Educational Peer Effects
Quantile Regression Evidence from Denmark with PISA2000 data

by

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Abstract

We combine data from the first wave of the OECD PISA sample with register data for Denmark to estimate educational peer effects. These datasets combined provide an unusually large set of background variables that help alleviate the usual problems of omitted variables bias, prevalent in much of the empirical literature on peer effects. Quantile regression results show that there may be differential peer group effects at different points of the conditional test score distribution: The positive and significant peer level effect is strongest for weak students and is steadily decreasing over the conditional test score distribution. The effect from a heterogenous peer composition on test scores does affect weak learners positively, while the effect for good readers is negative, but at all estimated quantiles, the effect is not significantly different from zero. These results combined suggest that mixing of abilities is the optimal policy to maximize average reading skills in the student population.

1 Introduction

Inputs in the process of accumulation of human capital operate at different levels. Some are determined at the level of individual families, others at the level of the whole economy, others again at the intermediate level of communities, neighborhoods, or social networks. This is the case with many forms of "social capital", e.g. peer effects and role models. Educational peer effects have long been of interest to social scientists because, if they exist, they potentially affect the optimal organization of schools. The learning process at schools depends crucially on how purchased inputs (teachers, buildings, books) combine with social capital in the production of education. One expects peers to affect achievement directly (e.g. helping each other with course work) and also via values (e.g. peers acting as rolemodels).

The question is how best to educate students whose backgrounds and abilities differ widely. However, the value of ability grouping in schools is a subject of much debate. Proponents of ability grouping argue that by narrowing the range of student abilities within a classroom, streaming allows teachers to target instruction at a level more closely aligned with student needs than is possible in more heterogeneous environments, and that student interest and participation are increased by ability grouping. On the other hand, those opposed to ability grouping argue that less gifted students need the presence of their higher level peers to stimulate learning, while average and even above average students do not derive substantial academic benefits from being grouped together.

If the individual student's scholastic achievement depends on the average quality of the individual's peer group, then, depending on the nature of the peer effects, there may be social gains from grouping together "high ability" students, or there could be social gains from spreading high ability students evenly among the population. A welfare question emerges: is it better to mix children's qualities in schools, or should one educate them in groups segregated by ability? Assuming that the social planner seeks to maximize average academic achievement, the optimal policy turns on whether there are increasing or decreasing returns to peer groups: with decreasing returns, one should mix children; with increasing returns they should be segregated by ability. Thus, the optimal policy becomes an empirical issue.

For governments to design policies that can improve socioeconomic outcomes, it is critical that the educational production function and the relative importance of peer effects versus other inputs such as teachers and infrastructure are better understood. It is clearly difficult to think about improving student outcomes in schools until we know which inputs matter. This study sheds some light on peer group effects in the Danish – non-selective – school system.

The challenges of the Danish school system School systems around the world differ in their extent of ability grouping of students during compulsory education. Some countries have non-selective school systems (e.g. the Scandinavian countries) that seek to provide all students with the same opportunities for learning. These countries limit parental choice of school through fixed school catchment areas, there are no tracks or streams, and automatic promotion regulates the transition from one grade to the next. Other countries respond to diversity explicitly by forming groups of students of similar performance levels through various mechanisms of the school system (e.g. Austria, Belgium, Germany): tracking, streaming, grade retention, to name a few[1], with the aim of serving students according to their specific needs. And in yet other countries, combinations of the two approaches occur.

While the issue whether or not to stream does not seem to be on the mind of many European educational researchers or politicians, ability grouping has been an ongoing debate in the United States for decades. Based on the research produced in the 1980s on tracking, many schools and school systems have made major efforts to detrack. In Denmark, a country with a completely non-selective school system, mixed-ability classes and schools have been the corner stone of the compulsory school system. However, the disappointing results for Denmark of the PISA (program for International Student Assessment) investigation conducted by the OECD (see OECD 2001a) in the year 2000 have

[1] Tracking usually means ability grouping of students either across schools or within schools for *all* subjects, while streaming typically covers ability grouping *within* schools and for *some* subjects only. The practice of making students repeat a grade when his/her results are poor is called grade retention.

led to a fervent discussion on how to improve the academic achievement of Danish students[1]. Compared to the other Scandinavian countries, Denmark produces relatively few well-performing students and relatively many poorly performing students. As Danish expenditure on education is among the highest among the OECD countries, any discussion to yield achievement gains by raising general expenditures is ruled out[2].

Denmark is a country with a completely non-selective school system with all students taught in mixed-ability classes during compulsory schooling. There is almost no ability grouping at all (only to a *very* small extent within the classroom), and there is almost no chance for implicit ability grouping by choosing more or less advanced courses. The only possibility for parents to choose a specific peer group for their child is at the school level – either by residential location or by opting out of public school and choosing a private school with a certain profile. However, even private schools are by no means elitist in Denmark, but certain types of private schools attract certain types of students (see Rangvid, 2003). For some years already, there has been an increasing awareness of the pitfalls of a completely non-selective school system, and this criticism has led to some within-class grouping. However, within-class grouping requires individual teachers to manage several different groups of students simultaneously, thereby necessitating that some groups of students spend considerable time working alone while the teacher is working with other groups. Also, the PISA results show that Danish schools are comparably weak at moderating the impact of socioeconomic disadvantage which might be due to the fact that teaching has been targeted to the average student, while the weak and the strong students have been left to themselves. For these groups, the parental background may play a relatively stronger role than for the average student to whom teaching is targeted. This discussion and the realization that a strong enough differentiation within the class is not possible without the supply of many more resources in the form of teacher hours has led to considerations whether to group students across grades by student ability at least for some of the time. The suggestion has been to stream children during up to three months of a school year, thus keeping the heterogenous classes intact for the remaining time. This – rather cautious – suggestion for reform is due to the fact that ability grouping is commonly viewed as increasing socioeconomic differences rather than moderating them. By introducing limited and flexible regrouping schemes, the hope is to retrieve the benefits from tailoring classes to each group's ability, while maintaining the positive peer group effects of a mixed-ability class.

[1] Actually, Denmark performed *at* the OECD average. However, for Denmark, the other Scandinavian countries have traditionally been the relevant standard and Denmark underperformed compared to these countries.

[2] Also, Graversen and Heinesen (2002) provide evidence based on Danish data that the level of general school expenditure at best has a very limited effect on educational outcomes.

In any event, choosing the optimal degree of ability grouping is difficult; and the optimal strategy is not only a matter of how and for how long to group students, but also one of teacher attitude toward classes of varying abilities, curriculum issues like keeping academic standards up also in low ability classes, etc.

Results from the literature The influence of peers on students' achievement has been a topic of extensive empirical and theoretical research. The peer group is undeniably important in the minds of parents. Even in a mixed-ability school system, peer group choice is exercised as residential location decisions of families have an implicit if not explicit peer group component (thus choosing between public schools). The role of peers has entered increasingly into theoretical analyses of school choice[3]. But do peers have a (measurable) impact on student performance? The findings in the existing literature on peer effects are ambiguous. The standard approach to measuring peer effects regresses students' own outcomes on the peer "quality" in the class or school (e.g. Summers and Wolfe 1977; Henderson, Miezkowski, and Sauvageau, 1978; Betts and Morell, 1999; Zimmer and Toma, 2000). These studies on peer effects in a variety of countries all find important educational peer effects[4].

Generally, it is difficult to interpret coefficients obtained from this standard approach, because individuals self-select into peer groups. This makes it difficult to distinguish the selection effect from any actual peer effect. There are several approaches to dealing with this sort of bias. Various authors attempt to solve the selection problem by designing instruments for the peer group that are assumed to be exogenous. However, finding credible instruments is difficult. Feinstein and Symons (1999) use local authority area dummies to instrument for peer groups. The instrument is tested to be valid, but no test of strength is reported. In a study of peer group effects on academic achievement, Robertson and Symons (1996) employ regions of birth indicators to deal with the bias. However, they do not present any formal or informal test of validity of the instruments. In a recent study on another subsample of the PISA 2000 data we use in the present study[5], Fertig (2003) estimates the effect of a heterogenous classroom on reading skills,

[3]Starting from the observation that many people express concern about other students, a variety of analyses (e.g. Benabou 1993, 1996; Caucutt, 2001, forthcoming; de Bartolome, 1990; Epple and Romano, 1998; Nechyba 1999, 2000) have examined how financing mechanisms, particularly vouchers, interact with demands of families for different peers. Epple, Newlon, and Romano (2002) analyze the effects of ability grouping on school competition. Also in club theory, peer group effects have been incorporated. Standard club theory shows that homogenous clubs are superior to mixed clubs on efficiency grounds (Berglas and Pines 1981). Brueckner and Lee (1989) show in a variation on the standard club model that mixed clubs can be optimal when there are peer group effects.

[4]However, the size and significance of the peer effect for different groups of students differs across studies.

[5]Fertig uses the US subsample, while we employ the Danish subsample of the international PISA 2000 data.

using school selectivity, whether the school is a private school, and variables reflecting the "caring behaviour" of parents as instruments. However, in spite of the fact that there are multiple instruments, no test of overidentifying restrictions is offered to validate the instruments.

In the closely related tracking literature, additional instruments for the peer group have been suggested, e.g. regional indicators, urbanicity indicators and student body characteristics (Argys, Rees and Brewer, 1996); two and three way interactions of three variables: the number of academic courses required for state graduation, the number of schools in the county, the fraction of voters in the county who voted for President Reagan in the 1984 election (Figlio and Page, 2000); the percentage of black students in the school, the percentage of students who receive full federal lunch assistance at the school, and the students' test score relative to the average for their grade (Betts and Shkolnik, 2000).

Some recent papers argue that estimating traditional "average effect" models do not give the full picture of the peer group effect, because they estimate the effect only for the average student[6]. This may produce misleading results if peer group effects differ for high and low ability students. By applying quantile regression techniques, it is possible to give a more complete picture, because the peer effect can be estimated at all points of the conditional test score distribution. Levin (2001) estimates peer group effects by quantile regression techniques and finds that the number of similar classmates has the largest effect on achievement of individuals at the lowest quantiles and that the peer effect experiences a considerable monotonic decrease as one goes up the conditional achievement distribution, and becomes insignificantly different from zero by the 90^{th} percentile. Levin argues that students at the lower end of the conditional achievement distribution are "dependent" learners relative to their higher achieving classmates for whom the effect of having similar peers is negligible.

Despite of the attempts made in the literature, finding credible instruments seems to be difficult[7]. Also, the results of the peer effect yielded in these studies are ambiguous.

Contribution In this study we provide evidence on the effect of ability grouping by exploiting the degree of ability grouping across schools that is present in the existing school system in Denmark: peer groups vary across schools due to geographical clustering of families with different socioeconomic background and due to parental choice between

[6] However, several studies conduct subsample analyses of the peer effect.

[7] This is especially true for observational cross section data. When panel data, natural experiments or even true experimental data are available, there are additional possibilities, see e.g. Hanushek et al. , 2003; Lavy, 1999; and Goldhaber and Brewer, 1997 for panel data and Boozer and Cacciola, 2001; Sacerdote, 2001; Zimmerman, 1999; and Hoxby (2000) for natural experiment; and Falk and Ichino (2003) for a true experiment in a different context.

the local public school and a range of private schools.

This is the first study on peer effects in Denmark, and one of the first studies on peer effects to exploit the riches of the newly released PISA dataset[8]. The first wave of the OECD PISA study provides a number of informative peer group variables as well as family background variables. We use data from the first wave of the PISA study combined with register-based data on the students' backgrounds to estimate the effects of the *level* and the *variance* of peer ability on the individual student's educational achievement.

A crucial technical question we raise in this paper concerns the estimation of these peer group effects by empirical methods. Due to the unavailability of suitable instruments to account for self-selection into a peer group, we turn to a different strategy. The ability to distinguish the separate effects of individual and school factors from those of peers depends crucially on observing and measuring the inputs into students' performance. The typical analysis, however, does not have perfect measures of either family background or school inputs. Thus, the endogeneity bias in our setting is basically an omitted variables bias. Our strategy is to eliminate as much as possible of this bias by including variables for parental characteristics that are typically thought to introduce the bias. While certainly not being "perfect" measures, yet, we have information available that permits us to control for these effects, e.g. data on so-called *parental academic interest*, on *home educational resources*, *cultural interest*, etc. (see section 2) – all variables which are unobserved in ordinary datasets, but are thought to affect both parental choice of peers and other (unobserved) parental input into the child's schooling. Also, we estimate the peer effect both for the average student and – using quantile regression techniques – at different points of the conditional test score distribution to find out, whether high and low performing students are affected differently by changes in the peer group. The effect of class composition on academic achievement is explained via two main effects. The first is the mean peer effect, namely externalities that are induced by the composition of teaching and learning environment. The second is the efficiency effect, which reflects the reduced ability of the teacher to teach and of the pupil to learn in a heterogenous environment.

We show that after controlling for a broad range of factors, there remains a statistically significant positive effect of the average of peer group quality with OLS estimation. However, peer group heterogeneity does not seem to play an important role in Danish classrooms, once the peer group level has been accounted for. Quantile regression evidence reveals important differences of the peer effects at different parts of the conditional

[8] Two OECD (2001a, 2002) reports based on the dataset we employ in this study briefly address the issue of peer effect. The effect of the peer group is also briefly discussed in the Danish National PISA report (2001). All studies find that peer group effects for Denmark exist, but the impact is far below the OECD average.

test score distribution. While the peer level effect is positive and significant at all estimated quantiles, the point estimates are highest for weak readers and decrease steadily over the conditional test score distribution. Also the peer heterogeneity effect is highest at the lower quantiles, but unlike the peer average effect, it is imprecisely estimated for all quantiles, and turns even negative (but is still insignificant) at the highest quantile.

The paper proceeds as follows. Section 2 presents the empirical framework. The following section details the data. Section 4 reports results and sensitivity analyses, while the last section concludes.

2 The empirical framework

To examine peer effects, we employ a standard education production function model. These models estimate outcomes, e.g. test scores, as a function of the cumulative influence of family and school inputs, the peers of the student, and individual characteristics of the student. Conceptually, the model to be estimated is:

$$A_{ij} = \beta X_{ij} + \gamma S_j + \delta P_j + \varepsilon_{ij} \tag{1}$$

where A_{ij} is achievement for student i at school j, X_{ij} is a vector of individual and family background influences, S_j is vector of school inputs, P_j is peer influence, and ε_{ij} is an error term.

If the student's peer group is randomly assigned, or at least not systematically correlated with unobserved factors influencing both the choice of the peer group and academic achievement, then equation 1 provides us with an unbiased estimate of the peer group effect, δ.

However, our concern is that the "peer group" is often itself a matter of individual choice, as is the implication of the classical Tiebout (1956) model of local finance. In a Tiebout world, individual households, in making their locational choice, are also choosing a peer group for many local services, including local public schools. In this setting, the peer group becomes an endogenous variable, determined in part by household choice. Once this is recognized, it is clear that models that do not correct for this kind of selectivity are inappropriate. Thus, if students (or their parents) select into ability groups on the basis of unobserved factors such as motivation or unobservable school quality, and if these factors are, in turn, correlated with achievement, this approach will yield biased estimates of the peer group effect. Formally, the error term in the selection equation may be correlated with the error term of the achievement equation.

We suspect that simple models are likely to overstate peer group effects. Consider a child with parents who do place a lot of importance on their child's education. This child will be a high-achiever in school for two reasons. First, the parents will choose a school for their child with a positive learning environment, including a good peer group. Second, the child will do better in school because the parents will encourage and back up their child to reach a high academic level, i.e. parents who are exceptionally ambitious for their children (in a manner unobserved by the econometrician) will choose high levels of the productive inputs, e.g. the peer group, as well as aiding their child directly. If this type of parental conduct is unobserved, empirical studies will tend mistakenly to allocate the effect of the unobserved extra parental influence to the peer group.

There are several possible solutions to this problem. As discussed in the Introduction, other authors have used such different approaches as instrumental variables methods, panel data fixed effects estimators and quasi-experimental methods. However, due to data limitations, (we do not have neither panel data, nor experimental data available for our study) we cannot employ panel data techniques, neither do we have experimental data. Moreover, according to our judgement, no study in the existing literature has come up with convincing instruments.

Another strategy is to find a dataset which provides the researcher with data for the relevant characteristics, which, when omitted due to data unavailability, introduce the selectivity bias into single equation estimates. This is the strategy we will adopt in the present paper: we shall deal with the bias, or at least reduce its effect, by including e.g. data on parental academic interest and home educational resources – variables which are unobserved in typical datasets, but are thought to affect both parental choice of peers and other parental input into the child's schooling.

We can do so, because basically, the selectivity problem is a problem of omitted variables. If we condition on all relevant variables, i.e. there were no unobservable characteristics related to both the peer choice and the academic achievement equation, then single equation models yield unbiased estimates. Thus, recall equation 1 and assume that the vector of relevant family background variables[9] can be decomposed into two parts: observable characteristics, X_1, such as gender, ethnicity, parental education, income, and family structure, which are included in the typical empirical estimation in the literature; and unobservable characteristics, X_2, such as parental ambition, parental time spend with their child, encouragement and support in their children's education, etc., which are omitted from the typical estimation in the literature, due to data unavailability.

[9] We suspect that in this setting the most important part of the bias is due to unobserved *family* characteristics – as the parents usually choose school for their child and at the same time parental characteristics are the most important predictor of the child's academic achievement.

Hence, the true model is

$$A_{ij} = (\beta_1 X_{1ij} + \beta_2 X_{2ij}) + \gamma S_j + \delta P_j + \varepsilon_{ij} \tag{2}$$

but the typical study estimates

$$A_{ij} = \beta^* X_{1ij} + \gamma^* S_j + \delta^* P_j + \nu_{ij} \tag{3}$$

where the error term now consists of unobservable family characteristics as well as a random component (where $\nu_{ij} = \beta_2' X_{2ij} + \varepsilon_{ij}$). Thus by omitting X_{2ij}, the estimation of (3) may overstate the total effect of the peer group on academic achievement, because omitted factors (X_{2ij}) will not be included in the explained portion of the variance in student achievement.

The obvious solution to this problem is to utilize data which include the relevant variables in X_{2ij}, or at least good proxies for them. In the present paper, the strategy is to eliminate as much as possible of this bias by including variables for particularly parental characteristics that are typically suspected of introducing the bias.

A main focus in the OECDs PISA study is the concept of family background influences. Parental interest in the child's education is regarded as one of the strongest predictors for achievement in schools, and several questions in the student questionnaire aim to tap information about the students' home social background. In PISA, apart from the usually collected variables on parental education, occupation, income and wealth, several constructs related to family background variables that typically remain unobserved have been derived from the student questionnaire. An important variable provided by the PISA data is on parents' support for their children's education, which is widely recognized to be an essential factor for success in school. When parents interact and communicate well with their children, they can offer encouragement, demonstrate their interest in their children's progress, and otherwise show their concern for how their children are doing in school. PISA divides parental involvement with their children into two indices: *parental academic interest* and *social communication. Parental academic interest* is intended to measure the level of academic and, more broadly, cultural competence in the student's home. Importantly, the construct also incorporates whether parents engage in these activities together with their children. *Social communication* is designed to explicitly measure home social capital, which means both parental involvement with their child in school matters specifically, but also interest in the child's life in general.

PISA also provides data on several other factors about the students' home background which are thought to be related to educational success. *Student's cultural activity* focuses on the student's own cultural activities. Even though the construct clearly focuses on the student's own activities, there are strong reasons to believe that the tendency among 15-year-olds to attend these kinds of activities is strongly linked to parental preferences and practices. *Home cultural possessions* like classical literature, poetry and paintings have frequently been shown to be related to educational success. *Home educational resources* such as a desk for studying, textbooks, and computers focus on home resources that are directly useful for the student's schoolwork. These aspects of the home environment could be said to indicate an academic orientation.

Thus, the PISA data give a wealth of information on the student parental background that is usually unobserved by the econometrician, but is a potential source of the kind of omitted variables bias described above. As will be shown, conditioning our estimates on this extensive set of explanatory variables will produce estimates, where the bias is significantly reduced.

Estimation methods We start by estimating peer effects by Ordinary Least Squares (OLS), i.e. in an "average causal effect" setting. We use sample weights as our sample is stratified.

Most of the empirical work on peer effects focuses on average peer effects. However, the standard methodology may miss how school resources affect achievement differently at different points of the conditional test score distribution. For example, while the peer group composition may not matter for average test scores, it would be useful to know whether the true effect is zero at all points of the conditional test score distribution, or whether the zero average effect covers up positive effects at some points of the distribution and negative ones at others. This is especially relevant in the setting of peer composition, if our analysis is to be relevant for policy advice: we must consider the best equilibrium outcome, as giving a student a good peer is to take that peer away from someone else. Thus, if weak readers profit from a better peer group, it is obvious that the best strategy is mixing only if high-ability students are unaffected. With quantile regression, we are not only able to address the question whether peer groups matter, but also for whom does the peer group matter?

Over and above of allowing the researcher to focus on quantile treatment effects rather than on average treatment effects, the quantile regression method has several other virtues. First, the quantile regression estimator gives less weight to outlier data points on the dependent variable than OLS, which weakens the impact such data points might have on the results. In other words, every observation of the dependent variable can be made arbitrarily big or small without changing the results, as long as it does not cross the estimated (hyper-) plane. Second, by allowing the parameter estimates for the marginal effects of the explanatory variables to differ across the quantiles of the dependent variable, robustness to potential heteroscedasticity is achieved. Third, when the error terms are non-normal, quantile regression estimators may be more efficient than least squares estimators. The main advantage, though, is the semi-parametric nature of the approach, which relaxes the restrictions on the parameters to be constant across the entire distribution of the dependent variables.

To appreciate quantile regression methods, it is worth emphasizing what quantile regression is *not* about. Something like quantile regression *cannot* be achieved by dividing the response variable into subsets according to its unconditional distribution and then doing OLS on these subsets. This form of "truncation on the dependent variable" is clearly not permissible. It is thus important to recognize that even for the extreme quantiles *all* the sample observations are used in the process of quantile regression fitting.

The only paper we are aware of estimating peer group effects by quantile regression methods is Levin (2001)[10]. However, in a slightly different context, Eide and Showalter (1998) have used quantile regression techniques to estimate the effect of school resources on test scores.

Originally, quantile regressions were suggested by Koenker and Basset (1978) as a "robust" regression technique alternative to Ordinary Least Squares for the case when the errors are not normally distributed. More recently, the quantile regression technique has been applied not because of its robust property, but because of its feature to estimate effects at different points of the conditional outcome distribution.

The basic quantile regression model specifies the conditional quantile as a linear function of covariates. For the θth quantile, a common way to write the model (see, e.g. Buchinsky, 1998) is

$$y_i = x_i'\beta_\theta + u_{\theta i}, \qquad Quant_\theta(y_i|x_i) = x_i'\beta_\theta \qquad \theta \in (0,1) \tag{4}$$

where $Quant_\theta(y_i|x_i)$ denotes the quantile of y_i, conditional on the regressor vector x_i. The distribution of the error term is left unspecified. It is only assumed that $u_{\theta i}$ satisfies the quantile restriction $Quant_\theta(u_{\theta i}|x_i) = 0$. The θth regression quantile $(0 < \theta < 1)$ of y is the solution to the minimization of the sum of absolute deviations residuals

$$\min_\beta \frac{1}{n} \left\{ \sum_{i:y_i \geq x_i'\beta} \theta \left|y_i - x_i'\beta\right| + \sum_{i:y_i < x_i'\beta} (1-\theta) \left|y_i - x_i'\beta\right| \right\} \tag{5}$$

The variation in the value of θ traces the entire distribution of test scores and one can

[10] However, several studies estimate peer effects on subgroup of students, where the sample is divided into subsets defined according to the independent variables.

estimate the peer effect on reading scores at any given percentile. The important feature of this framework is that the marginal effects of the covariates, given by β_θ, may differ over quantiles (i.e., different values of θ). In the special case where $y_i = x_i'\beta + u_i$ (with u_i homoscedastic), the marginal effects at every quantile will be the same[11]. Variation in the estimated peer effects across the quantiles of the conditional distribution of reading scores may be an indication of heterogenous peer effects. Therefore, we estimate an education production function at different quantiles ($\theta = 0.1, 0.25, 0.5, 0.75, 0.9$), and examine whether there is homogeneity in the effect of peers on individual reading literacy, by testing for the equality of the peer effect coefficients across the quantiles.

Different quantiles are estimated by weighting the residuals differently. For the median regression, all residuals receive equal weight. However, when estimating the 75th percentile, negative residuals are weighted by 0.25 and positive residuals by 0.75. The criterion is minimized, when 75 percent of the residuals are negative. This is set up as a linear programming problem and solved. Algorithms based on the least absolute deviations criterion are available in order to obtain estimates.

While finding the solution to (5) is straightforward, the estimation of the variance-covariance matrix is not. For the case of homoscedastic residuals, Koenker and Bassett (1982) propose a formula. However, Rogers (1992) and Gould (1992) argue that, while this method works adequately under the assumption of homoscedastic errors, it underestimates the standard errors in the presence of heteroscedasticity, which is especially troublesome as heteroscedasticity is often the reason for using quantile regression in the first place[12]. It is therefore important to use some other method for estimating standard errors, such as bootstrap re-sampling techniques. In the present study, standard errors are obtained by bootstrapping the entire vector of observations. We use the design matrix bootstrap, where pairs ($\mathbf{x}_{i}$,y_i) i=1,...,n are drawn at random from the original observations with replacement. For each of these samples drawn, an estimator of the parameters vector, β_θ is recomputed. Repeating this procedure B times yields a sample of B parameter vectors whose sample covariance matrix constitutes a valid estimator of the covariance matrix of the original estimator. This procedure is automated in the Stata statistical package. However, the question of how to choose the number of bootstrap repetitions remains. Andrews and Buchinsky (2000) suggest a three-step method to choose the number for which they provide Monte Carlo simulations in Andrews and Buchinsky (2001). Their findings are that the number of bootstrap repetitions commonly used in econometric applications is much less than needed to achieve accurate bootstrap quantities. As the

[11] The slope coefficients on any variable will be the same across quantiles, though the intercept will differ across quantiles.

[12] In our paper, the analytically calculated standard errors were up to 25% smaller than the bootstrapped errors.

sufficient number of bootstrap repetitions is inversely related to sample size, we chose the number of 200 bootstrap repetitions.

If peer effects are homogenous across the conditional distribution, we would expect the slope coefficients estimated at the quantiles to be equal. A method to formally test the presence of heterogeneity in the peer effects is to test whether the observed differences along the estimated coefficients are statistically significant across quantiles. Equality of selected subsets of parameters between quantiles may be tested by estimating all quantile equations simultaneously and thereby obtaining an estimate of the entire variance-covariance matrix (including between-quantiles blocks) of the estimators by bootstrapping. Thus, tests concerning coefficients across equations can be performed. For example, a test of the hypothesis of heterogeneity ($\beta_\theta \neq \beta_\delta$ for some θ) can be based on a test of whether the estimated peer effect coefficients differ across quantiles.

3 Data

Sample We use data for Denmark from the OECD program for International Student Assessment (PISA). The first PISA survey was conducted in the year 2000 in 32 countries. PISA 2000 surveyed reading literacy, mathematical literacy and scientific literacy. In this study, we use only reading test scores as our academic achievement measure[13]. The test focusses on the demonstration of knowledge and skills in a form that is relevant to everyday life challenges rather than how well students had mastered a specific school curriculum. Students were tested in three "type of reading" tasks: retrieving information, interpreting texts, and reflection and evaluation. To facilitate the interpretation of the scores assigned to students, scores from these three tasks were summarized in a combined reading literacy scale. This scale was designed to have an OECD average score of 500 points and a standard deviation of 100 points. In Denmark, the test score distribution is rather tight with a standard deviation of only 93 points compared to the OECD average of 100 points.

In addition to the reading literacy test, students answered a background questionnaire and school principals completed a questionnaire about their school. In Denmark, the sample is stratified by school size (very small, small and big schools). Within the strata, schools were selected in a specific way so each student had an equal opportunity to be selected irrespective of the size of the school or the class. Within each school, 28 students aged 15 were selected randomly for participation[14]. For Denmark, the final PISA sample consists of 223 participating schools with 4212 students corresponding to a responsrate of

[13] While the reading test was administered to every student participating in the assessment, the mathematics and the science tests were only given to one out of two students, thus rendering the mathematics and science samples too small to obtain reliable estimates.

[14] In schools with less than 28 15-year-old students, all of them participated.

87 percent.

Register data As our identification approach is to minimize the amount of unobserved characteristics which might influence both peer group choice and educational achievement, we use Danish register data in addition to the PISA survey data, to complement the picture of the family background available from the PISA data. Some variables are available both from the survey and from registers.

The use of register data added information on parents' income, type of housing, whether the parents were "teens" when their child was born, a more complete measure of parental education (e.g. detailing the length of tertiary education), and the amount of parental unemployment in the year when the student was 15 years old. Other information, such as information on the family structure, is also available from the PISA questionnaire, but can now be cross-checked with registerdata. In section 4, we conduct selectivity analyses to check for disparities between survey and register variables.

We relied on several criteria to select the sample for our study from the larger PISA sample. First, we restricted the sample to students attending schools for which PISA provided data for at least 15 students. This restriction is necessary because for use in the estimation, we construct peer measures from mean characteristics of the 15-year-old students in a school, and we prefer to be sure of some minimum number of observations for each school. The minimum number of 15 students might seem low, but was chosen as not to lose too many observations from the sample.

We also include dummy variables to control for missing values of some explanatory variables, in which case the explanatory variable was set to zero. E.g. in an important number of cases, data on a parent's occupational status were missing. Also, school background variables were missing in a significant number of cases, and we include dummy variables for these missing observations as well. In spite of the introduction of missing categories for some key variables, we lost 163 observations due to missing values in other variables. Our final sample includes 3666 cases.

Peer quality and heterogeneity The peer variable is of primary importance in this research. Building on the existing literature, we define the peer variable in terms of ability and measure it in several ways. When we talk about peer "quality", we have in mind both intellectual ability and other characteristics such as ambition, docility, punctuality and so on, which we believe are derived in large part from the child's home environment, and in the schooling setting perhaps especially from the educational level of the parents. In our basic estimations, we proxy peer quality by the average years of schooling of the classmates' mothers[15] [16]. We refrain from using the more obvious average level of reading scores of the classmates as a proxy for class ability due to potential problems with reverse causality. In this paper, we suggest that there are peer effects in the classroom. Consequently, if each single student in the classroom is affected by his peers, the average achievement of the classmates is affected by each single student, too – giving rise to reverse causality problems.

As part of the sensitivity analysis reported in section 4, we also use additional measures of peer quality.

We also square mean parental education to capture nonlinear as well as linear effects of the average on fellow students. It is generally assumed in the literature that the higher the mean score of fellow students in a class, the higher the level of educational attainment of a given student, but the effect may diminish (i.e. the squared term is expected to be negative.) Additional interaction variables to capture peer effects will be introduced in variations on the basic empirical model.

In addition to these peer *mean* variables, we control for the *heterogeneity* of the peer group, because if a better peer group in the data is correlated with a homogenous classroom, then a potentially positive effect from a homogenous peer group will be falsely attributed to a higher peer quality. The variance, or mix, of abilities within the classroom may affect individual student achievement and we are interested in assessing the effect of being schooled in a classroom with similarly skilled students relative to the effect of being schooled in a classroom that has a larger variance in student abilities. For example, a classroom in which all students have roughly the same ability may generate different peer effects than one with the same mean score, but a wide range of abilities. Teachers of a homogenous high ability class can provide them with more challenging material or present standard material at a faster pace than would be possible in a classroom where less-able students' needs also have to be met. At the same time, low-ability students are expected to benefit from the slower pace or alternative teaching methods that become feasible when teachers are not simultaneously responsible for engaging the students' high-ability peers. To analyze this, we include the standard deviation of parental education of all students in the classroom as a measure of classroom heterogeneity.

Our peer data is only approximately class peer data, as 15-year-old students were selected randomly within schools, irrespective of the grade or class within grades. However,

[15] We have tried other specification as well: years of schooling of classmates' fathers and average years of schooling of parents without a qualitative change in the results.

[16] Behrman and Rosenzweig's (2002) results suggest that the postive relationship between the schooling of mothers and their children is substantially biased upward due to correlations between schooling and heritable "ability" as well as assortative mating. In other words: more able women, who have more schooling, have more able children, who obtain more schooling; and: more schooled women marry more schooled (and thus more able) men.

in a system like the Danish, where students are not grouped by ability across classes, the peer composition of 15-year-olds within a school will be an acceptable proxy for the peer composition within the classes, the 15-year-old students attend.

Control variables Other variables than the peer group are important for academic achievement. Particularly parental inputs are commonly found to be immensely significant in determining both choice of peer group and academic achievement, while the importance of school level inputs is much more disputed in the literature. The control variables can be divided into two main categories: home-related factors and school-related factors.

– *Home-related inputs* The model controls for the typically employed background variables which are likely to affect test scores and/or peer group choice, e.g. age[17], gender, ethnicity, number of siblings, sibling status (only child; oldest, middle, youngest child), family structure, parental education, parental occupation, parental income, and parental wealth, and whether the student speaks Danish at home. In addition to the background variables available in a typical dataset, the PISA dataset provides us with variables, which are thought to be important for the parents' influence on the child's peer group and on his educational achievement. Such variables are e.g. the index[18] measuring *parental academic interest*, which was derived from students' reports on the frequency with which their parents engaged with them in discussing political or social issues, discussing books, films or television programs, and listen to classical music. The index for *cultural possessions* in a family's home was derived from students' reports on the availability of classical literature, books of poetry and works of art in the home, while the *social communication* index was based on the frequency with which parents discussed how well their child is doing at school, whether they eat the main meal with him around the table, and how often they spend time simply talking with him. The index for *home educational resources* was derived from students' reports on the availability and number of dictionaries, a quiet place to study, a desk for study, textbooks, and calculators. *Students' cultural activity* is an index on how often students had visited a museum or an art gallery during the preceding year, attended an opera, ballet or classical symphony concert, and watched live theatre. All these variables are proxies for parental interest in the child, either directly in education matters, or general interest. They, also, indicate general

[17] Although the students were tested in the year were they turned 15, the oldest students (born in the beginning of the year) will be almost a year older than the students born at the end of that year. We thus measure their age in months to take account of potential age effects.

[18] Several summary indices were created for the official PISA dataset. They summarize students' or school principals' responses to a series of related questions selected from larger constructs on the basis of theoretical considerations and previous research. Indices were standardised so that the mean of the index value for the OECD student population was zero and the standard deviation was one. See OECD (2001b) for details on the construction of the indices.

parental interest in culture and education. These are variables, which are thought to be crucial for both the peer group choice parents directly or indirectly make for their children, and at the same time do such factors proxy for how much, and how qualified parental support the child will receive to promote academic achievement. Moreover, information is available on time spent on homework for Danish lessons, type of housing, and whether the parents were in their teens when their child was born.

– *School-related inputs* Hanushek (2002) points out that "characterizing school quality has been difficult, and thus it is highly likely that standard estimation of educational production functions with peers will overstate peer influence". In the present study we will therefore include various variables at the school and class level which potentially influence achievement and school (peer) choice.
We expect students to benefit from teaching practices that demonstrate teachers' interest in the progress of their students, when teachers expect their students to attain reasonable achievement standards, and when they are willing to help all students to meet these standards. The responses were combined to create an index of *teacher support*. Also, *teacher behaviour* is of importance for the learning environment, such as whether learning is hindered by low expectations of teachers, poor student-teacher relations, absenteeism among teachers, staff resistance to change, teachers not meeting individual students' needs, and students not being encouraged to achieve their full potential. Factors related to teacher education are also generally considered important. We use PISA data on how many of the teachers teaching Danish at the school are certified in Danish, and how many teachers have participated in professional development within the last three months.
In the empirical literature, the effect of school resources on scholastic achievement has produced few significant results. Hanushek (1986, 1996) provides an extensive discussion of this evidence. However, some recent studies have found more positive evidence (e.g. Card and Krueger 1992, Angrist and Lavy 1999, Krueger 1999, Wilson 2002). To avoid any confounding influences from systematic variations of school resources, we include *class size* as a measure of school resources in our estimations. Also, the absence of a suitable physical infrastructure (e.g. buildings) and an adequate supply of educational resources (computers, library, teaching materials) might hinder learning. Two indices were created by the PISA consortium – one for the perceived quality of the *school's physical infrastructure* and the other for the perceived quality of *educational resources*. Also, the *number of Danish lessons* per week, the *total number of teaching lessons* per year and the *frequency with which students use school resources,* like e.g. the library, are used as controls.
Private schools are sometimes perceived to be more effective in teaching children.

Especially in the Danish context, private schools have a great liberty with respect to the choice of curriculum and teaching methods. On the other hand, in a non-selective school system, private schools might be used as an alternative if the local public school is of low (peer) quality. We control for the type of school to investigate how the institutional arrangement of schools is related to student performance. Also, *school size* might be related to student achievement, due, for example, to economies of scale in the use of educational and economic resources.

There is a range of other student and school variables available in the PISA dataset, e.g. whether students attend special courses to improve results. However, we suspect there to be a large amount of reverse causality between reading skills and these variables, which is the reason for their omission from the estimations.

Table 1 provides descriptive statistics of the dataset used for the estimations.

Table 1: Descriptive statistics

Variable description	*Mean*	*Std.dev.*	*Min*	*Max*
Main variables of interest				
PISA reading score	501.9	93.4	89.3	887.3
Mean parental education of peers (years)	12.34	1.30	7.53	15.03
Std.dev. of parental education of peers	3.83	1.02	2.00	6.83
Background variables				
Gender (Male=0, Female=1)	.50	.50	0	1
Student's age at test	15.72	.27	15.25	16.17
Ethnic Dane	.95	22	0	1
Immigrant	.03	.18	0	1
Parents immigrated	.02	.12	0	1
Does not speak Danish at home	.05	.22	0	1
Number of brothers/sisters	1.90	1.28	0	12
Student is only child	.06	.23	0	1
Student is oldest child	.36	.48	0	1
Student is middle child	.23	.42	0	1
Student is youngest child	.35	.48	0	1
Student lives in nuclear family	.67	.47	0	1
... lives with single mother	.13	.34	0	1
... lives with mother and stepdad	.10	.29	0	1
... lives with single dad	.02	.15	0	1
... lives with father and stepmum	.02	.13	0	1
... lives without parents	.06	.23	0	1
... lives without parents (but parents live together)	.01	.09	0	1
Student's mother was teen at birth	.09	.28	0	1
Student's father was teen at birth	.10	.29	0	1
Mother: unskilled	.21	.41	0	1
- high-school degree	.04	.20	0	1
- vocational education	.30	.46	0	1
- short college	.04	.20	0	1
- long college	.21	.41	0	1
- university	.03	.18	0	1
- PhD	.003	.05	0	1
Father unskilled	.29	.45	0	1
- high-school degree	.03	.18	0	1
- vocational education	.41	.49	0	1
- short college	.04	.20	0	1
- long college	.11	.31	0	1
- university	.07	.26	0	1
- PhD	.003	.029	0	1
Mother: Legislator, senior official or manager	.03	.17	0	1
- Professional	.11	.32	0	1
- Technician or associate professional	.19	.39	0	1
- Clerk	.15	.36	0	1
- Service worker/shop&market sales worker	.17	.37	0	1
- Skilled agricultural or fishery worker	.00	.07	0	1
- Craft or related trades worker	.02	.13	0	1

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Variable description	Mean	Std.dev.	Min	Max
- Plant or machine operator or assembler	.04	.20	0	1
- Elementary occupations	.14	.35	0	1
- Armed forces	<.00	.02	0	1
Father: Legislator, senior official or manager	.08	.28	0	1
- Professional	.13	.33	0	1
- Technician or associate professional	.11	.32	0	1
- Clerk	.03	.17	0	1
- Service worker/shop&market sales worker	.04	.20	0	1
- Skilled agricultural or fishery worker	.04	.21	0	1
- Craft or related trades worker	.17	.37	0	1
- Plant or machine operator or assembler	.08	.27	0	1
- Elementary occupations	.10	.29	0	1
- Armed forces	.01	.08	0	1
Mother's income	.19	.10	0	2.08
Father's income	.29	.21	0	3.12
Mother's unemployment at student's age 15	.05	.16	0	1
Father's unemployment at student's age 15	.03	.14	0	1
Family wealth	.53	.74	-3.00	3.38
Parents home-owners	.72	.45	0	1
Parents home-renters	.19	.39	0	1
Parents living in not categorized type of dwelling	.01	.12	0	1
Parents' type of dwelling unknown	.07	.26	0	1
Index of parental academic interest	.11	.98	-2.2	2.72
– missing	.001	.03	0	1
Index of social communication	.22	.89	-3.65	1.2
Index of cultural possessions	-.11	.97	-1.65	1.15
Index of cultural activity	.28	.90	-1.28	2.93
Student has quiet place to study	.87	.33	0	1
Index of home educational resources	-.20	.92	-3.99	.76
Class size in Danish lessons	17.63	3.52	1	28
Class size in Danish lessons, missing	.10	.30	0	1
Number of Danish lessons/week	7.09	1.81	1	20
Number of Danish lessons, miss.	.31	.46	0	1
Hours spent on homework in Danish	2.96	.71	1	4
Data on homework (Danish) missing	.014	.12	0	1
Index of teacher support	.12	.70	-3.03	1.95
Index of teacher behaviour	-.52	.77	-2.41	1.22
School physical infrastructure	.04	.73	-1.12	3.38
School educational resources	-.16	.63	-1.90	2.48
Certified Danish teachers	.41	.34	0	1
Frequency of library use etc.	.84	1.01	-2.46	4.43

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Variable description	Mean	Std.	Min	Max
Schooling hours per year	881.83	153.48	594	1400
Number of students at school	437.13	195.93	49	917
Percentage of teachers on professional development	.48	.31	.05	1
School type (Public school=0, private=1)	.12	.32	0	1
Indicator: Variables at school level missing	.33	.47	0	1
				3666

Before going on to the estimations, let's have a short look at raw correlations between the variables of primary interest. In Table 2, we report results from simple OLS models regressing reading scores on either the mean peer level of parental (mothers') years of schooling in each student's classroom, the classroom heterogeneity or both. The results show that the mean peer level (PeerMean) is positively correlated with reading scores, and peer heterogeneity is negatively so (favouring homogenous classrooms, i.e. streaming students), when they are entered separately.

Table 2: Simple models

Reading scores	(1)			(2)			(3)		
	Coeff.	Std.dev.		Coeff.	Std.dev.		Coeff.	Std.dev.	
PeerMean	15.35	1.16	***	-			18.41	1.63	***
PeerHetero	-			-11.03	1.51	***	5.57	2.09	***
Adj. R^2			.045			.014			.047
*** indicates significance at the .001 level.									

However, as we suspected earlier, peer level and peer heterogeneity are not uncorrelated. As can be seen from Figure 1, there is a clear tendency between the peer level of a class and the heterogeneity of the class peers. Classes with a high peer level tend to be more homogenous than classes with a lower peer level[19]. When we enter the peer level and peer heterogeneity variable jointly in a regression (see Table 2, model (3)), the coefficient on peer heterogeneity turns positive. So, the large negative correlation between achievement and class-heterogeneity is clearly a result of the association between more heterogenous classes and lower parental education among pupils[20].

The following section presents results.

4 Results

To begin with, we will motivate our choice of variables for the main estimations. We then report OLS results, followed by results from quantile regressions and discussion.

[19] In the extremes of the joint distribution, this could be a mechanical relation, as high-level classes must be quite homogenous to reach a very high level of peer average. However, when we trim the sample, so we drop the observations with the 5% highest peer mean level, results are basically unchanged.

[20] This finding is confirmed by Lavy (1999) for schools in Israel.

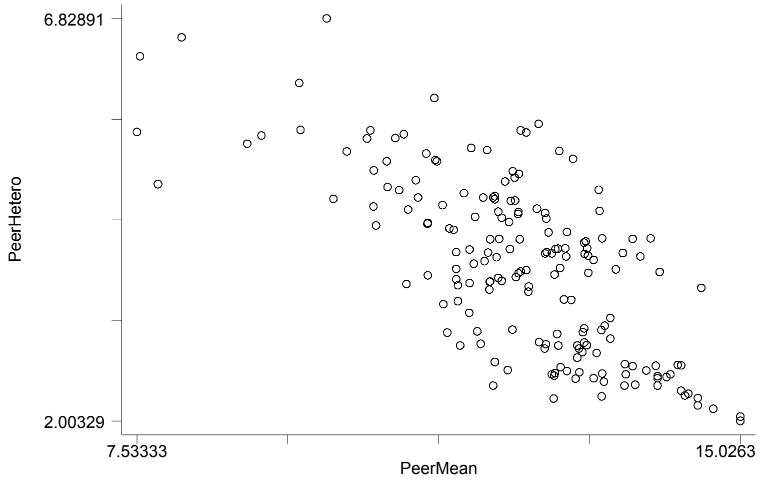

Figure 1: Correlation between peer mean and peer variance

4.1 Choosing the set of controls

As pointed out earlier, the estimation strategy for isolating the causal effect of school peers is to condition on a very broad set of factors that may affect both achievement and peer composition. This approach implies "washing out" the problem of unobserved ability by including a broad range of background variables in the regressions. Our data provides an extensive set of control variables that help avoid the usual problems of omitted variables bias. In addition to the standard set of controls (parental education, occupation, income and wealth; family structure, siblings, ethnicity, bilingual, gender, class size, teacher qualification = model (i) in Table 3), the PISA dataset provides us with other information on factors thought to be important for both the choice of peers and for reading skills. These variables are: parental academic interest, social communication, cultural possessions and activity, whether the student has a quiet place for study at home, the amount of time spend on homework and study for Danish lessons; and school resource variables like the number of Danish lessons per week, teacher support, teacher behaviour, the quality of the school building and the educational material in school, use of school resources, the student-teacher relationship, total hours of teaching, school size, the share of teachers having participated in extra qualification courses during the last three months, and whether the school is public or private (= model (ii) in Table 3).

We then add information from administrative registers on the parents' occupation, their housing conditions and whether they were teen-parents (model iii). Last, we exchange some survey variables with information from the registers, because they are more detailed and also, they are generally considered as being more reliable than survey data[21]

[21] There is a great deal of noise when we compare the parental education data from the survey and the register data, respectively. Also, with respect to the family structure variables, there is quite a bit of noise (the survey and register data deviate in about 20% of the observations). Some of the inconsistencies

(model iv). However, some information might be more useful in terms of measuring what we are really interested in when retrieved from surveys. One example of this is family structure. When asking the child who he/she lives with, the answer will perhaps be "coloured" be the child's own perception of the current situation in the family. However, this "subjective perception" will measure exactly what we are interested in, as we want to know whom of the parents and other family members the child feels most attached to.

Table 3: (Weighted) OLS results from different control sets

		PeerMean		PeerHeterogeneity	
Model	Set of controls	Coeff.	Std.dev.	Coeff.	Std.dev.
(i)	Standard controls	7.36	1.60**	2.36	1.94
(ii)	+ PISA variables	5.56	1.55**	3.14	1.85
(iii)	+ additional register information	4.68	1.55**	2.37	1.85
(iv)	+ survey exchanged with register data	4.97	1.58**	1.02	1.89
	Change (size of coefficients)	-33 %		-42 %	

** indicates significance at the .01 level. Model (i) includes as controls: parental education, wealth, income; family structure, siblings, age, ethnicity, bilingual, gender, teacher education, class size. Model (ii) adds parental academic interest, social communication, cultural possessions, cultural activities, home educational resources, number of Danish lessons/week, total number of teaching lessons/year, homework, teacher support, teacher behaviour, school building conditions, school education resources, school resource use, student-teacher relations, grade, school size, public/private school. Model (iii) adds parental occupation, type of dwelling, teen-parents. Model (iv) exchanges survey information on parental education and income.

In table 3, we report coefficients on the two peer variables (the mean and the variance of the peer group) from simple OLS estimations with four different sets of controls. When we compare the results from the standard set of controls (model i) with the full sets of controls available in this study (model iv), we see that the peer mean effect drops by 1/3, but is still significant at the 1% level, while the coefficient for the heterogeneity of the peer group has fallen by even more (and remains insignificant).

We conclude that the set of controls included in the estimations is of great importance for reducing selection bias in our peer effect model. Therefore, we choose to use the most extensive set of controls (model iv) throughout the empirical analysis in this paper.

4.2 Ordinary Least Squares

The results of a simple OLS estimation using our favorite control set (=model (iv) in Table 3) is shown in Table 4.

between the survey and register data might be due to the fact that they refer to the year, were the students were 14 years old, while the surveys where answered when the student was 15 years old.

Table 4: Ordinary least squares results (weighted)

PISA reading scores			
Independent variable	Coefficient	Std.err.	
Peer variables			
Mean parental education of peers (years)	4.97	1.58	**
Std.dev. of parental education of peers	1.02	1.89	
Individual and family background			
Male	(reference category)		
Female	22.81	2.76	**
Student's age at test	11.92	4.84	*
Ethnic Dane	(reference category)		
Immigrant	-17.73	10.24	(*)
Parents immigrated	-27.98	12.59	*
Does not speak Danish at home	-45.32	8.11	**
Number of brothers/sisters	-2.76	1.41	*
Student is only child	-14.08	6.34	*
Student is oldest child	(reference category)		
Student is middle child	-0.69	4.12	
Student is youngest child	-7.78	3.19	*
Student lives in nuclear family	(reference category)		
... lives with single mother	-6.69	4.74	
... lives with mother and stepdad	-5.41	5.05	
... lives with single dad	1.99	9.84	
... lives with father and stepmum	-4.71	10.16	
... lives without parents	-31.18	15.56	(*)
... lives without parents (but parents live together)	-31.85	19.35	
Student's mother was teen at birth	-14.92	9.37	
Student's father was teen at birth	19.56	20.74	
Mother: unskilled	(reference category)		
- high-school degree	15.47	6.94	*
- vocational education	3.21	3.60	
- short college	7.65	7.32	
- long college	17.60	5.29	**
- university	32.99	8.94	**
- PhD	-1.17	42.68	
Father: unskilled	(reference category)		
- high-school degree	25.45	7.97	**
- vocational education	8.55	3.56	*
- short college	18.35	7.18	*
- long college	23.02	5.76	**
- university	23.66	7.06	**
- PhD	55.59	22.02	*

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Independent variable	PISA reading scores		
	Coefficient	Std.err.	
Mother's income	3.06	11.2	
Father's income	8.98	8.15	
Mother's unemployment at student's age 15	-.19	8.41	
Father's unemployment at student's age 15	1.30	10.41	
Family wealth	-5.44	2.03	**
Parents home-owners	(reference category)		
Parents home-renters	-4.21	4.12	
Parents living in not categorized type of dwelling	-13.99	11.05	
Parents' type of dwelling unknown	11.59	9.51	
Index of parental academic interest	16.19	1.63	**
– missing	-98.87	40.49	*
Index of social communication	.18	1.67	
Index of cultural possessions	3.24	1.61	*
Index of cultural activity	1.91	1.69	
Student has quiet place to study	8.36	4.20	*
Index of home educational resources	2.56	1.91	
School variables			
Class size in Danish lessons	1.25	.41	**
– missing	-10.59	8.48	
Number of Danish lessons/week	.36	.93	
– missing	-29.1	7.21	**
Hours/week spent on homework in Danish	-2.36	1.93	
– missing	-15.11	13.02	
Index of teacher support	-.90	2.13	
Index of teacher behaviour	.26	2.14	
Index of school physical infrastructure	.67	2.27	
Index of school educational resources	-3.95	2.53	
Percentage of certified Danish teachers	1.24	7.81	
Index of frequency of library use etc.	6.60	1.69	**
Index of student/teacher relations	9.19	1.95	**
Schooling hours per year	.003	.01	
Number of students at school	.02	.01	(*)
Percentage of teachers on professional development	-.49	5.29	
School type (Public school=0, private=1)	-6.85	5.19	
Indicator: Variables at school level missing	20.26	12.76	
Constant	198.49	82.21	*
Job position dummies included and jointly significant			
Adjusted R^2			29.22
No. of obs.			3666

(*), * and ** indicate significance above the .10, .05 and .01 level, respectively.

Of greatest interest for our purposes are the coefficients on the peer effects. Both peer variables are positive, but only the peer *level* variable is significant. Thus, attending a school with peers from more well-educated homes is associated with higher reading scores. An increase in the average educational background of one's peers by one year is associated with an increase of one's reading scores by almost 5 points on the PISA scale, which amounts to 5% of the standard deviation of the scale. Stated alternatively, moving a student from a class with a mean peer background of 9 years of parental schooling (corresponding to compulsory education) to a class with a mean of 16 years of schooling will increase that student's achievement level by 35 points – or 37% of the standard deviation of the scale. Compared to important family effects, the peer effect is important in size. Students from households with a mother with a vocational education have predicted reading scores that are 37 points lower than their counterparts who have a mother with a university degree. Here, the peer group effect of moving from a peer group with mean parental education of 9 years to a peer group where this number is 16 is of the same size as the effect of own parental education by the same amount, which is remarkable.

Peer effects may be represented not only through the mean ability of classmates, but also via the standard deviation in the abilities of classmates. The results on the peer heterogeneity variable in Table 4 suggest that a student's achievement level is higher, when the heterogeneity of abilities within the classroom is greater suggesting that mixing ability types generates educational benefits. However, the effect is imprecisely estimated and thus not statistically significant different from zero.

While the primary interest of this study is on peer effects, let's have a brief look at the results for the control variables, anyway. Most of the estimated coefficients of these variables are statistically significant and are usually in the direction one might expect a priori. For example, parental education and occupation (not shown), not being the youngest child, having fewer siblings, and attending a bigger school are positively related to test scores – results typical of the literature. Also, female students have higher predicted scores on average, as do native speakers. The effect of growing up with only one natural parent is negative, but not (strongly) significant. Also, counterintuitively, being an only child is associated with lower reading scores, while the effect of having many siblings is also negative. Apparently, there is some "optimal" number of siblings, while being an only child might reflect some other factors in the family that have resulted in the decision not to have more children (e.g. (female) career decisions, personal/family problems). Interestingly, while income is not related to test scores, wealth is, but negatively so. We suspect this to be a result of the somewhat problematic way of assessing family wealth in the PISA data. The "wealth" variable has been derived from students' reports on the availability, in their home, of a dishwasher, a room of their own, educational software, a link to the Internet; and the number of cellular phones, television sets, computers, motor cars and bathrooms at home. We suspect these factors not to be good indicators of family wealth in the case of Denmark. Also, we know that the PISA index on family wealth has been constructed in a way so its mean is 0 and its standard deviation is 1 in the total OECD sample. In our Danish subsample, the mean is .53, while the standard deviation is only .74. Thus, there is a comparably high level of "wealth" as measured by the PISA index, but only little variation compared to the OECD average, which may contribute to the unexpected coefficient on the wealth variable in our estimation[22]. Another counterintuitive sign is the positive one on class size. We suspect that there is some selection of not so well doing students into smaller classes.

We also note that *parental academic interest* is positive and strongly significant, which underlines the important role of this typically unobserved characteristic. An increase of one standard deviation of the academic interest index is associated with a test score increase of 16 points, which is about half of the advantage of living with both natural parents instead of living alone at the age of 15. Also, cultural possessions like classical literature, the availability of a quiet place to study, and the frequency with which school resources, like e.g. the library, is used are all positively related to reading scores, while the coefficient on social communication, cultural activity, and home educational resources is positive, but insignificant.

The PISA dataset gives information about both school principal reported disciplinary climate at the school and student reported disciplinary climate in the classroom. Counterintuitively, disciplinary climate at both the school and classroom level are negatively related to outcomes – but are not significant when mean peer background is controlled for, indicating that disciplinary problems are more prevalent in classes with a lower peer environment.

4.3 Robustness

Finally, we have explored the sensitivity of our results to changes in the specification of our model. We tested for nonlinear effects of school resources by adding squares of the peer mean measure. The linear and the square of the peer mean measure were *partially insignificant* when entered in the equation together, but were *jointly strongly significant.* The square effect was negative, implying decreasing returns to peer quality – which makes mixing the optimal policy if the aim is to maximize the overall level of reading skills.

To determine whether the peer effect is dependent on the student's own parental background, we include a variable to make the peer mean effect interact with own parental background. The negative sign on this interaction term implies that a higher average peer quality (measured by years of schooling of the classmates' parents) has smaller effects on individual student achievement for students with high-educated parents than for students from low-educated homes. Stated differently, raising the average parental background of the peers yields greater benefits for students with less favourable parental background. However, both the main effects coefficients and those on the interaction are insignificant

[22] We have examined whether the perverse sign for wealth is the result of strong correlation of wealth with variables as parental income, education and occupation. This is not the case. In fact, these variables together explain only little of the variation in wealth (about 10%), and some of the single coefficients even have the "wrong" sign. This leaves as an explanation for the perverse sign on wealth the argument that wealth is measured poorly for the Danish PISA data.

(however, they are jointly significant), suggesting that the effects are intertwined and cannot credibly be estimated separately. We also wanted to estimate results on subsamples divided by parental education, but the sample size was too small. The coefficients on the peer variables were mostly insignificant and did not show any clear pattern.

In the existing literature, a range of variables have been used to proxy for the peer environment at schools, e.g. the racial composition, and the share of students from two-parent families. In reality, the peer environment at school is composed of many factors. As it is not obvious which variables to use as a proxy for peer quality, we have experimented with a range of characteristics. Using alternative measures of peer effects reinforces the earlier results. Whether we use the mean and variance of parental *occupational status*, the father's education, the share of bilingual students in the class, or the share of students living with both natural parents as our peer group indicator, we tend to find significant mean peer effects with the "right" sign and an insignificant peer heterogeneity effect.

4.4 Quantile regression results

Ordinary least squares methods estimate effects at the mean. By calculating regressions for different quantiles, it is possible to explore the entire shape of the conditional distribution. As will be shown, the quantile regression results suggest some important differences across different points in the conditional distribution of reading scores.

The partial derivative of the conditional quantile of y with respect to one of the regressors is to be interpreted as the marginal change in the θth conditional quantile due to a marginal change in x. One should be cautious in interpreting this result. It does not imply that a person who happens to be in the θth quantile of one conditional distribution will also find himself at the same quantile had his x changed. But this is mainly a problem with dummy explanatory variables, where a marginal change is from 0 (the lowest possible value) to 1 (the highest possible value). In the present study, both variables of main interest are continuous, making this potential problem less severe.

Results from the estimation of quantile regressions at the 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} conditional percentile are presented in the Appendix[23]. In Figure 2, we report only the coefficients on the main parameters of interest to this study – the peer level and peer heterogeneity. The 90% confidence bounds are also plotted. The thin lines show the OLS estimate and its confidence bands. Homogeneity in the peer level and heterogeneity effect over the conditional test score distribution would imply that it is possible to draw a horizontal line through the point estimates at different quantiles. However, in Figure 2 the point estimates suggest a pattern of effects that are larger in magnitude at the lower quantiles as compared to the higher quantiles. The quantile regression (QR) coefficients do clearly show a decreasing trend over the distribution suggesting that having good peers

[23] Estimates were calculated using the *sqreg* procedure of STATA version 7.0.

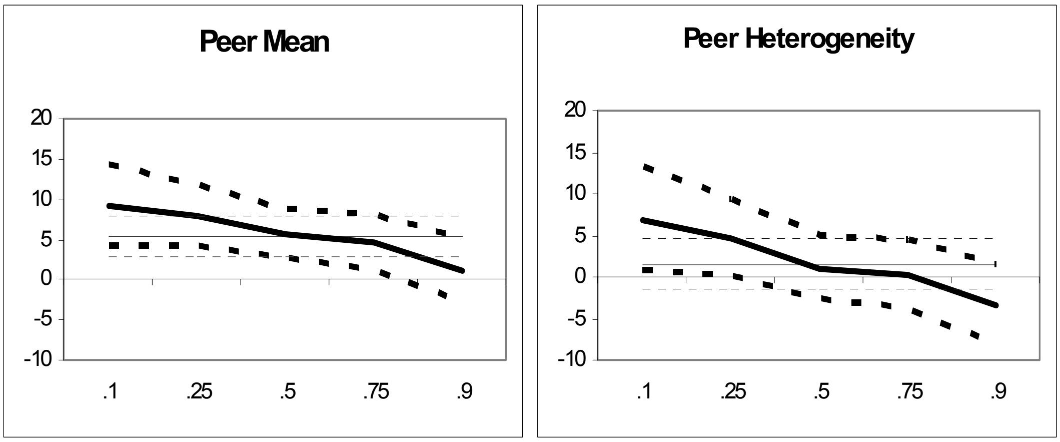

Figure 2: Quantile regression results for peer variables

is more important for low achievers than for high achievers. A test for the equality of coefficients at the 1th and 9th decile suggests that the effects are significantly different at the 10% level. A similar trend is present in the results for the heterogeneity of the peers in the classroom. However, while the effect of the average peer quality in the classroom remains positive and significant over most of the test score distribution, the effect of peer heterogeneity is only weakly significant at the 10% level for the two lowest quantiles, and is insignificant – and even turning negative – for higher quantiles. These results indicate that low achievers profit from a heterogenous classroom, while the effect for high achievers is not significantly different from zero.

Combining the results on peer level and class heterogeneity gives some suggestive policy advice. The QR coefficients show a decreasing trend over the distribution suggesting that having good peers is more important for low achievers than for high achievers making mixing the preferable policy if the overall aim is to maximize average student performance. However, increasing the ability mix does not only mean reducing disparities of the average educational background of classrooms, but does necessarily also imply increasing classroom heterogeneity. The peer level effect and the classroom heterogeneity effect are intertwined. As the heterogeneity coefficient is positive and weakly significant for weak readers, while being insignificantly different from zero for the upper part of the distribution, together with the result that the effect from good peers is more effective at the lower quantiles, suggests that mixing is the optimal policy for maximizing overall achievement.

It is important to stress that this solution is not pareto-optimal. However, when the aim of the social planner is to maximize overall reading scores, the marginal products must be equated across students at different points of the conditional test score distribution. As the results show, the marginal product from a better peer group is higher at lower quantiles than at higher quantiles. Equating marginal products by increased mixing will therefore increase the overall reading performance.

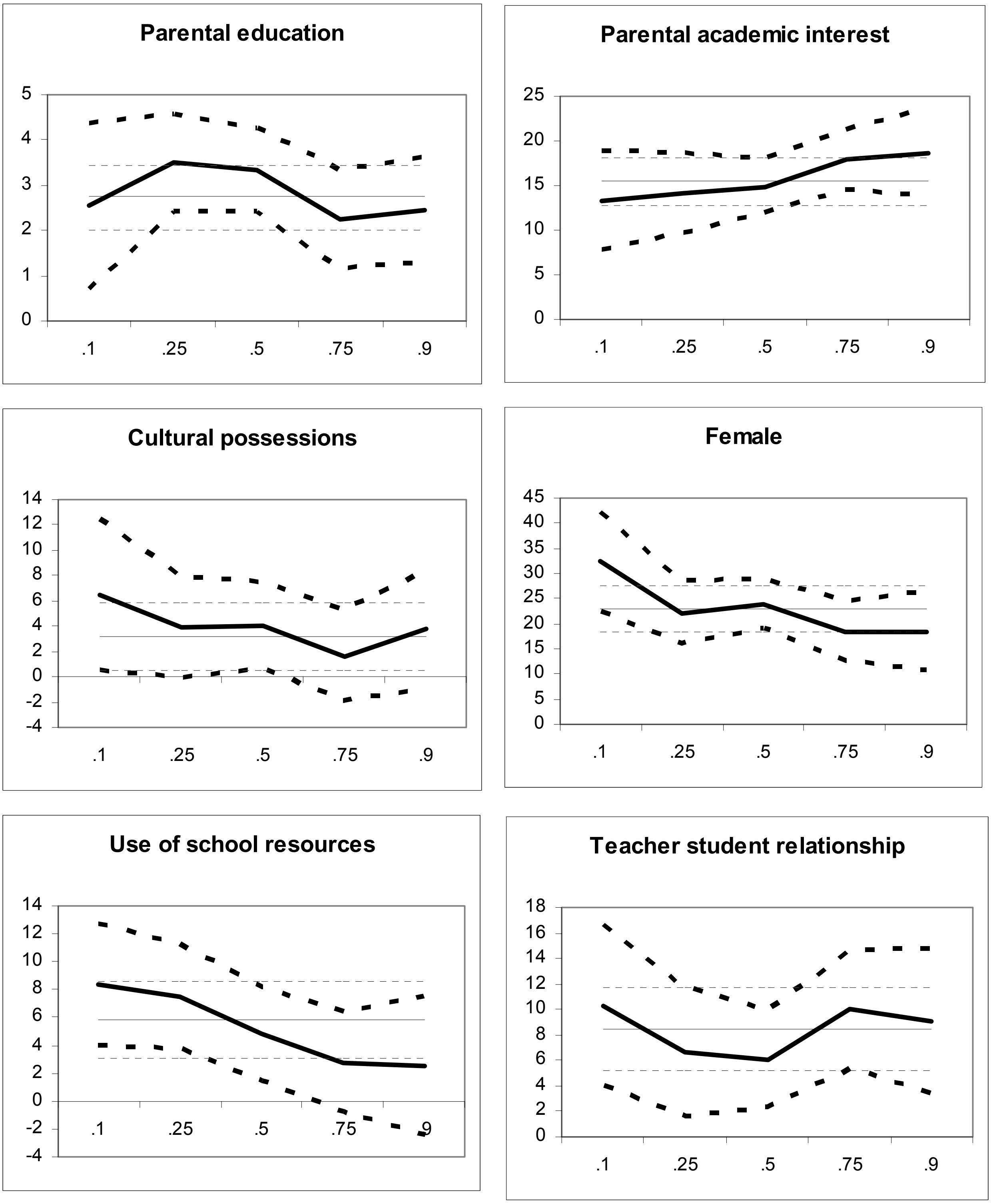

Figure 3: Quantile regression results for selected control variables

As the peer variables are of main interest in this paper, and the OLS results for the control variables have been reported in some detail, we will confine our discussion to only a few of the covariates. A full results table is provided in the Appendix. Figure 3 shows quantile regression results for six important control variables.

The effect of parental education on reading scores fluctuates somewhat around the OLS estimate (shown with thin lines), but without any clear pattern and it does not leave the OLS confidence interval[24]. The same is true for the index of parental academic interest (upper right panel of Figure 3), but here the pattern is a steady increase of the effect over the reading score distribution, suggesting that parental academic interest is an important determinant of reading scores for students over the whole test score distribution, but the effect seems to be more important for better readers. Interestingly, the opposite is true for cultural possessions (middle left panel), which are significantly positive related to reading scores only for weak readers, while the effect at the upper end of the distribution is insignificant. The middle right panel of Figure 3 shows that girls read better than boys over the whole range of the distribution, but the effect is particularly important at the lower tail, where the quantile regression point estimate is outside the OLS confidence interval, indicating that the effect of being a girl on reading scores may not be constant across the conditional distribution. The effect of using school resources like the library, the Internet etc. frequently (lower left panel) is positive and significant only at the lower end of the conditional test score distribution, while the effect is close to zero for the higher quartiles (0.75 and above). Apparently, especially weak readers profit from school resources over and above classroom teaching. Interestingly, the teacher student relationship (lower right panel) seems to be more important at *both* extremes of the test score distribution than for average students: both weak and strong readers respond strongly to teacher student relations, while students in between seem to be more independent from the relationship with their teachers.

In several panels in Figure 3, quantile regression estimates lie at some point outside the confidence intervals for the OLS regression, suggesting that the effects of these covariates may not be constant across the conditional distribution of the independent variable. However, formal testing suggests that the coefficients for the first and last quantile estimated is significantly different only for gender.

To recall, quantile regression techniques estimate peer effects at different values of the *conditional* test score distribution. This means that when we talk about "weak readers", we do not necessarily refer to students with low (absolute) test scores, but to students reading less well than we would expect them to according to their background characteristics. Thus, "weak readers" are, e.g., students from well-educated homes, who read less well than the average student with well-educated parents. However, for policy purposes, we are usually interested in students being poor readers *unconditional* on their

[24] For simplicity of the exposition of results, the parental education dummies were recoded into a continuous variable indicating the number of years of schooling attained by the parent with most schooling.

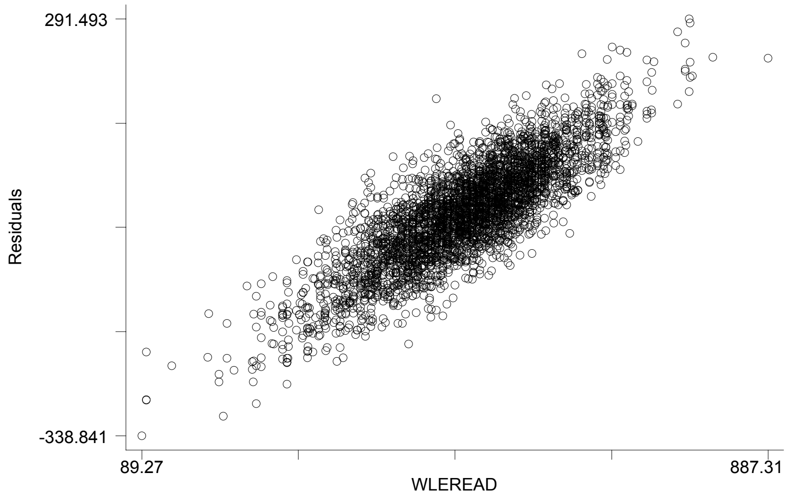

Figure 4: Unconditional versus conditional reading scores

background. It is thus useful to examine, how unconditionally poor readers are related to conditionally poor readers. If we, e.g., can show that the *conditionally* poor readers are typically identical with the *unconditionally* poor readers, then, our quantile regression results can be transferred directly to the unconditional distribution of test scores. To find out about the relationship between the conditional and unconditional distribution, we plot the residuals from the OLS regression (=conditional test scores) against raw test scores in Figure 4. Unconditional and conditional test scores are closely related, which illustrates the tendency of the conditionally poor readers to be identical with those who get low raw test scores. This means that all results extracted above for the conditional distributions are generally valid for the unconditional distribution, too. This relationship makes our results much more easy to interpret for policy advice.

5 Conclusion

We investigated empirical peer group effects in the Danish, non-selective, school system. Ordinary Least Squares results (mean effects) showed that there are sizeable positive effects of attending school with peers with better educated parents. However, it could not be shown that the heterogeneity of the classroom affects reading scores.

Also, our findings indicate for *whom* peers may matter, not just whether or not they matter on average. The paper uses quantile regressions to describe effects at different points of the conditional test score distribution. Quantile regressions provide "snap-shots" at different points of a conditional distribution and, thus, are a parsimonious way of describing the whole distribution. The estimation of education production functions at several points of the test scores distribution revealed several interesting aspects that would not be apparent by just examining a single regression equation, such as the mean. Quantile regression results suggest that there may be differential peer group effects at different points in the conditional test score distribution. Generally, peer group effects are stronger at the lower end of the conditional test score distribution, confirming the suggestion in the peer literature that low achievers are *dependent learners*. Such differences in the peer effect is probably due to students with an educationally rich home environment, being likely to do relatively well in most school environments. In contrast, the performance of students from more educationally impoverished backgrounds may depend more heavily on school factors as e.g. teacher quality, school resources, and their classmates.

Thus, quantile regression evidence reveals that the effect is stronger for weak readers. This suggests, together with the insignificance of the coefficient on classroom heterogeneity for the upper end of the conditional distribution of test scores that mixing, rather than ability grouping of students, is preferable, when the aim is to maximize overall reading performance.

In this paper, we show how our rich set of controls (we add variables form the PISA dataset and administrative data to the usual set of controls) decreases the peer effect coefficients compared to results from estimations with a smaller set of controls. This shows that the comprehensive dataset we use eliminates bias due to selection of better students into high-quality peer groups. However, while the bias is reduced, we cannot be sure that it is eliminated entirely. Also, having only data on reading skills as outcome measure of schooling, limits the scope of our results, if peer effects with respect to achievement in other subjects (e.g. math or science) differ from peer effects for reading skills. With the future release of data for the next PISA waves (PISA 2003, 2006) which will focus on mathematical (2003) and science skills (2006), analyses on whether peer effects differ over subjects will be feasible.

Finally, the peer effects we estimated are those present in a system with the deliberate policy not to be selective. Results could change, when ability grouping became the favoured policy approach, because this would constitute a shift in the educational system, with an entire range of possible accompanying changes. E.g. the psychological effect of being placed in a low ability track, the effect on teacher moral, teaching practices and curriculum changes, when ability grouping becomes a policy rather than a feature that is a product of differences in location pattern across different groups of families. How a fundamental change in the education system would affect the mean and distribution of test scores will depend crucially on the particular features of the system to be implemented.

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Appendix

Table A1: Quantile regression full results tabel

Decile	0.1		0.25		0.5		0.75		0.9	
Independent variable	Coef.	Std.	Coef.	Std.	Coef.	Std.	Coef.	Std.	Coef.	Std.
Peer variables										
Mean parental education of peers (years)	**9.284**	**3.076**	**8.069**	**2.332**	**5.649**	**1.953**	**4.423**	**2.227**	1.826	2.505
Std.dev. of parental education of peers	*6.821*	*4.032*	*4.727*	*2.867*	1.119	2.247	-.025	2.659	-3.112	3.144
Individual and family background										
Male					(reference category)					
Female	**33.758**	**5.564**	**24.147**	**4.302**	**21.431**	**3.205**	**17.768**	**3.765**	**21.696**	**4.755**
Student's age at test	7.124	9.937	10.760	8.049	6.908	6.540	10.553	6.644	6.117	8.693
Ethnic Dane					(reference category)					
Immigrant	-35.813	24.508	-25.668	18.949	-16.842	15.687	-.455	14.903	-3.592	16.854
Parents immigrated	-52.810	32.749	-11.309	22.007	-8.224	13.904	*-28.499*	*14.906*	-13.910	31.360
Does not speak Danish at home	**-45.832**	**20.636**	**-47.013**	**14.680**	**-50.428**	**12.224**	**-39.527**	**12.204**	**-44.640**	**13.707**
Number of brothers/sisters	-4.256	3.169	-2.546	2.422	-3.148	1.950	-3.258	2.166	2.409	2.931
Student is only child	-19.180	13.597	*-18.049*	*10.395*	*-17.389*	*9.272*	-9.180	8.584	7.320	10.524
Student is oldest child					(reference category)					
Student is middle child	-.807	8.452	5.756	6.387	-.966	6.368	-.542	5.924	-3.563	7.151
Student is youngest child	*-12.111*	*6.335*	-4.446	5.454	*-6.906*	*3.864*	**-9.951**	**4.412**	-6.010	5.256
Student lives in nuclear family					(reference category)					
... lives with single mother	-8.436	9.551	-1.045	7.507	-8.222	6.050	5.695	7.423	-2.402	9.439
... with mother and stepdad	-4.283	9.168	-4.608	7.495	-4.715	5.884	-4.539	6.382	*-15.869*	*8.839*
... with single dad	.020	15.346	-4.439	19.679	11.291	11.562	-4.319	11.476	-6.237	16.516
... with father and stepmum	-14.297	18.908	-8.736	15.384	-8.105	12.781	-1.610	13.734	-7.746	20.757
... without parents (parents together)	-30.257	35.650	-9.622	36.239	-14.938	30.649	-25.967	25.850	-30.762	34.596
... without parents (parents not together)	-39.161	46.370	30.973	27.543	25.594	22.357	21.691	17.781	-11.054	27.179
Student's mother was teen at birth	-13.033	19.313	-10.064	13.033	-15.616	10.511	-20.122	13.733	-.571	16.052
Student's father was teen at birth	18.641	32.924	48.268	31.361	17.691	15.859	2.028	24.403	-17.153	24.584
Mother: unskilled					(reference category)					
- high-school degree	**36.38**	**12.44**	**25.732**	**10.055**	*15.865*	*9.328*	7.995	10.186	**32.411**	**14.717**
- vocational education	8.95	6.77	*10.341*	*5.953*	3.331	4.086	.367	4.762	-1.958	5.77
- short college	14.64	13.03	5.605	11.149	2.692	9.525	15.511	11.606	11.977	11.086
- long college	**27.14**	**10.90**	**22.366**	**8.092**	**13.757**	**6.440**	**18.235**	**7.878**	**18.616**	**8.251**
- university	**53.70**	**16.22**	**38.974**	**13.035**	**36.677**	**10.748**	**33.573**	**13.278**	**35.358**	**14.957**
- PhD	*65.96*	*38.18*	-1.374	35.879	-.911	39.067	15.678	47.522	-34.042	49.749
Father: unskilled					(reference category)					
- high-school degree	23.89	19.11	**28.515**	**10.555**	19.247	12.473	**27.900**	**10.674**	**42.090**	**12.509**
- vocational education	2.58	6.99	5.664	5.567	6.105	5.093	**13.305**	**4.218**	**19.353**	**5.430**
- short college	8.51	13.64	16.454	10.230	*18.437*	*10.889*	**21.598**	**8.280**	*19.341*	*10.712*

(...continued)

... continued

	0.1		0.25		0.5		0.75		0.9	
	Coef.	Std.	Coef.	Std.	Coef.	Std.	Coef.	Std.	Coef.	Std.
Father: long college	**23.91**	**9.68**	**25.774**	**7.868**	**23.527**	**8.295**	**22.635**	**7.112**	**21.960**	**9.075**
- university	**38.46**	**13.01**	**27.579**	**9.934**	**21.295**	**9.484**	**23.924**	**9.643**	*23.566*	*12.574*
- PhD	-14.38	29.66	-46.312	43.918	-31.256	51.595	50.221	98.428	143.698	93.919
Mother's income	27.9	27.5	3.96	18.6	-10.9	17.7	4.55	21.9	8.83	24.3
Father's income	-1.62	14.9	1.11	1.16	2.69	9.90	10.9	11.8	6.22	13.8
Mother's unemployment at student's age 15	14.761	13.862	-8.736	11.334	2.662	11.610	7.249	11.135	-1.522	11.492
Father's unemployment at student's age 15	-6.557	26.777	-18.857	18.582	6.793	14.501	10.940	16.921	23.406	16.097
Family wealth	**-10.554**	**4.112**	**-6.095**	**2.825**	-3.957	2.758	-3.516	*2.564*	*-6.689*	3.787
Parents home-owners					(reference category)					
Parents home-renters	4.425	7.558	-3.616	6.165	*-9.277*	*4.851*	**-14.052**	**5.406**	*-13.877*	*8.246*
Parents living in not categorized type of dwelling	-13.924	33.135	1.549	16.865	-8.334	11.319	-21.226	14.086	-28.503	19.069
Parents type of dwelling unknown	15.496	13.012	7.392	10.719	4.237	12.831	-6.153	14.114	2.538	20.078
Index of parental academic interest	**13.324**	**3.406**	**14.337**	**2.753**	**14.818**	**1.820**	**17.321**	**2.072**	**17.359**	2.615
Index of social communication	3.229	3.377	1.960	2.435	1.373	2.149	-2.732	2.172	-2.118	2.946
Index of cultural possessions	**6.479**	**3.233**	**6.107**	**2.444**	**3.730**	**1.981**	.541	2.073	1.423	2.513
Index of cultural activity	-.031	3.609	1.342	2.853	2.088	2.193	2.268	2.603	3.642	2.822
Index of home educational resources	4.369	3.957	5.84	3.03	-.248	2.248	.631	2.556	*5.536*	*2.885*
Student has quite place to study	15.714	11.015	6.820	5.821	6.660	5.864	5.738	5.583	4.921	7.135
School variables										
Class size in Danish lessons	.093	.819	.847	.680	**1.176**	**.588**	.708	.523	**1.483**	**.752**
Number of Danish lessons/week	1.180	1.551	.847	1.520	.948	1.148	-.129	1.050	-.887	1.573
Hours/week spent on homework in Danish	.303	3.996	-1.064	3.049	-.340	2.363	-3.263	2.767	-5.574	3.468
Index of teacher support	1.167	3.919	1.773	3.777	1.586	2.802	-2.071	2.873	-3.276	3.877
Index of teacher behaviour	-.922	3.771	-.617	3.116	2.323	2.749	-.390	3.108	3.582	3.481
Index of school physical infrastructure	1.673	4.217	4.432	3.182	2.041	2.628	-2.283	2.929	**-8.519**	**3.876**
Index of school educational resources	-6.127	4.849	**-8.623**	**4.112**	-3.572	3.057	-2.269	3.629	.072	4.050
Percentage of certified Danish teachers	12.537	16.071	-2.630	11.641	-2.710	10.267	5.427	9.884	14.406	10.078
Index of school resource use (e.g. library)	**10.048**	**2.889**	**7.803**	**2.468**	**4.648**	**2.131**	1.898	2.382	3.504	2.694
Index of student/teacher relations	**10.001**	**3.879**	**6.629**	**3.165**	**5.637**	**2.472**	**9.986**	**2.323**	**8.764**	**3.809**
Schooling hours per year	-.024	.022	-.006	.018	.001	.015	.010	.015	*.035*	*.020*
Number of students at school	.016	.019	.010	.016	.026	.015	.026	.016	.019	.018
Percentage of teachers on professional development	6.566	9.142	-8.598	7.970	-.195	7.268	-5.095	7.400	-11.338	9.765
School type (Public school=0, private=1)	-3.629	9.248	-4.689	7.657	-5.576	6.679	-9.067	7.379	-12.941	10.450
No. of obs.										3666

Bold (italic) figures indicate significance above the .05 (.10) level, respectively. Missing categories for parental education, ethnicity, parental academic interest, class size in Danish lessons, number of Danish lessons/week, hours/week spent on homework in Danish, missing school level variablesand a constant included. Job position dummies included and jointly significant.