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Inside the Black of Box of Ability Peer Effects: Evidence from Variation in Low Achievers in the Classroom"

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Abstract

In this paper, we estimate the extent of ability peer effects in the classroom and explore the underlying mechanisms through which these peer effects operate. We identify as low ability students those who are enrolled at least one year behind their birth cohort ("repeaters"). We show that there are marked differences between the academic performance and behavior of repeaters and regular students. The status of repeaters is mostly determined by first grade; therefore, it is unlikely to have been affected by their classroom peers, and our estimates will not suffer from the reflection problem. Using within school variation in the proportion of these low ability students across cohorts of middle and high school students in Israel, we find that the proportion of low achieving peers has a negative effect on the performance of regular students, especially those located at the lower end of the ability distribution. An exploration of the underlying mechanisms of these peer effects shows that, relative to regular students, repeaters report that teachers are better in the individual treatment of students and in the instilment of capacity for individual study. However, a higher proportion of these low achieving students results in a deterioration of teachers' pedagogical practices, has detrimental effects on the quality of inter-student relationships and the relationships between teachers and students, and increases the level of violence and classroom disruptions.

Keywords: Peer effects, Education production function.
JEL Codes: 12, 121, J24

1. Introduction

This paper studies peer effects in educational outcomes between classmates in high schools and in middle schools. It investigates the existence and magnitude of peer effects, and explores some of the potential mechanisms that may explain their emergence. Specifically, we investigate whether having unusually low-achieving classmates has any effect on the educational outcomes of regular pupils. Using students' exact date of birth and the rules governing the enrollment in first grade, we identify as potentially low ability students those who were born substantially before the relevant threshold date for their cohort ("repeaters").[1] We show that repeaters have substantially lower academic achievements relative to both regular students and relative to any other group of students identified based on their socio-economic background.

It is well known that the estimation of peer effects entails a number of difficult econometric problems. First, we need to solve the obvious selection problem stemming from the fact that the proportion of a student's peers who are repeaters is not determined randomly; rather, repeater status is correlated with low socioeconomic background and therefore repeaters are typically clustered in low-achieving schools. In order to overcome this selection problem, we exploit idiosyncratic variations in the proportion of repeating students across adjacent cohorts within the same schools.

By using multiple cohorts and conditioning on school fixed-effects and school specific time trends we are able to eliminate all the observable correlations that can confound the repeater peer effects in schools. We show that within schools, there is considerable cohort-to-cohort variation in the proportion of repeaters, and demonstrate that this within school variation is not related to variation in student background characteristics. We are also able to enhance the credibility of this identification strategy by contrasting the estimated treatment effects to those based on two alternative "placebo" treatments (i.e., measuring the key treatment variable not in the cohort of interest, but in adjacent cohorts within the same school).

A second difficulty involves the measurement of peer ability. The direct approach that regresses own achievement on contemporaneous or lagged achievement of peers is problematic, since these variables are determined simultaneously with own achievement (Manski, 1993). Therefore, the empirical evidence on ability peer effects in schools comes primarily from studies that examine the effect of average background characteristics, such as parental schooling, race, and ethnicity on student outcomes. A wide variety of approaches are used in these studies to identify peer effects. The papers closest in spirit to ours are the ones by Hoxby (2000) for the US, and Ammermueller and Pischke (2006) for several European countries. Similarly to us, these papers rely on differences in the composition of individual

[1] The terms are used somewhat loosely: a large majority of repeaters never really repeated a grade, but rather entered first grade one year after their normative entry date. In a sense, they repeated the last year of kindergarten.

classes within a school, which come about by chance. [2] A limitation of these studies is that they do not measure directly the ability of students' peers but rely on socio-economic background characteristics as proxies for ability.

An advantage of our study is in the use of a closer proxy of peers' ability, which we believe captures some of the most important dimensions of academic ability, and is unlikely to have been affected by own ability. As we argue below, the status of being a repeater is determined primarily during elementary school, usually as early as kindergarten and first grade, and it is highly correlated with academic achievements, especially in subjects that demand high levels of abstract reasoning, such as mathematics and science.

The first part of the paper explores how classroom ability composition, as measured by the proportion of repeaters in the class, affects scholastic achievements of middle and high school students, as measured by test scores in English, Hebrew, math, and science (for middle schools), and students' performance in the matriculation exams completed by the end of 12^{th} grade (for high schools).

The second part of the paper identifies mechanisms by which the ability peer composition affects academic outcomes. Using a unique national survey administered to middle school students, we are able to identify whether peer composition affects the teachers' pedagogical methods in the classroom, the level of disruption and violence, and the quality of inter-student and student-teacher interactions. With the exception of Lavy and Schlosser (2007) who apply a similar research design to study the extent and mechanisms of gender peer effects in the classroom, we are not aware of other studies that have attempted to explore empirically the "black box" of peer effects. Investigating inside the black box is important in its own right, as there is abundant evidence that parents place a high value on a good classroom environment.

The results show that the proportion of repeaters in class has a negative and significant effect on the academic achievements of regular middle and high school students. We also find that the negative effect of repeaters is larger on students with low socio-economic background. When we replace the actual treatment variable with two alternative "placebo" treatments – the proportion of repeaters in either the previous or the subsequent cohorts – we find no effect at all. The lack of any discerned effects when using the placebo treatments strongly suggests that our estimates are not spuriously picking up any short term effects of unobserved confounders at the school level.

The exploration of the underlying mechanisms of these peer effects shows that, relative to regular students, repeaters report that teachers are better in the individual treatment of students and in the

[2] A number of recent studies have also used explicit random or quasi-random assignment to classes or schools, or other natural experiments, for example, Sacerdote (2001), Zimmerman (2003), Angrist and Lang (2004), Arcidiacono and Nicholson (2005), Hanushek et al. (2003) and Gould, Lavy and Paserman (forthcoming).

instilment of capacity for individual study. However, a higher proportion of such students results in a deterioration of teachers' pedagogical practices and the relationships between teachers and students, and increases the level of violence and classroom disruptions.

The rest of the paper is organized as follows. The next section reviews the relevant recent literature on peer effects and the following section describes the identification strategy. Section 4 discusses the data and the construction of the analysis samples, while section 5 presents the main OLS and school fixed effect estimates of ability peer effects on middle and high school students' achievements. Section 6 presents evidence on the possible mechanisms driving the negative peer effects of low ability students on other students' achievement. Section 7 concludes.

2. Related Literature on Peer-Effects

Social scientists have long recognized that peer effects may be among the most important determinants of student outcomes. However, it has been often difficult to convincingly isolate peer effects in empirical studies, because students from similar backgrounds typically tend to associate together, so that one's peer group is almost always self-selected. Recent years have seen a flurry of research that has attempted to use natural and quasi-experimental settings to identify peer effects in the classroom: these studies include Boozer and Cacciola (2001), Lefgren (2004), Vigdor and Nechyba (2004), Nechyba and Vigdor (2005), Burke and Sass (2004), Gibbons and Telhaj (2005), and Lavy and Schlosser (2007). Other closely related papers in the educational setting are those by Sacerdote (2001) and Zimmerman (2003), who study residential peer effects by exploiting the random assignment of college roommates; the literature on the effects of desegregation on the educational outcomes of white students (Angrist and Lang, 2004; Guryan, 2004); and the literature on the effects of immigrants on natives' educational outcomes (Betts, 1998; Hoxby, 1998a; Borjas, 2004; Gould, Lavy and Paserman, forthcoming).[3] More recent education peer effect studies are Duflo, Dupas and Kremer (2008), Carrell and Hoekstra (2008), Aizer (2008) and Bifulco, Fletcher and Ross (2008).

The papers closest in spirit to ours are the ones by Hoxby (2000) and Ammermueller and Pischke (2006). Hoxby (2000) relies on the exogenous variation across cohorts in peer composition at the school grade-level in Texas elementary schools and finds evidence for the existence of gender and race peer effects. Ammermueller and Pischke (2006) estimate peer effects across classes within primary schools of six European countries and argue that classes within schools are formed randomly with respect to family

[3] Other studies have instead looked at more broadly defined peer effects on a variety of children and adult outcomes (Katz, Kling and Liebman, 2001; Oreopoulos, 2003; Jacob, 2004; Weinberg, Reagan and Yankow, 2004). In a similar vein are also the works by Bayer, Pintoff and Pozen (2004), who study the effects of being in the same correctional facility on recidivism of young criminals, and Bayer, Ross and Topa (2005) who consider peer effects in place of employment for neighbors who live on the same residential block.

background. They find strong positive effects of the student background measure of peer composition on reading test scores of fourth graders.[4]

A limitation of the approaches taken by Hoxby and Ammermueller-Pischke is that they measure peer quality using fixed and exogenous demographic characteristics that are not necessarily an indicator of academic ability. This point is highlighted by Hoxby and Weingarth (2005) who find that, when properly accounting for the effects of peers' achievement, the race, ethnicity, parental income and education of one's peers have little or no effects on students' academic outcomes.

As an alternative strategy, other studies measure peer quality directly using lagged academic achievements (Lefgren, 2004; Burke and Sass, 2006; Nechyba and Vigdor, 2005). However, since a student's peer group usually does not change during his or her time in elementary school if not for mobility-related reasons, this approach may still suffer from the reflection problem. The reason for this is that a student in, say, 4th grade, will have already influenced her peers in 3rd grade: hence, using lagged achievement necessarily embodies the reflection problem. One advantage of our estimation approach is that we measure peer quality using a predetermined student characteristic, which is nonetheless very strongly correlated with academic ability.[5]

Finally, our paper is also related to the recent literature on the effects of age at school entry on educational and labor market outcomes. Many studies have shown that being relatively older and more mature when entering the compulsory school system might have positive effects on academic performance although the evidence on long-term outcomes has found no or even negative effects.[6] None of these studies, however, investigates the effects of late school entrants on their peers, which is the focus of the current paper. Moreover, while these studies compare the outcomes of students born just within the relevant school entry threshold dates for their cohort, our focus is on the effects of peers born several months outside the relevant threshold dates. We will show that these students are unquestionably weaker academically relative to their peers.

3. Empirical Strategy

[4] In a more recent paper, which uses a similar identification strategy to ours, Carrell and Hoekstra (2008) exploit cross-cohort within-school variation in the proportion of students exposed to domestic violence, and find that the presence of these students has a significant negative effect on their peers' test scores.

[5] This same point is made by Hoxby and Weingarth (2005) to justify their identification strategy. They instrument lagged achievement with the *initial* achievement of each peer in the "simulated" cohort that would have resulted solely from the random reassignment of students across Wake County districts (if any subsequent endogenous movements had not been allowed). Hoxby and Weingarth state that in this case "the reflection problem does not occur, because the reassigned peers had not experienced the student when their initial achievement was determined."

[6] See for example, Angrist and Krueger (1992), Datar (2006), Fredrikkson and Öckert (2005), Puhani and Weber (2005), Bedard and Dhuey (2006), Cascio and Whitmore Schanzenbach (2007), and Black, Devereux, and Salvanes (2008).

The effect of ability composition is usually confounded by effects of unobserved correlated factors that affect students' outcomes. This correlation could result if there is selection and sorting of students across schools based on ability or if there is a correlation between average students' ability in a school and other characteristics of the school that affect students' outcomes. A feasible approach that avoids both sources of confounding factors in the estimation of peer effects is to rely on within school variations in the ability distribution of students across adjacent cohorts provided that these variations are purely idiosyncratic and uncorrelated with students' potential outcomes. Based on this approach we examine whether cohort-to-cohort changes in students' outcomes within the same grade and school are systematically associated with cohort-to-cohort changes in the distribution of student ability.

The key requirement for our empirical approach is the identification of a group of low-ability students based on a predetermined proxy for student ability that has not been affected by the ability of his/her peers and therefore, does not suffer from the reflection problem. For this purpose, we define as low ability students, those who were born at least three months before the relevant cutoff date for their cohort.[7] These students have essentially been held back or "repeated" a grade – mostly because they were held back in kindergarten or repeated first grade – and therefore we dub them "repeaters." We will later show that repeaters have indeed substantially lower academic outcomes relative to "regular" students, and that repeater status is usually determined very early in a student's school career.

The basic idea of our empirical strategy is to compare the outcomes of students from adjacent cohorts who have similar characteristics and face the same school environment, except for the fact that one cohort has a relatively high proportion of low ability students (repeaters) than the other due to purely random factors.

In implementing this methodology we use the proportion of repeaters measured at the grade and not at the class level because the latter might be endogenous, as parents and school authorities may have some discretion in placing students in different classes within a grade. This is not a very restrictive compromise because within a given school the proportion of repeaters in a grade is highly correlated with their proportions in a class.

Using repeated cross-sectional data we estimate the following equation for the sample of regular students:

$$y_{igst} = \alpha_g + \beta_s + \gamma_t + X'_{igst}\delta_1 + S'_{gst}\delta_2 + \delta_3 LA_{gst} + \varepsilon_{igst} \qquad (1)$$

where i denotes individuals, g denotes grades, s denotes schools, and t denotes time. y_{igst} is an achievement measure for student i in grade g, school s, and year t; α_g is a grade effect, β_s is a school

[7] We defer the exact definition of "repeaters," as well as institutional details of the Israeli school system to the next section.

effect, γ_t is a time effect, X_{igst} is a vector of student's covariates that includes gender, mother's and father's years of schooling, number of siblings, immigration status, ethnic origin, and indicators for missing values of these covariates, S_{gst} is a vector of characteristics of a grade *g* in school *s* and time *t* and includes a quadratic function of enrollment and set of variables for the average characteristics of the regular students in the grade; LA_{gst} is the proportion of low ability students in grade *g*, school *s*, and year *t*, and ε_{igst} is the error term which is composed of a school-specific random element that allows for any type of correlation within observations of the same school across time and an individual random element. The coefficient of interest is δ_3 which captures the effect of having more low-ability students as peers on students' achievement.

For the estimates in equation (1) to have a causal interpretation, the unobserved determinant of achievement must be uncorrelated with the treatment variable. Including school fixed effects controls for the most obvious potential confounding factor – the endogenous sorting of students across schools based on socio-economic background. However, one may be concerned that there are time-varying unobserved factors that are also correlated with the proportion of high achieving and low achieving students.[8] Therefore, in our preferred specification we add to equation (1) a full set of school-specific linear time trends. Hence, identification is achieved from the deviation in the proportion of low-achieving students from its long-term trend within a school.

4. Data and Descriptive Statistics

The empirical analysis is based on two samples that include high and middle school students, respectively. We limit the analysis to samples that include only the Jewish state run schools in Israel. Below we describe the samples.

The high school data

We use administrative records collected by the Israel Ministry of Education for 7 consecutive cohorts (from 1994 to 2000) of 10th grade students. The data are based on annual reports submitted by school authorities to the Ministry of Education at the beginning of the school year. Each record contains an individual identifier, a school and class identifier, and detailed demographic information on the student: date of birth, gender, parental education, number of siblings, year of immigration (where relevant), and ethnicity. We use 10th grade to define the base population because it is the first year of high

[8] These concerns are particularly relevant for the high school outcomes equation because we have a longer panel and also because secular trends in school proportion of repeaters is more likely to exist in high schools since there is school choice at this level of education.

school and the last year of compulsory schooling. The sample is restricted to students in non-special education schools that have a matriculation track. As a further restriction, we drop all schools that experienced a change in enrollment of 80 percent or more between two consecutive years of the analyzed period to avoid changes in the proportion of school repeaters that might be originated by structural changes of the school. Finally, we only keep schools that appear in all 7 years, and omit schools with extremely small reported grade size.

Israeli high school students are enrolled either in an academic track leading to a matriculation certificate (*Bagrut* in Hebrew) or in an alternative track leading only to a high school diploma.[9] The *Bagrut* is completed by passing a series of national exams in core and elective subjects taken by the students between 10^{th} and 12^{th} grade. Students choose to be tested at various levels of proficiency, with each test awarding from one to five credit units per subject, depending on difficulty. Some subjects are mandatory, and for many the most basic level is three credit units. Advanced level subjects are those subjects taken at a level of 4 or 5 credit units. A minimum of 20 credit units is required to qualify for a matriculation certificate. We link the students' file with administrative records that include the results (test scores) of these matriculation exams.

We focus on the following matriculation outcomes that are available for all the years: the average score in the matriculation exams, matriculation status (equals 1 if the student was awarded the matriculation diploma, and 0 otherwise), the number of credit units, the number of advanced level subjects in math and science, and a matriculation status that meets university entrance requirements (at least 4 credits in English and another subject at a level of 4 or 5 credits, in addition to being awarded with the diploma).[10] We also constructed indicator variables for student enrollment in advanced courses in math, physics, computer science, biology, and chemistry.

The middle school data

Data for middle schools is based on the GEMS (Growth and Effectiveness Measures for Schools - *Meizav* in Hebrew) datasets for the years 2002-2005. The GEMS includes a series of tests and questionnaires administered by the Division of Evaluation and Measurement of the Ministry of

[9] The matriculation certificate is a prerequisite for university admission and receiving it is one of the most economically important educational milestones. Similar high school matriculation exams are found in many countries and in some states in the United States. Examples include the French Baccalaureate, the German Certificate of Maturity, the Italian Diploma di Maturità, and the New York State Regents examinations.

[10] Roughly, 10 percent of the students in the sample did not take any of the matriculation exams. These students get zero values in the average score. None of the other four matriculation outcomes that we use require such imputation since the zero values that these students get for these outcomes, for example, number of credit units, is a real and not an imputed measure of their achievements.

Education.[11] The GEMS is administered at the mid term of each school year to a representative 1-in-2 sample of all elementary and junior high schools in Israel, so that each school participates in GEMS once every two years.

The GEMS student data include tests scores of 8th graders in math, science, Hebrew and English, as well as the responses of 7th through 9th grade students to questionnaires. In principle, all students except those in special education classes are tested and administered the questionnaires. The rate of tested students is above 90 percent and the rate of questionnaire completion is roughly 91 percent. Student test scores are originally in a 1 to 100 scale and we transformed them into standardized z-scores to facilitate the interpretation of the results.

The student questionnaire includes 71 questions addressing various aspects of the school and the learning environment. We focus on two sections of the questionnaire which address issues related to teaching pedagogy and the school learning environment. In these two sections students are asked to rate the extent to which they agree with a series of statements on a 6-point scale ranging from *"strongly disagree"* to *"strongly agree"*. We transformed students' responses to these items into standardized z-scores.

The student questionnaire data and test scores for the years 2002-2005 were linked to student administrative records collected by the Israeli Ministry of Education (identical in structure to the data used for high school students). The administrative records include student background characteristics and are used to construct the peer composition variables.

A large proportion of religious middle schools have separate classes by gender. Since we are unable to observe whether the students study in single-sex or mixed- sex classes in these schools we cannot measure the proportion of repeaters accurately. In addition, a large proportion of religious boys leave the public school system to attend Yeshiva institutions during middle school grades. This creates more measurement error and instability in the treatment variables within the religious schools. We therefore, drop all religious schools from the middle school sample.

Since we have multiple grades for each school in the student's questionnaire data, we exploit within school variation in the proportion of repeaters across years and grades to gain more variability in this variable. To get a more stable population within schools across years, we further restrict our sample to a balanced panel keeping only schools that have complete data for the 3 grades (7th, 8th and 9th) at least two years. We therefore have six observations of the same school for middle schools (7th, 8th and 9th grade

[11] The GEMS are not administered for school accountability purposes and only aggregated results at the district level are published. For more information on the GEMS see the Division of Evaluation and Measurement website: *http://cms.education.gov.il/educationcms/units/rama/odotrama/odot.htm*.

for two years). The analysis on students tests scores for middle schools has more limited power since only 8th graders were tested leaving us with only two observations per school.

Measuring Treatment

In Israel, children roughly enter first grade in September of the calendar year in which they turn six years old. We say "roughly" because the relevant threshold date is based on the Hebrew calendar. For example, the first grade class of September 2007 is composed of children born between the 1st of Tevet 5761 (December 27th, 2000) and the 30th of Kislev 5762 (December 15th, 2001).[12] However, parents have some discretion in deciding when to send their children to first grade. The parents of a gifted child who can already read and write may decide to have her skip the last class of kindergarten, or make her enter school directly in second grade. More commonly, some parents who think that their child is not cognitively and emotionally mature enough for first grade, may decide to hold their child back and delay entry into first grade.

For children born between September 1st and the cutoff date, the process of delaying entry into first grade has a relatively low cost: all that is needed is a written request by the parents, accompanied by a letter from the kindergarten teacher in support of delayed entry. On the other hand, children born before September 1st, whose parents would like to delay their entry into first grade, need also a certification by an external counselor that the child is not ready for first grade.

Therefore, we define as "repeaters" children who are enrolled in a grade that is one year below their expected grade *and* were born before September 1st, as well as children who are enrolled in a grade that is two or more years below their expected grade. Note that we do not define as repeaters children who are one year behind but were born between September 1st and the cutoff date. As a result, we are confident that our sample of repeaters includes primarily children with low cognitive or emotional maturity, rather than children who are maybe deliberately kept one additional year in kindergarten to obtain a competitive advantage in school.[13] We also exclude from the count of repeaters students who are new immigrants, since the proportion of new immigrants who are repeaters is very high, and it is unclear whether repeater status among new immigrants indeed reflects low academic ability.

"Repeater" status is determined primarily while in elementary school, even before entering first grade. Our data does not allow following a student from first grade to the end of high school, so we cannot determine for each student the exact timing of becoming a repeater. However, we can assemble some evidence in support of the claim of early determination of these indicators. For example, we traced

[12] For conversion between Hebrew dates and Gregorian dates, see http://www.hebcal.com.

[13] In Israel, as elsewhere, there have been reports in the popular press that an increasing number of parents delay their children's entry into the school system (red-shirting), and that this phenomenon is particularly common among affluent parents (see, Deming and Dynarski, 2008, for a review).

the repeater status for three cohorts of 10th graders (1997-1999) back to their last year of elementary school and found that around 85 percent of repeaters are already in this status by 6th grade. We also traced back a cohort of middle school students and found that 85 percent of repeaters were already in this status in elementary school with 60 percent being held back in kindergarten or first grade.

Table 1 shows mean outcomes for repeaters and regular high school students. Column 1 presents the means of the repeaters, column 2 for a group with low parental education (both father and mother did not complete primary school), and column 3 reports outcome means for all students who progressed in school regularly according their age. The sample includes 310 high schools and 377,527 students from seven cohorts. The average proportion of repeaters is 3.7 percent and the proportion of students from low education families is 3.4 percent.

It is immediately apparent that repeaters have substantially lower mean outcomes relative to all regular students, as well as relative to children from low education families. For example, on average for the whole sample period, 60.6% of regular students in the sample were awarded a matriculation certificate, versus only 23.0% among repeaters. Regular students accumulated, on average, 21.4 credit units while repeaters accumulated only 12.4. The achievement gap is much larger in science and math: regular students' matriculation curriculum includes 0.54 advanced level subjects in math and science while repeaters had only 0.16 such subjects.

A more specific illustration of the large gap between these groups is presented in the lower panel of Table 1. The enrollment rate of repeaters in advanced level math classes in high school is extremely low, 1.6%, versus 14.0% among regular students. Similarly, the enrollment rate in advanced physics is 9.8% among regular students, and only 1.6% among repeaters. A similar pattern is seen in advanced computer science and chemistry classes while the gaps in biology are somewhat smaller.

These findings provide strong support for our working hypothesis that repeaters have low cognitive ability. The means presented in column 2 suggest that the weakest group in terms of socio-economic background (based on parental schooling) has better average outcomes than the group of repeaters. In fact, the gap between columns 2 and 3 is narrower than the gap between column 1 and 2 suggesting that the low parental education group is more similar to the regular students than to the repeaters. It is also worth noting that the mean years of parental schooling in the repeaters group is 10.2 while the mean in the low education group is less than 7, proving that repeaters have significantly lower outcomes, even though they are not the most disadvantaged students in terms of parental education.

The lower academic outcomes of repeaters compared to those in the lowest end of the distribution of parental education suggests that the strategy we propose may be more successful in identifying the truly low ability peers than alternative strategies that identify low-ability students based only on measures of low socioeconomic status.

5. Evidence on the Validity of the Identification Strategy in the High School Sample

A. Is there sufficient variation in the proportion of repeaters?

The identification strategy outlined in section 3 raises a number of concerns. The first is related to precision: since identification relies on within school variation in the proportion of repeaters, sufficient variation in peer composition across cohorts within schools is needed to obtain precise estimates. We find that there is substantial cohort-to-cohort variation in the proportion of repeaters, which can be exploited in the empirical analysis: a variance decomposition of the proportion of repeaters in high schools shows that the within school variation accounts for 34% of the total variance. As our results show, this amount of variation enables us to obtain sufficiently precise estimates even in specifications with a full set of school fixed effects or school specific trends. We also find that this variation is evident not only in small schools but also in medium and large schools as well as in large and medium sized towns. This evidence is important because it suggests that the identification of the ability peer effects will not rely solely on variation in small schools and towns, which are mainly situated in the periphery of the country, but will rely also on variation from medium and large schools and towns, including the large metropolitan areas in the center of the country.

B. Is observed within school variation consistent with a random process?

A second important question is what explains the within school variation in the proportion of repeaters. We argue that natural fluctuations in the number of low ability students in a cohort generate within school variation in the proportion of repeaters. To assess this issue, we checked whether the observed within school variation in the proportion of repeaters is consistent with a random process by performing Monte Carlo simulations. For each school, we randomly generated the repeater status of the students in each cohort using a binomial distribution function with p equal to the average proportion of repeaters in the school across all years. We then computed the within school standard deviation of the proportion repeaters. We repeated this process 1,000 times to obtain an empirical 95 percent confidence interval for the standard deviation for each school.[14]

To illustrate this procedure we plotted in Figure 1 the actual standard deviation in the proportion of repeaters and the 95 percent confidence interval computed for each school. It is clear from the figure that most of the standard deviations fall within the confidence interval. Overall, 93% percent of the high

[14] Since the models at the high school level control also for school specific time trends, the within school standard deviations in the proportion of repeaters is computed based on the residuals from a regression of the proportion of repeaters on school fixed effects and school specific time trends.

schools had a standard deviation in the proportion of repeaters that fell within the 95% confidence interval. We further re-estimated all models by restricting the samples to schools that had a standard deviation within the confidence interval and obtained virtually identical results to those based on the full sample and reported below.

C. Does variation in the proportion of repeaters affect school mobility?

Another concern is whether the within school variation in the proportion of repeaters affects the mobility of students across schools. The lack of school choice at the middle school level and the very limited scope of private schooling in Israel diminish significantly the possibility of such selection. Such selection could occur in high schools, though it is very unlikely since, while parents may know the average repeaters' proportion at a school, it will be difficult for them to predict in advance these proportions for a specific cohort. Nevertheless, parents might still respond to cohort peer composition once they observe its actual realization in the first year of high school.

We address this concern by checking whether the likelihood that a student leaves a school (by moving to another school or dropping out) is associated with the proportion of repeaters in his/her initial grade. Using the sample of 10^{th} grade students (which is the first grade of high school) we constructed a dummy variable that equals to one if the student left the school in the following year.[15] Using this indicator as a dependent variable, we estimated models similar to equation (1) to asses the effects of the proportion of repeaters in the grade on the likelihood that a 10^{th} grade student leaves his/her initial school.

It is worth noting in this regard is that the rate of students' mobility is relatively low. Roughly, 2.7 percent of the students left their school at the transition between 10^{th} and 11^{th} grade. This relatively low mobility rate (in comparison, for example, to the US) makes the implementation of an identification strategy based on within school variation in peer composition especially appealing in the Israeli context.[16] The estimates of the effects of the proportion repeaters on the likelihood of leaving the initial high school are small and insignificant regardless of the specification used. For example, the estimate based on a specification that includes school fixed effects and school time trends is -0.018 (s.e. = 0.056). Overall,

[15] In order to avoid classifying as school movements or drop-outs those cases that arise from structural school changes (closures, merges, etc.) or from data collection problems, we follow Hanushek et al. (2004) and exclude from school leavers those cases where the student moved to a school attended by more than 30 percent of the students of his/her former grade. We further excluded from school leavers those cases were 100 percent of the students in the grade left the school. Less than half percent of the sample's observations are affected by these two adjustments.

[16] A US national study reports that 40 percent of third graders have changed schools at least once since 1st grade (US General Accounting Office, 1994). Hanushek et al. (2004) report an annual rate of student mobility of 24% in Texas elementary schools. Similar annual rates are reported for Ohio by Rhodes (2005) and for Florida (personal communication with David Figlio).

this suggests that the likelihood that a student leaves his/her initial school is unrelated to the proportion of repeaters in his/her cohort.

D. Is the variation in proportion repeaters associated with cohort compositional changes?

Finally, we test directly whether the within school variation in the proportion of repeaters is associated with changes in the characteristics of regular students in the cohort. In particular, we checked whether the proportion of repeaters within a school is correlated with students' background characteristics such as parental education, family size, and proportion of new immigrants. Table 2 provides evidence on these balancing tests and reports the estimated coefficients from regressions of various student characteristics on the proportion of repeaters in high school. We present estimates from three specifications: simple OLS regressions, a specification with a full set of school fixed effects, and a specification with both school fixed effects and school-specific time trends.[17]

The OLS estimates show strong negative associations between the proportions of repeaters and student background characteristics. However, these correlations are much smaller and become insignificant in most of the within school regressions, where some of the estimates even change signs. The addition of school specific linear time trends wipes away almost all associations. For example, the coefficient of mother's years of schooling on the proportion of repeaters is -26.692 (s.e. 2.272) in the OLS regression. It drops to -0.523 (s.e. 1.435) in the within school regression and it is further reduced to -0.149 (s.e. 1.367) when adding school specific linear time trends.

Overall, by conditioning on school fixed effects and school specific linear time trends we are able to eliminate most of the observed associations between the proportion of repeaters and family background characteristics. There are some imbalances for students' ethnicity, but they are relatively small, only marginally significant and are of inconsistent signs. For example, the coefficient of Asia/Africa ethnic origin on the proportion of repeaters (in the full specification) is 0.152 (s.e. 0.068) and the coefficient of Europe/America ethnic origin is 0.101 (s.e. 0.049). This means that a change in the proportion of repeaters is positively associated with a change in the proportion of students from Asia/Africa, which is a relatively disadvantaged group, but it is also positively associated with a change in the proportion of students from America or Europe ethnic origin, which is a relatively privileged group. It is also worth noting that the magnitude of the estimates is very small relative to the magnitude of the independent variable. For example, a one standard deviation increase in the proportion of repeaters (0.03) is associated with a 0.3 percentage points increase in the proportion of students with Asian/African ethnic origin and with a similar increase in the proportion of students whose parents were born in America or Europe. In

[17] Balancing tests for middle schools are reported in the next section.

any case, in the outcome regressions, we will control for the student background covariates and for the average background characteristics of the regular students.

5. Results in the High School Sample

A. Effects on High School Students' Achievement

Table 3 reports the effects of the proportions of repeaters on the high school achievements of regular students. Each cell in the table shows the estimated coefficient on the proportion of repeaters in a grade from a separate regression. Column 1 presents the outcome means for regular students. Columns 2-5 report the results for the effect of the proportion of repeaters. The estimates presented are based on four different specifications. Columns 2 report OLS estimates when only year dummies are included as controls. In column 3 school fixed effects are added, in column 4 individual and school time varying controls are added and in column 5 school specific time trends are added as controls.

We see a common pattern for the effect of repeaters for most outcomes, as we move from the first to the fourth specification. Adding school fixed effects dramatically reduces the negative point estimates obtained from simple OLS regressions, though they remain negative and statistically significant. This decline, by about a factor of ten, suggests that selection and sorting play a large role in these OLS correlations. Adding the individual and school time varying controls leaves the estimates almost unchanged, suggesting that the school fixed effects eliminate essentially all the effect of the observables characteristics on the outcomes. This pattern is consistent with the findings reported in Table 2 that suggested that the proportion of repeaters is not correlated with observable students- characteristics. Adding the school specific time trends, though, leads to a further decline in the point estimates, with standard errors staying roughly unchanged. For example, the simple OLS estimate for the effect of the proportion repeaters on the average score is -193.370, it declines to -20.059 when school fixed effects are added, then it is changed to -26.059 when student and school characteristics are added, and finally drops to -11.183 when the school specific trends are added. It should be noted that inclusion of school-specific time trends may severely exacerbate attenuation bias in the presence of classical measurement error (Griliches and Mairesse, 1995). Hence, our estimates may well represent a lower bound on the true effect of repeaters on their peers.

Four out of the five point estimates are significantly different from zero (at 10% level of significance) in the fourth and most complete specification (column 5). Only the effect of proportion repeaters on the number of advanced level subjects in science becomes non-significant. This evidence suggests that having a larger proportion of low ability students in class harms the achievements of the

regular students. [18] These negative effects are, however, moderate. For example, increasing the proportion of repeaters in class from 0 to 5 percent will lead to a decline of 1 percentage points in the matriculation rate of the regular students. For comparison purposes, this is equivalent to the change in matriculation rates associated with a two-thirds of a year drop in father's or mother's years of schooling, with one additional sibling, or about half of the change associated with being of Asia-Africa origin.

B. Falsification tests

To check whether our results are being confounded by any short term trend that is not captured by the school specific linear time trend, we perform falsification tests by replacing the proportion of repeaters in cohort (t) by the proportion of repeaters in adjacent cohorts - the younger cohort (*t-1*) and the older cohort (*t+1*).

The evidence on these falsification tests is presented in the last two columns of Table 3: in columns 6-7 we show the effect on outcomes when we use as the measure of peer quality the proportion of repeaters in cohorts *t-1* and *t+1* respectively. The results based on the *t-1* or *t+1* measure of treatment show no effect at all on any of the outcomes, for both types of the placebo treatment. Moreover, the sign of the estimates does not have a consistent pattern. For example, some of the estimates for the effect of repeaters when the *t-1* measure is used are positive while the sign of all the estimates obtained with the true treatment measure are negative. When the *t+1* measure is used, two of the point estimates are negative and three are positive and none of them is significantly different from zero. We interpret these falsification tests as compelling evidence that our main results are not capturing a spurious correlation between the proportion of repeaters and time-varying school factors.

C. Allowing for Heterogeneous Effects by Students Socio-Economic Status

We now test for the presence of heterogeneous effects: specifically, we look at whether repeaters differently affect students with different socio-economic background and academic ability. For this purpose, we stratify the sample into two groups as follows: students who had both parents with 12 or more years of schooling (approximately 60% percent of students) and the rest. We then re-estimate model 1 separately for the two groups reporting the results in Table 4.

[18] One possible explanation for our results is that repeaters have a negative effect on their peers' outcomes not because of low academic ability, but because of emotional immaturity, since repeater status reflects mostly late entry to first grade, which may occur because the child is not ready for first grade, either cognitively or emotionally, at the end of kindergarten. Alternatively, the negative effect may arise simply because repeaters are older than regular students, and we are picking up an effect of age rather than of academic ability. The likelihood of certain types of misbehavior (truancy, smoking, sexual activity, etc.), which may harm academic performance, is probably higher among older students. We examined this potential channel by dividing the repeaters into two age groups and found that there are no significant differences in the treatment effect by age of repeaters.

The first two columns of Table 4 present the mean of the dependent variables for the two subsamples. Unsurprisingly, students with highly educated parents have substantially higher outcomes. The next two columns present the estimates of equation (1) on the two subsamples. We include the full set of control variables, as well as school fixed effects and school-specific time trends. Overall, the estimates suggest that low ability peers have a negative impact mainly on students from low socio-economic background. The estimated negative effects for this group are larger than those for students with highly educated parents, and those reported in Table 3 for the whole sample. For example, a change from 0 to 5 percent repeaters will reduce the number of credit units among students from low socio-economic background by 0.35, a change of almost 2 percent relative to the respective mean. The reduction in the matriculation rate will be of almost 3 percent relative to the outcome mean of this group. On the other hand, we find substantially smaller and mostly insignificant effects of the proportion repeaters on the outcomes of students with high-education parents.

As argued before, parental education may be only an imperfect measure of student ability. Therefore in Table 5 we propose an alternative test to assess whether regular students of different academic ability are affected differently by the proportion of repeaters in their grade. We exploit an institutional feature of Israeli high schools whereby students are tracked in most compulsory and elective subjects into study groups defined by the level of the curriculum. The assignment of students into the advanced placement class in each subject is shaped both by student's self-selection and by teachers' recommendations and usually takes place at the beginning of 10^{th} grade. Once students are tracked according to their academic orientation, they are usually assigned to classes for the remaining subjects with the same group of students.

Based on this idea, we stratified the sample of regular students according to their enrollment in advanced math and science programs (biology, chemistry, computer science and physics). We define three different groups: the first group includes students with no enrollment in any of these programs and it accounts for about two thirds of the overall sample. These students can be thought of as relatively low ability students. The next two groups comprise relatively high ability students: the second group includes students enrolled in at least two advanced programs (a sixth of the sample) while the third group (less than a tenth of the sample) includes students enrolled in three or more programs.

Though enrollment in such programs is determined relatively early in high school, it could still be affected by the proportion of repeaters in a grade. If this is the case, an estimation based on stratified samples by the number of advanced math and science programs may involve a selection bias. In the first row of Table 5 we present evidence on this issue by reporting the effect of the proportion of repeaters on students' enrollment in advanced math and science programs. All these estimates are small and not statistically different from zero, suggesting that there is no significant association between the number of

math and science advanced programs that a student is enrolled in and the proportion of repeaters in his/her grade. These results imply that we can stratify the sample by number of math and science advanced programs without a concern about potential selection bias.

The bottom panel of Table 5 presents the estimates of the effect of repeaters on students' outcomes for the three groups we defined above. In columns 1, 3, and 5, we report the outcome means for each of the three groups and in columns 2, 4, and 6 we report the estimates for the proportion of repeaters. The mean of each of the outcomes increases sharply and monotonically as we move from the first to the third group. For example, the matriculation rate in the third group is 0.972, more than twice the respective rate (0.460) in the first group. This pattern strongly supports the notion that the number of advanced programs in math and science that students are enrolled in is a good proxy of students' cognitive ability.

The results reported in columns 2, 4, and 6 are quite striking: the proportion of repeaters affects negatively only students in the first group, those who are not enrolled in any math and science advanced program. In contrast, the effect of the proportion of repeaters on students who are enrolled in 2+ or 3+ math and science programs are small, not significantly different from zero and some even have a positive sign.[19] This evidence indicates that a higher proportion of low ability students particularly harms regular students who are not tracked into advanced programs in math and science. This contrast persists even if we restrict the sample to schools that have at least some students enrolled in 3 or more advanced classes (column 8 in the Table), indicating that the result is not driven by a different composition of schools.[20]

One possible interpretation of this result is that high ability students are simply better able to cope with the difficulties associated with learning with a high proportion of repeaters. A more appealing explanation is that high ability students may be unaffected by the presence of repeaters because they hardly interact with them. Because of the tracking system, repeaters are hardly ever enrolled in the advanced classes in any of the subjects; consequently, those students who do enroll in these classes have almost no exposure or classroom interaction with repeaters. Indeed, only 2 percent of the total number of repeaters is enrolled in 2 or more advanced classes while only 0.75 percent of repeaters are enrolled in 3 or more advanced classes.

In this sense, the results in this table can be seen an additional falsification test, where we are able to identify a group of students belonging to the same cohort as the repeaters, but who in practice share

[19] It is important to note in this regard, that the standard errors of the estimates are similar across subsamples proving that the insignificant effects of repeaters on the subsample of students enrolled in advanced classes stems from the low magnitude of the estimates and not from a lack of statistical power.

[20] We also checked the balancing based on characteristics in each of the four ability groups and found that overall, after conditioning on school fixed effects and school specific linear time trends, there are no observed associations between the proportion of repeaters and family background characteristics in each of the four sub-samples (results available from the authors).

almost no classroom interaction with them. If there were any type of school-cohort specific shocks correlated with the proportion of repeaters and outcomes, or if peer effects operated mostly in non-classroom activities, we should expect a negative effect of the proportion of repeaters even among the group of students enrolled in 2 or more advanced classes. The lack of such an effect indicates that the negative effect of repeaters occurs because of what happens inside the classroom. In the next section we attempt to investigate in more depth the mechanisms that might give rise to this negative peer effect.

D. The Middle School Sample

The samples we have for middle schools test scores pool together only two cohorts of 8th grade. Therefore, the within school estimation of the effect of the proportion of repeaters is less powerful in this sample as we have only two data points per school (as opposed to the high school sample where we have seven cohorts per school). We present in appendix Table A1 the estimates for the effects of the proportion of repeaters on 8th grade test scores. The format of the table is similar to the previous ones.[21]

Despite the reduced power of our empirical strategy, we do find negative effects of the proportion of repeaters on test scores in all four subjects but the estimates are less precise than those found for high school outcomes. The most comprehensive specification (column 5) tells us that a 5 percentage point increase in the proportion of repeaters reduces test scores in math and Hebrew by 0.05 of a standard deviation, and in English by 0.03 of a standard deviation. We also report in the table the effect on the average score of the four subjects: again, the estimate is negative and only marginally significant. Altogether, the results are in line with those obtained in the high school sample, although our estimates are less precise, as expected.

6. Identifying Mechanisms of Ability Peer Effects

The results reported above show that the proportion of low ability students in class lowers the scholastic achievements of regular students. In this section we attempt to explore the mechanisms through which repeaters in class impact their peers. Ability peer effects could operate through various channels: this could include effects through changes in the pedagogical methods used by teachers, the classroom climate, the quality of interactions among students and between students and teachers, and the level of motivation and self confidence of students. We examine here these possible mediating channels by estimating equation (1) using as dependent variables the responses to a detailed questionnaire related to teachers' pedagogy and the classroom environment, administered to the sample of 7th through 9th grade students described above.

[21] In this case, we do not control for school specific time trends since we have only two observations per school.

We grouped the individual items of the student questionnaire under eight categories. The first five describe teachers' pedagogical practices in the classroom: (1) instilment of knowledge and enhancement of comprehension; (2) instilment of applicative, analytical and critical skills; (3) transparency, fairness and feedback; (4) individual treatment of students; and (5) instilment of capacity for individual study. These categories of teacher's pedagogical practices are common and accepted terminology in the literature of educational psychology. The remaining three categories describe the classroom environment: (6) classroom disruption and violence; (7) teacher-student relationships; and (8) inter-student relationships.

We recognize that evidence on the effect of repeaters on these behavioral outcomes is not a definite proof for a specific form of the educational production function. Nevertheless, since the following behavioral outcomes are highly correlated with students' test scores, observing treatment effects on these mediating factors provides suggestive evidence on the possible channels by which peer effects operate.

A. Overall Pattern of Differences between Repeaters and Regular Students

We begin by presenting balancing tests of the covariates in Table 6. Once again, we observe that the coefficients on the treatment variable drop by a substantial amount when we control for school fixed effects and all of them become insignificantly different from zero. Some of the coefficients even change sign. Once again, it appears that controlling for school fixed effects removes almost all of the correlation between the proportion repeaters and the background variables. As usual, in the outcome regressions we will still control for the full set of background covariates to increase the precision of our estimates.

Table 7 reports the differences in means (in standard deviation units) of the eight categories between repeaters and regular students. These statistics permit assessing how the two different groups of students perceive their learning and classroom environment. Column 1 reports the differences after controlling for year and grade dummies and columns 2 reports the controlled differences after adding individual controls, as well as grade, year, and school-grade-year fixed effects. The controlled differences for the individual questionnaire items included in each category are reported in column 1 of Table A2.

First, it is seen in column 1 that repeaters appreciate more their teachers' pedagogical methods relative to regular students. These differences in the teachers' assessment by repeaters and by regular students remain almost unchanged even after controlling for the full set of individual covariates and for school-grade-year fixed effects. Repeaters give to their teachers higher scores on their teaching methods (category 1 and 2), and they also perceive their teachers as being more fair and transparent (category 3). In addition, there are striking differences between repeaters and other students in the items grouped under the categories of "individual treatment of students" and "instilment of capacity for individual study" (categories 4 and 5), with repeaters substantially more likely to evaluate their teachers positively.

More insight about these differences is gained from the differences in the individual questionnaire items reported in columns 1 of Table A2. There are large differences in items 17, 21, 22 and 23, which all relate to whether teachers adapt their teaching methods and pace to individual student needs. Repeaters value highly the individualized attention bestowed upon them by their teachers, and therefore rate their teachers highly in this dimension. We conclude from this pattern of differences that teachers pay more attention and time to underachieving students (repeaters), perhaps at the expense of time and attention given to regular students. This crowding out of instruction time from regular students will be shown to be intensified as the proportion of repeaters rises.

The differences in category 6 (*discipline and lack of violence*) reveal that repeaters have worse behavior and are exposed more to violence than other students.[22] This is seen more explicitly in items 34-36 of table A2. Repeaters are more likely to report being involved in physical fights, they report a higher incidence of fights among their classmates, and they are more intimidated and often scared to go to school because there are violent students.

In contrast to the worse behavior of repeaters, it is interesting that we find that they report better student teacher-relationships relative to the regular students. In particular, repeaters are more likely to report that they can turn to their teachers and a counselor if they have a problem at school (item 41 in table A2). Finally, repeaters report a lower quality of inter-student relationships compared to regular students. They seem to be less socially adjusted and to have a lower level of satisfaction with school than regular students.

Overall, the pattern of differences between repeaters and regular students regarding their relationships with teachers is consistent with the pattern of differences in the perception of teaching methods. It seems that teachers give more attention to the special needs of repeaters while overlooking the attention demanded by other students in the class.

B. Effects of Repeaters on Learning and Classroom Environment

We now turn to the analysis of the effects of repeaters on the learning and classroom environment faced by the regular students. We report within school estimates of the proportion of repeaters for each of the categories (column 3 in Table 7) as well as for individual items of the student questionnaire (column 2 Table A2) using the sub-sample of the regular students.[23] Following Kling et al. (2007) we compute the average effect τ_c for each category c by averaging across the standardized effects of the individual

[22] In constructing the mean of the grouped item, all variables are transformed so that high values indicate a more disciplined and less violent learning environment.

[23] We have also estimated falsification or placebo regressions for all students' questionnaire items similarly to the respected models estimated for the high school outcomes reported in Table 3. The results for these tests are not reported here but they indicate that the estimates of the placebo treatment measures are always small, have sometimes the wrong sign, and are not significantly different from zero.

outcomes included in that category. That is, the average effect of the proportion of repeaters for category c is defined as $\tau_c = \frac{1}{K_c}\sum_{k=1}^{K_c}\frac{\pi_{kc}}{\sigma_{kc}}$ where K_c is the number of outcomes included in category c, π_{kc} is the effect on outcome k included in category c, and σ_{kc} is the standard deviation of the outcome. To calculate the variance of τ_c it is necessary to estimate the covariance matrix of the individual effects within each category. We do so by estimating a system of seemingly unrelated regressions for the outcomes in each category.[24] By averaging across the effects on different outcomes within a category, we implicitly attribute equal weight to all outcomes. Since there is no prior information to justify a particular weighting, we assign equal weight to all outcomes as it provides a more transparent interpretation.[25]

Focusing first on the effects on teachers' pedagogical methods, we see that the sign of the repeaters' estimates is always negative. The coefficients are almost always significant at the 5 percent level. These results support the notion that a high proportion of low achieving students induces teachers to modify their pedagogy and their personalized attention to students. For example, the detailed results in Appendix Table A2 show that a higher proportion of repeaters leads teachers to focus less on real comprehension and more on memorizing the material; and it induces less focus on developing analytical skills and more effort on instilling technical understanding of concepts. A higher proportion of repeaters also induces teachers to devote less time to the individual support of the regular students and less emphasis on teaching them the skills needed for individual study.

The analysis on classroom violence and discipline shows that a higher proportion of repeaters increases sharply the level of disruption. This result is consistent with the descriptive statistics from table A2 that showed that repeaters are more likely to be involved in physical fights. The estimate on the aggregate measure of "discipline and lack of violence" is -0.409 (s.e 0.158). This effect summarizes the negative estimates of all the seven items that are included in this group (shown in table A2).

The good relationship between repeaters and their teachers stands in sharp contrast to the negative effects of repeaters on the relationship between regular students and their teachers. The estimated effect for repeaters is -0.622 (s.e. 0.230). The negative influence of the repeaters on student-teacher relationships is manifested in particular in how often students are perceived to be rude to their teachers

[24] This method treats the standard deviation of the outcomes (σ_{kc}) as known. It is possible to account for the sampling variance of σ_{kc} by applying the delta method or bootstrapping. Kling and Liebman (2004) show that the estimates that result from the delta method or bootstrapping are similar to those obtained under the assumption of known σ_{kc} in a study that evaluates the effects of the Moving to Opportunity program on youth outcomes. Based on their results and given the large sample size of our study, we treat σ_{kc} as known.

[25] As an alternative strategy, we also constructed aggregate outcomes by averaging across the standardized outcomes included in each category and estimated the effects of the proportion of repeaters on these aggregate outcomes. The results for these averaged outcomes (not reported here to save space) are virtually identical to the average effects for each category reported in Table 7. In practice, both methods provide identical estimates when there are no missing values in item responses and the model has no additional covariates besides the treatment variable.

(item 37 in Table A2, estimate -0.863, s.e. 0.404) and on the lack of respect between teachers and students (estimate. -0.773, s.e. 0.406). Such 'bad blood' between students and teachers is reflected also in the negative effect of repeaters on the overall relationship between teachers and students (estimate -0.838, s.e. 0.281). Lastly, we find that a higher proportion of repeaters in class has a detrimental effect on inter-students relationships but the effect is only marginally significant. The effect on the average of these items is -0.331 (s.e. 0.194). We can conclude that while repeaters have good relationships with their teachers, they seem to be crowding out teachers' attention to regular students.

Overall, the evidence in this section is strongly suggestive of the fact that lower quality instruction and crowding out of teachers' attention are among the reasons behind the overall negative impact of repeaters on their peers' test scores and matriculation rates, even though it is difficult to prove the link conclusively with the data at our disposal. Regardless, the results in this section are still quite informative: the effect of repeaters on instruction methods, disruption, and violence are very interesting outcomes in their own right, as exemplified by the numerous studies showing that parents are willing to pay for a better classroom environment (Hoxby, 1998a; Black, 1999; Kane, Riegg and Staiger, 2006, Cullen, Jacob and Levitt, 2006).

7. Conclusions

In this paper we have estimated the effects of being in school with a high proportion of low-ability peers on the outcomes of regular students and on the learning environment. We view our main contribution as twofold: first, we are able to proxy for peer ability using a variable that is strongly related to academic ability but that is determined before school entry, so that it is relatively unlikely to suffer from the reflection problem; second, by means of a unique survey on the schooling environment, we are able to explore the "black box" of the educational production function, and investigate the possible mechanisms that underlie the estimated peer effects.

We find that an unusually high concentration of low ability students lowers the academic achievements of regular students. In particular, this negative impact is concentrated among the students located at the left tail of the socio-economic distribution. The schooling environment survey reveals that a high proportion of low ability students has a significant detrimental effect on teachers' pedagogical practices, it raises sharply the level of disruption and violence within the classroom and it deteriorates teacher-student relationships. These results are quite striking, since low ability students generally report a *higher* level of satisfaction with their teachers' pedagogical practices and with the quality of teacher-student relationships. These findings, combined, suggest that one of the main channels through which low-achieving students negatively affect their peers is by diverting teacher attention from regular to struggling students. While we cannot of course rule out that there are direct spillover effects from low-

ability to regular students ("endogenous peer effects"), it is important to be aware that contextual peer effects are also quantitatively important.

Overall, our results enhance our understanding of the operation of peer effects in educational settings, and can have important implications for the design of many educational policies. Though the nature of interaction of repeaters with their peers and their teachers might be specific to the Israeli education system, high rates of repetition are observed in many developed countries. For example, in France as many as 45 per cent of pupils repeat at least one grade before the end of junior high school (Maurin, 2005) and in the US approximately 10 percent of young adults aged 16-19 report to have been retained at least once in their school career (NCES, 2006). In these countries, as in Israel, repeaters have, on average, low cognitive and non-cognitive skills and therefore it is necessary to understand how educational outcomes are affected by the interaction of these students with their peers.

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Figure 1. Monte Carlo Simulations for the Within School Standard Deviation in the Proportion of Repeaters in High Schools

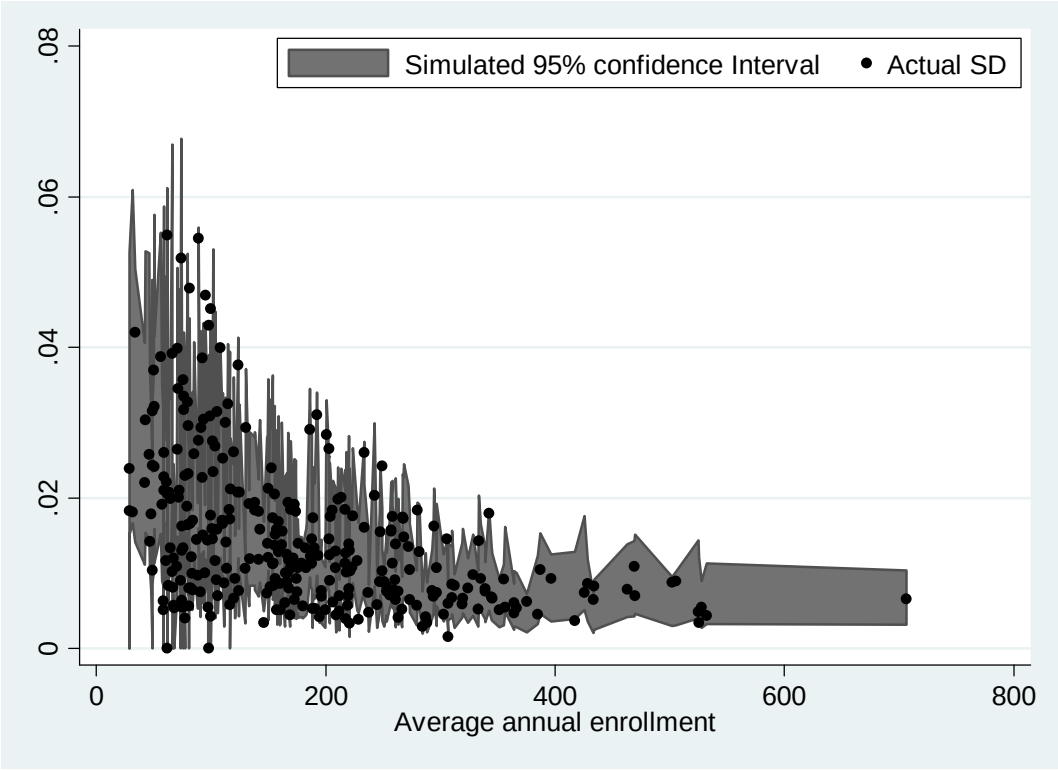

Table 1: Descriptive Statistics: Student's Achievements in High Schools

	Repeaters (1)	Low parental education (2)	Others (3)
Main matriculation outcomes			
Average score	49.7	59.8	69.0
Matriculation status	0.230	0.474	0.606
Number of credit units	12.4	18.0	21.4
Number of advanced level subjects in math and science	0.111	0.428	0.615
Matriculation diploma that meets university requirements	0.159	0.381	0.541
Enrollment in advanced classes			
Math	0.016	0.095	0.140
Physics	0.016	0.062	0.098
Computers	0.040	0.071	0.126
Biology	0.030	0.079	0.110
Chemistry	0.017	0.067	0.098
Number of students	13,814	12,768	363,713

Notes: The table reports descriptive statistics for students outcomes by group for the years 1994 through 2000. The sample includes all public Jewish high schools that have a matriculation track.

Table 2. Balancing Tests for the Proportion of Repeaters in High School

	Outcome means		Treatment: Proportion of Repeaters		
	Repeaters	Others	OLS	FE	Detrend
	(1)	(2)	(3)	(4)	(5)
Boy	0.589	0.472	0.910	0.022	-0.007
	[13.973]		(0.292)	(0.069)	(0.067)
Father's years of schooling	10.184	12.158	-30.080	-1.153	-0.556
	[-20.457]		(2.426)	(1.378)	(1.289)
Mother's years of schooling	10.200	12.255	-26.692	-0.523	-0.149
	[-21.480]		(2.272)	(1.435)	(1.367)
Number of siblings	2.839	2.406	4.017	1.227	0.099
	[12.557]		(1.225)	(0.738)	(0.576)
Immigrant	**	0.124	0.712	-0.481	-0.167
			(0.171)	(0.133)	(0.078)
Ethnic Origin:					
Israel	0.373	0.414	-1.549	0.107	-0.084
	[-6.235]		(0.180)	(0.100)	(0.073)
Asia or Africa	0.399	0.272	1.858	0.280	0.152
	[17.118]		(0.178)	(0.075)	(0.068)
Europe, the Americas or Oceania	0.176	0.190	-1.192	0.092	0.101
	[-2.443]		(0.108)	(0.048)	(0.049)
Ethiopia	0.052	0.010	0.318	-0.068	-0.049
	[6.377]		(0.067)	(0.030)	(0.031)
Soviet Union	**	0.103	0.607	-0.446	-0.165
			(0.164)	(0.132)	(0.073)
Other	**	0.012	-0.042	0.035	0.044
			(0.023)	(0.016)	(0.015)
Number of students	13,814	363,713			

Notes: the table reports means of the dependent variables in columns 1 and 2. T-statistics for repeaters differences in means from the other students, clustered at the school level, are reported in squared brackets. Columns 3-5 report OLS and school fixed effects estimates from separate regressions of the relevant dependent variable on the proportions of repeaters. All regressions include year dummies. Regressions in column 4 include also school fixed effects. Regressions in column 5 include school fixed effects and school specific linear time trends. Standard errors are adjusted for clustering at the school level.

**: By definition, immigrants are never repeaters

Table 3: Estimates of the Effects of the Proportion of Repeaters on Bagrut Outcomes

						Placebo regressions	
	Outcome means	Treatment effects: Proportion of Repeaters				In year t-1	In year t+1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average Score	68.995	-193.370	-20.908	-26.059	-11.183	-1.919	4.334
		(12.972)	(6.659)	(5.941)	(4.513)	(4.847)	(4.529)
Matriculation status	0.606	-4.638	-0.580	-0.542	-0.179	0.019	-0.016
		(0.261)	(0.142)	(0.122)	(0.098)	(0.089)	(0.089)
Number of credit units	21.441	-87.085	-13.315	-13.226	-4.695	-0.220	0.880
		(6.512)	(3.818)	(2.976)	(1.914)	(2.083)	(1.897)
Number of advanced level subjects in science	0.615	-6.941	-0.685	-0.617	-0.177	-0.032	0.164
		(0.493)	(0.205)	(0.182)	(0.129)	(0.122)	(0.119)
Matriculation diploma that meets university requirements	0.541	-5.275	-0.476	-0.473	-0.148	0.031	-0.006
		(0.279)	(0.132)	(0.113)	(0.077)	(0.088)	(0.078)
Year Fixed-Effects		✓	✓	✓	✓	✓	✓
School Fixed Effects			✓	✓	✓	✓	✓
Enrollment (2nd Poly.)				✓	✓	✓	✓
Individual Pupil Controls				✓	✓	✓	✓
Cohort Mean Controls				✓	✓	✓	✓
School Time Trend					✓	✓	✓
Number of students	363,713						
Number of schools	310						

Notes: The table reports means of the dependent variables (columns 1), OLS (columns 2 and 6) and school fixed effects (columns 3-5 and 7-9) estimates of the effects of the proportions of skipping and repeating students in a grade on their peers' achieveme

Table 4: Estimates of the Effects of Proportion of Repeaters on Matriculation Outcomes Estimated Separately for Students with Low and High Educated Parents

	Outcome means		Proportion of Repeaters	
	Low Educ. Parents	High Educ. Parents	Low Educ. Parents	High Educ. Parents
	(1)	(2)	(3)	(4)
Average Score	63.1	73.1	-13.670	-9.830
			(6.834)	(4.780)
Matriculation status	0.468	0.701	-0.268	-0.082
			(0.128)	(0.112)
Number of credit units	18.581	23.423	-6.902	-2.814
			(2.719)	(2.192)
Number of advanced level subjects in math and science	0.356	0.795	-0.239	-0.128
			(0.126)	(0.205)
Matriculation diploma that meets university requirements	0.377	0.654	-0.234	-0.045
			(0.096)	(0.110)
Number of students	148,851	214,862		
Number of schools	310	310		

Notes: The table reports means of the dependent variables (columns 1 and 2) and school specific time trends estimates (columns 3-6) of the effects of the proportion of skipping and repeating students on matriculation outcomes. Columns 3 and 5 report the estimates on students with low-education parents (one of their parents has less than 12 years of schooling). Columns 4 and 6 report the estimates on students with high-education parents (both their parents have at least 12 years of schooling). The regressions control for students background characteristics and school time varying controls detailed in table 5. The regressions include school and year fixed effects and school specific time trends and control also for a quadratic function of enrollment. Robust standard errors clustered at the school level are reported in parentheses.

Table 5: Estimates of the Effects of the Proportions of Repeaters on Bagrut Outcomes by Groups Defined on Enrollment in Advanced Science Courses

	All sample		0 advanced courses		2+ advanced courses		3+ advanced courses		0 advanced course, restricted sample of schools	
	Outcome means	Prop. of Repeaters	Outcome means	Prop. of Repeaters	Outcome means	Prop. of Repeaters	Outcome means	Prop. of Repeaters	Outcome means	Prop. of Repeaters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Effect on Probability to Enroll in Advanced Science Courses				0.022		-0.030		-0.021		0.044
				(0.081)		(0.053)		(0.033)		(0.106)
Outcomes:										
Average Score	68.995	-11.183	62.910	-12.525	84.347	-0.032	86.434	-2.257	63.752	-12.670
		(4.513)		(5.375)		(4.140)		(5.617)		(6.582)
Matriculation status	0.606	-0.179	0.460	-0.216	0.953	0.104	0.972	0.048	0.479	-0.262
		(0.098)		(0.109)		(0.122)		(0.168)		(0.143)
Number of credit units	21.441	-4.695	18.109	-5.804	29.790	1.309	31.402	2.333	18.279	-6.121
		(1.914)		(2.126)		(2.741)		(3.526)		(2.991)
Number of advanced level subjects in science	0.615	-0.177	0.125	-0.059	2.051	-0.407	2.320	-0.499	0.134	-0.100
		(0.129)		(0.060)		(0.327)		(0.446)		(0.080)
Matriculation diploma that meets university requirements	0.541	-0.148	0.377	-0.184	0.942	0.141	0.968	0.090	0.405	-0.236
		(0.077)		(0.079)		(0.138)		(0.173)		(0.112)
Number of students in group	363,713		243,321		60,506		24,683		202,183	
Number of schools in group	310		310		276		235		235	

Notes: The table reports means of the dependent variables (columns 1,3,5,7,9) and estimates (other columns) of the effects of the proportion of repeaters on matriculation outcomes. Columns 1-2 report the estimates on the whole sample. Columns 2-3 report the estimates for students who were not enrolled in any advanced science course. Columns 4-5 report the estimates for students enrolled in least one advanced science course. Columns 5-6 report the estimates for students enrolled in at least two courses and columns 7-8 report the estimates for students enrolled in at least three. The regressions control for students background characteristics and school time varying controls detailed in table 3. The regressions include school and year fixed effects and school specific time trends and control also for a quadratic function of enrollment. Robust standard errors clustered at the school level are reported in parentheses.

Table 6. Balancing Tests for the Proportions of Repeaters in Secular Middle Schools (7^{th} through 9^{th} grades)

	Outcome means		Proportion of Repeaters	
	Repeaters	Others	OLS	School fixed effects
	(1)	(2)	(3)	(4)
Male	0.648	0.496	0.089	0.126
	[20.123]		(0.073)	(0.078)
Father's years of schooling	11.921	13.007	-13.158	-0.679
	[-20.441]		(2.339)	(0.728)
Mother's years of schooling	12.095	13.183	-11.738	-0.750
	[-22.329]		(2.159)	(0.695)
Number of siblings	2.461	2.212	2.540	-0.194
	[10.485]		(0.803)	(0.542)
Immigrant	**	0.140	0.331	-0.071
			(0.235)	(0.064)
Ethnic origin from Israel	0.489	0.498	-0.732	-0.041
	[-1.290]		(0.262)	(0.071)
Ethnic origin from Asia or Africa	0.258	0.180	0.754	0.032
	[10.392]		(0.185)	(0.059)
Ethnic origin from Europe, the Americas or Oceania	0.227	0.174	-0.386	0.075
	[6.315]		(0.155)	(0.061)
Ethnic origin from Ethiopia	0.026	0.014	0.063	0.005
	[4.725]		(0.042)	(0.023)
Ethnic origin from the former Soviet Union	**	0.120	0.347	-0.061
			(0.229)	(0.062)
Immigrant from country other than Ethiopia or former Soviet Union nations	**	0.014	-0.046	-0.010
			(0.025)	(0.018)
Number of students	8,541	240,506	240,506	
Proportion of students in grade	0.0357			

Notes: The table reports means of the dependent variables in columns 1-2. The numbers in brackets in columns 1 and 2 are the t-statistics (clustered at the school level) for the difference in the mean of the relevant variable between skippers/repeaters and the other students. Columns 4 and 5 report OLS and school fixed effects estimates of the proportions of repeaters on students background characteristics, where the repeaters themselves are excluded from the sample. The regressions control for grade and year fixed effects. Standard errors clustered at the school level are reported in parentheses.

**: By definition, immigrants are never repeaters

Table 7. Effects on the Learning and Classroom Environment in Secular Middle Schools
(7th through 9th grades)

		Repeaters relative to others		
		Raw diffs (1)	Controlled diffs. (2)	Treatment effects: Proportion of repetares (3)
Pedagogy				
1	Instilment of knowledge and enhancement of comprehension	0.025	0.038	-0.542
		(0.009)	(0.009)	(0.186)
2	Instilment of analytical and critical skills	0.006	0.014	-0.204
		(0.004)	(0.004)	(0.068)
3	Transparency, fairness and feedback	0.042	0.029	-0.410
		(0.010)	(0.009)	(0.193)
4	Individual treatment of students	0.154	0.124	-0.417
		(0.009)	(0.008)	(0.173)
5	Instilment of capacity for individual study	0.111	0.109	-0.634
		(0.009)	(0.009)	(0.201)
Classroom environment				
6	Discipline and lack of violence	-0.048	-0.016	-0.409
		(0.007)	(0.007)	(0.158)
7	Student-teacher relationships	0.065	0.077	-0.622
		(0.009)	(0.008)	(0.230)
8	Inter-student relationships	-0.025	-0.025	-0.331
		(0.010)	(0.010)	(0.194)

Notes: Columns 1 and 2 report controlled differences between repeaters or skippers and the other students on their views on the classroom environment. The estimates are from regressions that control for year and grade effects. In addition, regressions in column 2 control for individual background characteristics and include grade, year and school-grade-year fixed effects. Columns 3 report school fixed effects estimates of the proportion of repeaters on classroom environment. The estimates are for the average effects of the individual items reported in table A4. The regressions control for student background characteristics (a female dummy, both parents' years of schooling, number of siblings, immigration status, ethnic origin and indicators for missing values in these covariates), cohort mean characteristics (students individuals controls averaged by school and year), a quadratic function of enrollment, year and grade dummies, and school fixed effects. Robust standard errors clustered at the school level are reported in parentheses.

Table A1: Estimates of the Effects of Proportions of Repeaters on Test Scores of 8th Graders in Secular Middle Schools

	Outcome means		Treatment effects: Proportion of repeaters		
	Repeaters	Others			
	(1)	(2)	(3)	(4)	(5)
Math	-0.666	0.018	-4.225	-1.162	-1.075
	[-30.751]		(0.641)	(0.668)	(0.665)
Science and Technology	-0.568	0.017	-2.670	-0.606	-0.388
	[-22.383]		(0.602)	(0.751)	(0.748)
Hebrew	-0.646	0.021	-3.275	-1.057	-0.968
	[-28.695]		(0.595)	(0.629)	(0.628)
English	-0.676	0.018	-4.326	-0.756	-0.597
	[-28.800]		(0.663)	(0.524)	(0.530)
Mean of Four Subjects			-3.662	-0.937	-0.812
			(0.561)	(0.519)	(0.528)
Common Time Trend			✓	✓	✓
School Fixed Effects				✓	✓
Enrollment (2nd Poly.)				✓	✓
Individual Pupil Controls					✓
Cohort Mean Controls					✓
Number of students	3,475	96,763			
Proportion of students in grade	0.0350				
Number of schools		354			

Notes: The table reports means of the dependent variables in columns 1 and 2. T-statistics for repeaters and skippers differences in means from the other students, clustered at the school level, are reported in squared brackets. Column 3 report OLS and Columns 4 and 5 report school fixed effects estimates of the proportion of repeaters on students standardized tests scores in 8th grade. Repeaters themselves are excluded from the sample. Individual controls include: a female dummy, both parents' years of schooling, number of siblings, immigration status, and ethnic origin. Cohort mean controls include students individual controls averaged by school and year. Robust standard errors clustered at the school level are reported in parentheses.

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Table A2: Estimates of the Effects of Proportion of Repeaters on the Learning and Classroom Environment in Secular Middle Schools (7th through 9th grades)

		Controlled difference in means relative to regular students	Treatment: Proportion of repeaters
		(1)	(2)
Instilment of knowledge and enhancement of comprehension			
1	The teachers give exercises and assignments that help memorize the material	0.047 (0.013)	-0.676 (0.240)
2	The teachers ask many questions in class that check whether we know the material well	0.047 (0.013)	-0.756 (0.247)
3	The teachers commend students who know the material well	0.054 (0.014)	-0.364 (0.239)
4	The teachers provide many examples that help understand the material	0.071 (0.013)	-0.668 (0.234)
5	The teachers hold discussions in class that help understand the material	0.063 (0.013)	-0.525 (0.229)
6	During lessons, the teachers ask many questions that check whether we understand the material well	0.028 (0.013)	-0.581 (0.245)
7	I understand the teachers' scholastic requirements well	-0.043 (0.013)	-0.194 (0.217)
Instilment of analytical and critical skills			
8	The teachers give exercises and assignments whose answers have not been studied in class and are not in the textbooks	-0.016 (0.019)	0.734 (0.312)
9	The teachers require that we use what we have studied to explain various phenomena	0.043 (0.012)	-0.270 (0.221)
10	The teachers ask that we find new examples by ourselves for the material we have studied	0.088 (0.019)	-0.415 (0.287)
11	The teachers ask that we try to find several ways to solve a certain problem	0.058 (0.013)	-0.544 (0.186)
12	The teachers teach us to find a single common explanation for different phenomena	0.075 (0.017)	-0.559 (0.263)
13	The teachers give assignments where it is required to analyze material and to relate it to other things we have studied	0.054 (0.013)	-0.291 (0.217)
14	When there are several ways to solve a problem, the teachers require that we check them all and find the best one	0.085 (0.012)	-0.448 (0.218)
15	The teachers expect us to ask ourselves whether what we have learned is correct	0.180 (0.018)	-0.109 (0.322)
16	The teachers teach us how to know whether information we have found is important, relevant and can be used	0.104 (0.013)	-0.521 (0.208)

Note: Column 1 reports the controlled differences between repeaters and other students on their reports about the classroom environment. The estimates come from regressions that control for the full set of individual covariates detailed in table 11 and include also year, grade, school-grade-year fixed effects. Column 2 reports school fixed effects estimates of the proportions of repeaters on classroom environment where repeaters themselves are excluded from the sample. The regressions control for students background characteristics and school time varying controls detailed in table 11. The regressions also include school fixed effects and grade and year dummies. Robust standard errors clustered at the school level are reported in parentheses.

Table A2: Estimates of the Effects of Proportion of Repeaters on the Learning and Classroom Environment in Secular Middle Schools (7th through 9th grades)

		Controlled difference in means relative to regular students	Treatment: Proportion of repeaters
		(1)	(2)
Transparency, fairness and feedback			
17	The teachers explain to me exactly what I have to do to improve my studies	0.112 (0.013)	-0.561 (0.219)
18	The teachers explain according to what they determine the grades / assessments	-0.059 (0.012)	-0.288 (0.260)
19	The teachers often tell me what my situation is regarding schoolwork	0.033 (0.012)	-0.362 (0.239)
Individual treatment of students			
20	The teachers know what the educational difficulties of each student are	0.080 (0.013)	-0.622 (0.236)
21	When a student has difficulty with a certain topic the teachers give him more time to study it	0.122 (0.012)	-0.367 (0.219)
22	The teachers give every student homework according to his place in the material	0.224 (0.015)	-0.086 (0.209)
23	The teachers help every student to learn topics interest him	0.176 (0.014)	-0.163 (0.202)
24	The teachers give me a feeling that if I make an effort I will succeed more at studies	0.090 (0.013)	-0.556 (0.236)
25	When a student fails, the teachers encourage him to try again and again	0.097 (0.012)	-0.537 (0.238)
26	The teachers always assist me when I need help with studies	0.082 (0.012)	-0.542 (0.247)
Instilment of capacity for individual study			
27	The teachers teach us how to learn new topics by ourselves	0.124 (0.012)	-0.574 (0.214)
28	The teachers require students to utilize many and varied sources of information (newspapers, books, databases etc.)	0.073 (0.012)	-0.800 (0.272)
29	The teachers teach us to observe our environment and to follow phenomena that occur in it	0.130 (0.013)	-0.509 (0.228)

Note: Column 1 reports the controlled differences between repeaters and other students on their reports about the classroom environment. The estimates come from regressions that control for the full set of individual covariates detailed in table 11 and include also year, grade, school-grade-year fixed effects. Column 2 reports school fixed effects estimates of the proportions of repeaters on classroom environment where repeaters themselves are excluded from the sample. The regressions control for students background characteristics and school time varying controls detailed in table 11. The regressions also include school fixed effects and grade and year dummies. Robust standard errors clustered at the school level are reported in parentheses.

Table A2: Estimates of the Effects of Proportion of Repeaters on the Learning and Classroom Environment in Secular Middle Schools (7th through 9th grades)

		Controlled difference in means relative to regular students	Treatment: Proportion of repeaters
		(1)	(2)
Violence and discipline			
30	I know what behavior is allowed or forbidden in school	0.033 (0.012)	-0.436 (0.217)
31	Student discipline is strictly maintained at school	0.058 (0.013)	-0.695 (0.267)
32	The classroom is frequently noisy and non-conducive to learning	-0.031 (0.015)	0.617 (0.333)
33	Students are frequently late or truant	-0.012 (0.017)	0.087 (0.385)
34	There are many fights among students in my class	0.057 (0.018)	0.784 (0.449)
35	I was involved in violence many times this year (physical fights)	0.107 (0.016)	0.355 (0.248)
36	Sometimes I am scared to go to school because there are violent students	0.123 (0.016)	0.318 (0.243)
Student-teacher relationships			
37	Students are frequently rude to the teachers	-0.010 (0.016)	0.863 (0.404)
38	Sometimes the teachers treat me in an insulting or hurtful way	-0.090 (0.021)	0.733 (0.406)
39	There are good relationships between the teachers and the students	0.117 (0.012)	-0.812 (0.302)
40	There is mutual respect between the teachers and the students	0.108 (0.013)	-0.838 (0.281)
41	When I have a problem I have whom to turn to at school (teachers, advisor)	0.102 (0.011)	-0.324 (0.281)
Inter-student relationships			
42	I feel well adjusted socially in my class	-0.104 (0.013)	-0.112 (0.197)
43	Students in my class help each other	0.054 (0.011)	-0.520 (0.253)
44	I am generally well off at school	-0.023 (0.013)	-0.348 (0.236)

Note: Column 1 reports the controlled differences between repeaters and other students on their reports about the classroom environment. The estimates come from regressions that control for the full set of individual covariates detailed in table 11 and include also year, grade, school-grade-year fixed effects. Column 2 reports school fixed effects estimates of the proportions of repeaters on classroom environment where repeaters themselves are excluded from the sample. The regressions control for students background characteristics and school time varying controls detailed in table 11. The regressions also include school fixed effects and grade and year dummies. Robust standard errors clustered at the school level are reported in parentheses.