Inside the Black of Box of Ability Peer Effects: Evidence from Variation in the Proportion of 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.

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1. Introduction

This paper studies peer effects in educational outcomes in middle schools and high 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 school 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.

The paper makes two contributions to the literature on peer effects. The first part of the paper explores how classroom ability composition, as measured by the proportion of repeaters in the grade, affects scholastic achievements of high school students, as measured by performance in the matriculation exams completed by the end of 12^{th} grade .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 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.

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 low ability is not determined randomly; rather, repeater status is correlated with low socioeconomic background and therefore repeaters are typically clustered in low-achieving schools.[2] In order to overcome this selection problem, we exploit idiosyncratic variations in the proportion of repeating students across adjacent cohorts within the same schools.

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[1] The term "repeater" is 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.

[2] We later show that in Israel repeater status is strongly negatively correlated with socioeconomic status. This is in contrast to the U.S., for example, where "redshirting" (i.e., holding back children one more year before they enter kindergarten), is mostly a high socioeconomic status phenomenon (Deming and Dynarski, 2008; Dobkin and Ferreira, 2009; Elder and Lubotsky, 2009).

By using multiple cohorts and conditioning on school fixed-effects and school specific time trends we are able to control for unobserved factors that might confound the repeater peer effect 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, which measure the key treatment variable not in the cohort of interest, but in adjacent cohorts within the same school.

A second difficulty in the estimation of peer effects 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 peers' 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 (2009) for several European countries. Similarly to us, these papers rely on differences in the composition of cohorts within a school, which come about by chance. [3] 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. 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. On the other hand, the alternative approach that measures peer quality directly using lagged academic achievements has its own shortcomings: since a student's peer group is usually constant during his or her time in school if not for mobility-related reasons, lagged peer achievement is unlikely to be exogenous to own current achievement and therefore it may still suffer from the reflection problem.

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.

[3] 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).

Our 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 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. These findings 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.

The rest of the paper is organized as follows. The next section reviews the relevant recent literature on peer effects. The following section describes the data and the construction of the analysis samples, and explains the definition of the treatment variable and identification strategy. Section 4 discusses the validity of the underlying assumptions required for identification, 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 the achievement of their peers. 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. Most models of the educational production function used to analyze the effects of different school policies (school choice, subsidies, tracking, etc.) assume some form of peer effects, which can take on a linear-in-means form (Epple and Romano, 1997), or be highly nonlinear (Lazear, 2001). 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. Hoxby (2000), Ammermueller and Pischke (2009), Burke and Sass (2006), Lavy and Schlosser (2007) and Bifulco et al., (2009) measure peer quality by fixed student characteristics and identify the effects of peers by exploiting within-school variation in

these characteristics across cohorts. A variation in this approach is present in Aizer (2008) and Carrell and Hoekstra (2008), who study specifically the effects of students with a high potential of being disruptive (students with ADD or exposed to domestic violence) on their classmates' outcomes. Lefgren (2004) and Gibbons and Telhaj (2008) use lagged outcome measures as the key peer characteristic. Cooley (2008) exploits the introduction of student accountability policies in North Carolina, which induced low achieving students to increase studying efforts to estimate behavioral spillovers effects of peers. Boozer and Cacciola (2001) and Duflo et al., (2008) exploit variation in peer characteristics generated by actual randomization. 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). A non-exhaustive summary of the recent literature is given in Gibbons and Telhaj (2008). They conclude that peer effects, even when statistically significant, are rather small: most studies find that a one standard deviation increase in peer quality raises outcomes by less than 10 percent of a standard deviation.

Finally, our paper is also related to the recent literature on the effects of age at school entry on educational and labor market outcomes. Most studies find that being relatively older and more mature when entering the compulsory school system has positive effects on early academic performance (Datar, 2006; Bedard and Dhuey, 2006; Elder and Lubotsky, 2009) although the evidence on long-term outcomes is more mixed.[4] Almost none of these studies, however, investigate the effects of late school entrants on their peers, which is the focus of the current paper.[5] 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. Data and Empirical Strategy

In this section, we first briefly describe the high school data that we use to estimate the effect of the proportion of low-ability students on high school outcomes. We then describe the construction of our

[4] Fredrikkson and Öckert (2005), Bedard and Dhuey (2006), and Puhani and Weber (2007) find that a higher age at school entry is beneficial for long-term outcomes; Cascio and Whitmore Schanzenbach (2007) find no effects; Black, Devereux, and Salvanes (2008), Dynarski and Deming (2008) and Dobkin and Ferreira (2009) find small negative effects of later school entry, operating mostly through higher dropout rates because of compulsory schooling laws. This was the mechanism originally argued for by Angrist and Krueger (1992).

[5] The only exception is Elder and Lubotsky (2009), who find that having relatively more advanced peers increases the probability of grade repetition and diagnoses of learning disabilities.

key treatment variable and explain why we believe it is a good measure of the proportion of low-ability students. Finally, we present the econometric specification and discuss the identification strategy.

A. 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. We use 10th grade to define the base population because it is the first year of high school and the last year of compulsory schooling. We limit the analysis to all Jewish state-run schools. The sample is restricted to students in non-special education schools that have a matriculation track.[6] 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. Each student 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.

Israeli high school students study during upper secondary schooling towards the matriculation certificate (*Bagrut* in Hebrew).[7] The *Bagrut* is completed by passing a series of national exams in core and elective subjects taken by the students between 10th and 12th 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 four or five 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

[6] This excludes about 15 percent of Jewish students who attend ultra-orthodox schools, yeshivot, and other independent schools and schools that offer low tech vocational programs, some of which allow also students to work part time.

[7] 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.

the diploma).[8] We also constructed indicator variables for student enrollment in advanced courses in math, physics, computer science, biology, and chemistry.

B. Empirical Approach and Definition of the Treatment Variable

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 proportion of low ability students.

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. 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 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.

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).[9] Virtually all students enroll in 1st grade after attending a public kindergarten at age 5; kindergarten is free, compulsory and universally provided by the local authorities and regulated by the Ministry of Education.[10] Therefore, the transition to 1st grade is mostly supervised by the Ministry of Education and the Municipality. However, parents have some discretion in deciding when to send their children to first grade. Parents of a gifted

[8] 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. Results are not sensitive to dropping these students from the sample. 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.

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

[10] The enrollment rate of Jewish children aged 5 at public kindergartens is 98 percent (Central Bureau of Statistics, 2008).

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 is relatively simple: all that is needed is a written request by the parents, accompanied by a letter from the kindergarten teacher in support of delayed entry. In contrast, the decision to delay entry into first grade for children born before September 1st is closely regulated and supervised by the Ministry of Education. The Ministry protocol specifies that the kindergarten psychologist and teachers are in charge of identifying those children who are not ready for entry into first grade and who would rather spend an extra year in kindergarten. Parents are advised to take their child to an external educational psychologist to evaluate the case, diagnose the child outside the kindergarten environment, and write an independent report. The retention decision is then taken jointly by the kindergarten psychologist, the teachers, and parents, and in case of disagreement the final word is reserved to the parents.[11]

We therefore focus on 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, and define them as "repeaters." To the extent that these children repeated kindergarten, we are focusing on children who are identified both by the school and the external educational psychologist as having some cognitive and non-cognitive deficiencies. 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.[12] 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.

Even though we cannot ascertain that all "repeaters" were held back in kindergarten we know that grade repetition is highly unlikely at higher grades.[13] In fact, since the early 1970's repetition was

[11] For more information about this process, see the Ministry of Education regulations (in Hebrew) at : http://cms.education.gov.il/EducationCMS/applications/mankal/arc/se9ak3_10_11.htm.

[12] 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 also Deming and Dynarski, 2008).

[13] 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 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

abandoned in Israel as a pedagogical tool in all the compulsory schooling grades (until 10th grade). The number of repeaters has declined since then dramatically and repetition in primary and middle school has become a very rare phenomenon. It is therefore not surprising that the most recent Ministry of Education documents that discuss repetition in primary school and middle school were published in 1963 and in 1971, respectively, and that both documents are no longer in effect.[14]

Since we do observe in our data some grade repetition in primary and middle schools, we inquired about the guidelines for making such decisions by interviewing the Ministry of Education's Head Superintendent and other officials at the Jerusalem school authority. The essence of the information we learned from these officials is that repeating a grade is a very unusual event and should be approved by the Central Authorities at the Ministry of Education.[15] We infer from this information that the decision of grade retention is unlikely to be determined at the school level and therefore also unlikely to be potentially correlated with unobserved determinants of learning by other pupils in the grade.

Table 1 shows mean outcomes for repeaters and regular high school students.[16] 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 students with low parental education. For example, on average for the whole sample period, 60.6% of regular students in the sample were awarded a matriculation

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of repeaters were already in this status in elementary school with 60 percent being held back in kindergarten or first grade.

[14] The most recent regulations at the post compulsory level (after the completion of 10th grade) are specified in Ministry of Education publication from December 1994, and specify that grade retention should be based only on academic considerations and not on pupil's discipline and behavior. The decision is determined by a pedagogical council that includes all the pupil's teachers, the school educational counselor and the school head master. The council decision should depend on the number of courses the pupil failed, his exact grades in each failing course, the level of these courses, and whether the transition considered is to 11th or to 12th grade. However, these guidelines allow the council a degree of discretion to deviate from the specified rules in cases of special personal circumstances of the pupil such as new immigrants, parental recent chronic illness, death or divorce or other unusual personal crisis. For more information about this process, see the Ministry of Education regulations, Special Director General Circular, December 1994 (in Hebrew).

[15] The guidelines described to us by the Ministry staff are that the school's pedagogic council has to discuss and approve every case for grade repetition. The school headmaster, the school educational counselor, the psychologist and the home class teacher have each to present to the council the relevant learning, emotional, and family background aspects of the case at hand and explain why they think that repeating the grade would be beneficial to the pupil. The council makes a decision and forwards it to the Ministry of Education District superintendent who reviews the evidence and the various recommendations, and must then approve or reject the recommendation. The same guidelines apply for skipping grades. Contrary to this process, the rules that were in effect until the early 1970's gave the school's pedagogic council the authority to decide on these matters.

[16] For a more detailed description of the high school data and the outcome variables, see Section 4.

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 use low socioeconomic status as a proxy for low-achievers

C. Econometric specification

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. 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 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 a higher proportion of low-ability peers on student's achievement.

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 low achieving students.[17] 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.

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.

For equation (1) to yield valid causal estimates of the parameter of interest, the key identifying assumption is that cohort-to-cohort variation in the proportion repeaters is random within school. This assumption is fairly reasonable for a number of reasons. First, our analysis is mostly centered on high school and middle school students: these cohorts have inherited a given proportion of repeaters from the feeder schools at the lower level. As we explained before, repeater status is determined very early, often at the time of elementary school entry, and the decision is taken independently of the staffing and academic needs of the school. Therefore, it is unlikely that, within a school, the proportion of repeaters in a cohort would be correlated with unobserved determinants of ability of that cohort.

Second, it is important to note that our key identifying assumption would hold as long as the threshold for determining repeater status is fixed, and any year-to-year shocks in the density of ability below the threshold within a school are uncorrelated with shocks to the density above the threshold. In

[17] 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.

other words, we require that the proportion of repeaters does not change because the entire ability distribution within schools shifts from year to year, or because the threshold for determining repeater status changes from year to year at the school level. Since the decision of grade retention is mostly centralized and involves the intervention of external professionals who evaluate the student independently of the school environment, this assumption is very likely to hold. Notice also that, even if there were a time-varying threshold at the school level, it would induce a correlation between the proportion of repeaters in cohort *t,* and the ability distribution of students in cohort *t-1*, the cohort to which repeaters originally belonged. Therefore, we can test for violation of the key identifying assumption by looking at the correlation between the actual proportion of repeaters in cohort *t* and outcomes of regular students in cohort *t-1*.

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

A. What is the source of variation in the proportion of repeaters?

The identification strategy outlined in the previous section 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. 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 where we randomly generated the proportion of repeaters in each cohort and compared the simulated within school standard deviation with the standard deviation observed in the data.[18] .We also computed an empirical confidence interval for the

[18] 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

standard deviation in the proportion of repeaters for each school finding that 93% percent of the high schools had a standard deviation in the proportion of repeaters that fell within the 95% confidence interval.[19] 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.

B. 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 proportion of repeaters 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.[20] 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.[21] The estimates of the effects of the proportion repeaters on the likelihood of leaving the initial high school

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computed the within school standard deviation of the proportion repeaters based on the simulated data and repeated this process 1,000 times.

[19] We computed within school standard deviations using residuals from a regression of the proportion of repeaters on school fixed effects and school specific time trends.

[20] 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.

[21] 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).

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

C. 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.

The OLS estimates show strong negative associations between the proportions of repeaters and student background characteristics. These correlations show unambiguously that in Israel repeater status is strongly negatively correlated with socioeconomic status, in contrast with the United States, where "redshirting" is more common among high socioeconomic status families (see, e.g., Dobkin and Ferreira, 2009).[22] 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, a relatively

[22] We also did not find any evidence of bimodality, with some repeaters having high socioeconomic status, and others low socioeconomic status. Rather, the entire distribution of parents' education for repeaters is shifted to the left relative to the distribution of parents' education for regular students.

disadvantaged group in Israel, but it is also positively associated with a change in the proportion of students from American or European origin, 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 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.[23]

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.[24] 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

[23] The individual-level control variables are: a gender dummy, father's and mother's years of schooling and indicators for missing parents' years of schooling, number of siblings, a dummy for post 1989 immigrants, and ethnic origin dummies. The grade-level control variables are: enrollment and its square, the percentage of boys, average parental education, average number of siblings, percentage of immigrants, and percentage belonging to each ethnic group.

[24] The control variables enter the regression with the predicted signs: females, children of highly educated parents, and children from smaller families have better educational outcomes; children of Asia-Africa origin and immigrants have worse educational outcomes; and high socioeconomic status peers are associated with positive outcomes. The individual-level and grade-level controls on their own explain about 12% of the overall variation in educational outcomes.

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. [25] These negative effects are, however, moderate. For example, a one standard deviation increase (i.e., a 0.032 increase) in the proportion of repeaters leads to a decrease of between 0.012 and 0.036 of a standard deviation in the matriculation rate, and to a decrease of between 0.015 and 0.036 of a standard deviation in the average score of regular students.[26] These effects are on the lower end of the range reported in Gibbons and Telhaj (2008), but are not all that unusual. For comparison purposes, a one standard deviation increase in the proportion of boys in a grade reduces the matriculation rate by between 0.038 and 0.050 of a standard deviation, while a one standard deviation increase in mother's education raises matriculation rates by about 0.12 of a standard deviation.

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

[25] 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 the transition to elementary school, 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 no significant differences in the treatment effect by age of repeaters.

[26] The standard deviation in the proportion of repeaters is calculated using one observation per school-year.

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. The absence of correlation between the proportion of repeaters in cohort *t+1* and the outcomes of regular students in cohort *t* can also be interpreted as evidence that variation in the proportion repeaters is not due to a shift (within schools) in the threshold determining repeater status.

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 equation (1) separately for the two groups reporting the results in Table 4.

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 one standard deviation increase in proportion repeaters reduces the number of credit units among students with low parental education by 0.224 units, or 0.022 of a standard deviation; and reduces the matriculation rate by 0.9 percentage points, or 0.017 of a standard deviation. On the other hand, we find substantially smaller and mostly insignificant effects of the proportion repeaters on the outcomes of students with highly educated 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 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 effects 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.[27] 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.[28]

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 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 Effect of Proportion Repeaters on Middle School Test Scores

In the next section we discuss the mechanisms through which low ability students affect the outcomes of their peers using a unique survey on teachers' pedagogical methods and the classroom environment administered to all middle school students. Here we investigate whether a high proportion of repeaters affects the test scores of middle school students using information on national test scores of 8th graders in math, science, Hebrew and English. The data is based on the GEMS (Growth and Effectiveness Measures for Schools - *Meizav* in Hebrew) datasets for the years 2002-2005. The GEMS includes exams to 8th graders in four subjects and a student questionnaire provided to all students from 7th through 9th

[27] 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.

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

grade (described in the next section) and is administered by the Division of Evaluation and Measurement of the Ministry of Education at the mid-term of each school year to a representative 1-in-2 sample of all elementary and middle schools in Israel, so that each school participates in GEMS once every two years.[29] 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.

We linked the test score data 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. We dropped from the sample all religious schools due to the high instability of the student population and the measurement error in the treatment variable in these types of schools at the middle school level.[30]

The samples we have for middle schools test scores pool together only two cohorts of 8th grade students. 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.[31]

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 one standard deviation increase (i.e., a 0.031 increase) in the proportion of repeaters, reduces test scores in math and Hebrew by 0.03 of a standard deviation, and in English by 0.02 of a standard deviation. The magnitude of these effects are very much in line with what we obtained for the high school sample although they are less precisely estimated since we only have two observations per school. 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.

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[29] 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*.

[30] 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.

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

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 within 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 students' responses to the GEMS questionnaire. We first describe the middle school questionnaire and, in similar fashion to the high school data, we report results from balancing tests showing that changes in the proportion of repeaters are uncorrelated with changes in student's background characteristics within middle schools. We then show striking differences in behavior and perception of the classroom environment between repeaters and regular students and report the effects of the proportion of repeaters on on teachers' pedagogical methods and on the classroom environment.

A. The middle school data

The student questionnaire is administered to all students from grades 7th through 9th and includes several questions addressing various aspects of the school and the learning environment. We concentrate on two sections of the questionnaire which focus on 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.

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 (see Bloom, 1956). 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.

Similar to the procedure done for the test score data, we linked the student questionnaire with student administrative records to obtain information in student's background characteristics and peer composition. We also restrict the sample to secular students by dropping all religious schools for the reasons described in footnote 30. In addition, 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 (7^{th}, 8^{th} and 9^{th}) at least two years. We therefore have six observations of the same school for middle schools (7^{th}, 8^{th} and 9^{th} grade for two years). Since we have only two observations per grade and school in this dataset, we pool all grades and years and exploit within school variation in the proportion of repeaters across grades and years to gain more variability in the treatment variable while controlling for year, school, and grade fixed effects.

B. Balancing Tests

We begin our analysis of the effect of repeaters on teaching methods and classroom environment by presenting balancing tests of the covariates for the sample of 7^{th} to 9^{th} graders to whom the student questionnaire was administered. The results of these tests are presented in Table 6 where we report the estimated coefficients from regressions of student's background characteristics on the proportion of repeaters in their cohort. 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. Overall, 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 still control for the full set of covariates to increase the precision of our estimates.[32]

As with the middle school test score data, we cannot control for school specific time trends since we have only two observations per grade and school. Nevertheless, given that the balancing tests show no associations between student's covariates and the proportion of repeaters in the school fixed effects specification, we feel pretty confident that this specification is powerful enough to control for possible confounders. Similar to the tests conducted with the high school data, we also performed falsification tests where we replace the treatment variable with the proportion of repeaters in the previous or following

[32] The individual-level covariates include: a gender dummy, mother's and father's years of schooling, number of siblings and indicators for missing values in these covariates, a dummy for post 1989 immigrants, and ethnic origin dummies. The grade-level control variables are: enrollment and its square, the percentage of boys, average parental education, average number of siblings, percentage of immigrants, and percentage belonging to each ethnic group.

grade. The results show no cross-cohort effects for the proportion of repeaters in school suggesting that our results are not being confounded by other short-term school shocks.

C. Differences between Repeaters and Regular Students in the Assessment of Teachers and Classroom Environment

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, column 1 clearly shows that repeaters have a higher appreciation of 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 the quality of their feedback and formative assessment as 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 remarkable contrast between repeaters and regular students in teachers' assessments 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 intensify 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 regular students.[33] 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

[33] 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.

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.

D. The Effect of Proportion Repeaters on Teachers' Pedagogy 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 in Table A2) using the sub-sample of the regular students.[34] Following Kling et al. (2007) we also compute the average effect for each category by averaging across the standardized effects of the individual outcomes included in that category and estimating a system of seemingly unrelated regressions.[35] As there is no prior information to justify a particular weighting, we assign equal weight to all outcomes within a category as this provides a more transparent interpretation.[36]

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

[34] 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.

[35] That is, we define the average effect τ_c of the proportion of repeaters for category c 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. We treat (σ_{kc}) as known based on the results of Kling and Liebman (2004) and given that we have a large sample.

[36] 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.

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 treatment effect 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 (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 and by the central role attributed to the school environment on school choice decisions (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 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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Table 1: Descriptive Statistics: Student's Achievements in High Schools

	Repeaters (1)	Low parental education (2)	Others (non-repeaters) (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. The "others" subsample reported in column 3 includes also the low parental education group reported in column 2.

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

	Outcome means		Treatment: Proportion of Repeaters		
	Repeaters (1)	Others (2)	OLS (3)	School Fixed effects (4)	School specific time trends (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: Columns 1 and 2 report means for repeaters and regular students. T-statistics for differences in means between repeaters and regular students adjusted for clustering 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 variables 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 adjusted for clustering at the school level are reported in parentheses.

**: By definition, immigrants are never repeaters.

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

						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 estimates (column 2), and estimates that include school fixed effects (columns 3-4), and school-specific time trends (column 5) of the effects of the proportion of repeating students in a grade on their peers' achievements in high school. Columns 6 and 7 report estimates from falsification tests using the proportion of repeaters of the t-1 or t+1 cohort respectively. The proportion of repeaters is measured in 10th grade. Individual controls include: 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 controls include students individual controls averaged by school and year and a quadratic function of enrollment. Robust standard errors clustered at the school level are reported in parentheses.

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

	Outcome means		Effect of Proportion 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 3-4) of the effects of the proportion of repeaters on matriculation outcomes of regular students. Column 3 reports estimates on students with low parental education (one of their parents has less than 12 years of schooling). Column 4 reports estimates on students with high parental education (both their parents have at least 12 years of schooling). All regressions include school and year fixed effects and school specific time trends. The regressions also control for students background characteristics and school time varying controls detailed in Table 3. 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 of Regular Students by Groups Defined on Enrollment in Advanced Math and Science Courses

	0 advanced courses		2+ advanced courses		3+ advanced courses		0 advanced courses, restricted sample of schools	
	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)
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	62.910	-12.525	84.347	-0.032	86.434	-2.257	63.752	-12.670
		(5.375)		(4.140)		(5.617)		(6.582)
Matriculation status	0.460	-0.216	0.953	0.104	0.972	0.048	0.479	-0.262
		(0.109)		(0.122)		(0.168)		(0.143)
Number of credit units	18.109	-5.804	29.790	1.309	31.402	2.333	18.279	-6.121
		(2.126)		(2.741)		(3.526)		(2.991)
Number of advanced level subjects in science	0.125	-0.059	2.051	-0.407	2.320	-0.499	0.134	-0.100
		(0.060)		(0.327)		(0.446)		(0.080)
Matriculation diploma that meets university requirements	0.377	-0.184	0.942	0.141	0.968	0.090	0.405	-0.236
		(0.079)		(0.138)		(0.173)		(0.112)
Number of students in group	243,321		60,506		24,683		202,183	
Number of schools in group	310		276		235		235	

Notes: The table reports means of the dependent variables (odd columns) and estimates (even columns) of the effects of the proportion of repeaters on matriculation outcomes of regular students stratified by their enrollment in advanced courses in math and science. The sample for columns 1-2 includes students who were not enrolled in any advanced math or science course. The sample for columns 3-4 report includes students enrolled in least two advanced courses and the sample for columns 5-6 includes students enrolled in three or more courses. The sample for columns 9 and 10 includes students not enrolled in any of these courses who attend the same school as the students included in columns 7-8. The first row of the table reports the effect of repeaters on the likelihood of being included in each of the samples. All regressions include school and year fixed effects and school specific time trends. The regressions also control for students background characteristics and school time varying controls detailed in Table 3. Robust standard errors clustered at the school level are reported in parentheses.

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

	Outcome means		Effect of Proportion of Repeaters	
	Repeaters (1)	Others (2)	OLS (3)	School fixed effects (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: Columns 1 and 2 report means for repeaters and regular students. T-statistics for differences in means between repeaters and regular students adjusted for clustering at the school level are reported in squared brackets. Columns 3 and 4 report OLS and school fixed effects estimates from separate regressions of the relevant variables on the proportion of repeaters. The sample includes regular students in grades 7-9. The regressions control for grade and year fixed effects. Standard errors adjusted for clustering at the school level are reported in parentheses.

**: By definition, immigrants are never repeaters.

Table 7. Effects of Repeaters 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 and regular students on their views on the learning and 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. Column 3 reports school fixed effects estimates of the proportion of repeaters on the learning and classroom environment. The estimates are for the average effects of the individual items reported in table A2 and come from the sample of regular students. 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: Columns 1 and 2 report means the dependent variables for repeaters and regular students. T-statistics for differences in means between repeaters and regular students adjusted for clustering 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 the standardized tests scores of regular students 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.

Table A2: Estimates of the Effects of the 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 (1)	Treatment: Proportion of repeaters (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 7 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 7. 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 (cont.): 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 (1)	Treatment: Proportion of repeaters (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 7 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 7. 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 (cont.): 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 (1)	Treatment: Proportion of repeaters (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 7 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 7. The regressions also include school fixed effects and grade and year dummies. Robust standard errors clustered at the school level are reported in parentheses.