contrast, the estimates surrounding Baccalaureate performance (Column 4) generally suggest a positive impact from having the opportunity to attend a higher ranked school. The magnitude of the effect is larger and only significant in the top tercile, but cannot be distinguished from that in the bottom tercile, which is itself statistically insignificant. The bottom line is that gaining access to a better school might be valuable to both high and low-scoring children, but statistical power

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

seems to quickly constrain our ability to explore such heterogeneity. More generally, school effects are difficult to identify, and sample size issues alone might account for some of the heterogeneity in conclusions observed in the literature.

5.4. **Mechanisms and behavioral responses.** To summarize the results thus far, students who receive a transition score above school/track cut-offs are on average more likely to attend better school/tracks. Four years later these students are not more likely to take the Baccalaureate exam, but on average they perform better.

Using our survey of principals, parents, and children, this section turns to investigating the mechanisms that might account for these effects. It is important to underline that although we examine various potential channels, our analysis cannot attribute the effects to one or another mechanism.[30] Rather, we present suggestive evidence on which might be operative and which might not. Nevertheless we believe that our results contribute towards opening up the "black-box" of school effects with useful implications for the literature.

Before presenting results, we make some notes on the structure of all remaining tables and figures. In each table, Panel A aggregates outcomes to the school level; Panel B aggregates the outcomes 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. In Panel A this variable reflects the average proportion of Language teachers that passed this exam at a child's school; in Panel B, the measure is the average proportion of teachers who passed it in her track; and in Panel C the dependent variable indicates whether the actual Language teacher assigned to a particular student passed the exam.[31] Note that the variables from the principal survey only vary at the school level, so Panels B and C are blank in tables covering them.

As above, in all figures the panels on the left hand side present simple means of the outcome variables, while the right hand side panels show fitted values of residuals from regressions of outcomes on a linear trend in transition scores and cutoff fixed effects. 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. In each

[30] This would be hard to do without strong assumptions, given that we have one source of exogenous variation in the treatment (the opportunity to attend a better school) and many variables that might mediate its effect.

[31] 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.

figure, 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.

Our preferred estimates are based on the second specification, that restricted to individuals within one transition grade from the cutoff; we usually use these when discussing the results. Finally, the econometric specifications are the same as used above, although 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), as the two school towns of course contain only one cutoff.[32]

5.4.1. *Teacher characteristics.* Figure 5 and Table 6 describe the impact that scoring above a school cut-off has on the teacher characteristics that students experience. The first three columns of Table 6 show that students above the cutoff are about 13 percent more likely to attend a school which the principal declares has the best teachers in town. The remaining columns describe Language teacher qualifications as provided by their schools based on administrative records.[33]

The dependent variable in columns 4-6 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 5, 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 differences disappear when one considers the actual teachers experienced by students at the margin. This is consistent with teachers sorting both across and within schools in a way clearly associated with student stratification. For example, the pattern of results could reflect the highest certification

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

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

teachers having a preference for—and through seniority gravitating towards—the highest academic ability children. Consistent with this columns 7-9 (Table 6) reveal a similar patterns when teacher quality is measured using years of experience.

However, it is worth noting that differences at the margin persist for some of our measures of teacher quality. Columns 10-12 show that attending a better school decreases the probability of having a "novice" teacher (one with two or fewer years of experience) not just on average, but also on the margin.[34] In short, while teacher sorting may undo some of the teacher-related benefits from getting into a better school, this does not happen along all observable dimensions of teacher quality.

5.4.2. *Parental effort.* Table 7 and Figures 6 and 7 describe the impact that being able to attend a better school has on measures of parental involvement and effort. We focus on measures of parents' participation at school and also on variables intended to capture their interactions with their own children, such as the willingness to help with homework or pay for tutoring services.[35] To illustrate the variation in these dimensions, 11 percent of parents report that during the last year, they volunteered in their child's classroom, school office, or library. About 24 percent of parents report having paid for private tutoring lessons, a common practice in Romania. In addition, roughly 20 percent of parents claim to help with homework on a daily or almost daily basis.

The first six 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 1-3, Panel A) and in parents' self-reports on volunteering (columns 4-6, Panel A). However, the impacts at the track and individual level become small and statistically insignificant (columns 4-6, panels B and C). Figure 6 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. Thus, part of the benefits of attending a better school might be the result of behavior on the part of other parents in the school—for instance, higher average participation might incentivize school principals—but not of changes in behavior by the parents whose child just makes it in. A similar conclusion emerges in columns 7-9, where we use the frequency of parental expenditures on tutoring as the outcome variable.

[34] On average teachers have about five years of experience, with only six percent having less than two years.
[35] Secondary education is free in Romania; hence we do not consider tution expenses as a measure of parental effort.

For a final measure of parents' involvement, Table 7 considers the extent to which they help their children with homework. Columns 10-12 (Panel A) point to no differences at the cutoffs on the average likelihood that parents help on a daily or almost-daily basis. This might not be surprising given that the need for help might depend on children's academic ability. However, the most striking result is shown in Panel C, as well as in Panel F of Figure 7—a *reduction* in average parental help with homework for children just above cutoffs. This suggests that at least in our setting, parents might view their own effort and school quality as substitutes.

Taken together, the results in Table 7 and figures 6-7 suggest that while making it into a better school might allow a student to enjoy positive spillovers from other parents, some of the benefit might be mitigated by behavioral responses on the part of the child's own parents.

5.4.3. *Interactions with peers.* Peer effects have been a focus in the educational literature, with a large number of papers attempting to empirically determine their presence and functional form (see Hoxby and Weingarth (2006) and Lavy, Paserman, and Schlosser (2007) for discussions on channels through which peer effects may operate). However, there is little evidence or consensus on the channels peer effects operate through, and here we try to contribute to this discussion.

Specifically, Section 5.1 made the point that children who score above cutoffs are on average exposed to peers that have higher average transition scores. This first stage is confirmed and expanded upon by columns 1-3 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, which assumes homogeneous treatment effects, these findings would imply positive peer effects for the children who make it into a better school. However, scoring above a cutoff could adversely impact children if their relative ability ranking matters, since this will make 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 cuttoffs actually perceive being lower in their peer ability distribution. Columns 4-6 explore this by running regressions in which children are asked about their rank within their track. The responses ranged from 1 to 7 with higher numbers indicating a better rank in terms of academic ability. Panels A-B in columns 4-6 show that on average children in better schools are more likely to feel they are strong relative to their peers, and as might be expected if they 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 also significant) in this case. This might not be surprising given that Romania's meritocratic student allocation system is well understood by many if not most students.

Finally we explore whether attending a better school has an impact on children's relationship with their peers. We measure this using an index of negative interactions that averages four indicators for whether children report 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 (happened daily) to 5 (did not happen at all); the average of 4.87 across all four indicators suggests that these events are fairly rare. The results in Table 8 (columns 7-9) 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 Figure 8.

In short, these results leave open the possibility that getting into a better school might result in feelings of insecurity or marginalization, which could mitigate the advantages of having access to a better educational environment. It is also possible that the realization of such effects is behind the fact that not all students take up the opportunity to enroll in a more selective school (Figure A.1).

5.4.4. *Other behavioral responses.* Our analysis so far implies that some salient behavioral responses on the part of teachers, parents, and students might diminish the net impact of attending a better school. In this section we present evidence on additional behavioral responses that might play a role in shaping school effects.

First we focus on student homework-related effort. 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 six columns of Table 9, where panel A suggests that students in the better schools do more homework on average, suggesting higher effort in such settings. 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. Finally, columns 7-9 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—this may be yet another reason not all children attend the highest-ranked school they are eligible for.

Next we examine the role of infrastructure availability and use. For this, we asked children to state whether their school has seven physical inputs: Gym, computer laboratory, internet connection, science laboratory, art studio, music room and library. We created an index summarizing this information, one which ranges between 0 and 1 and takes on a value of 1 if students indicate their schools have all these inputs. We calculated a similar index using principals' analogous responses, and the two are highly correlated. As Table 10 shows, we find few if any significant differences in infrastructure availability at all three levels of aggregations and across both principal and student reports. First, columns 1-3 show that in contrast to when they are asked about student quality, teacher quality, and parental participation, principals at better schools do not on average state that their institutions have better facilities. Their responses do suggest small differences in our index (columns 4-6), but children themselves perceive no such differences (columns 7-9). The bottom line is that we find little evidence that going to a more selective school gives children access to better infrastructure. These results are not surprising in Romania, where essentially all schools are public and in principle similarly equipped.

We then investigated the *use* of the mentioned inputs, something that is feasible because we also asked students whether they had actually utilized each of the seven inputs listed above. Specifically, we asked students how often in the last month they had used these inputs, and aggregated their answers to an index that ranges from 0 to 5 (0 if they did not use any of these in the past month and 5 if they used each of them every day). The mean and mode of this index are close to 1.3, perhaps reflecting that these are not inputs that one would expect students to use on a daily basis.

In the event, the results on input use are strikingly different from those on input availability. The final three columns of Table 10 show that on average children who score above a cutoff gain access to environments in which inputs are used more extensively. Interestingly, this result persists at the track level (Panel B) and also in Panel C, suggesting the marginal child also uses infrastructure more, leaving this as a mechanism that may account for some of our positive school effects.

5.5. **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 during the application process, students request high school/track combinations. In this section, we consider the effects of being able to enroll in a better *class* within a given track.

As stated, the Ministry of Education stipulates that after students are admitted to a particular track within a school, they should be allocated to classes containing at most 28 students.[36] In fact the track-specific slot availabilities which schools submit prior to the allocation process need to be multiples of 28. Since the Ministry does not specify how the allocation of students to classes within tracks is to be implemented, each school has the authority to decide its own allocation. Our data suggests that many schools decide to further stratify children into classes also based on their transition scores.[37]

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, 112, etc.), and which were also completely filled at the time of the admission process.[38] 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 28^{th}, (or 56^{th} or 84^{th} student, etc.). As above, we normalized the transition scores relative to the cutoffs, and 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, our analysis focuses on intent to treat estimates of scoring above a particular class level cutoff.[39]

Understanding these effects is interesting for two reasons. First, by looking at children in different classes but in the same track, we are able to make comparisons between children who are exposed to the same curriculum. These results therefore provide a robustness check for our school and track level analysis, in which we cannot control for potential curriculum differences. Second, considering classes allows us to analyze behavioral responses in a setting that even more closely approximates the experimental setting of Duflo, Dupas, and Kremer (forthcoming), where an RD analysis compares students who are on the margin of being assigned to low or high achieving classes.

Table 11 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,

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[36] After being allocated to a particular class, students usually spend the next four years with the same peers, taking all subjects together.

[37] Our conversations with headmasters anecdotally confirm that many schools have this policy.

[38] The number of children who apply for entrance into high school is always smaller than the number of slots published by the Ministry prior to the computerized allocation process. As a result, enrollment in many of the less desirable schools is often less than the number of initial slot offerings.

[39] 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.

the results in panels A and B 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 transition score 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 is not surprising in that there is a lot less variability in the transition scores between classes within school/tracks.

Columns 2-4 (Table 11) 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 0.5 more years of experience.[40]

The remaining columns present all the other outcome variables featured in our previous analysis of the survey data (measures of parental participation, children's interaction with peers, child homework effort, and infrastructure availability and use) with results that are qualitatively similar to those found in Tables 7-10. 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 (as seen in Column 1) are less stark than in the school or track level analysis. Nevertheless, the bottom line is that many of the behavioral responses we observed previously—particularly the sorting of more qualified teachers to better classes—can also be observed across classes within the same track.

6. Conclusion

Whether students would benefit from attending higher-achieving schools is a classic question in education, one which the literature has struggled with mainly because it is difficult to identify situations in which otherwise 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

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

observed around the world. This mechanism yields a large number of RD-based quasi-experiments, enabling us to contribute to the literature with unusually large sample sizes, and with the ability to explore 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, and to use the information collected from students, parents, teachers and principals to begin exploring what mechanisms and behavioral responses might account for any observed effects.

The central reduced form result is that access to a better school has a positive impact on cognitive outcomes when these are measured using achievement in the high-stakes Baccalaureate exam. This finding points to the existence of positive school effects. This has not been a consistent finding in the literature, as some papers—including some which also rely on an RD approach—find little evidence that enrolling in a higher-achievement school or class raises learning.

Our second set of results provides a potential explanation for this, namely, that in at least some settings behavioral responses on the part of teachers, students and parents might operate in ways that reduce the net benefit of getting into a better school. Specifically, we find that teachers sort in response to the stratification of students in a way that determines that a student who just makes it into a more selective school encounters a teachers who is less qualified than the average instructor at the school, and possibly no different than the most qualified teacher at the school he just avoided. Similarly, while children who make it into a better school 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 better schools are exposed to better peers, they also seem to realize they are weaker and to feel marginalized.

While these findings may well reflect institutions that are specific to Romania, we do believe they make a clear case that large scale interventions may result in behavioral responses by actors involved in educational markets. These responses are often held constant in research focused on partial equilibrium interventions, and so our results add credibility to the concern that the experimental approach to studying educational policy may face limitations related to external validity and equilibrium effects (see Banerjee and Duflo (2008) and Deaton (2010) for discussions). More broadly, these findings imply educational interventions might productively be analyzed with reference to how their design affects the behavior of different agents involved in the educational process.

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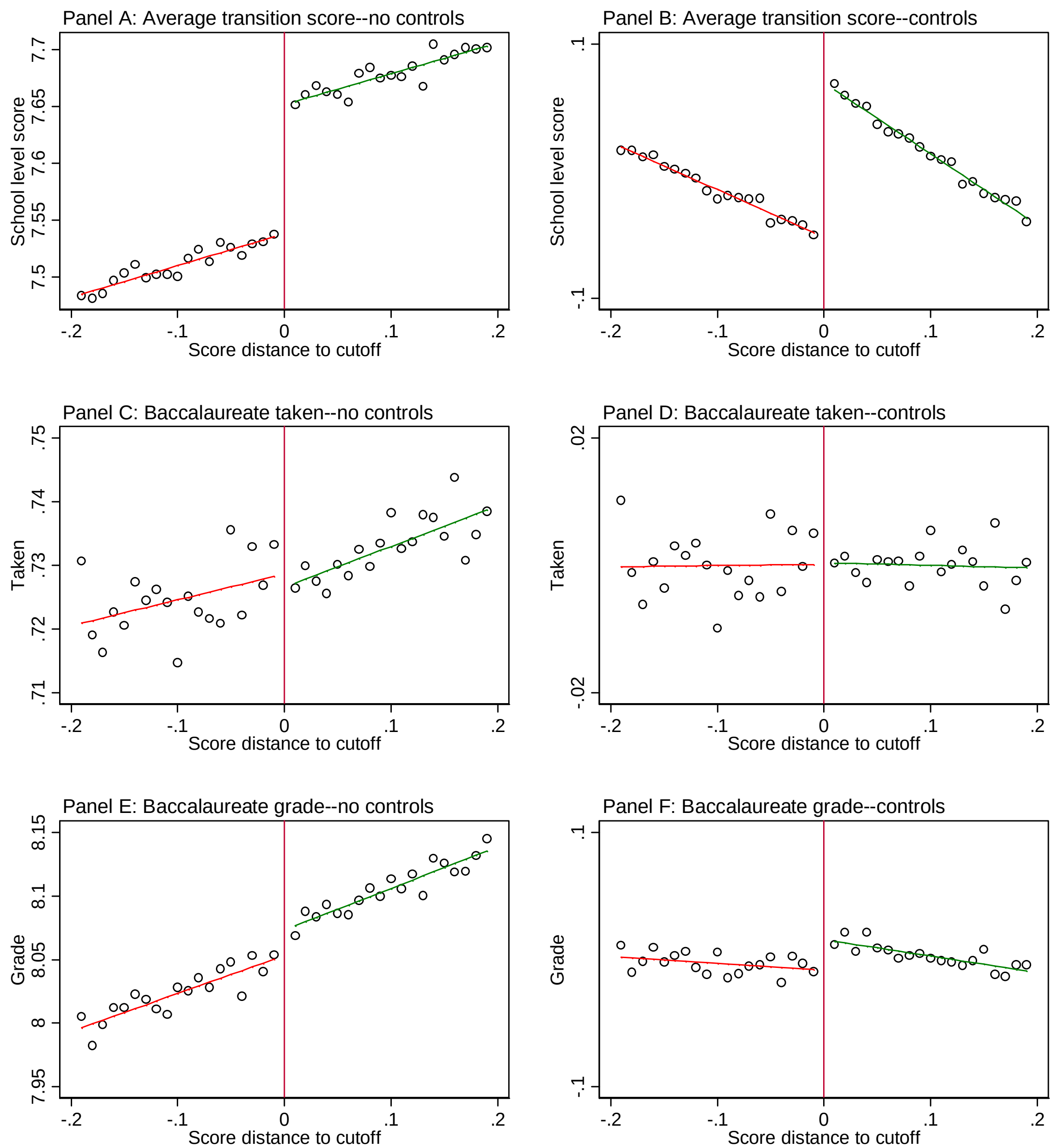
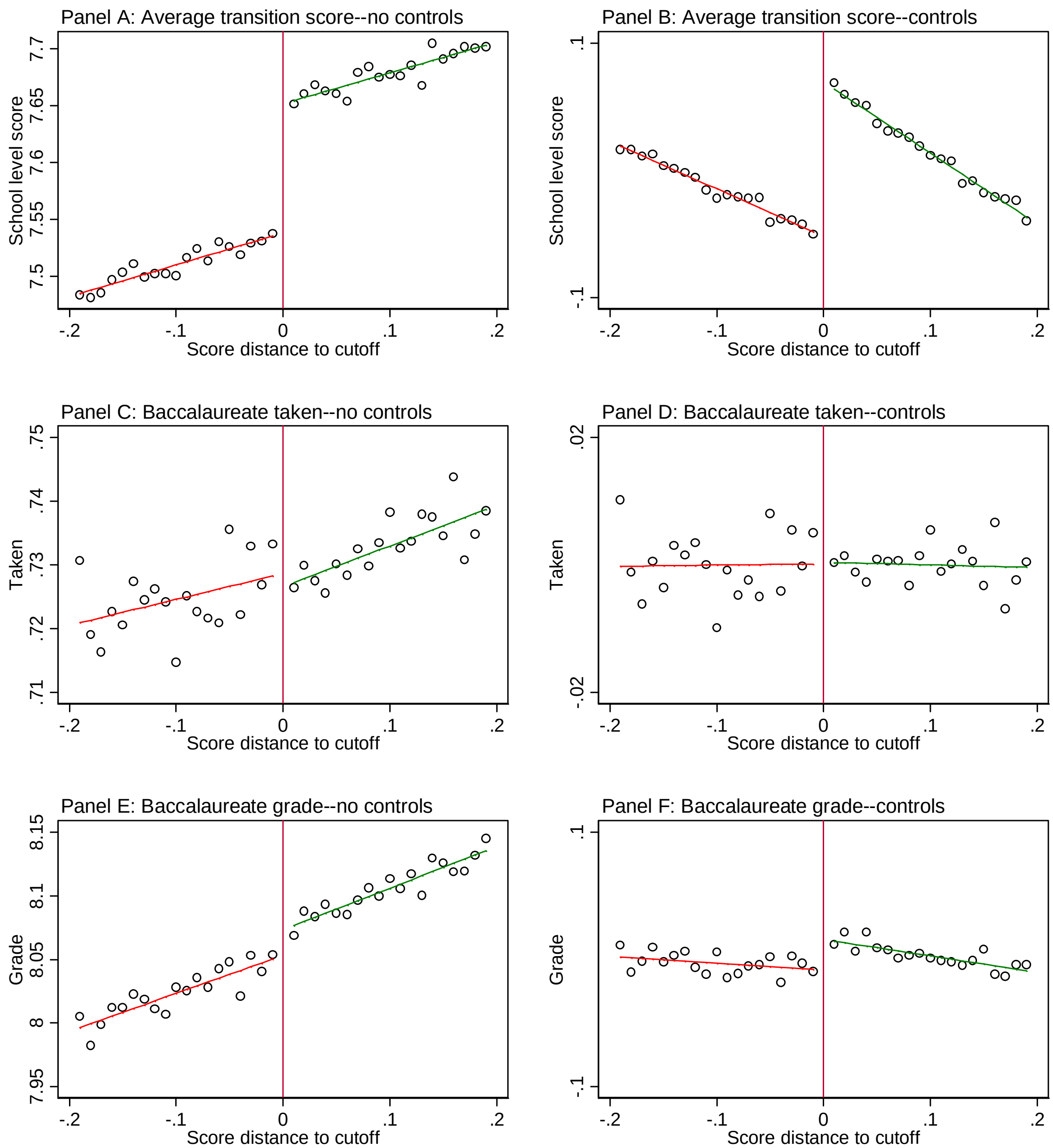
Figure 1: Between-school cutoffs, all towns



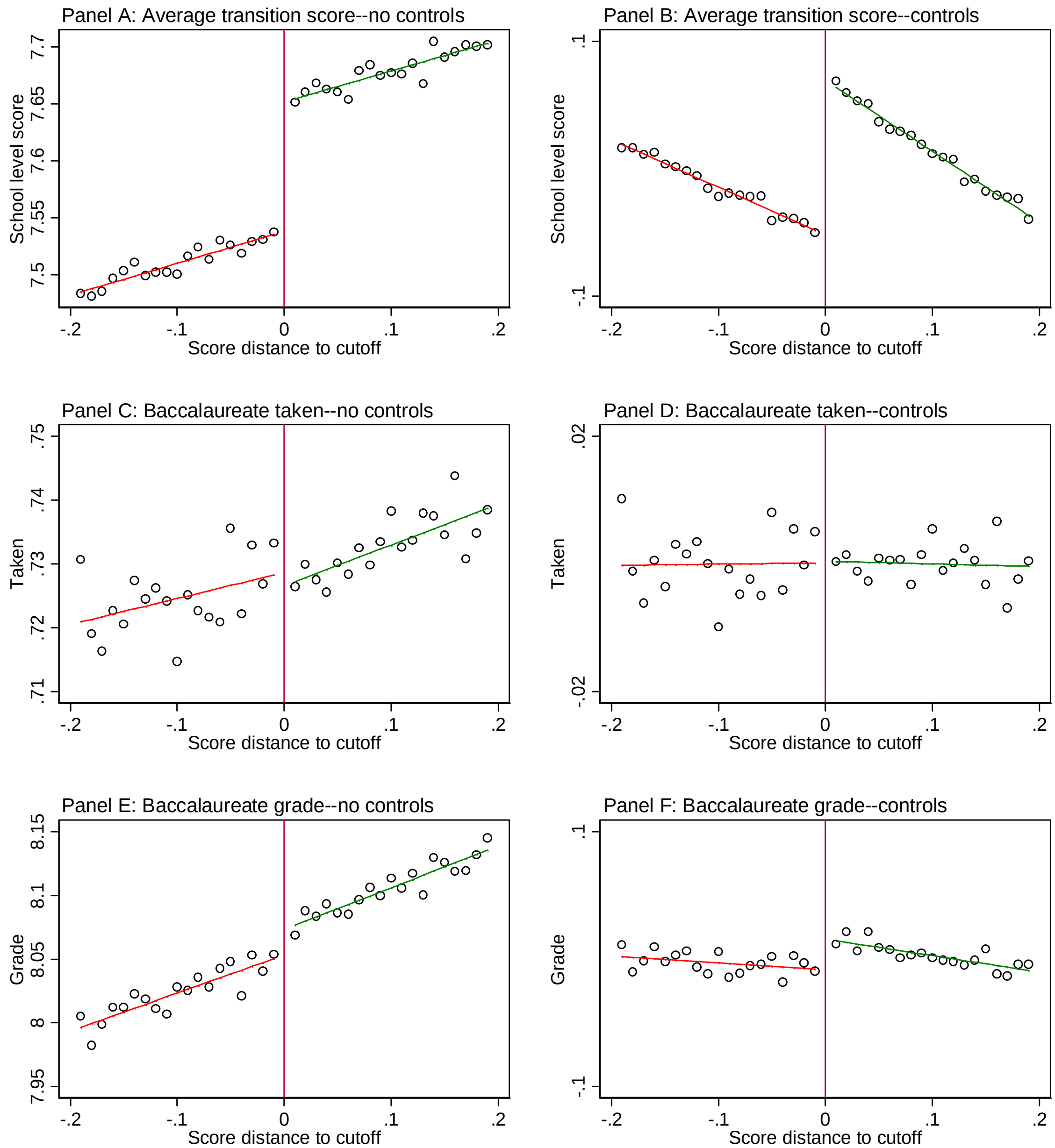
Note: All panels are based on administrative data for the 2001-2003 admission cohorts, and restrict observations to individuals with transition scores within 0.2 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.01 point) transition score cell means of the dependent variable. The right hand side panels plot analogous means of residuals from a regression of the dependent variable on a linear trend in the transition score and 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.

Figure 2: Between-school cutoffs, survey sample towns



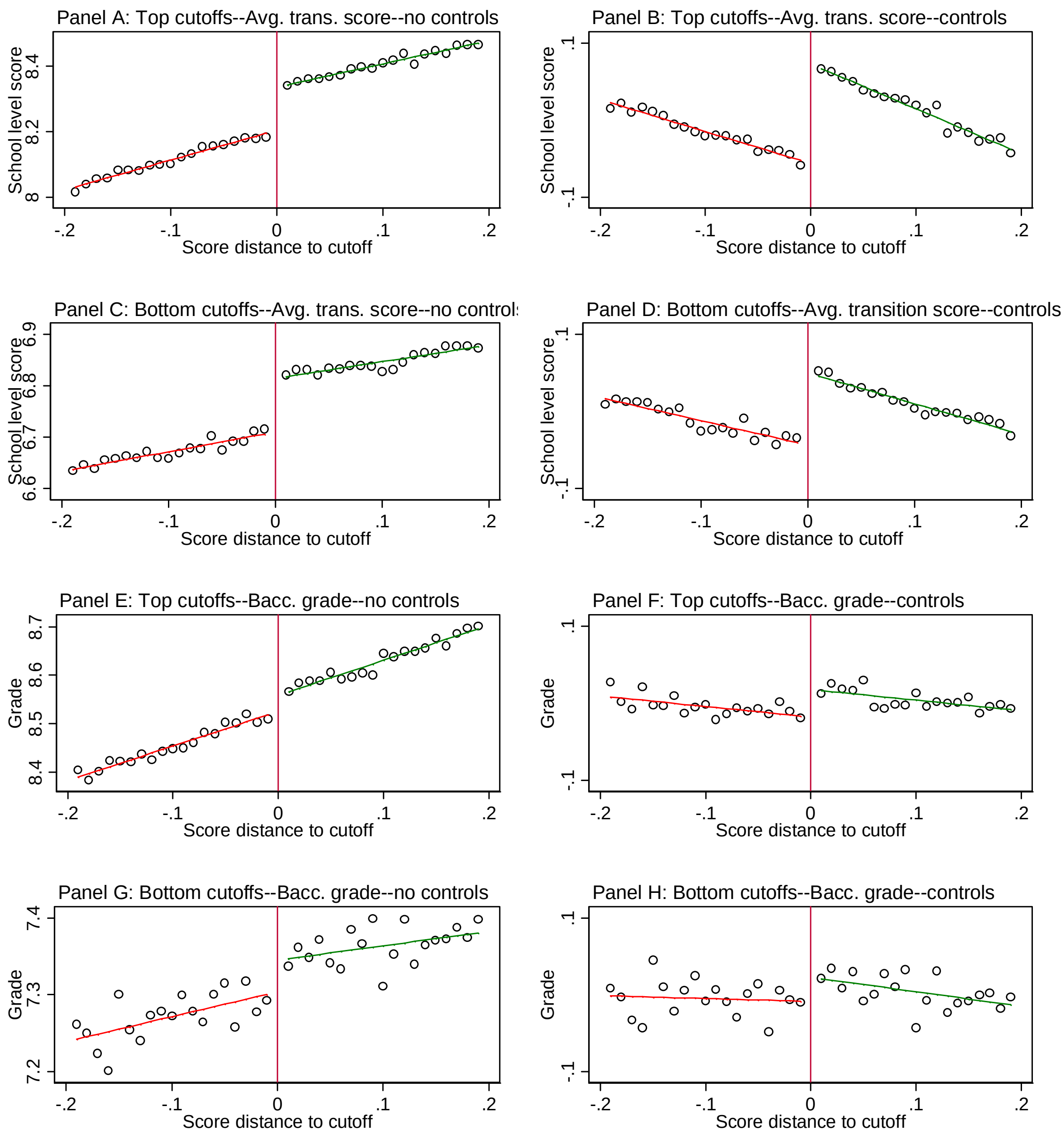
Note: All panels are based on administrative data for the 2001-2003 admission cohorts, and restrict observations to individuals with transition scores within 0.2 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.01 point) transition score cell means of the dependent variable. The right hand side panels plot analogous means of residuals from a regression of the dependent variable on a linear trend in the transition score and a 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.

Figure 3: Between-track cutoffs, all towns



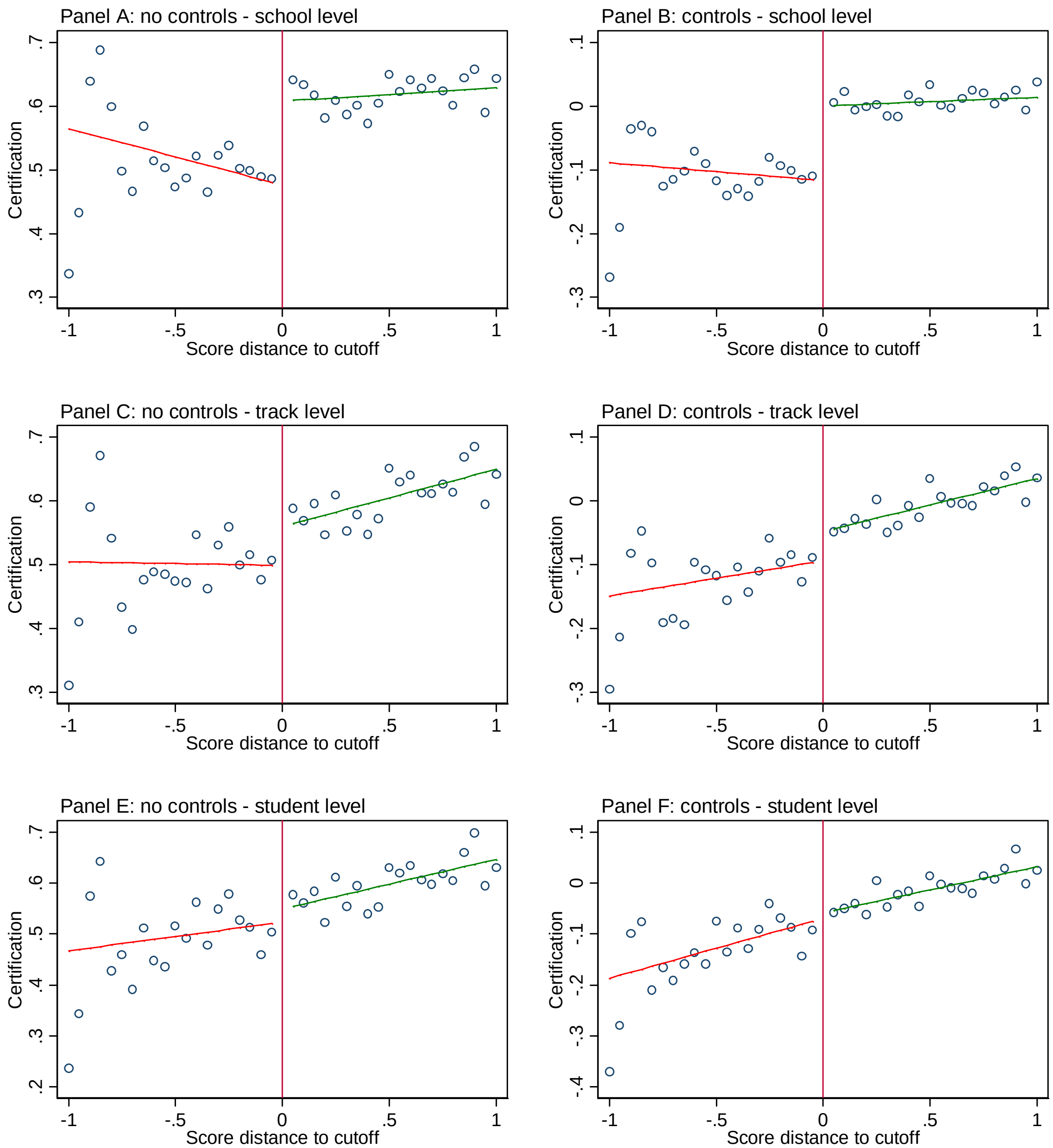
Note: All panels are based on administrative data for the 2001-2003 admission cohorts, and restrict observations to individuals with transition scores within 0.2 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.01 point) transition score cell means of the dependent variable. The right hand side panels plot analogous means of residuals from a regression of the dependent variable on a linear trend in the transition score and 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.

Figure 4: Top and bottom terciles of between-school cutoffs



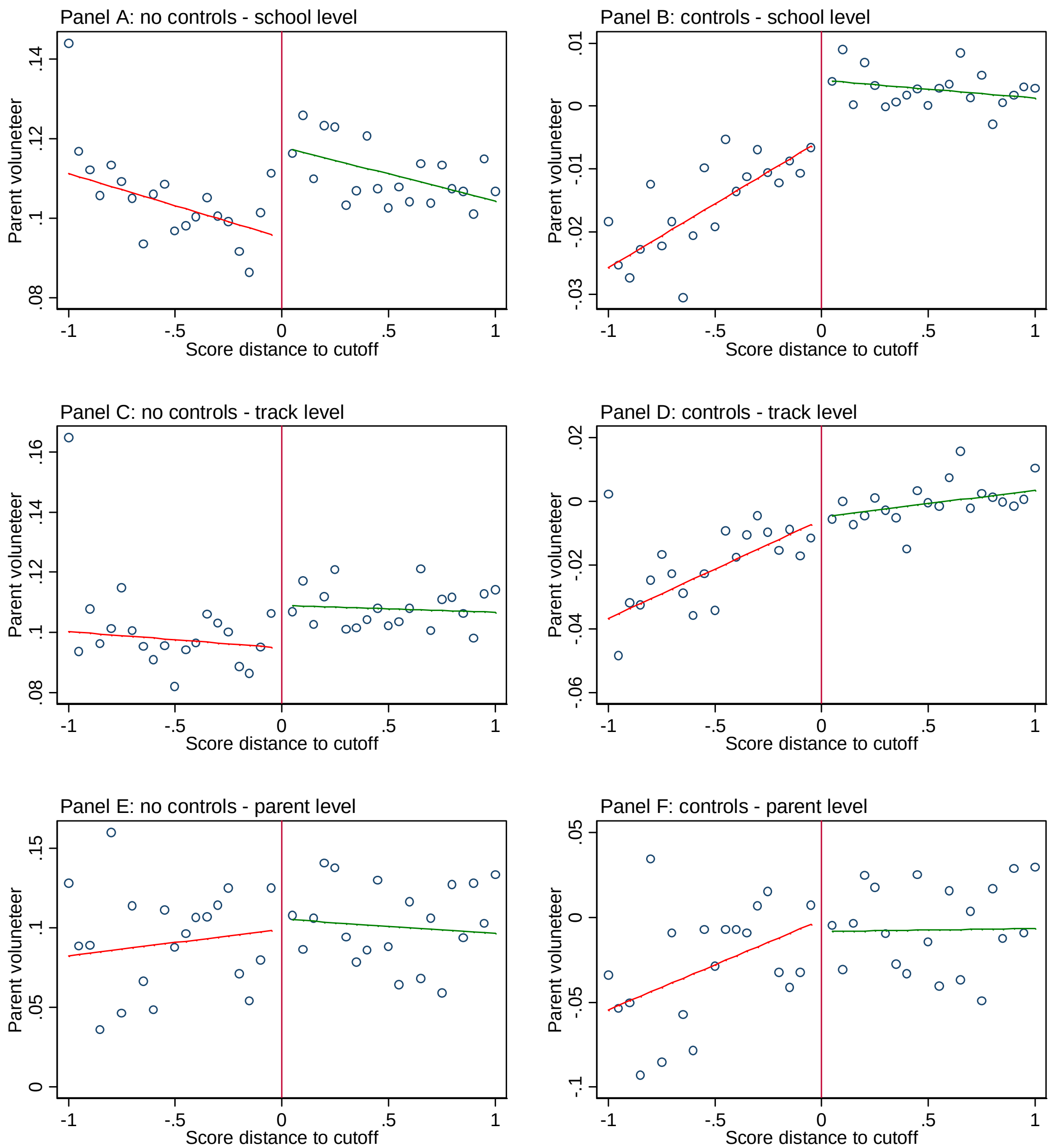
Note: All panels are based on administrative data for the 2001-2003 admission cohorts, and restrict observations to individuals with transition scores within 0.2 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.01 point) transition score cell means of the dependent variable. The right hand side panels plot analogous means of residuals from a regression of the dependent variable on a linear trend in the transition score and 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.

Figure 5: Teacher certification



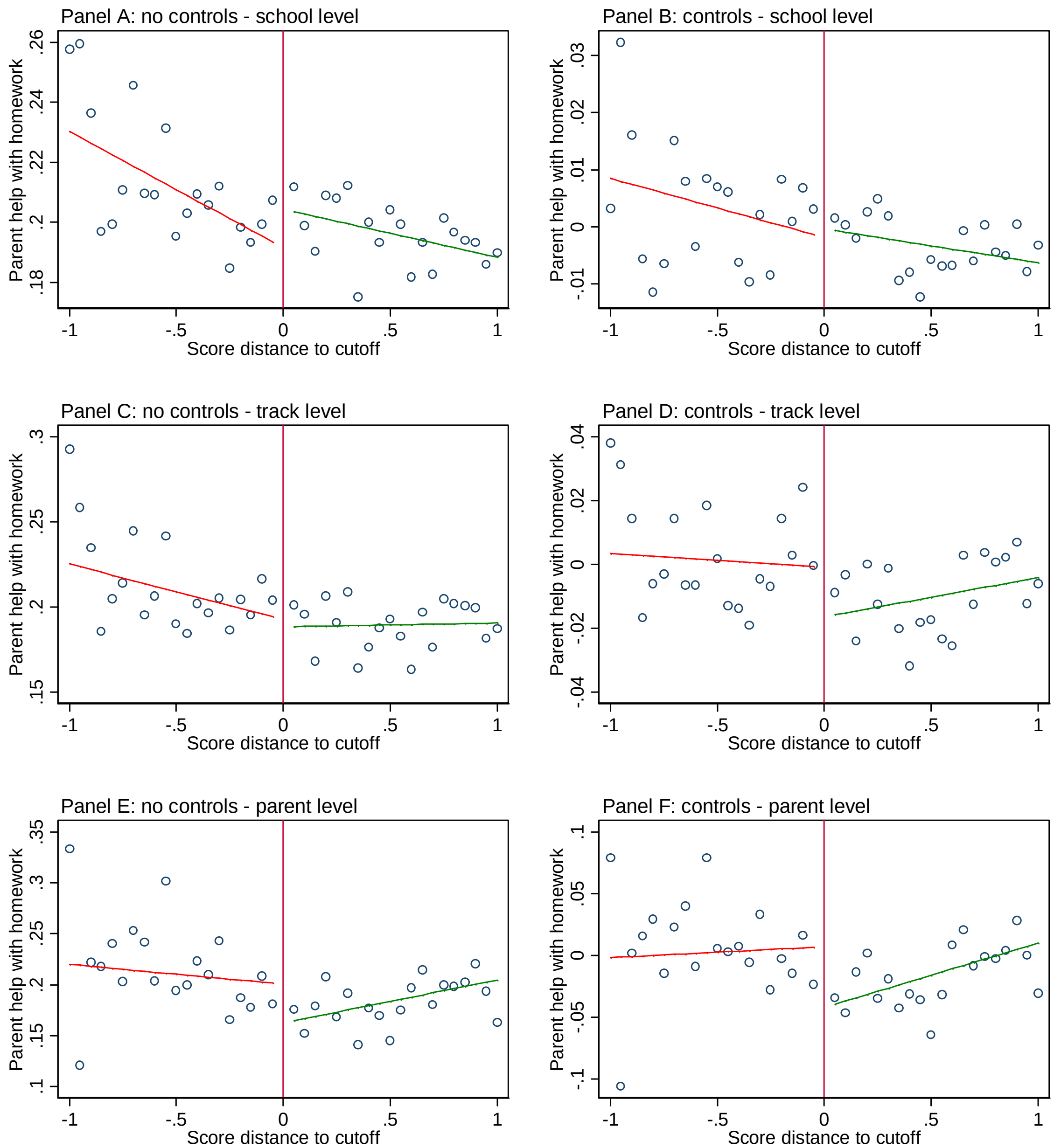
Note: All panels are based on survey data for the 2005-2007 admission cohorts, and restrict observations to individuals with transition scores within 1 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.05 point) transition score cell means of an indicator for whether 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 a linear trend in the transition score and 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.

Figure 6: Parental volunteering at school



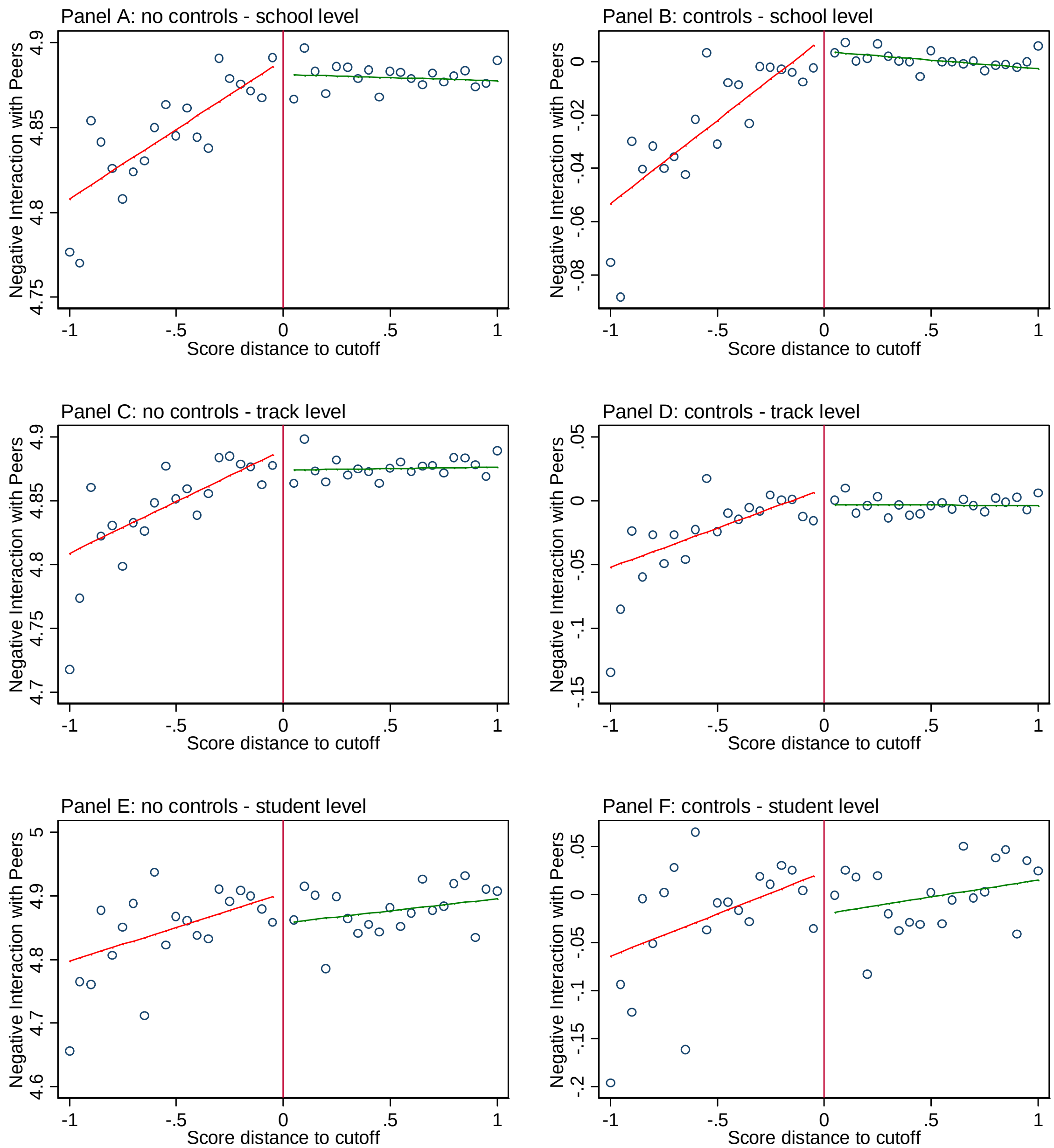
Note: All panels are based on survey data for the 2005-2007 admission cohorts, and restrict observations to individuals with transition scores within 1 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.05 point) transition score cell means of an indicator for whether parents have volunteered at school in the past year. The right hand side panels plot analogous means of residuals from a regression of the dependent variable on a linear trend in the transition score and 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.

Figure 7: Parental help with homework



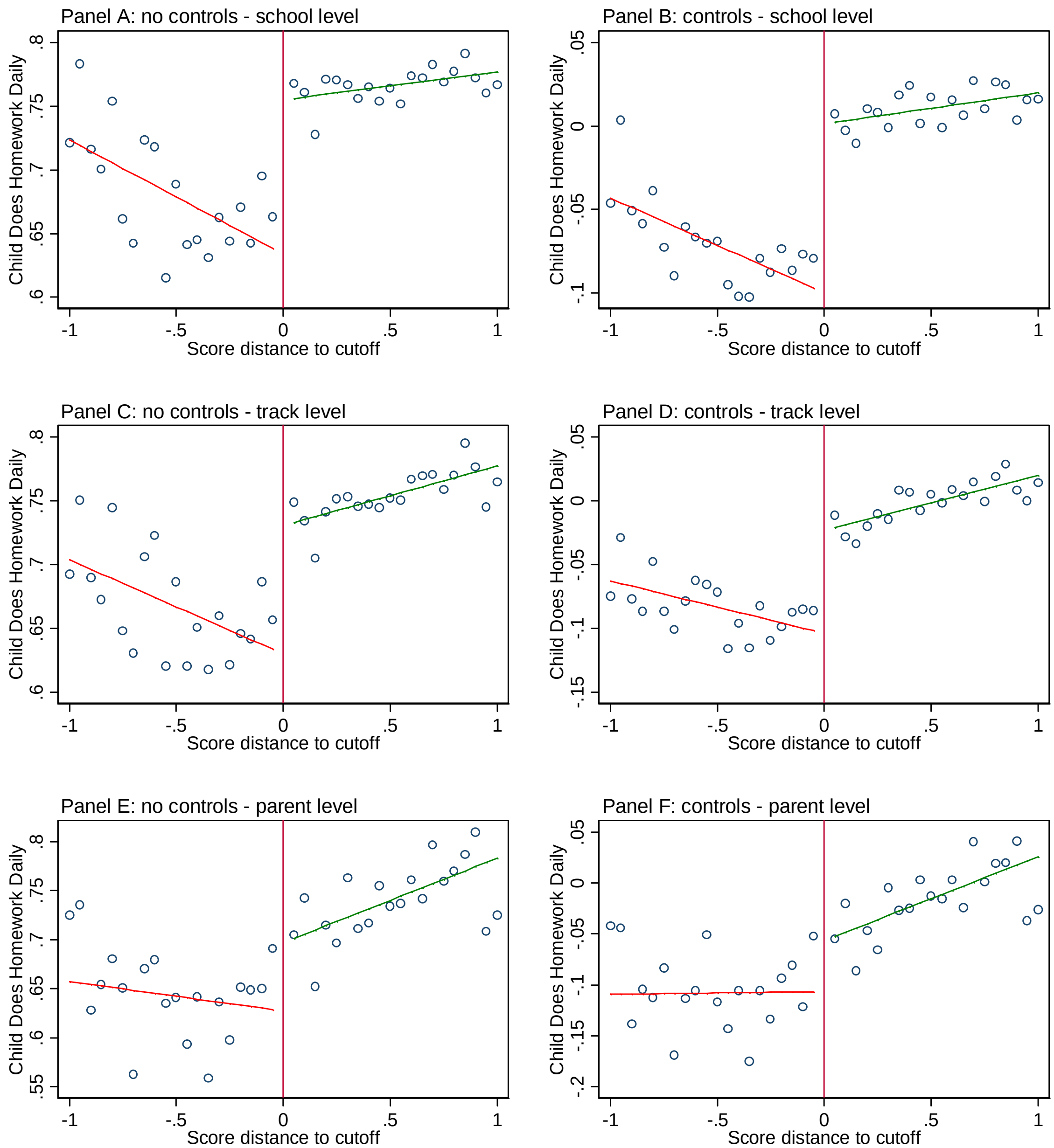
Note: All panels are based on survey data for the 2005-2007 admission cohorts, and restrict observations to individuals with transition scores within 1 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.05 point) transition score cell means of an indicator for whether, in the month before the survey, parents declare helping their children with homework on a daily or almost daily basis. The right hand side panels plot analogous means of residuals from a regression of the dependent variable on a linear trend in the transition score and 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.

Figure 8: Negative interaction with peers at school



Note: All panels are based on survey data for the 2005-2007 admission cohorts, and restrict observations to individuals with transition scores within 1 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.05 point) transition score cell means of an index of children's negative interactions with peers at school (Section 4). The right hand side panels plot analogous means of residuals from a regression of the dependent variable on a linear trend in the transition score and 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.

Figure 9: Children's homework effort (parental report)



Note: All panels are based on survey data for the 2005-2007 admission cohorts, and restrict observations to individuals with transition scores within 1 points of a cutoff (normalized to zero in all cases). The left hand side panels plot (0.05 point) transition score cell means of an indicator for whether, based on parental reports, children do homework on a daily or almost daily basis. The right hand side panels plot analogous means of residuals from a regression of the dependent variable on a linear trend in the transition score and 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.

Table 1: Descriptive statistics, administrative data

	High school admission cohort														
	2001					2002					2003				
	Mean	S.D.	Min	Max	N	Mean	S.D.	Min	Max	N	Mean	S.D.	Min	Max	N
Panel A.1: Individual level															
Transition grade	7.68	0.80	5.90	9.52	107,812	7.87	0.75	6.03	9.41	110,912	7.96	0.97	5.13	10	115,413
Baccalaureate taken	0.847	0.360	0	1	107,812	0.822	0.383	0	1	110,912	0.809	0.393	0	1	115,413
Baccalaureate grade	8.31	0.93	5.19	10.00	87,411	8.28	0.95	5.18	10.00	85,946	8.51	0.88	5.27	10.00	84,110
Panel A.2: Track level															
9th grade enrollment	62.6	49.0	1	276	1,722	66.6	50.6	1	280	1,665	71.5	53.3	1	329	1,615
Panel A.3: School level															
9th grade enrollment	135.3	61.4	2	352	797	140.6	63.1	9	420	789	144.1	69.2	3	432	801
Number of tracks	2.2	1.2	1	5	797	2.1	1.2	1	5	789	2.0	1.2	1	5	801
Panel A.4: Town level															
9th grade enrollment	804.6	849.6	62	3,819	134	827.7	875.5	60	4,088	134	854.9	919.5	45	4,169	135
No. of schools	5.9	6.0	2	29	134	5.9	5.8	2	28	134	5.9	5.9	2	29	135
No. of tracks	12.9	11.9	2	58	134	12.4	11.4	2	56	134	12.0	10.9	2	52	135
	2005					2006					2007				
	Mean	S.D.	Min	Max	N	Mean	S.D.	Min	Max	N	Mean	S.D.	Min	Max	N
Panel B.1: Individual level															
Transition grade	8.14	0.80	6.50	9.64	105,737	8.26	0.76	6.41	9.67	98,647	8.39	0.77	3.68	9.74	97,069
Baccalaureate taken	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
Baccalaureate grade	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
Panel B.2: Track level															
9th grade enrollment	62.8	47.8	1	308	1,684	60.4	44.3	1	280	1,634	59.1	42.4	3	252	1,644
Panel B.3: School level															
9th grade enrollment	129.4	70.6	2	420	817	120.3	64.8	1	336	820	116.8	62.2	3	308	831
Number of tracks	2.1	1.2	1	5	817	2.0	1.2	1	5	820	2.0	1.2	1	5	831
Panel B.4: Town level															
9th grade enrollment	766.2	839.8	45	3,767	138	720.1	764.6	46	3,650	137	683.5	728.5	57	3,462	142
No. of schools	5.9	6.0	2	32	138	6.0	6.2	2	34	137	5.9	6.0	2	33	142
No. of tracks	12.2	11.4	2	58	138	11.9	11.2	2	60	137	11.6	10.9	2	59	142

Note: This table describes data covering the universe of Romanian towns with two exceptions (discussed in Section 3): i) towns that make up Bucharest, and ii) towns that contain a single school. Panel A.1 presents student level statistics for the 2001-2003 cohorts. 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 2005-2007 cohorts.

Table 2: Descriptive statistics, survey data

	Mean	Std. Dev.	N
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)			
Infrastructure availability index[1]	0.826	0.157	11,376
Infrastructure use index[2]	1.312	0.527	11,376
Relative rank among peers (1-7, with 7 better ranked)	4.745	1.300	11,798
Index of negative interactions with peers[3]	4.879	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	4.873	2.944	11,169
Proportion of teachers who are "novices" (less than 2 years of experience)	0.061	0.238	11,169

Notes: This table describes a specialized survey collected in 135 schools among 59 towns for the 2005-2007 admissions cohorts (see Section 3).

[1] Index based on whether children indicate seven physical inputs (gym, computer lab, internet connection, science lab, art studio, music lab, and library) are available at school. The index ranges from 0 to 1, taking on a value of 1 if students indicate all inputs are available.

[2] Index based on how often in the last month students indicate they have used each of the seven inputs listed in note 1. The index ranges from 0 to 7.

[3] 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 (happened daily) and 5 (did not happen in the past month).

Table 3: First stages

Dependent variable:	Administrative data						Survey data		
	All towns			Survey towns					
	Full sample	Within 1 point of cutoff	Within IK bound	Survey towns sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: **School**-level avg. transition grade - **2001-2003** cohorts - **between school** cutoffs									
1{Trans. grade≥Cutoff}	0.094***	0.107***	0.115***	0.454***	0.446***	0.447***			
	(0.001)	(0.001)	(0.001)	(0.005)	(0.007)	(0.007)			
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes			
R^2	0.817	0.790	0.792	0.729	0.754	0.754			
N	3,609,572	1,857,376	1,160,458	64,052	39,363	39,104			
Panel B: **Track**-level avg. transition grade - **2001-2003** cohorts - **between track** cutoffs									
1{Trans. grade≥Cutoff}	0.067***	0.063***	0.065***	0.203***	0.188***	0.187***			
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)			
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes			
R^2	0.873	0.857	0.857	0.779	0.792	0.793			
N	8,802,699	4,845,812	4,400,772	265,896	172,656	154,366			
Panel C: **Track**-level avg. transition grade - **2001-2003** cohorts - **between school** cutoffs									
1{Trans. grade≥Cutoff}	0.068***	0.073***	0.082***	0.283***	0.266***	0.266***			
	(0.001)	(0.001)	(0.001)	(0.005)	(0.006)	(0.006)			
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes			
R^2	0.870	0.849	0.852	0.783	0.811	0.812			
N	3,609,572	1,857,376	1,160,458	64,052	39,363	39,104			
Panel D: **School**-level avg. transition grade - **2005-2007** cohorts - **between school** cutoffs									
1{Trans. grade≥Cutoff}	0.107***	0.107***	0.106***	0.435***	0.414***	0.438***	0.516***	0.477***	0.477***
	(0.001)	(0.001)	(0.001)	(0.006)	(0.007)	(0.009)	(0.014)	(0.018)	(0.018)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.830	0.808	0.811	0.669	0.700	0.691	0.700	0.700	0.700
N	3,302,846	1,611,388	1,822,434	62,503	34,855	22,485	11,838	6,559	6,382

Note: All regressions are clustered at the student level and include cutoff fixed effects. 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 (normalized to zero).

Table 4: Effects on Baccalaureate taking and performance

Dependent variable:	All towns			Survey towns		
	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Bacc. taken dummy - 2001-2003 cohorts - between school cutoffs						
1{Trans. grade≥Cutoff}	0.001 ***	0.000	0.001	0.021 ***	0.012	0.012
	(0.001)	(0.001)	(0.001)	(0.007)	(0.009)	(0.009)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.056	0.054	0.059	0.086	0.081	0.080
N	3,609,572	1,857,376	1,160,458	64,052	39,363	39,104
Panel B: Bacc. grade - 2001-2003 cohorts - between school cutoffs						
1{Trans. grade≥Cutoff}	0.037 ***	0.018 ***	0.015 ***	0.144 ***	0.105 ***	0.104 ***
	(0.001)	(0.002)	(0.003)	(0.012)	(0.015)	(0.015)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.567	0.483	0.472	0.566	0.494	0.494
N	2,546,208	1,256,038	840,750	44,115	25,393	25,201
Panel C: Bacc. taken dummy - 2001-2003 cohorts - between track cutoffs						
1{Trans. grade≥Cutoff}	-0.005 ***	-0.001	-0.001	0.007 *	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.060	0.057	0.059	0.091	0.084	0.086
N	8,802,699	4,845,812	4,400,772	265,896	172,656	154,366
Panel D: Bacc. grade - 2001-2003 cohorts - between track cutoffs						
1{Trans. grade≥Cutoff}	0.042 ***	0.011 ***	0.015 ***	0.062 ***	0.036 ***	0.037 ***
	(0.001)	(0.001)	(0.001)	(0.006)	(0.007)	(0.007)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.567	0.490	0.506	0.559	0.495	0.498
N	6,165,081	3,371,726	3,923,073	183,321	117,179	122,320

Note: All regressions are clustered at the student level and include cutoff fixed effects. 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 (normalized to zero).

Table 5: Heterogeneity in Baccalaureate effects (all specifications within IK bands)

	School level average transition score (1)	Track-level average transition score (2)	Baccalaureate taken (3)	Baccalaureate grade (4)
Panel A: Full sample				
1{Grade≥Cutoff}	0.115 ***	0.082 ***	0.001	0.015 ***
	(0.001)	(0.001)	(0.001)	(0.003)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.792	0.852	0.039	0.472
N	1,160,458	1,160,458	1,160,458	840,750
Panel B: Top tercile				
1{Grade≥Cutoff}	0.135 ***	0.087 ***	0.002	0.025 ***
	(0.002)	(0.002)	(0.003)	(0.005)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.615	0.709	0.267	0.333
N	329,062	329,062	329,062	277,183
Panel C: Bottom tercile				
1{Grade≥Cutoff}	0.096 ***	0.084 ***	-0.004	0.011
	(0.003)	(0.002)	(0.004)	(0.009)
Linear spline	Yes	Yes	Yes	Yes
R^2	0.398	0.471	0.052	0.197
N	220,613	220,613	220,613	131,734

Note: All regressions are clustered at the student level and include cutoff fixed effects. 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 (normalized to zero). For comparison, panel A replicates the I-K bound specifications in tables 3 and 4. Panels B and C present analogous specifications for the top and bottom tercile of cutoffs, respectively.

Table 6: Teachers

Dependent variable:	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)		
	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: School level												
1{Trans. grade≥Cutoff}	0.148 ***	0.128 ***	0.112 ***	0.079 ***	0.095 ***	0.087 ***	0.208 ***	0.275 ***	0.217 ***	-0.015 **	0.037 ***	0.033 ***
	(0.013)	(0.014)	(0.013)	(0.011)	(0.012)	(0.012)	(0.071)	(0.077)	(0.077)	(0.006)	(0.006)	(0.006)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.380	0.490	0.470	0.570	0.610	0.600	0.550	0.570	0.550	0.510	0.600	0.590
N	11,430	6,289	7,379	11,084	6,065	6,736	11,084	6,065	6,962	11,084	6,065	6,128
Panel B: Track level												
1{Trans. grade≥Cutoff}				0.048 ***	0.026	0.037 **	0.173 **	0.021	0.078	-0.018 **	-0.036 ***	0.027 ***
				(0.014)	(0.016)	(0.015)	(0.088)	(0.104)	(0.095)	(0.008)	(0.009)	(0.008)
Linear spline				Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2				0.410	0.480	0.450	0.380	0.410	0.370	0.340	0.450	0.390
N				11,084	6,065	7,977	11,084	6,065	8,978	11,084	6,065	8,305
Panel C: Student/parent level												
1{Trans. grade≥Cutoff}				0.026	-0.005	-0.011	0.030	-0.112	-0.160	-0.012	-0.036	-0.026 **
				(0.017)	(0.021)	(0.021)	(0.106)	(0.129)	(0.118)	(0.010)	(0.011)	(0.010)
Linear spline				Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2				0.320	0.370	0.370	0.300	0.320	0.300	0.270	0.360	0.340
N				11,084	6,065	5,955	11,084	6,065	8,120	11,084	6,065	7,242

Note: All regressions are clustered at the student level and include cutoff fixed effects. 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 (normalized to zero). Panel A presents outcome variables that are aggegated 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.

Table 7: Parents

Dependent variable:	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		
	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: School level												
1{Trans. grade≥Cutoff}	0.129 ***	0.125 ***	0.130 ***	0.011 ***	0.012 ***	0.012 ***	0.064 ***	0.063 ***	0.064 ***	-0.003	0.000	0.000
	(0.016)	(0.019)	(0.020)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.460	0.480	0.490	0.730	0.710	0.700	0.770	0.750	0.750	0.720	0.740	0.730
N	11,046	6,138	5,557	11,776	6,522	7,142	11,757	6,501	7,255	11,723	6,488	9,674
Panel B: Track level												
1{Trans. grade≥Cutoff}				0.009 ***	0.005	0.006 *	0.022 ***	0.009	0.020 ***	-0.015 ***	0.026 ***	0.022 ***
				(0.003)	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.005)	(0.006)	(0.006)
Linear spline				Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2				0.470	0.490	0.470	0.590	0.550	0.560	0.480	0.500	0.500
N				11,776	6,522	7,606	11,757	6,501	5,363	11,723	6,488	7,141
Panel C: Student/parent level												
1{Trans. grade≥Cutoff}				-0.001	-0.002	0.007	-0.009	-0.003	-0.003	-0.021	-0.043 **	-0.033 **
				(0.011)	(0.015)	(0.014)	(0.014)	(0.018)	(0.017)	(0.015)	(0.019)	(0.017)
Linear spline				Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2				0.070	0.070	0.070	0.170	0.130	0.130	0.080	0.090	0.090
N				11,776	6,522	7,905	11,757	6,501	6,771	11,723	6,488	8,840

Note: All regressions are clustered at the student level and include cutoff fixed effects. 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 (normalized to zero). Panel A presents outcome variables that are aggegated at the school level. Panel B presents outcome variables that are aggegated at the track level. Panel C presents outcome variables that are at the child or parent level.

Table 8: Peers

Dependent variable:	**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**		
	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School level									
1{Trans. grade≥Cutoff}	0.342 ***	0.336 ***	0.335 ***	0.172 ***	0.172 ***	0.159 ***	-0.002	-0.004	-0.005
	(0.014)	(0.018)	(0.017)	(0.010)	(0.012)	(0.011)	(0.003)	(0.004)	(0.004)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.450	0.470	0.470	0.670	0.690	0.680	0.660	0.690	0.690
N	11,732	6,512	6,736	11,708	6,478	8,264	11,745	6,500	6,612
Panel B: Track level									
1{Trans. grade≥Cutoff}				0.117 ***	0.090 ***	0.100 ***	-0.010 **	-0.012 **	0.014 ***
				(0.015)	(0.018)	(0.016)	(0.004)	(0.005)	(0.005)
Linear spline				Yes	Yes	Yes	Yes	Yes	Yes
R^2				0.500	0.520	0.520	0.450	0.480	0.490
N				11,708	6,478	9,666	11,745	6,500	8,162
Panel C: Student/parent level									
1{Trans. grade≥Cutoff}				-0.214 ***	-0.134 ***	-0.126 ***	-0.027 *	-0.045 **	-0.036 **
				(0.046)	(0.059)	(0.051)	(0.014)	(0.019)	(0.017)
Linear spline				Yes	Yes	Yes	Yes	Yes	Yes
R^2				0.170	0.120	0.130	0.070	0.080	0.080
N				11,708	6,478	9,009	11,745	6,500	8,289

Note: All regressions are clustered at the student level and include cutoff fixed effects. 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 (normalized to zero). Panel A presents outcome variables that are aggegated at the school level. Panel B presents outcome variables that are aggegated at the track level. Panel C presents outcome variables that are at the child or parent level.

Table 9: Child homework

Dependent variable:	Child does homework every day or almost every day (child report)			Child does homework every day or almost every day (parent report)			Index: Child perceives homework to be easy		
	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School level									
1{Trans. grade≥Cutoff}	0.078 ***	0.072 ***	0.068 ***	0.101 ***	0.091 ***	0.092 ***	0.059 ***	0.038 ***	0.027 ***
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.009)	(0.011)	(0.010)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.730	0.770	0.750	0.750	0.760	0.760	0.730	0.760	0.750
N	11,815	6,544	8,262	11,689	6,471	6,584	9,556	5,468	6,557
Panel B: Track level									
1{Trans. grade≥Cutoff}	0.067 ***	0.050 ***	0.056 ***	0.093 ***	0.075 ***	0.075 ***	0.045 ***	0.011	0.011
	(0.006)	(0.008)	(0.007)	(0.006)	(0.007)	(0.007)	(0.014)	(0.017)	(0.016)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.570	0.610	0.600	0.600	0.640	0.630	0.510	0.560	0.550
N	11,815	6,544	8,565	11,689	6,471	6,793	9,556	5,468	6,478
Panel C: Student/parent level									
1{Trans. grade≥Cutoff}	0.020	0.024	0.019	0.081 ***	0.051 **	0.046 **	-0.023	-0.021	-0.020
	(0.018)	(0.023)	(0.019)	(0.017)	(0.021)	(0.020)	(0.042)	(0.053)	(0.047)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.160	0.180	0.170	0.170	0.200	0.190	0.120	0.140	0.130
N	11,815	6,544	9,999	11,689	6,471	7,177	9,556	5,468	7,042

Note: All regressions are clustered at the student level and include cutoff fixed effects. 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 (normalized to zero). Panel A presents outcome variables that are aggegated at the school level. Panel B presents outcome variables that are aggegated at the track level. Panel C presents outcome variables that are at the child or parent level.

Table 10: Infrastructure

Dependent variable:	School perceived to have the best facilities in town (by principals)			School input availability index (as perceived by principals)			School input availability index (as perceived by students)			School input ***use*** index (as perceived by students)		
	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: School level												
1{Trans. grade≥Cutoff}	0.007	-0.027	-0.030 *	0.017 ***	0.014 ***	0.015 ***	0.001	-0.001	0.003	0.162 ***	0.132 ***	0.125 ***
	(0.015)	(0.017)	(0.017)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.008)	(0.009)	(0.010)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.320	0.340	0.340	0.520	0.530	0.540	0.710	0.680	0.680	0.660	0.670	0.670
N	11,732	6,512	6,282	11,833	6,555	7,027	11,288	6,205	7,867	11,288	6,205	5,608
Panel B: Track level												
1{Trans. grade≥Cutoff}							0.001	0.001	0.005	0.117 ***	0.101 ***	0.099 ***
							(0.030)	(0.004)	(0.004)	(0.010)	(0.012)	(0.012)
Linear spline							Yes	Yes	Yes	Yes	Yes	Yes
R^2							0.650	0.620	0.630	0.510	0.520	0.520
N							11,288	6,205	7,663	11,288	6,205	6,032
Panel C: Student/parent level												
1{Trans. grade≥Cutoff}							0.000	-0.003	0.003	0.122 ***	0.121 ***	0.110 ***
							(0.006)	(0.007)	(0.006)	(0.019)	(0.025)	(0.025)
Linear spline							Yes	Yes	Yes	Yes	Yes	Yes
R^2							0.300	0.290	0.290	0.210	0.200	0.200
N							11,288	6,205	8,967	11,288	6,205	6,470

Note: All regressions are clustered at the student level and include cutoff fixed effects. 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 (normalized to zero). Panel A presents outcome variables that are aggegated at the school level. Panel B presents outcome variables that are aggegated at the track level. Panel C presents outcome variables that are at the child or parent level.

Table 11: Class effects (all specifications within 1 point of cutoffs)

	Category						
	First Stage	Teachers			Parents		
Dependent variable:	Class level transition socre	Language teacher has the certification certification	Language teacher experience (in years)	Language teacher has two or less years experience	Parents have volunteered in past year	Parents have paid for tutoring services	Parents help child with homework often
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Class level							
1{Trans. grade≥Cutoff}	0.127***	0.046***	0.449***	-0.006	0.006	0.026***	0.004
	(0.010)	(0.013)	(0.080)	(0.006)	(0.004)	(0.005)	(0.004)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.890	0.560	0.590	0.530	0.560	0.740	0.590
N	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
N					5,373	5,357	5,360

	Peers		Child			Infrastructure	
Dependent variable:	Child's perception of his/her rank in track	Child's negative interaction with peers	Child does homework every day or almost every (child report)	Child does homework every day or almost every (parent report)	Index: Child perceives homework to be easy	School input availability index (as perceived by students)	School input availability index (as perceived by principals)
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Class level							
1{Trans. grade≥Cutoff}	0.044***	-0.007	0.021***	0.017***	0.032**	-0.003*	0.002
	(0.015)	(0.004)	(0.006)	(0.005)	(0.015)	(0.002)	(0.007)
Linear spline	Yes	Yes	Yes	Yes	Yes		Yes
R^2	0.620	0.520	0.720	0.700	0.620	0.870	0.820
N	5,342	5,350	5,385	5,355	4,443	5,194	5,194
Panel B: Student/parent level							
1{Trans. grade≥Cutoff}	-0.142***	-0.018	0.003	0.010	-0.032	-0.008*	0.025
	(0.047)	(0.013)	(0.017)	(0.015)	(0.042)	(0.005)	(0.017)
Linear spline	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.200	0.100	0.240	0.230	0.190	0.420	0.400
N	5,353	5,361	5,396	5,366	4,453	5,205	5,205

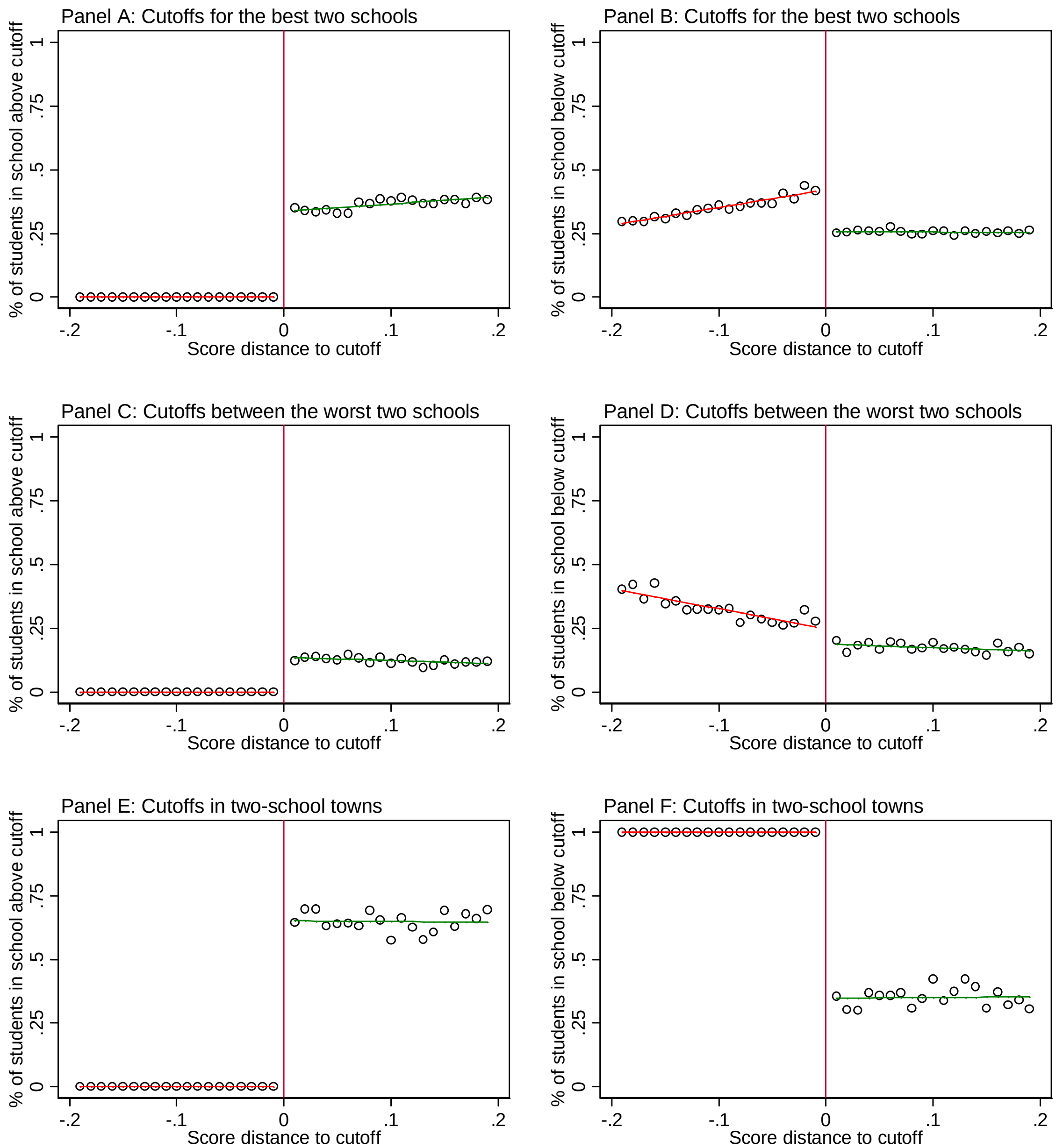
Note: All regressions are clustered at the student level and include cutoff fixed effects. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff (normalized to zero). Panel A presents outcome variables that are aggregated at the class level. Panel B presents outcome variables that are at the child or parent level.

Table A.1

	Dependent variable								
	Mother's birthyear	Mother is Romanian	Mother is Hungarian	Mother is Roma	Mother has has primary education	Mother has has secondary education	Mother has has tertiary education	Child Gender gender	Child's birthyear
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample	-0.297	-0.001	0.001	-0.002	-0.002	0.039*	-0.022**	0.026	0.013
	[0.240]	[0.008]	[0.007]	[0.003]	[0.005]	[0.020]	[0.009]	[0.019]	[0.019]
Panel B: Within 1 point of cutoff	-0.222	0.003	0.000	0.000	0.001	-0.003	-0.005	0.025	0.032
	[0.317]	[0.011]	[0.009]	[0.004]	[0.006]	[0.026]	[0.011]	[0.024]	[0.025]
Panel C: Within IK bounds	-0.325	0.003	0.003	-0.002	0.001	0.018	-0.014	0.027	0.017
	[0.278]	[0.010]	[0.008]	[0.004]	[0.006]	[0.023]	[0.009]	[0.021]	[0.023]

Note: All regressions are clustered at the student level and include cutoff fixed effects. All results are based on 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 (normalized to zero). All outcome variables are at the child or parent level.

Figure A.1: Top and bottom cutoffs in towns with 3 or more schools; 2-school towns



Note: 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 ones 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.