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Teacher characteristics and student performance in India: A pupil fixed effects approach

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Abstract

This paper exploits unique data that permits the matching of students' test scores in different subjects to the teachers that teach those subjects. Within-pupil (across-subject, rather than across-time) variation is used to examine whether the characteristics of different subject teachers are related to a student's marks across subjects. There are four main contributions. *Firstly* the findings, using a credible methodology for identification, give only modest grounds for optimism about the effects of teacher policies. A teacher's possession of Masters level qualification and pre-service training have well identified but small effects on student achievement. While a teacher's union membership strongly reduces pupil achievement, union membership is typically not a policy variable. The bulk of the variation in student achievement is a school fixed effect and observed school characteristics explain less than 30% of this fixed effect. The *second* main contribution of the paper is to highlight the importance of 'controlling for' the non-random matching of students to schools and teachers. The finding that within-pupil effects of many teacher variables differ very significantly from the across school effects indicates that much of the extant achievement production function literature – which perforce relies on across school estimation – leads to incorrect inferences because it confounds the effect of unobserved school and pupil heterogeneity with the effect of teacher characteristics. This underlines the importance of finding credible sources of within school and preferably within-student variation in future research. *Thirdly*, the paper showcases the use of an across-subject estimator of the achievement production function which is similar to the more familiar panel data approach but which circumvents the problem of non-random attrition of students/teachers over time and the problem of non-random matching of students to teachers, and which permits the identification of teacher effects in cross-section data that are readily available. *Finally*, a school fixed effects equation of teacher pay shows that while teacher compensation is efficient in some respects, i.e. teachers are rewarded for characteristics that raise student achievement, it is not so in other respects. In particular, union membership is substantially rewarded when in fact it is associated with significantly lower student achievement, raising the question whether teachers' right to unionize pits teacher interests against student interests.

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Teacher pay and student performance: A pupil fixed effects approach

1. Introduction

Do higher teacher certification and greater teacher pay raise student achievement, and to what extent? Are teacher compensation policies efficient, i.e. are teachers rewarded for possession of characteristics that raise student achievement? These and similar questions about the effect of teacher policies on pupil achievement are of intense interest to education policy makers. While the question whether teacher characteristics affect student achievement has also deeply interested researchers, so far results have been mixed and one influential strand of research is pessimistic that teacher input based policies (increasing teacher education, training, experience and pay) can make a difference to student performance.

In the existing literature, one type of study measures teacher quality and then relates teacher characteristics to it. It measures teacher quality as a teacher fixed effect in a student achievement equation using data where a teacher is matched to students in the various classes of a given grade she/he taught in a year or the cohorts she/he taught over various years. Studies by Aaronson, Barrow and Sander (2003), Rockoff (2004), Hanushek, et. al. (2005) and Rivkin et. al. (2005) follow this approach. They find that teacher quality measured in this way is reasonably stable over time. While students taught by ‘high quality’ teachers have significantly higher achievement, resumè characteristics on which teacher compensation is based – such as teacher education, training, and experience – explain little of the variation in teacher quality.

Another type of study examines the relationship between teacher characteristics and student achievement *directly*. Some such studies have used experimental methods, mainly investigating the effect of teacher incentives (Duflo and Hanna, 2005; Lavy, 2003; Glewwe, Ilias and Kremer, 2005; Muralidharan, 2006). Other studies have used statistical approaches such as an instrumental variable approach (Hoxby, 1996; Kingdon and Teal, 2005; Sprietsma and Waltenberg, 2005), a value-added approach (see >30 value added studies summarized in Hanushek, 2003) or a panel data approach (Clotfelter, Ladd and Vigdor, 2006). The evidence from these studies is not undisputed. The lack of agreement in findings has sometimes led to impassioned disagreements about interpreting research results (Krueger, 2003; Hanushek, 2003).

The disputes about the effects of teacher (and school) inputs are due to the ubiquitous problem that students may match to schools and teachers endogenously. While randomized experiments, propensity score matching techniques, IV methods, discontinuity-design approaches and panel data methods have been used in various recent studies, each has its drawbacks and each approach is dependent on the availability of certain types of data. For instance, while randomized experiments provide a good solution to the problem of endogeneity, they are not above criticism[1] and, in any case, experimental data are still uncommon. Although quasi-experimental approaches such as propensity score matching methods have occasionally been used to evaluate the impact of education programs (e.g. Machin and McNally, 2004; Jalan and Glinskaya, 2002), they require the questionable assumption that matching based on pre-treatment observables adequately captures all relevant characteristics of treated units. Valid instrumental variables are difficult to find and few studies have convincingly tested impact effects in education using the IV approach. While discontinuity design studies (e.g. Angrist and Lavy, 1999) provide a promising way out of the endogeneity conundrum, this approach is often infeasible in many developing countries because even where official (exogenous) rules, e.g. about maximum class-size or school start age etc., exist in law, they are rarely adhered to in practice. The panel data approach, even if we set aside the heightened problem of measurement error bias, is often infeasible because of the absence of longitudinal education data in most countries.

The contribution of this paper is to show-case a pupil fixed effects model of student achievement using *cross-section* data on students' achievement scores in different subjects. This approach has both methodological and cost advantages over the panel data approach. The methodological advantages of the approach are avoiding the problem of non-random attrition of students and teachers over time and circumventing the non-random matching of students to teachers within the school, both problems which plague panel data studies. These advantages are discussed in more detail near the end of Section 2. The cost advantage arises because this approach permits the identification of the effects of teacher characteristics using *cross-section* data on pupil achievement by subject. Such data are readily available (for instance in the TIMSS and PISA tests for many countries) or, where not available, are much more cheaply collected than longitudinal data.

I examine the effect of teacher characteristics on pupil learning using the standard cross-section achievement production function, but the innovation is to allow for pupil fixed effects. This is possible because the unique data for India here provide each student's marks in five different subjects (English, second language, history/geography, math, science). Since each subject is taught by different teachers,

[1] While Angrist (2004) shows that the ethical problems cited by detractors of the experimental methodology can be dealt with by using an 'intention to treat' estimator, behavioral response can still undermine impact estimation if participants in an experiment alter their usual effort level. For instance, teachers and schools in a class size experiment could change their behavior during the experiment because they know that future class-sizes would depend on outcomes in the study (Hoxby, 2000), though careful studies do test robustness to this problem, e.g. Krueger (1999). There is also the issue of generalising findings outside of the experiment (Todd and Wolpin, 2003).

there is for each student-subject row, linked average characteristics of the teachers *who teach the student that subject* within the school. This approach allows us to control for all subject-invariant student and family unobservables. Our cross-section data allow us to examine whether the characteristics of different subject teachers *in a given year* in a school are related to a student's marks across those subjects within the school. In other words, we estimate a within-pupil across-subject equation of the achievement production function rather than a within-pupil across-time one.

Having discovered the teacher characteristics that most increase pupil learning, the paper also examines the teacher pay schedule to examine whether teachers are rewarded for possessing those characteristics that raise student achievement. In other words, we ask whether the teacher pay schedule is efficient. We estimate a school fixed effects equation of teacher pay to investigate this issue.

The paper is laid out as follows: Section 2 discusses data. Section 3 presents estimates of the achievement production function. Section 4 discusses the results of the teacher pay analysis and the last section concludes.

2. Data and estimation issues

The data for this study come from a sample of 186 schools affiliated to the Council for Indian Secondary Certificate Examinations (CISCE) which is an English Medium exam board. The schools were chosen by a stratified random sampling procedure within 16 major Indian states (the strata). The sampling procedure is explained in Appendix 1. Postal questionnaires sent by the Exam Board were filled by all students of grade 10 in the sample schools, and by the teachers that teach them as well as by the school Principal. Grade 10 students are aged approximately 16 years old and the grade 10 board examination in CISCE schools is equivalent to the High School board examination in other Indian exam boards, such as the state examination boards or the Central Board of Secondary Education. The overwhelming proportion of CICSE affiliated schools are private unaided schools, i.e. run without state aid (95%), 3.2% are aided schools (mainly in West Bengal), and only 1.6% are government or local body schools. Thus the sample represents mainly English Medium private secondary schools in India.

The student questionnaire captured information on the child's personal characteristics such as age, gender, health, disability, and time-use, as well as detailed family characteristics such as household demographics, asset/wealth ownership, parental education and occupation, etc. Examination results data were subsequently provided by the exam board and matched to students using a unique pupil identifier code. The teacher questionnaire collected information on a range of teacher characteristics and the school questionnaire elicited data on student and teacher numbers, school facilities and resources, length of the instructional program, school fees, and management and teacher motivation aspects.

Given the importance of innate ability in explaining student achievement, pupils were also given a test of ability/IQ based on 36-items in Sets A, C and D of the Ravens Progressive Matrices test. The score on this test provides an important control for innate ability in the achievement production function.

The objective is to estimate an educational production function in a consistent manner. The standard achievement function is specified as follows:

$$A_{ik} = \alpha + \beta X_{ik} + \delta S_k + \mu_i + \eta_k \qquad (1)$$

where the achievement level of the i^{th} student in the k^{th} school is determined by a vector of his/her personal characteristics (X) and by a vector of school and teacher characteristics (S). μ_i captures all student level and η_k all school level unobservables. Typically in the literature, teacher characteristics averaged across the relevant set of teachers that teach a grade, are included in the vector S, i.e. teacher variables have a k subscript. Since all students in a grade are taught a given subject by the same teacher (or set of teachers), for any individual student the teacher variables are in fact school-level variables, i.e. they do not vary by student. Thus, one could not estimate a relationship between student achievement and teacher characteristics *within* a school, i.e. with a school fixed effects model[2]. This is the reason why the existing achievement production function literature using cross-section data invariably provides across-school estimation and not within-school estimation.

However, we have data on student achievement *by subject* for all students of grade 10 in the sample schools, i.e. for each student in the grade there are as many rows of data as there are number of subjects. Thus, there is variation in teacher characteristics for a student within the school since different teachers teach different subjects, and it is possible to include teacher characteristics as explanatory variables in their own right in a school fixed effects equation. With this approach it is also possible to include pupil fixed effects whereby the only variables retained in the achievement equation will be the teacher characteristics since it is only these that will vary within pupil (across subject). This is the approach we follow. However, unfortunately, we do not know the class assignment of each student within grade 10 and the classes of grade 10 that a teacher teaches. As a result, we have to treat all students of grade 10

[2] In order to identify teacher effects on student achievement *within* a school, there should be variation in the set of students exposed to a teacher within the school in a given year, for instance different sets of students should be exposed to the same teacher for different lengths of time. Aaronson et. al. (2003) find just such variation in the Chicago school district and they use it to derive teacher fixed effects from an achievement production function. However, this approach can only estimate the total effect of a teacher. It does not identify which particular traits raise achievement.

in the school as if they are in one class, taught a given subject by 1 teacher. That is, we have to assign the average characteristics of all teachers in the school that teach a given subject to grade 10, to all students of grade 10, for that subject. This implies that for grade 10, there is no (across-subject) variation in teacher characteristics within the school which is not also within the pupil. Given this, the school fixed effects result will be identical to the pupil fixed effects results except if we have an unbalanced panel[3]. While the lack of *class*-level matched student-subject-teacher data is unfortunate, assignment of the average of subject-teachers' characteristics to all students of grade 10 in the school in the given subject also has an advantage: it enables us to circumvent the problem of the potential non-random sorting of students (with say high ability in a given subject) into classes that are taught by the particular teachers of that subject.

I estimate a simple pupil fixed effects equation of achievement, possible because of the data available on pupil achievement in 5 different subjects and matched data on teachers that teach each subject.

$$A_{ijk} = \alpha + \beta X_{ik} + \gamma T_{jk} + \delta S_k + (\mu_{ij} + \varepsilon_{jk} + \eta_{jk}) \qquad (2)$$

A_{ijk} is achievement of the i^{th} student in the j^{th} subject in the k^{th} school, X is a vector of characteristics of the i^{th} student, T a vector of characteristics of the teacher of the j^{th} subject and S a vector of characteristics of the k^{th} school. The composite error term is in the brackets. μ_{ij}, ε_{jk} and η_{jk} represent respectively the unobserved characteristics of the student, the subject teacher and the school. A pupil fixed effects model implies, for the simplified case of two subjects, 1 and 2:

$$(A_{i2k} - A_{i1k}) = \beta(T_{2k} - T_{1k}) + \{(\mu_{i2} - \mu_{i1}) + (\varepsilon_{k2} - \varepsilon_{k1}) + (\eta_{k2} - \eta_{k1})\} \quad (3)$$

Pupil fixed effects implies within-school estimation since a student necessarily studies within a single school. If school unobservables are not subject specific (η does not have a *j* subscript) and if pupil unobservables are also not subject specific (μ does not have a *j* subscript) then within the k^{th} school,

$$(A_{i2} - A_{i1}) = \beta(T_2 - T_1) + (\varepsilon_2 - \varepsilon_1) \qquad (4)$$

and regressing *difference* in a pupil's test scores across subjects on the *difference* in characteristics of teachers across subjects nets out the effect of all student unobserved characteristics. However, if student ability is subject-varying, it not netted out but $(\mu_{i2} - \mu_{i1})$ remains in the error term and could in

[3] If the pupil fixed effects results are to differ from the school fixed effects results then, given a sample of grade 10 students only, there should be some variation in teacher characteristics within the school that is not also within the pupil. This can happen if there is teacher data matched to the class (within the grade). Then the teacher characteristics in any row of data will be the characteristics of only those teachers that taught the student's class. This approach will provide within-grade (i.e. within school) variation in teacher characteristics, and will permit the school fixed effects estimation to differ from pupil fixed effects estimation.

principle cause omitted variable bias if it were correlated with $(T_2 - T_1)$. But for that to happen, students should be able to match to teachers on the basis of their unobserved characteristics. In our approach, by construction this is ruled out since each subject row has the averaged characteristics of all the teachers that teach that subject to the student's class. Thus, the presence of subject-varying pupil ability is not a source of bias in our approach. Nevertheless, subject-varying *school* unobservables $(\eta_{k2} - \eta_{k1})$ remain in the error term and may be correlated with $(T_2 - T_1)$ [4]. In a pupil fixed effects equation such bias remains a possibility but only at a stretch since most subject varying aspects of schools would be due to teacher characteristics, not school characteristics, i.e. schools are known to be good in a subject because of good teachers in that subject and this will show up in the teachers' subject specific effects.

Even if we do not need to worry about subject-specific student and school unobservables, for consistent estimation of the effect of teacher characteristics, it is required that teachers' unobserved characteristics be unrelated to included teacher characteristics:

$$E[(\varepsilon_2 - \varepsilon_1)(T_2 - T_1)] = 0 \qquad (5)$$

Since omitted teacher characteristics in ε_1, ε_2 may be correlated both with included teacher characteristics T_1, T_2 and with student achievement A_1, A_2, we cannot say that pupil fixed effects estimation of achievement – even with no subject-specific student and school unobserved heterogeneity – permits us to interpret the effects of teacher characteristics as causal. While across-subject pupil fixed effects estimation solves one source of endogeneity (the correlation between subject-invariant μ and *T*), it does not solve the second potential source of endogeneity (the possible correlation between ε and T). This is analogous to the situation with panel data analysis where teacher unobservables remain in the error term, though this drawback with the otherwise stringent differencing approach is rarely highlighted. Of course, studies that do not aim to identify the effect of separate teacher characteristics but are concerned only with identifying 'teacher quality' (the total effect of a teacher) do not have to worry about this source of endogeneity since they do not include individual teacher characteristics in achievement gain equations, e.g. Aaronson et al (2003), Rockoff (2004) and Rivkin et al (2005).

The approach outlined above closely resembles the familiar panel data approach in several respects. In both approaches, teacher unobservables remain in the error term and the coefficients on included teacher variables will suffer from endogeneity bias. Both across-subject and across-time approaches difference

[4] For instance, suppose schools with a reputation for excellence in math teaching attract pupils with high math ability and suppose that in order to maintain this reputation, such schools also pay math teachers more than other teachers. Then students' subject specific ability in the error term will be correlated with both math achievement as well as with teacher pay, leading to an upward bias in the coefficient of teacher pay.

out pupil unobservables (subject-invariant and time-invariant ones, respectively), which is their main strength. However, being differenced, both also suffer from possible measurement error attenuation bias[5]. The across-subject approach retains subject-varying pupil and school unobservables in the error term, and equivalently, the across-time approach retains time-varying pupil and school unobservables in the error term, thus both cases suffer from potential omitted variable bias.

However, across-subject differencing has two important methodological advantages over across-time differencing. Firstly, the across-subject approach does not suffer from the problem of non-random attrition of teachers and students over time that plagues panel data studies. For instance, in their panel study relating student achievement to teacher characteristics using North Carolina data, Clotfelter, Ladd and Vigdor (2006) highlight the difficulty of determining whether a higher coefficient on teacher experience reflects improvement with experience or the differentially higher attrition of the less effective teachers[6], and Rivkin et al (2005) also address non-random attrition[7]. Across-subject estimation obviates this problem since estimation is within pupil at one point in time. While the potential for endogenous selection into the 'surviving' teachers' group is the same in both approaches, the across-time technique relies on change in teacher over time (over which non-random attrition can take place) as part of the estimation strategy, while the across-subject technique does not.

The second methodological advantage is that the across-subject approach provides a means to circumvent the potential problem of non-random matching of students to particular teachers within the school on the basis of their unobserved characteristics – whether it be brighter students matching to abler teachers or school policy deliberately matching slower students to abler teachers. Across-subject estimation bypasses the problem either by averaging the characteristics of all teachers by grade and subject within the school, or by restricting the sample to schools where any given subject is taught to the student's grade by only *one* teacher within the school. Either way, the student is by construction

[5] Within a school, teacher characteristics are likely to be highly correlated both over time and across subjects, but measurement error will not be highly correlated.
[6] They say that due to the technological difficulty of including both pupil and teacher fixed effects in one equation, they attempt to address the problem by using the sub-sample of teachers who remain teachers for 3 or more years.
[7] According to Rivkin et al (2005, p429), the effect of endogenous attrition is to cause upward bias in the coefficients of teacher characteristics. Suppose that high quality teachers are more likely to exit than low quality ones. In this case, schools that obtain a particularly good draw of teachers in one year will tend to have both a greater turnover at the end of the year and a larger average gain in pupil achievement than would be the case with random attrition. Similarly, in the more intuitive case where higher quality teachers are more likely to be retained. Thus, non-random attrition would upward bias estimates, irrespective of whether higher quality teachers are more likely to exit or remain.

matched to a single set of teacher characteristics in each subject within the school. An equivalent is not possible in panel data[8].

Students of grade 10 in the CISCE exam board have 5 compulsory and one optional subject. The optional subject is chosen from among a large number of subject choices and thus varies a great deal between students. We wish to use exam marks of students in the 5 compulsory subjects: English, Second language, History/Geography, Math and Science[9]. Table 1 shows mean mark by subject. Figure 1 shows Epanechnikov kernel densities of marks in different subjects. It is clear that the distribution of marks in different subjects differs appreciably. For instance, the distribution of the second-language mark is quite different to that of other subjects. In order to render the marks in the different subjects comparable, we standardize the mark in each subject by the national mean mark in the subject, i.e. we use the z-score of achievement as our dependent variable. The z-score is a student's mark in a subject less the national mean mark in that subject, divided by the standard deviation of mark in the subject. Thus, by construction, mean z score in any given subject is 0 and its standard deviation is 1. Figure 2 shows the distribution of z-score for all subjects. Table 2 shows descriptive statistics of variables used in the achievement production functions.

3. The achievement production function

Table 3 shows the first cut OLS regressions of standardized achievement by subject. Most pupil variables have the expected signs. Number of siblings systematically reduces achievement, consistent with tradeoff and competition among children within a family, rather than with positive externalities. Mother's education matters significantly more than father's education in explaining English language marks but the difference is not statistically significant for the other subjects. Ability as measured by the Raven's progressive matrices test has a large and statistically the most significant coefficients in the regressions. Student achievement is not invariant to caste and religion. Christian students have a modest advantage over Hindu students (base category) in English achievement but a strong disadvantage in second language achievement, consistent with the fact that English is often the first language in

[8] The estimated results are similar when we restrict the sample to only those schools with a single teacher for each subject. This confirms that results are not driven by the non-random matching of students to particular teachers within the school. The results are available from the author.

[9] As the ICSE exam board is an English Medium exam board, all subjects in affiliated schools are taught in English. The local state language is considered the 'Second language' and it is typically a child's mother tongue. While History and Geography are tested separately, their marks are pooled by the examination board and provided together, i.e. they are treated as one subject. Similarly, while physics, chemistry and biology are tested separately, their marks are lumped together. When matching student marks in a subject to the characteristics of the teacher that taught them that subject, we have taken the *average* characteristics of the teachers that taught them that subject. Thus, if history and geography were taught to grade 10 by different teachers, we have taken the average characteristics of the history and geography teachers as the relevant characteristics to match with a student's history-geography subject-row.

Christian homes in India. However, they also have much lower achievement than Hindus in math and science. In comparison, the achievement disadvantage of Muslim children *vis a vis* Hindus is smaller. Scheduled tribe children have much lower achievement than general caste children. Household wealth and child age both have an inverted U shaped relationship with achievement across all subjects. Interestingly, the results corroborate the common finding that girls outperform boys in languages but achieve less in math and science than boys. Time use also has significant association with achievement: while greater hours of home study profits math and science achievement, it makes little difference to achievement in languages and history/geography. Hours of domestic work strongly reduces achievement across all subjects other than second language. Longer hours in school raise learning levels in math, history / geography and second language but not in English or science.

The main results of this paper are presented in Table 4, which pools all five subjects. The first column is an OLS achievement production function with state dummy variables, corresponding to equation (2). The second and third columns show school and pupil fixed effects results. Only the parameter estimates of the teacher variables are shown. The fact that the results of the school and pupil fixed effects equations are very similar is unsurprising: were it not for an unbalanced panel, the two would be identical since there is no variation in teacher characteristics within the school which is not also within the pupil[10]. The results of main interest are in the final column of Table 4.

It is conspicuous that some of the results change dramatically when moving from across-school to within-pupil analysis, i.e. from column 1 to column 3. For instance, the log of teacher's monthly pay (t_logpay) has a coefficient of 0.3607 and a robust t-value of 12.0 in the first column. This suggests that doubling absolute teacher pay increases standardized achievement by a large 0.25 standard deviations. However, across school correlation picks up the effect of unobserved school characteristics that are related with pay. When we estimate a pupil fixed effects model (column 3), the coefficient on teacher pay falls to one fifth its former size, though it is still statistically significant. The point estimate on the teacher pay variable is 0.07 in the pupil-fixed effects analysis, implying that – even if we ignore simultaneity issues – a doubling of teacher pay raises student mark by only 0.05 of one standard deviation, which is a tiny effect for a large implied increase in school expenditures. The coefficient on union membership (t_memunion) turns from about zero in column 1 to a strong negative in column 3, suggesting that some higher scoring schools have unionized teachers but that within school, the students of unionized teachers have sharply lower achievement levels. The positive and very significant effect of

[10] This is because we did not know the assignment of the different teachers of a given subject to the different classes of grade 10 within a school, and have thus averaged the characteristics of the teachers who teach a given subject to (any class of) grade 10 in the school. Thus, teacher characteristics within a school are subject specific, not pupil-specific. This limitation implies that we cannot test whether a school fixed effects model differs from a pupil fixed effects one.

teacher's average division in board exams (t_av_div, a measure of teacher's own exam performance) in the first column disappears in the pupil fixed effects estimation and indeed turns negative. Teacher pre-service training (t_training) appears to have a small effect and be only weakly related to student achievement in the first column but in within-pupil analysis, the effect is statistically significantly larger and more precisely determined.

The very different results from pupil fixed effects analysis suggest that across school estimation – which is all that is available in much of the achievement production function literature – leads to misleading inferences about the relationship between teacher characteristics and student achievement, confounding the effect of teacher traits with the effect of pupil and school unobservables.

Focusing on the pupil fixed effects results, a student's mark in a subject taught by a teacher who has masters (MA) level or above qualifications, is about 0.09 standard deviations higher than his mark in a subject taught by a teacher who does not have a master's qualification. The effect of pre-service teacher training is of a similar magnitude. Length of sick leave taken by teacher reduces student achievement very statistically significantly, though the size of the effect is small: raising days of sick leave taken from one SD below mean sick leave days to one SD above reduces achievement mark by only 0.03 SD. Having a teacher who is member of a teacher union is very inimical to student achievement: it reduces achievement by 0.25 SDs, the largest effect of all teacher variables. It remains possible that teachers with low student achievement tend to become unionized as a way of guarding against dismissal for inefficiency but the results are nevertheless interesting. Variables such as teacher experience and tenure are included merely as control variables as they are likely to be jointly determined with student achievement and, as such, we do not interpret their coefficients. For instance, the coefficient on tenure could reflect greater teacher learning with greater experience (causation from tenure to pupil achievement) or it could reflect that more effective teachers' contracts are more likely to be renewed (causation from pupil achievement to tenure).

Figure 1 showed that the distribution of second language mark differed a great deal from the distribution of mark for the other four subjects. Thus, it is natural to ask whether our across-subject (i.e. fixed effects) results could be being driven by this difference, despite using z-scores. To check this, we drop second language and re-estimate equations. Table 5 shows that when second language is omitted, the results of most interest are qualitatively unchanged. In the state fixed effects equation, the parameter vector is very similar across the two columns though in the pupil fixed effects equation, the effects of a few variables differ, e.g. the size of the effect of MA and pre-service training falls to about half.

An issue of policy interest has been whether raising teacher pay can increase student achievement. In assessing the effect of teacher pay, we consider the biases first. While pupil fixed effects solves the most commonly talked-about source of omitted variable bias OVB namely (subject-invariant) student ability, two further sources of OVB still remain: (i) teacher unobservables which may be correlated with teacher measured characteristics and (ii) pupils' and schools' subject-specific unobservables that could also be correlated with measured teacher characteristics for reasons given in Section 2[11]. The likely effect of both of these is upward OVB. Moreover, the estimates may suffer from upward simultaneity bias since teachers whose students perform well may be paid more, i.e. there may be performance related pay. In the case of teacher pay, despite the existence of upward bias due to both OVB and reverse causation, the size of the coefficient on pay is small, i.e. it appears that endogeneity is not a big issue in this data[12]. The finding of only a small positive effect of pay on student achievement has the rather pessimistic implication that raising pay is not a powerful way to motivate teachers to apply more effort. This could be because efficiency wages serve as an effort motivating device only when the threat of dismissal is credible. Due to strong labor laws in India, even private schools may find it difficult to lay-off non-performing teachers.

Another issue of long-standing interest and debate in the economics of education has been whether certification measures such as teacher's educational qualifications and pre-service professional training – commonly used as measures of teacher quality – actually matter to student achievement. The pupil fixed effects equation in Table 4 already helps to address this issue but it could be that teacher training and qualifications impact student achievement at least partly via their effect on teacher salary. In order to measure the total effect of these variables, the first column of Table 6 re-estimates the equations of Table 4 without the teacher pay variable. It shows that when pay is omitted, none of the other coefficients are significantly affected and 'Masters level or higher' qualification and possession of pre-service teacher training still each raise pupil achievement by 0.09 standard deviations (statistically very significantly). Thus, a student's achievement score is about one-fifth of a SD higher when taught by a teacher who has both MA/higher qualification as well as a year of pre-service training, compared with

[11] If a child is particularly able in math and chooses a school that especially champions math teaching and thus, say, pays math teachers more than other teachers in order to attract particularly good math teachers. Then the teacher pay variable in our achievement equation will be upward bias reflecting the fact that student and school unmeasured emphasis on math (which is in the error term) is positively correlated with both math teacher pay and student achievement in math.

[12] Even if teacher pay is the only explanatory variable, the size of the effect of pay in a pupil fixed effects regression is small, despite upward endogeneity bias due to both OVB and reverse causation. A doubling of teacher pay raises pupil achievement by only 0.11 SD. In a 2SLS model of achievement (not reported) the instruments (teacher's gender, caste, tenure, and permanent status) were accepted as valid by the Sargan test of overidentifying restrictions. Instrumenting teacher pay did not alter the coefficients of the other variables but the coefficient on pay increased to 0.24. While we would have expected IV estimation to *reduce* the coefficient on pay, the fact that it increased is consistent with correction for measurement error. However, even a coefficient of 0.24 on logpay still implies a relatively small effect from pay onto achievement: doubling absolute teacher pay would raise pupil achievement by 0.16 SD.

his/her score if taught by a teacher with neither of these certificates. While this size of effect on achievement is not trivial – being roughly equal to the effect of parental education[13] – it is possible that the coefficients on certification variables suffer upward omitted variable bias so that these are upper bound estimates[14], though because of differencing they may also suffer from attenuation bias.

Extensions

Schools with the freedom to set salary schedules such as private schools and non unionized schools are the most likely to have within school variation in pay, in order either to reward teachers for effort or for student performance (Ballou, 2001). In other words, we would expect the correlation between pay and achievement to be greater in schools that have greater discretion to set salary levels for teachers. Schools in which teachers are unionized arguably have less latitude to alter individual teacher pay than schools without unionized teachers. We have data on whether individual teachers are members of a teacher union. Only less than 4% of teachers of grade 10 in our private school sample report being union members. Schools in which any teacher is unionized is taken to be a unionized school since, in such schools, school management are likely to experience union-related pressures. By this definition, 11% of all sample schools are unionized. Table 7 shows school and pupil fixed effects estimates of the achievement production function separately for unionized and non-unionized schools. It is clear that correlation between pay and achievement exists only in the non-unionized schools and is altogether absent in unionized schools[15]. This is consistent with findings in Hoxby (1996) where the beneficial effect of teacher salary on pupil achievement is significantly lower in unionized school districts than in non-unionized ones.

The effects of several other variables differ sharply by whether the school has unionized teachers. For instance, while possession of MA raises pupil achievement in non-unionized schools, it does not in union schools. While longer days of sick-leave taken reduce student learning in non-union schools, they raise it in union schools. Years of teacher experience reduces student learning by nearly 8 times as much in union schools as in non-union schools implying that each extra year a teacher spends in school lowers student achievement but it does so nearly 8 times more in unionized than in non-unionized schools. Overall, the productivity of teacher inputs is lower in unionized than non-unionized schools[16].

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[13] The coefficient on 'Father has MA' is 0.124 and on 'Mother has MA' is 0.119 in a school fixed effects equation of achievement, with the base category for each parent being 'education less than MA'

[14] For example, if more motivated teachers are both more likely to get MA/training as well as have higher student achievement then the observed significant cross-section correlation between MA/training and student achievement could be due to this unmeasured teacher motivation rather than a causal effect from MA/training onto student achievement.

[15] In non-unionized schools, doubling teacher pay would raise student achievement by just under one tenth of a SD.

[16] Results for non-unionized schools remain very similar when estimated on the sample of non-unionized schools only in states that have any unionized schools.

Having estimated the school fixed effect in Table 4, we wish to ask to what extent observed school characteristics explain the school fixed effect, which we take to be a measure of school quality. In Table 8 column 1, about 28% of the school fixed effect is explained by observed school and average teacher characteristics. This remains similar (27%) with a more parsimonious specification in the next column. Finally, when we include hours per week the school operates (hours_week) and the number of days for which the school operates per week (ndayop), the adjusted R-square jumps to 57%, i.e. these two variables together have a great deal of explanatory power. While this equation should be read with caution since it is estimated for a much smaller sample (these variables are missing for many schools), the results are striking and worthy of further research. Discounting this equation, Table 8 shows that less than 30% of the school fixed effect is explained by observed school factors. Much of what makes a school effective is unobserved, perhaps consisting of principal and teacher quality, management style, motivation levels, ethos etc. Nevertheless, the analysis gives helpful pointers about specific school and teacher inputs that explain variations in school quality. Results suggest that larger schools (log_totstren), girls schools (girlschool) and schools that open for longer hours per week (hours_week) have significantly larger school fixed effects than their opposite numbers, and that higher pupil teacher ratios and Saturday opening are inimical to school quality. Of course, the coefficients do not necessarily represent causality. For instance, larger schools are associated with higher school quality but it could be that higher quality schools attract more students and so become large.

Finally, having estimated the pupil fixed effect it is of interest to see what percentage of this measure of 'pupil quality' is explained by pupils' observed characteristics. Table 9 shows that our rich set of controls for pupils' personal and home background characteristics explains only about a quarter of what makes a pupil a high achiever. When we add school fixed effects in the second column, they greatly raise the explained variation in 'pupil quality' to 43.45% and the school dummies are jointly highly significant (F=18.33). Clearly there is a good deal of sorting by high quality pupils into particular schools and it lends further support to our earlier conclusion that across-school estimation – which is all that is available in most of the achievement production function literature – confounds the effect of school/teacher variables with the effect of pupil quality[17].

[17] The fact that within-school estimates of many variables in Table 9 (second column) are typically significantly smaller than the across-school estimates (first column) confirms sorting of students to higher and lower quality schools on the basis of *observed* characteristics. For example, the coefficient on mother's education level of 0.117 in column 1 falls to 0.070 in column 2, suggesting that more educated mothers send their children to significantly better quality schools.

4. Teacher pay schedule

How efficient is the system of teacher compensation in the English medium private secondary schools in India? In other words, are teachers rewarded for possessing characteristics that raise student achievement? We attempt to answer this question by examining the teacher pay schedule. All employers with more than 10 workers in India have to *de jure* abide by minimum wage laws and ‘recognized’ private schools are required to pay teachers salaries on a par with the government teacher salary scales. *De facto*, many private schools get away with paying teachers significantly less than the government prescribed minima. For instance, at the middle school level, Kingdon (1996) found that private teachers’ mean salary was only 60% of the public teachers’ mean salary. However, private schools do have latitude to set their own compensation schedules. Kingdon and Teal (2006) find that private and public school salary schedules differ substantially in India.

The survey for this study collected information on all teachers that taught grade 10 students in sample schools. This yielded a sample of 2103 teachers. The first column of Table 10 presents an OLS equation of log of teacher pay but our data also have within-school variation in teacher pay, which is not commonly available. The size of coefficients on several variables (age, age-square, union-membership and sick-leave days) changes statistically significantly when I estimate a school fixed effects equation in the second column. Since teachers may sort into high and low paying schools on the basis of their unobserved characteristics, we rely more on the school fixed effects regression in the second column, though omitted variable bias due to teachers’ unobserved characteristics remains possible even in this.

The equation shows that the teacher pay structure in our sample of schools is efficient in some respects but not in all. Teachers who are older, have MA level or higher qualifications, pre-service teacher training, permanent status and longer years of tenure in their present school have significantly and substantially higher pay than those without these. In this respect, the teacher pay schedule is efficient since these characteristics are each associated with higher student achievement in the pupil fixed effects achievement equation of Table 4. However, while days of sick-leave taken reduces student achievement, it does not reduce teacher pay presumably because there are rules about teachers’ sick leave entitlement without loss of pay. Finally, union membership and total experience significantly lower student achievement in Table 4 and yet these characteristics are significantly positively *rewarded* in the teacher pay schedule. This inefficiency could be because even private schools have to, or choose to, follow rules (mandated for public schools) about higher pay for greater years of work experience or because union members can pressurize schools to raise pay irrespective of productivity considerations.

When we examine the salary structure by whether the school is unionized (Table 11), we find that the effects of some variables differ statistically significantly by unionization status. Firstly, both tenure and permanency are rewarded significantly more heavily in unionized schools. Secondly, non-unionized schools gender-discriminate but unionized schools do not. Thirdly, while the age-earnings profile has the familiar concave shape in non-unionized schools (with the maxima reached at age 38 years old), in unionized schools, pay has no relationship with age. This is akin to the finding of Pritchett and Murgai (2006) in India that the relationship of teacher pay and experience is shallower in public sector versus both private sector teaching jobs and private sector other jobs. Lastly, while teachers of English, history/geography, math and science all receive significantly higher pay than teachers of other subjects (base category) in non-union schools, in unionized schools this is not the case at all, suggesting that in unionized schools, there are pressures to equalize pay irrespective of perceived differences in the relative importance (or scarcity) of teachers of different subjects.

5. Conclusions

This paper exploits within-pupil variation to estimate the effects of teacher characteristics on student achievement. It utilizes data that permits the matching of students' test scores in different subjects to the teachers that teach those subjects, and permits examination of whether the characteristics of different subject teachers are related to a student's marks across subjects.

The paper makes four contributions. *Firstly*, the findings, using a credible methodology for identification, give only modest grounds for optimism about the effects of teacher policies. Pre-service teacher training and possession by teacher of a Master's level qualification together raise student achievement by about one fifth of a standard deviation – roughly equal to the effect of parental education on student achievement. However, this seems to be an upper bound estimate. The bulk of the variation in student achievement is a school fixed effect and observed school and teacher characteristics explain only less than 30% of it. One particularly interesting result is that a teacher's membership of a teacher union lowers student achievement score by 0.25 standard deviations and the productivity of teacher inputs is lower in unionized than non-unionized schools. This is specially striking when juxtaposed with the finding that unionized teachers have significantly higher pay, as seen in Table 10. This begs the question whether teachers' right to unionize pits their interests against students' interests.

Secondly, the paper highlights the importance of 'controlling for' the non-random matching of students to schools and teachers. The finding that within-pupil effects of many teacher variables differ very significantly from the across-school effects indicates that much of the extant achievement production function literature – which perforce relies on across school estimation – leads to incorrect inferences because it confounds the effect of unobserved school and pupil heterogeneity with the effect of teacher characteristics. This underlines the importance of finding credible sources of within school and preferably within-student variation in future research.

Thirdly, the paper showcases the use of an across-subject estimator of the achievement production function that is superior to the more familiar panel data estimator in that it circumvents the problem of non-random attrition of students and teachers over time and also the problem of non-random matching of students to teachers.

Fourthly, the paper examines the efficiency of teacher compensation by examining the structure of teacher pay in a way that avoids biases due to the non-random matching of teachers to schools. School fixed effects results show that while sample schools follow efficient teacher compensation policies in some respects, e.g. by rewarding teachers for the possession of characteristics – such as MA qualifications and pre-service training, etc. – which raise student achievement, they are also inefficient in other respects, for instance due to not relating pay to days of sick leave taken (which is inimical to pupil achievement), and paying significantly higher salaries to unionized teachers when in fact union membership is associated with very significantly *lower* student achievement.

While the approach in this paper demonstrates a way forward, it has an important drawback. Differences between *teachers* in their unobserved characteristics still remain a source of endogeneity and undermine the ability to attribute causality to teacher certification or teacher pay variables. Data on pupils' subject marks and teacher traits at two points in time could enable the researcher to address at least the time invariant aspects of unobserved teacher heterogeneity and the time invariant aspects of subject-specific pupil unobserved heterogeneity, though such panel data would raise its own attendant issues of non-random attrition of students and teachers over time and the potential non-random matching of students to teachers.

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Appendix 1

Sampling procedure

The Council for Indian School Certificate Examinations (CISCE) supported the research by enabling the collection of data from its affiliated schools. Schools were sampled from among schools affiliated to the Council for Secondary Certificate Examinations (CISCE) in each of 15 major states (the strata). The second stage stratum was by whether the highest class taught in the school was grade 10 (ICSE school) or grade 12 (ISC school). CISCE permitted a maximum sample of 300 schools out of their total of just over 1000 affiliated schools. Given 15 major states in India, this implied that 20 schools could be chosen in each state, 10 from among the ICSE schools and 10 from among the ISC schools. However, there were few CISCE affiliated schools in Gujarat and Rajasthan so only 10 schools were sampled in these. Conversely, there were a large number of schools affiliated to CISCE in the states of Uttar Pradesh and West Bengal and thus I sampled a total of 30 schools each from these two states. Since in Madhya Pradesh the number of CISCE affiliated schools is only small, I counted schools in the newly formed state of Chattisgarh as if they were MP schools. Thus, although not precisely, the sampling is close to probability proportional to size. The number of schools sampled and responding by state was as follows:

	Number sampled			Number	
	ICSE	ISC	Total	Responded	% Response
Andhra Pradesh	10	10	20	13	0.65
Bihar	10	10	20	8	0.40
Gujarat	5	5	10	3	0.30
Haryana	14	6	20	13	0.65
Himachal Pradesh	10	10	20	12	0.60
Karnataka	10	10	20	17	0.85
Kerala	10	10	20	15	0.75
Madhya Pradesh	6	14	20	7	0.35
Maharashtra	14	6	20	13	0.65
Orissa	10	10	20	12	0.60
Punjab	11	9	20	10	0.50
Rajasthan	5	5	10	4	0.40
Tamil Nadu	10	10	20	10	0.50
Uttar Pradesh	15	15	30	24	0.80
West Bengal	15	15	30	25	0.83
Total	155	145	300	186	0.62

The total number of ICSE (ISC) schools in a state was divided by the number of ICSE (ISC) schools I wanted from that state in my sample (usually 10), and the resulting figure was the interval. I then chose a starting number randomly between 1 and 5 and picked schools at regular intervals after that. For example, for Andhra Pradesh, there are 69 ICSE schools and I wanted to pick 10 ICSE schools for my sample. So I divided 69 by 10, giving me the interval 6.9 (or 7). I picked the first school at random as the 4th school on the list of schools within Andhra, and then picked every 7th school thereafter. Out of the 300 sampled schools, 186 returned all components of the study, i.e. school, teacher and student questionnaires as well as Ravens ability tests taken by students, i.e. a response rate of 62%.

Table 1
Mean mark, by subject

Subject	Mean mark	SD	Minimum	Maximum
English	67.15	15.3	20	97
Second language	79.73	8.9	28	99
History-geography	70.09	14.3	20	99
Maths	68.92	18.4	15	99
Science	65.72	17.0	20	99

Table 2
Descriptive Statistics

Variable	Definition	Mean	SD	Minimum	Maximum
Pupil characteristics					
stdmark	Standardized mark	0.000	1.00	-5.83	2.17
hhsize	Household size	5.361	2.62	2.00	43.00
ybrother	Number of younger brothers	0.418	0.60	0.00	5.00
obrother	Number of older brothers	0.327	0.64	0.00	9.00
ysister	Number of younger sisters	0.333	0.57	0.00	6.00
osister	Number of older sisters	0.365	0.70	0.00	8.00
faedu	Father’s education	4.049	0.98	1.00	5.00
maedu	Mother’s education	3.518	1.15	1.00	5.00
specs	Child wears spectacles*	0.282	0.45	0.00	1.00
disabled	Child is disabled*	0.003	0.06	0.00	1.00
sibling_icse	Any sibling studies in ICSE school	0.301	0.46	0.00	1.00
raven	Score on Raven’s ability test	23.824	6.40	1.00	36.00
sikh	Religion is Sikh*	0.058	0.23	0.00	1.00
christn	Religion is Christian*	0.078	0.27	0.00	1.00
muslim	Religion is Muslim*	0.069	0.25	0.00	1.00
wealth	Index of asset ownership	21.629	6.31	0.00	33.00
wealth_sq	Wealth squared	507.608	256.85	0.00	1089.00
c_agemo	Child’s age in months	195.055	8.90	164.00	264.00
agemosq	Age squared	38.126	3.56	26.90	69.70
c_obc	‘other backward caste’*	0.062	0.24	0.00	1.00
c_sc	‘scheduled caste’*	0.012	0.11	0.00	1.00
c_st	‘scheduled tribe’*	0.017	0.13	0.00	1.00
male	Child is male*	0.491	0.50	0.00	1.00
h_study	Minutes per day spent in study at home	4.033	1.61	0.17	8.50
h_domes	Minutes per day spent in domestic work	0.692	0.57	0.00	2.50
h_play	Minutes per day spent in playing	1.631	0.86	0.00	4.50
h_trav	Minutes per day spent in travel to school	0.614	0.48	0.00	2.25
h_sch	Minutes per day spent in school	6.223	0.73	4.75	8.00
Teacher characteristics					
t_logpay	Log of teacher’s gross pay	8.884	0.40	7.50	9.67
t_grpay	Teachers’ gross pay	7864.421	2997.72	1800.00	15,800.00
t_ma	Teacher has MA/MSc/PhD*	0.741	0.35	0.00	1.00
t_av_div	Average grades of teacher in exams 1st division=3; 2nd division=2; 3rd div.=1	2.311	0.42	1.00	3.00
t_sickleav	Days of sick leave taken last year	2.182	2.70	0.00	15.00
t_memunion	Teacher is member of a teacher union*	0.036	0.16	0.00	1.00
t_training	Years of teacher training	0.920	0.36	0.00	2.00
t_christn	Teacher is Christian*	0.251	0.36	0.00	1.00
t_female	Teacher is female*	0.723	0.39	0.00	1.00
t_age	Teacher’s age	41.122	7.92	22.00	76.00
t_agesq	Teachers age squared	1778.512	667.40	484.00	5776.00
t_exptotal	Years of work experience in teaching	13.388	7.17	0.00	42.00
t_expthiss	Years of work experience in this school	10.037	6.67	0.00	40.00
t_permanen	Teacher’s contract is permanent*	0.878	0.27	0.00	1.00

Table 2, continued

<u>**School characteristics**</u>					
totstren	Total number of pupils in the school	1648.789	852.18	96.00	4111.00
pupilx	Number of pupils in grade 10	111.175	59.55	2.00	292.00
pinflu	index of principal's influence in school decision-making: lowest=1, highest=5	4.161	0.93	1.00	5.00
logmfeex	Log of monthly fee (rupees)	6.349	0.48	5.22	7.69
girlschool	Is a girls-only school*	0.210	0.41	0.00	1.00
highersec	Is a higher secondary school*	0.567	0.50	0.00	1.00
resource4	Index of school resources	20.598	14.56	1.00	74.00
School characteristics used in Table 8**					
school_fe	School fixed effect	0.965	0.76	-1.28	3.30
totstren	Total number of pupils in the school	1132.430	775.52	96.00	4111.00
pupilx	Number of pupils in grade 10	68.674	53.80	2.00	292.00
ptr	Class-size: no. of students per class 10	24.708	8.28	5.35	41.67
logmfeex	Log of monthly fee in school	6.295	0.46	5.22	7.69
highersec	Is a higher secondary school*	0.465	0.50	0.00	1.00
resource4	Index of school resources	14.976	12.35	1.10	74.30
pinflu	Principal’s influence in teacher appointments 1=very little; 5= decisive	4.215	0.90	1.00	5.00
girlschool	Is a girls-only school*	0.145	0.35	0.00	1.00
t_mot_index	Teacher motivation index 1=low; 4=high	3.058	0.71	1.00	4.00
comp_index	Competition index: number of secondary schools within 3 km radius of school	7.380	6.09	0.00	26.00
t_female	Percentage of teachers that are female	0.675	0.25	0.00	1.00
t_memunion	Percentage of teachers that are unionised	0.030	0.13	0.00	1.00
t_training	Average years of teacher training	0.839	0.22	0.00	1.60
hours_week	No. of hours school operates per week	35.115	4.48	26.67	51.50
workdays	No. of school days in past school year	212.262	21.75	160.00	290.00
ndayop	No. of days school opens per week	5.559	0.50	5.00	6.00

Note: Variables marked with a * are 0/1 indicator variables with yes=1 and no=0. If a teacher got 1st division in every board exam (high school, grade 12, BA and MA), her average division would be 3 [(3+3+3+3)/4]. If she got 1st division in BA but 2nd division in each of high school, grade 12 and MA, then her average ‘division’ would be 2.25 [(2+2+3+2)/4]. If she got 3rd division in all four board exams, her average division would be 1 [(1+1+1+1)/4].

** School characteristics used in the regression in Table 8 are not weighted by the number of grade 10 students. Thus, the averages are taken across the 172 schools included in the achievement regressions. Some variables were included in preliminary runs but not retained in the final reported equations.

Table 3
OLS Achievement production function, by subject

	english		second language		history-geography		mathematics		science	
	coeff	robust t	coeff	robust t	coeff	robust t	coeff	robust t	coeff	robust t
Pupil characteristics										
hhsize	-0.006	-1.7	0.000	-0.1	-0.001	-0.4	-0.003	-0.7	-0.006	-1.6
ybrother	-0.048	-3.4	-0.029	-1.7	-0.021	-1.3	-0.027	-1.6	-0.032	-2.0
obrother	-0.038	-2.9	-0.039	-2.2	-0.029	-1.9	-0.027	-1.7	-0.037	-2.4
ysister	-0.025	-1.6	-0.037	-2.1	-0.018	-1.1	-0.033	-2.0	-0.045	-2.7
osister	-0.022	-1.8	0.001	0.1	-0.021	-1.6	-0.016	-1.1	-0.017	-1.2
faedu	0.078	7.7	0.049	4.1	0.076	7.0	0.071	6.3	0.083	7.6
maedu	0.111	12.3	0.045	4.3	0.086	8.8	0.087	8.6	0.098	10.0
specs	0.138	8.0	0.076	4.0	0.112	6.0	0.120	6.3	0.117	6.3
disabled	-0.045	-0.5	-0.065	-0.4	-0.001	0.0	-0.170	-1.1	0.064	0.4
sibling_icse	0.020	1.1	-0.071	-3.2	0.039	1.9	0.040	1.9	0.030	1.4
raven	0.033	27.0	0.025	16.8	0.035	24.4	0.044	30.2	0.040	28.3
sikh	-0.102	-2.6	-0.094	-2.1	-0.081	-1.7	-0.130	-2.8	-0.065	-1.4
christn	0.051	1.5	-0.347	-8.4	-0.074	-2.1	-0.224	-5.9	-0.195	-5.5
muslim	-0.028	-0.9	-0.187	-5.0	-0.084	-2.5	-0.157	-4.3	-0.111	-3.2
wealth	0.012	2.1	0.000	-0.1	0.018	2.7	0.021	3.1	0.012	1.8
wealth_sq	0.000	-2.5	0.000	-1.7	-0.001	-3.7	-0.001	-4.1	-0.001	-3.2
c_agemo	0.127	6.6	0.084	3.3	0.147	6.3	0.174	7.2	0.171	7.2
agemosq	-0.347	-7.2	-0.247	-3.9	-0.409	-7.0	-0.479	-7.9	-0.475	-8.0
c_obc	-0.055	-1.6	-0.011	-0.3	-0.105	-2.8	-0.101	-2.7	-0.076	-2.1
c_sc	0.076	1.0	-0.020	-0.2	0.000	0.0	-0.038	-0.5	0.030	0.4
c_st	-0.114	-1.7	-0.003	0.0	-0.234	-3.2	-0.259	-3.5	-0.175	-2.5
male	-0.168	-8.7	-0.362	-16.2	-0.015	-0.7	0.035	1.7	0.036	1.7
h_hstudy	-0.003	-0.5	0.005	0.8	-0.007	-1.2	0.018	3.1	0.014	2.3
h_domes	-0.058	-4.0	-0.027	-1.6	-0.073	-4.6	-0.086	-5.3	-0.083	-5.2
h_play	-0.022	-2.3	-0.019	-1.7	-0.017	-1.6	-0.025	-2.3	-0.026	-2.4
h_travel	0.017	1.1	-0.001	-0.1	-0.008	-0.4	0.020	1.1	0.028	1.6
h_school	0.012	0.7	0.075	3.8	0.059	3.5	0.048	2.6	0.011	0.6
Teacher characteristics										
t_logpay	0.437	10.5	0.365	8.1	0.440	9.4	0.245	5.9	0.357	8.2
t_ma	-0.009	-0.3	0.207	6.1	0.039	1.2	0.019	0.7	0.127	3.0
t_training	0.001	0.1	-0.030	-0.8	0.057	1.7	0.031	1.2	-0.075	-2.3
t_av_div	0.168	6.3	0.065	2.3	-0.014	-0.5	0.134	5.2	0.297	7.2
t_sickleav	-0.006	-1.6	-0.014	-4.6	-0.007	-1.7	-0.012	-3.2	-0.010	-2.2
t_memunion	0.088	1.3	0.254	3.9	0.079	1.0	0.030	0.5	-0.032	-0.6
t_christn	0.078	3.4	0.122	2.9	-0.155	-4.8	-0.142	-4.5	0.009	0.2
t_female	0.003	0.1	-0.055	-1.6	-0.107	-3.7	-0.092	-3.5	-0.039	-0.9
t_age	0.019	2.1	-0.015	-1.0	0.021	1.4	0.038	3.1	0.002	0.1
t_agesq	0.000	-2.6	0.000	1.1	0.000	-1.0	-0.001	-3.3	0.000	-0.1
t_exptotal	0.005	2.0	0.000	0.0	-0.021	-7.2	0.011	4.2	0.018	4.4
t_expthiss	-0.004	-1.6	-0.003	-1.5	-0.008	-3.1	-0.004	-1.9	-0.004	-1.0
t_permanen	-0.005	-0.2	0.515	7.7	0.292	6.4	0.128	3.3	0.031	0.7

School characteristics										
totstren	0.000	6.1	0.000	6.0	0.000	8.9	0.000	13.6	0.000	9.7
pupilx	-0.001	-2.6	-0.001	-2.7	-0.001	-3.9	-0.003	-8.9	-0.002	-5.9
pinflu	0.050	4.6	0.060	4.5	0.074	5.7	0.040	3.4	0.025	2.1
logmfeex	0.087	3.2	-0.024	-0.8	-0.113	-3.9	-0.056	-2.0	-0.059	-2.1
girlschool	0.329	13.0	0.029	1.0	0.183	6.5	0.127	4.2	0.131	4.4
higherseco~y	-0.093	-4.7	-0.088	-3.8	-0.062	-2.9	0.059	2.8	0.007	0.3
resource4	0.014	14.3	-0.002	-1.6	0.008	7.0	0.004	3.6	0.004	4.2
State dummies										
ap	-0.331	-5.5	-0.989	-13.0	-0.451	-7.2	-0.381	-5.9	-0.192	-3.1
bi	-0.511	-8.9	-0.387	-5.1	-0.395	-6.4	-0.145	-2.1	-0.170	-2.3
gu	-0.807	-9.1	-1.046	-10.0	-0.696	-6.7	-0.399	-4.1	-0.337	-3.7
ha	-0.814	-13.6	-1.039	-14.2	-0.448	-6.5	-0.240	-3.4	-0.256	-3.4
hp	-0.369	-6.4	-0.764	-10.7	-0.197	-3.2	-0.329	-5.2	-0.244	-3.8
ka	-0.315	-5.9	-1.231	-17.9	-0.301	-5.2	-0.546	-9.0	-0.369	-6.1
ma_	-0.600	-11.0	-1.021	-14.1	-0.416	-6.5	-0.451	-7.0	-0.315	-4.9
mp	-1.011	-15.3	-0.909	-11.8	-0.835	-11.7	-0.557	-7.3	-0.541	-7.3
or	-0.333	-6.1	-0.517	-7.8	-0.281	-4.9	-0.160	-2.7	-0.235	-3.7
pu	-0.809	-13.4	-0.668	-9.2	-0.459	-6.5	-0.393	-5.9	-0.483	-6.7
ra	-0.866	-10.4	-0.754	-7.8	-0.081	-1.0	-0.389	-3.9	-0.395	-3.9
tn	-0.361	-5.3	-0.510	-5.8	-0.292	-3.7	-0.202	-2.8	-0.171	-2.4
up	-0.838	-16.3	-0.874	-13.8	-0.733	-13.2	-0.534	-9.0	-0.605	-9.6
wb	-0.446	-8.5	-1.084	-16.5	-0.562	-9.7	-0.457	-7.6	-0.443	-7.2
ch	-0.781	-10.3	-1.133	-10.4	-0.748	-7.2	-0.711	-6.9	-0.631	-5.8
_cons	-18.320	-9.2	-10.912	-4.3	-18.513	-8.0	-20.693	-8.5	-20.607	-8.6
N	9772		9864		9833		9903		9717	
R-square	0.4418		0.2416		0.3248		0.3019		0.3317	

Note: the standard errors and thus t-values are corrected for the correlation of the errors between pupils within a school, i.e. we have used the pupil id as the clustering variable.

Table 4
Achievement production functions
(five subjects pooled)

	State fixed effects		School fixed effects		Pupil fixed effects	
	coeff	robust t	coeff	robust t	coeff	robust t
t_logpay	0.3677	12.0	0.0722	2.9	0.0711	2.8
t_ma	0.0828	6.2	0.0880	8.9	0.0870	9.3
t_training	0.0277	2.2	0.0857	9.7	0.0869	9.7
t_av_div	0.0754	6.4	-0.0105	-1.2	-0.0114	-1.3
t_sickleav	-0.0092	-4.4	-0.0050	-3.4	-0.0057	-4.1
t_memunion	0.0299	0.7	-0.2591	-7.6	-0.2498	-6.9
t_christn	0.0028	0.2	0.0216	2.1	0.0214	2.1
t_female	-0.0565	-4.0	-0.0010	-0.1	-0.0017	-0.2
t_age	0.0284	5.7	0.0241	6.7	0.0228	6.5
t_agesq	-0.0003	-5.4	-0.0003	-6.3	-0.0002	-6.0
t_exptotal	-0.0033	-3.2	-0.0036	-4.9	-0.0036	-4.7
t_expthiss	-0.0032	-2.9	0.0020	2.5	0.0021	2.6
t_permanen	0.1319	5.9	0.0271	1.9	0.0241	1.7
_cons	-18.4865	-9.8	-9.3228	-4.0	-1.2746	-5.7
Subject dummies	yes		yes		yes	
Pupil variables	yes		yes		no	
School variables	yes		no		no	
N	49089		49089		49089	
Adjusted R-sq	0.2784		0.3491		---	
No. of groups	16		172		10016	
F (p-value)	96.90 (0.000)		31.03 (0.000)		11.33 (0.000)	

Note: the standard errors and t-values are corrected for the clustering of errors between subjects within a pupil, i.e. we have used the pupil id as the clustering variable. The last row reports the F-statistic of the joint significance of the fixed effects, and the value in parentheses next to it is the corresponding p-value of the F-test.

Table 5
Achievement equation with and without second language
Using state and pupil fixed effects estimators

	All five subjects*		Excluding second language	
	coeff	robust t	coeff	robust t
State FE estimator				
t_logpay	0.368	12.0	0.385	11.9
t_ma	0.083	6.2	0.062	4.4
t_training	0.028	2.2	0.004	0.3
t_av_div	0.075	6.4	0.095	7.2
t_sickleav	-0.009	-4.4	-0.010	-4.2
t_memunion	0.030	0.7	0.010	0.2
t_christn	0.003	0.2	-0.033	-2.1
t_female	-0.057	-4.0	-0.100	-6.9
t_age	0.028	5.7	0.029	5.4
t_agesq	0.000	-5.4	0.000	-5.1
t_exptotal	-0.003	-3.2	-0.004	-3.3
t_expthiss	-0.003	-2.9	-0.005	-3.8
t_permanen	0.132	5.9	0.079	3.5
Pupil FE estimator				
t_logpay	0.071	2.8	0.094	3.6
t_ma	0.087	9.3	0.043	4.6
t_training	0.087	9.7	0.046	5.0
t_av_div	-0.011	-1.3	-0.019	-2.2
t_sickleav	-0.006	-4.1	-0.006	-4.0
t_memunion	-0.250	-6.9	-0.167	-4.8
t_christn	0.021	2.1	-0.034	-3.4
t_female	-0.002	-0.2	-0.072	-7.7
t_age	0.023	6.5	-0.002	-0.5
t_agesq	-0.000	-6.0	0.000	1.1
t_exptotal	-0.004	-4.7	-0.006	-7.4
t_expthiss	0.002	2.6	0.002	3.3
t_permanen	0.024	1.7	-0.051	-3.8
N	49089		39225	

Note: * This replicates the main results (of Table 4). All equations include subject dummies and a constant. State fixed effects equations include pupil and school variables.

Table 6

Pupil fixed effects achievement function without teacher pay

	With teacher pay		Without teacher pay	
	Coeff	t-value	Coeff	t-value
t_logpay	0.0711	2.8	---	---
t_ma	0.0870	9.3	0.0896	9.6
t_training	0.0869	9.7	0.0903	10.2
t_av_div	-0.0114	-1.3	-0.0108	-1.3
t_sickleav	-0.0057	-4.1	-0.0058	-4.2
t_memunion	-0.2498	-6.9	-0.2419	-6.7
t_christn	0.0214	2.1	0.0230	2.3
t_female	-0.0017	-0.2	-0.0028	-0.3
t_age	0.0228	6.5	0.0235	6.7
t_agesq	-0.0002	-6.0	-0.0002	-6.1
t_exptotal	-0.0036	-4.7	-0.0035	-4.5
t_expthiss	0.0021	2.6	0.0028	3.8
t_permanen	0.0241	1.7	0.0331	2.4
Subject dummies	yes		yes	
N	49089		49089	
Number of groups	10016		10016	
F (p-value)	11.33 (0.000)		11.77 (0.000)	

Note: The last row reports the F-statistic of the joint significance of the fixed effects, and the value in parentheses next to it is the corresponding p-value of the F-test.

Table 7
Achievement production functions, unionized and non-unionized schools
(Pupil fixed effects)

	Non-unionised schools		Unionised schools	
	coeff	robust t	coeff	robust t
t_logpay	0.1228	4.4	0.0198	0.3
t_ma	0.0898	9.5	0.0254	0.8
t_av_div	-0.0022	-0.3	0.0508	2.0
t_sickleav	-0.0098	-6.7	0.0238	5.5
t_memunion	---	---	-0.1997	-4.8
t_training	0.0893	9.5	0.0946	3.3
t_christn	0.0238	2.3	0.0003	0.0
t_female	0.0139	1.5	-0.2217	-8.0
t_age	0.0187	5.2	0.0635	4.0
t_agesq	-0.0002	-5.0	-0.0006	-3.2
t_exptotal	-0.0024	-3.0	-0.0181	-7.0
t_expthiss	0.0015	1.8	0.0059	2.1
t_permanen	0.0081	0.6	-0.0185	-0.4
_cons	-1.6158	-6.6	-1.6223	-2.4
N	44299		4790	
Adj R-sq	---		---	
R-sq within	0.0102		0.0500	
between	0.0680		0.0107	
overall	0.0427		0.0192	

Notes: Unionized schools are those in which any teacher is unionized.

Table 8
Determinants of the school fixed effect

	Full specification		Parsimonious specification		Including variables with missing values	
	coeff	t-value	coeff	t-value	coeff	t-value
t_age	0.138	1.3				
t_agesq	-0.001	-1.0				
t_christn	0.487	2.4				
t_female	0.439	1.6	0.339	1.3	0.131	0.6
t_ma	-0.577	-2.0	-0.767	-2.8	-0.605	-2.4
t_training	-0.344	-1.4	-0.275	-1.1	-0.371	-1.6
t_logpay	0.318	1.4	0.528	2.8	0.343	2.1
t_av_div	0.816	2.9	0.720	2.6	0.530	2.2
t_sickleav	-0.028	-0.9				
t_memunion	0.144	0.3				
t_exptotal	-0.016	-0.5				
t_expthiss	0.008	0.3				
t_permanen	0.059	0.2				
log_totstren	0.140	1.0	0.244	1.9	0.277	2.6
pinflu	-0.030	-0.5	-0.080	-1.3	-0.050	-1.0
logmfeex	-0.015	-0.1	-0.066	-0.4	-0.323	-2.4
girlschool	0.285	1.7	0.333	2.0	0.194	1.4
resource4	0.002	0.3	0.000	-0.1	0.004	0.8
ptr	-0.024	-2.2	-0.030	-2.9	-0.016	-1.8
hours_week					0.124	10.0
ndayop					-0.682	-6.0
_cons	-7.897	-3.7	-6.044	-4.6	-3.512	-2.8
N	172		172		143	
Adjusted R-sq	0.2776		0.2660		0.5748	

Note: The school fixed effect is extracted from the achievement equation in the second column of Table 4, which included student and teacher characteristics.

Table 9
Regressing the 'Pupil Fixed Effect' on pupils' observed characteristics

	Without school fixed effects		With school fixed effects	
	coeff	t-value	coeff	t-value
hhsize	-0.003	-0.9	-0.003	-1.2
ybrother	-0.087	-6.3	-0.028	-2.2
obrother	-0.075	-5.7	-0.027	-2.3
ysister	-0.063	-4.4	-0.018	-1.4
osister	-0.043	-3.7	-0.007	-0.6
faedu	0.086	9.0	0.056	6.5
maedu	0.117	13.8	0.070	9.0
specs	0.159	9.5	0.090	6.0
disabled	-0.056	-0.4	-0.039	-0.3
sibling_icse	0.030	1.6	0.000	0.0
raven	0.037	31.3	0.039	33.9
sikh	-0.159	-4.9	-0.082	-2.3
christn	-0.079	-2.7	-0.201	-6.8
muslim	-0.078	-2.5	-0.089	-3.0
wealth	0.017	3.0	0.009	1.8
wealth_sq	-0.001	-4.2	0.000	-3.3
c_agemo	0.214	11.3	0.123	7.1
agemosq	-0.566	-12.0	-0.349	-8.1
c_obc	-0.020	-0.6	-0.052	-1.7
c_sc	-0.022	-0.3	0.033	0.5
c_st	-0.178	-3.0	-0.145	-2.6
male	-0.191	-12.3	-0.099	-6.1
N	10016		10016	
Adjusted R-square	0.2525		0.4346	
F (joint-sig of school FE)	---		19.83	
p-value of F-test	---		0.0000	

Note: There are 172 groups (i.e. schools). Mean number of observations per group (pupils per school) is 58.2.

Table 10
Regression of log of teacher's monthly pay

	Across school (OLS) coefficient	t-value	Within school (school fixed effects) coefficient	t-value
Teacher characteristics				
t_ma	0.0548	3.3	0.0524	4.9
t_training	0.0513	3.1	0.0481	4.8
t_av_div	0.0365	2.4	0.0166	1.5
t_sickleav	-0.0068	-2.2	-0.0007	-0.4
t_memunion	0.2696	2.4	0.0826	2.3
t_christn	0.0506	2.1	0.0352	2.8
t_female	-0.0282	-1.3	-0.0238	-2.0
t_age	0.0225	3.8	0.0098	2.5
t_agesq	-0.0002	-3.6	-0.0001	-2.3
t_exptotal	0.0052	2.5	0.0064	5.0
t_expthiss	0.0129	6.4	0.0096	9.1
t_permanen	0.1589	4.9	0.1694	11.1
Average student mark				
stdmark	0.1200	4.0		
_cons	5.7190	20.3	8.1044	94.1
Subject dummies included	Yes		Yes	
State dummies included	Yes		Not applicable	
School variables included	Yes		Not applicable	
N	2033		2033	
Mean of dependent var.	8.755		8.755	
			R-sq within 0.3808	
Adjusted R-square	0.6436		R-sq between 0.3695	
			R-sq overall 0.2908	

Note: For School fixed-effects estimation, the number of groups is 183, i.e. estimation is within 183 schools. Mean number of teachers within a school is 11.1 (minimum=5, maximum=32). F-value of the joint significance of the school dummies in the first set of columns is 35.3 and in the second set of columns is 35.8, i.e. the school dummies are jointly significant at the 0.0000 level in both cases. The base category for the subject dummies is 'other subjects'.

Table 11
School fixed effects regression of log of teacher's monthly pay,
by whether school is unionized

	Unionized		Non-unionized	
	coefficient	t-value	coefficient	t-value
Teacher characteristics				
t_ma	0.0676	1.9	0.0535	4.7
t_training	0.0422	1.3	0.0472	4.4
t_av_div	0.0650	1.9	0.0196	1.7
t_sickleav	-0.0054	-1.4	0.0002	0.1
t_memunion	0.0381	1.0	---	---
t_christn	0.0392	1.0	0.0402	3.0
t_female	0.0070	0.2	-0.0291	-2.3
t_age	-0.0061	-0.5	0.0151	3.6
t_agesq	0.0001	0.7	-0.0002	-3.5
t_exptotal	0.0080	1.9	0.0072	5.3
t_expthiss	0.0116	4.1	0.0082	7.1
t_permanen	0.2995	5.6	0.1669	10.3
_cons	8.3164	27.8	7.9719	88.7
Subject dummies included	yes		yes	
N	230		1832	
Mean dependent variable	8.929		8.731	
Mean monthly pay	8641.19		6867.89	
R-sq within	0.5758		0.3655	
R-sq between	0.5317		0.3545	
R-sq overall	0.3395		0.2806	

Note: A school is taken to be unionized if any of its teachers (in our sample of grade 10 teachers) are unionized. 11% of teachers are unionized.

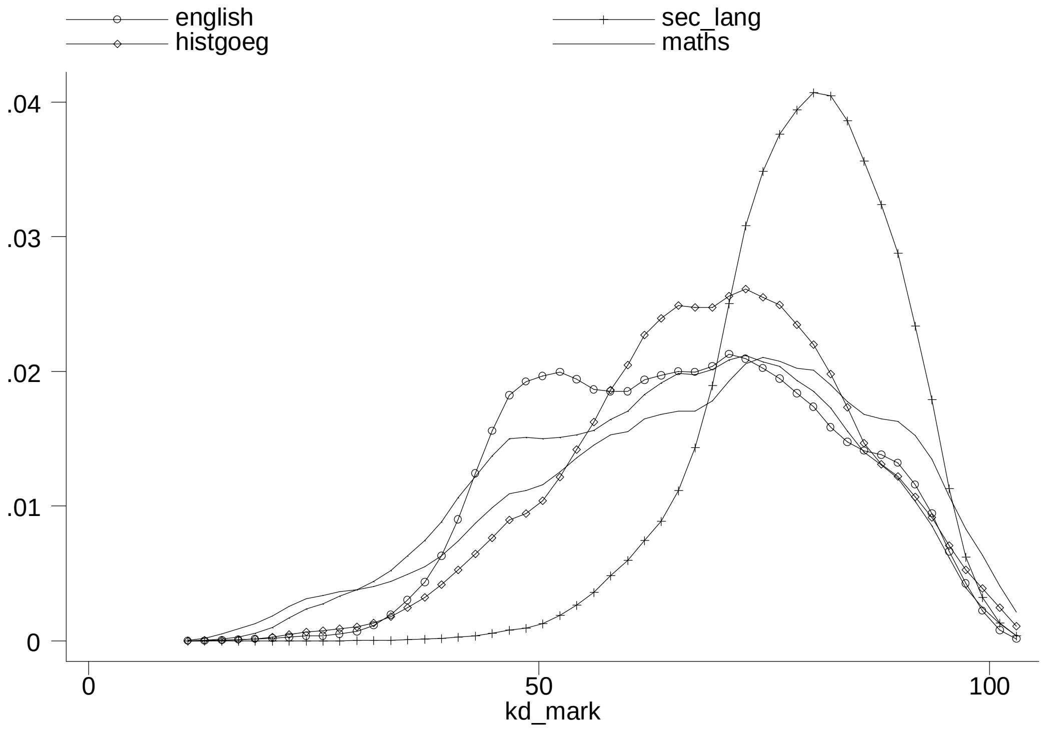

Figure 1

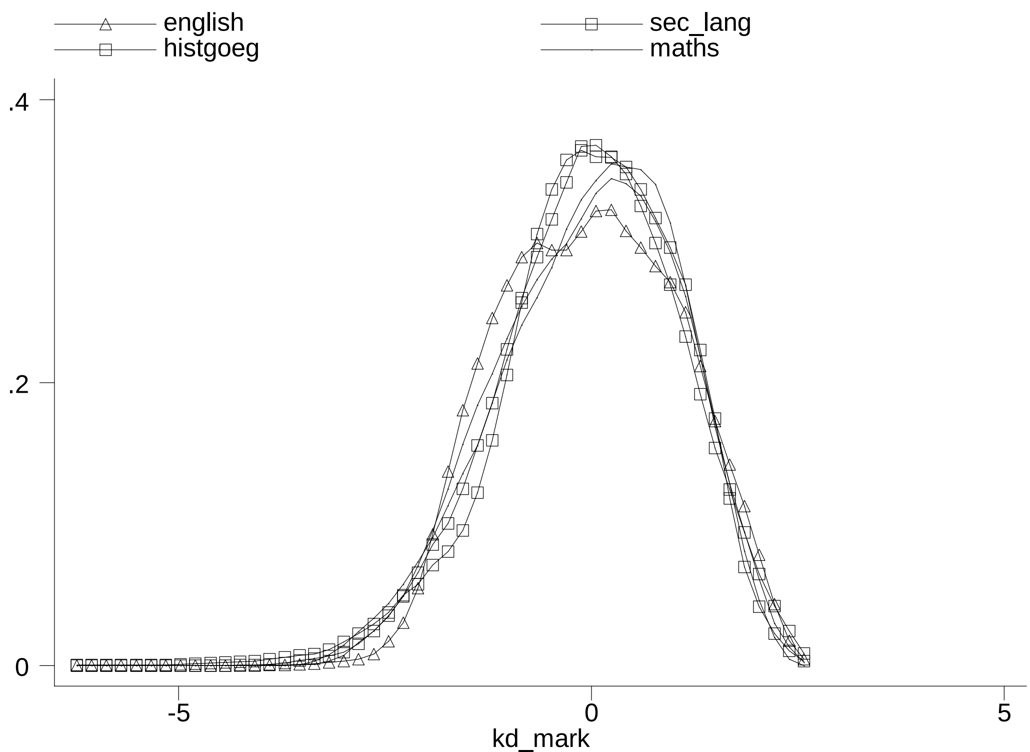

Figure 2