Additional Resources versus Organizational Changes in Education: Experimental Evidence from Kenya

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Abstract

Several studies have found that resources alone have limited impact on the quality of education in developing countries, while others have found that changes in pedagogy and incentives can have significant and large impacts. This paper compares increases in resources alone (changes in pupil-teacher ratios) to two frequently advocated (and implemented) changes in the organization of teaching: the use of locally hired teachers on short contracts, and the involvement of parents in the management of schools. We use data from 140 schools in Western Kenya, 70 of which were randomly selected to receive an extra contract teacher, and find that reducing the pupil-teacher ratio (from 82 to 43 on average), in the absence of any other changes, leads to reduced teacher effort, and to small and insignificant increases in test scores. In contrast, students who were (randomly) assigned to the contract teachers experience significant improvement in test scores, as did those who were in schools were school committees were given extra training.

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1. Introduction

In the last 15 years, primary school enrollment rates have increased significantly in several developing countries. Between 1999 and 2004 the number of new primary school entrants in sub-Saharan Africa increased by more than 30 percent. In Kenya alone, following the abolition of school fees, enrollment in primary schools rose from 5.9 million to 7.6 million between 2002 and 2005, an increase of nearly 30 percent (UNESCO, 2006, 2007).

The quality of education, however, has not progressed as rapidly. According to Filmer, Hasan and Pritchett (2006), a household survey in Ghana found that only a quarter of 15-19 year olds could answer more than four out of eight very simple math questions, such as 2x3=?, 10/5=?, 24+17=?, etc.); in South Africa, only 37 percent could answer at least half of a set of simple "real-life" mathematics questions, such as "A shop has 126 liters of milk. 87 liters are sold. How many liters remain?" The observation that increased school participation does not necessarily translate into increased competency in basic skills is not specific to Africa. In India, a 2008 nationwide survey showed that, by grade 5, only 56 percent of children in rural areas can read a simple story (grade 2 level), and 19 percent cannot read beyond a word (Pratham, 2009). Similar finding were found in a large scale survey in Pakistan (Andrabi, Das and Khwaja, 2009). Furthermore, policies or programs that managed to increase child presence at school without changing the teaching methodology or environment (e.g., deworming, school meals) typically did not lead to increased test scores, calling into question the usefulness of increasing school participation along the intensive margin (Miguel and Kremer, 2004; Kremer and Vermeesch, 2004). The campaign to achieve universal primary education might have been a victim of its own success: budgets have not kept pace with enrollment, leading to large pupil-teacher ratios, over-stretched physical infrastructure, insufficient number of textbooks, etc. However, this lack of resources might not explain much of the quality deficit. Contrary to the evidence in richer countries (Krueger and Whitmore 2002, Angrist and Lavy 1999, Piketty 2004), the evidence for developed countries suggest that, in many settings, providing extra resources such as textbooks, flipcharts, or remedial education without changing either pedagogy or incentives faced by teachers or students does not lead to an increase in test scores (see Glewwe and Kremer, 2006, for a review).

In contrast, changing the incentives faced by parents, teachers, or students, or using new resources to change the pedagogy (or at least what part of the curriculum is emphasized in school), or doing both at the same time, has been shown to have significant effects on test scores (Banerjee et al, 2007). This suggests that reforming school systems and curricula may be more effective than simply pouring resources into public schools. As a matter of fact, many developing countries are experimenting with new ways of

publicly funding education (such as vouchers, or public-private partnerships), greater involvement of parents in schooling decisions (through school committees which are given control over resources), and new ways of hiring teachers. Many countries now favor hiring less experienced, some times less trained, teachers on short contract and low pay, both for budgetary reasons and because these teachers are thought to be easier to motivate (De Laat and Vegas, 2003; Muralidharan and Sundararaman, 2008).

While these two sets of studies may suggest that changing the teaching organization might be more effective than changing the level of resources available, few studies have been explicitly conceived and designed to compare the effectiveness of these two sets of reforms within a given, common context. Most often, a particular study either examines the effect of providing a new resource, without any other exogenous change (there can of course be endogenous changes to providing an input), or it examines the effect of a new teaching method or of a new incentive system.[5] Since these specific studies take place in various places and times, a direct comparison between these two types of intervention can be difficult. In this paper, we explicitly compare, in the same setting and over the same time period, the effectiveness of the three education reforms that are the most frequently discussed and implemented in primary education systems: reducing the pupil-teacher ratio, hiring teachers on short-term contracts, and granting parents greater involvement in the school management.[6]

To do so, we exploit data from an experimental program conducted in Western Kenya (the "extra teacher program", or ETP) which provided 140 schools, randomly selected out of 210, the funding sufficient to hire a contract teacher under the control of the school committee. The ETP program started a few months into the 2005 school year, and for the remainder of that year, schools that received the funds (henceforth, the "ETP schools") were instructed to create an additional section in grade 1, to be taught by the contact teacher, thereby reducing the pupil-teacher ratio in that grade by half. At the beginning of the following school year (2006), schools were instructed to assign the contract teacher to grade 2, in order to maintain a low pupil-teacher ratio for the cohort of students affected in the first year. The "treatment" thus affected one cohort of students (the students enrolled in grade 1 in 2005) over two years.

At the end of the second year, a standardized test administered to students in that cohort revealed that scores were 0.16 standard deviations higher in ETP schools than in the control schools. Muralidharan and

[5] An exception is Muralidharan and Sundararaman (2008a), which compares teacher incentives to block grant in India, and finds that teacher incentives have a larger effect on test scores. Banerjee et al. (2007) try to separately estimate the effect of a pull out remedial education system.

[6] In a companion paper, we also study the impact of a change in a choice pedagogy in the same setting: sorting students by levels of initial preparedness, often called "tracking".

Sundararaman (2008b), evaluate a similar intervention in India, and find that the addition of contract teachers to primary schools led to 0.15 and 0.09 standard deviations increases in math and language tests, respectively.[7]

Funding schools to enable them to hire a contract teacher for grade 1 is a complex intervention, however: additional resources are provided to reduce the pupil-teacher ratio, but these resources are controlled locally (since the school committee, made of parents and teachers, is given the responsibility to hire and fire the teacher); and these resources are directed towards hiring a contract teacher, as opposed to a regular civil-service teacher. Without additional variation, it is thus impossible to disentangle the respective roles of resources, community control, and teacher type, in the education production function. For this reason, we designed the ETP program and its evaluation in a way that enables separate identification of each of these three effects, as well as that of another education policy of interest, the sorting of students into "tracks" based on their scores.

First, 70 of the 140 ETP schools were randomly selected to be "tracking schools": in these schools, after the new teacher was hired, grade 1 students were assigned to a section based on their first-trimester grade. The specific effect of tracking is studied in a companion paper (Duflo, Dupas, and Kremer, 2009), and the 70 tracking schools are dropped from the analysis in the present paper The present paper thus focuses on the comparison between the remaining 70 ("non-tracked") ETP schools, and the 70 control schools. In the 70 non-tracked ETP schools (henceforth, "ETP schools"), students were randomly assigned to a section. Furthermore, in half of those schools, the school committee was trained on how to monitor and assess the performance of the contract teacher, and encouraged to hold a performance review at the end of the year to decide whether or not to renew the contract for the ETP-funded teacher. These schools are called "SBM schools", for "school-based management". This design allows us to separately estimate the effect of a change in class size, a change in class size accompanied by a change in teacher incentives, and a change in class size accompanied by a greater involvement of parents in teacher management.

Our findings corroborate the conclusion of the previous literature. In schools that did not receive the extra training for school committee members, students assigned to the regular teacher in ETP schools score only 0.09 standard deviations higher than students in comparison schools, and this estimate is insignificant. In contrast, students assigned to the contract teachers in the ETP schools score 0.18 standard deviations higher than students assigned to the regular teacher in the same schools (a significant

[7] Other evaluations of contract teachers include see De Laat and Vegas, 2003; and Chaudhury et al., 2005. In the US, A study of higher education in the United States suggests that students taught by adjunct professors do as well as students taught by tenure-track professors (Bettinger and Long, 2006).

difference) and 0.27 standard deviations higher than students in comparison schools. Moreover, in schools where school committees were trained, students assigned to the regular and to the extra teacher do almost equally well, and significantly better than students in comparison schools (they score about 0.21 standard deviations higher in mathematics). These effects do not persist, however. A long-term follow-up shows that these differences had attenuated when students were tested again one year after the end of the program (i.e. in November 2007). The positive effects remain significant only for children who had been originally assigned to the contract teacher in the schools where school committees were trained.

It is tempting to attribute the difference in performance between students assigned to contract teachers and students assigned to regular teachers to the different incentives their teachers faced. The contract structure, however, is not the only difference between contract and regular teachers. First, contract teachers are generally younger; they thus have less experienced, but they were also trained more recently, perhaps with better methods. Second, many schools have a teacher rotation system (where one teacher teaches math, another one teaches language, etc.); the ETP program required that the contract teacher would be fully in charge of the section she was assigned to (teaching all subjects to one set of students), but allowed the schools to maintain their rotation scheme between regular teachers for the other sections. Thus students assigned to the contract teacher we also assigned, *de facto*, to a single teacher instead of an array of teachers, and this could have contributed to their learning experience.

That said, we do observe important differences in effort levels across teacher types. Contract teachers were significantly more likely to be in class and teaching than regular teachers in their own school, as well as regular teachers in control schools. In contrast, regular teachers in ETP schools *reduced* their effort in response to the introduction of the contract teacher: during unannounced visits conducted during school hours, regular teachers in ETP, non-SBM schools were 13 percentage points (22 percent) *less* likely to be found in class and teaching than in control schools.

The positive effect of the SBM training on students' scores can also be linked to a change in teachers' effort. The likelihood that regular teachers decreased effort in response to the introduction of a contract teacher was significantly lower in ETP, SBM schools than in the ETP, non-SBM schools.

Overall, these results suggest that at least part of the positive impacts on learning of the contract teacher and of the SBM training were channeled through additional teacher effort. More generally, they also provide a potential reason (not exclusive of others) why adding extra resources without any other changes seem to have little impact on learning in developing countries: if part of the resources are diverted by

teachers (as here, in the form of lower effort), schools, or parents, the overall effect of a change in resources will be dampened, compared to the direct effect the added resources would have on learning if everything else could be kept constant.[8]

The remainder of this paper proceeds as follows: Section 2 provides background and describes the project and data; Section 3 presents results on the intermediate outcomes of teacher and student effort; Section 4 discusses the impact on student achievement; and Section 5 draws conclusions.

2. Background

2.1 Primary Education in Kenya

In Kenya, primary school fees were abolished in 2003.Subsequently, enrolment in primary schools rose from 5.9 million to 7.6 million between 2002 and 2005, an increase of nearly 30 percent, (UNESCO, 2006). Yet this progress creates its own new challenges. The influx of new students has raised pupil-teacher ratios. By 2005 in the area we examine in Kenya, the average class size in first grade was 83, the median was 74, and 28 percent of first grade classes had more than 100 students.

Historically, most teachers in Kenya have been hired centrally through the Ministry of Education Science and Technology (MoEST) Teachers Service Commission (TSC). Teachers hired through the TSC have civil service protection and receive wages and benefits that are considerably above levels that would clear the market, and there is considerable queuing for these positions. For teachers in these positions, promotions, transfers, and disciplinary actions are decided through MoEST, rather than by more local bodies such as the school committees associated with every Kenyan school, which are primarily composed of elected representatives of parents.

Some school committees hire teachers locally using parent contributions to supplement the teachers hired by the Ministry of Education. In the area of study, we observed that these locally hired teachers, called PTA (Parent-Teacher Association) teachers, received compensation in the range of 2000 Kenyan shillings, or US$ 30 per month. In comparison, the average civil service teacher receives around 7,000 shillings (US$ 120) per month plus generous benefits including housing allowances, provisions for retirement, and medical coverage (Glewwe et al., 2003). New graduates of teacher training colleges often work for several years as PTA teachers and then obtain positions as civil service teachers.

[8] Das et al, (2004) demonstrate similar effects for parental investments in response to an increase in non-teacher inputs: parents invested less in non-teaching inputs in response to a program that provided grants to them.

Since the introduction of free primary education in 2003, however, parents can no longer be required to pay fees for their children to attend school, and thus school committees are generally unable to raise the funds necessary to hire PTA teachers. In 2004, 80 percent of primary schools in our Western Kenya sample had no PTA teachers. Free primary education has thus raised the pupil-teacher ratio both by increasing the number of students and by reducing the number of teachers. It also led to a large influx of students who had little or no previous schooling.[9]

2.2 Experimental Design

The experimental project we study was modeled on the school committees' experience hiring extra teachers. With funding from the World Bank, the non-profit organization ICS provided school committees with funds to locally hire an additional first grade teacher who had the same academic qualifications as a civil service teacher. Schools that participated in the Extra Teacher Program (ETP) received funding to create an additional class in first grade at the start of the second school term in May of 2005 (the school year runs from January to December in Kenya). In each school, ICS held a meeting with parents and teachers to explain program rules regarding the hiring of an extra teacher. Once a teacher had been hired, ICS disbursed funds to the school committees. School committees then paid extra teachers each month. The monthly allowance totaled 2,500 Kenyan shillings (around US$ 35), putting it at the top of the range of what is typically paid to extra-teachers by school committees in Kenya. When the program continued the following school year, school committees were free to replace or keep the original extra teachers and were encouraged to move the teachers to second grade with the same group of students.[10]

A list of all students enrolled in first grade in each of the 140 schools was collected at the end of the first school term.[11] In the treatment schools, the contract teacher started teaching at the beginning of the second term. Most of these schools had only one first grade class and were instructed to split it in two, one to be taught by the contract teacher and one to be taught by the regular, civil service teacher. First grade pupils were randomly assigned to one of the two classes. The contract teacher was then randomly assigned to one class, and the civil service teacher took the other one. Schools with two or more classes in first grade before the program were also encouraged to create an additional class to be taught by the contract teacher, and to randomly assign the students across the three (or more) classes. After the random assignment of students had been made, transfers from one class to the next were systematically granted

[9] Pre-school is not sponsored by the MoEST and therefore not free, since parents have to contribute to the pre-school teacher salary.

[10] Students enrolled in grade 2 in 2005 and who repeated grade 2 in 2006 were randomly assigned to either the contract teacher or the civil service teacher in 2006.

[11] The school year in Kenya starts in January and is divided in three terms of three months, with three month-long breaks in April, August, and December.

for siblings who wanted to stay together. Other transfers were in principle possible upon parental request, but they had to be granted pro-forma approval from the ICS program administrator. In practice, though, there were very few requests of this kind.[12]

In addition, among the 70 ETP schools, 35 schools were randomly selected to participate in a School-Based Management (SBM) initiative designed to empower the school committees to monitor teachers' performance. In those schools, school committees held a formal review meeting at the end of the first school year of the program (in November 2005) to assess the contract teacher's performance and decide whether to renew his or her contract or to replace her. To prepare the school committee, ICS provided its members a short, focused training on monitoring the contract teacher's performance. The school committee members were taught techniques for soliciting input from parents and checking teacher attendance. A formal sub-committee of first grade parents was formed to evaluate the contract teacher and deliver a performance report at the end of the first year.

Overall, the program was implemented successfully. While average class size in the comparison schools was 82 in grade 1 in 2005, it was only 43 in ETP schools (see Table 1). This significant gap in class size was partially maintained the following year in second grade: in 2006, average class size in second grade was 68 in comparison schools and 42 in ETP schools.

2.3 Data Collection

Sample and Summary Statistics

The sample includes about 13,500 students who were enrolled in first grade at the end of the first school term in one of the 140 primary schools enrolled in this study. Slightly less than half are girls (49 percent). On average, students were seven years old at the outset of the program (with a standard deviation of 1.3 years), but ages ranged from 5 to 14.

In addition, we have data on 754 teachers who taught lessons in first grade in 2005, in second grade in 2006, or both. The vast majority of these teachers were centrally hired civil service teachers (653 in total). The average age of civil service teachers was 42 (with a standard deviation of 9 years), and 67 percent were female.

[12] New pupils who joined the school after the introduction of the program were assigned to a class on a random basis. However, since the decision for these children to enroll in a treatment or comparison school might be endogenous, they are excluded from the analysis.

Finally, 101 contract teachers were hired through the ETP program by the 70 ETP schools over the course of the experiment. On average, these contract teachers were 27 years old (with a standard deviation of 4.5 years), and 48 percent were female. They all had received a teacher's certificate from a governmental teacher training college, as all civil-service teachers do.

Outcomes

Our key outcome of interest is student achievement, as measured by scores on a standardized math and language test administered in all schools after 18 months, just before the program ended. The same test was administered again during a follow-up one year after the program ended. Exams were administered by trained enumerators and graded blindly by data processors. In each school, 60 students were randomly drawn from the initial sample to participate in the tests. Students were asked math and literacy questions ranging from counting and identifying letters to subtracting two-digit numbers and writing words. To limit attrition, enumerators were instructed to go to the homes of students who had dropped out or were absent on the day of the test and bring them to school for the test. This was not always possible, however.

The attrition rate for the endline test was 18 percent, and 23 percent for the one-year follow-up test. Appendix Table A1 regresses attrition on treatment dummies, as well as treatment dummies interacted with students' gender and students' initial attainment. It shows that attrition is not significantly different across ETP and non-ETP schools. However, students of contract teachers are significantly less likely to have missed the endline test, especially boys.

In addition, data was collected on pupils' attendance and dropout/grade promotion through unannounced school visits made by enumerators on a quarterly basis. Overall, the dropout rate among first grade students in our sample was low (below 0.5 percent).

Baseline data on students' initial achievement was collected from the school records. While internally consistent within schools, this data is not comparable across schools.

2.4 Conceptual Framework

The following framework may be useful in thinking about the impact of the program. In particular, suppose that educational outcomes for student i, Y_i, are given by:

$$Y_i = f(T, P, X_i, \underline{X}, E, W)$$

where T is the number of teachers in the grade, P is the number of pupils, X is the pretest score, bars denote the average in the class, E is an index of teacher effort and behavior, and W represents other factors, possibly stochastic.

Suppose also that teacher effort, E, is given by $E = g(T, P, \underline{X}, I, O)$, where I represents the incentive system in place and O represents other unmodeled factors.

In this framework, to understand the impact of hiring extra teachers on educational outcomes, it is important to look at the total derivative of f with respect to $\underline{X}$, P, and T, not just the partial derivative, to see how the teacher-pupil ratio also affects teacher effort and not just student outcomes directly. Of course, the partial derivative of g with respect to T, P, and $\underline{X}$ can potentially vary with the context and in particular the incentive system. Thus, Section 3 examines how teacher (and student) behavior responds to the interventions we study, before Section 4 examines the overall impact of these interventions on educational outcomes.

3. Intermediate Outcomes: Teacher and Student Effort

Before looking at student achievement, we investigate the effect of the program on the intermediate outcomes of teacher and student effort to help us interpret the results on achievement.

Teacher Effort

Table 2 shows regressions estimating the impact of various program variants on teachers' effort, measured by their presence in school on the day of a random visit and whether or not they were found in class teaching when the observers entered the school compound. The omitted category in this regression is civil service teachers in the comparison schools. The first row in the table is thus the difference between civil service teachers in the ETP schools that did not receive the SBM intervention and those in the comparison schools. This difference tells us the effect of reducing the pupil-teacher ratio. The second row interacts the ETP treatment with the SBM treatment, and the coefficient estimates the difference, among ETP schools, between civil service teachers in SBM schools and non-SBM schools. Row 3 presents the coefficient for contract teachers, and Row 4 presents the effect of SBM on contract teachers.

While civil service teachers in ETP schools were as likely to be present in school as teachers in comparison schools, the likelihood that civil service teachers in ETP schools with no SBM were found in class teaching if they were present fell 15 percentage points (column 3) relative to the (civil service)

teachers in comparison schools.[13] This effect, significant at the 5 percent confidence level, corresponds to a 22 percent decrease in teacher presence in class compared with civil service teachers in comparison schools. This finding suggests that civil service teachers took advantage of the presence of the extra contract teachers to work less. While this crowding-out effect may be exacerbated by the vulnerable position of the contract teachers, who are presumably easy for civil service teachers to order around, it might have occurred even if the additional hired teachers had civil service protection.

Contract teachers in ETP schools with no SBM were 30 percentage points more likely to be found in class teaching when at school than their civil service counterparts in the same schools (Table 2, Row 3, Column 3). A natural interpretation of this is that the strong incentives to perform that contract teachers face for contract renewal play a large role.

The SBM initiative reinforced the role of parents (as opposed to only headmasters, who often dominate those committees) in hiring, monitoring, and retaining the contract teachers. Parents were instructed on how to monitor the contract teacher, and the SBM initiative had a significant impact on the attendance records of contract teachers (Row 4, column 1), but it did not improve the effort of contract teachers as measured by overall presence in the classroom, which is very high to start with (Row 4, column 5).

Compared with civil service teachers in non-SBM ETP schools, civil service teachers in SBM schools were 9 percentage points more likely to be found in class teaching during random spot checks by the research team. This effect is significant at the 10 percent level (Row 2, Column 3) but loses significance when controls for individual characteristics are added (column 3). One potential explanation for the effect of SBM on civil service teachers' effort is that the SBM initiative emphasized the responsibility of the contract teacher with respect to the specific class to which she was assigned and thus made it more difficult for the head teacher or the civil service teachers in those schools to use the extra teacher to relieve themselves of their own duties when they actually did show up to school. The contract teachers in these schools had a greater incentive to please the school committee and less of an incentive to please the other teachers and the headmaster.

Student Attendance

The reduction in pupil-teacher ratio had no impact on student attendance for students assigned to the civil service teachers in ETP schools (see Table 3, Row 1). However, students of contract teachers were 1.5

[13] When teachers are not in class teaching, they are usually in the teachers' room. When a teacher is absent, it is common for classes to be combined and taught by the teacher who is present.

percentage points more likely to be in school than students of civil-service teachers in the same schools. This corresponds to an 11 percent decrease in absenteeism among students of contract teachers, significant at the 5 percent level. Since we found in Table 2 that the ETP program reduced the rate of class presence by civil service teachers, while that of contract teachers was significantly higher than the class presence of teachers in comparison schools, a plausible interpretation of the effect of the program on student's presence is that students come to school more if their teacher teaches more.

4. Effect on Test Scores

In this section, we will review the overall effect of the program and the separate effects of pupil-teacher ratio, teacher type, and SBM on test scores. Table 4 presents average treatment effects on test scores at the endline test (panel A) and one year after the end of the program (panel B). Test scores were normalized such that the mean and standard deviation of in the non-ETP group are zero and one, respectively. All standard errors are clustered at the school level. Tables 5 and 6 present regression estimates with individualized and school-level controls. For each outcome, we present the results of four different regression specifications: the first specification estimates average treatment effects with treatment dummies. The second specification adds controls for age, gender and school size. The third specification adds a control for the student's initial attainment (which is missing for 14 schools, therefore adding this control reduces the sample size). The fourth specification adds controls for the teachers' characteristics (age and gender).

4.1 Overall Impact

Adding contract teachers substantially improved average test scores in the short-run. Eighteen months into the program, students in ETP schools had, on average, test scores that were 16 percent of a standard deviation higher than students in the comparison schools (Table 4, column 2). This difference is significant at the 10 percent level.

Simply introducing a new contract teacher and randomly assigning students to either this new teacher or the civil service teacher, without training the school committee and without tracking by initial achievement has a small (13 percent of a standard deviation) but insignificant effect on test scores, despite a reduction in average class size of about 40 students. The effect is larger (19 percent of a standard deviation, significant at the 10 percent level) when the school committees are trained on teacher

management (SBM).[14] Few of these effects persist after the program ends, however. Results from the long-term follow-up presented in Panel B of Table 4 show no significant differences across groups.

Overall, these results suggest that reducing class size alone is not key to improving school quality; the environment in which the size reduction is done appears to be an essential element of the success of such an intervention. We now turn to more detailed evidence to unpack these results.

4. 2 Pupil-Teacher Ratio

In order to isolate the relationship between pupil-teacher ratio reduction and school achievement, we compare the test scores of students assigned to civil service teachers in ETP schools with those of pupils in the non-ETP schools. Because students were randomly assigned to either civil service teachers or contract teachers, this comparison can be interpreted as the causal effect of being assigned to a smaller class with a civil service teacher.

While pupils in schools with a reduced pupil-teacher ratio scored somewhat higher on both the math and language tests compared to pupils in the non-ETP schools (9 percent of a standard deviation overall), the null hypothesis of no effect cannot be rejected (see column 5 of Table 4). These results are confirmed by the regression results in Tables 5 and 6 (first row).

At the sample mean, these results suggest that, in lower grades, reducing class size from 80 to 40 students without any other change does not lead to a significant increase in test scores. This is a striking result, in particular since the literature in richer countries suggests that there are significant effects of pupil-teacher ratio reduction on test scores in some contexts (see e.g. Krueger and Whitmore, 2002; Angrist and Lavy, 1999; Piketty, 2004). However, this finding does echo the results from India by Banerjee et al. (2007), which showed no impact of the reduction in pupil-teacher ratio achieved through the hiring of a remedial education teacher for students who remained with the regular (civil service) teacher. There are several possible explanations for these results. One is that, at 40, the class size remains too large and too diverse for the teacher to be able to devote any individual attention to each student, so that having 40 rather than 80 students still does not make any difference. Another possible explanation, however, is that in this program, class size reduction was accompanied by a reduction in teacher effort, as discussed in Section 3

[14] The effect of the program is much larger and very significant when the class is not divided in two streams randomly but when students are tracked by initial achievement, with or without training of the school committees (between 25 and 31 percent of a standard deviation, significant at the 1 percent level). We do not present this data in this paper. The impact of tracking by initial achievement is discussed at length in a companion paper (Duflo, Dupas and Kremer, 2009).

where we noted that civil service teachers were less likely to be in class teaching in ETP schools than in non-ETP schools.

It should be noted that these results do not tell us what would have been the impact of achieving the same pupil-teacher ratio reduction by adding civil service teachers, as opposed to contract teachers. For example, it is possible that if additional civil service teachers had been hired, existing civil service teachers would be less inclined to start shirking, while with this program they can more easily pass work off to the contract teachers.

4.3 Comparing Contract Teachers and Civil Service Teachers

To measure the impact of being taught by a contract teacher rather than by a civil service teacher, keeping pupil-teacher ratio constant, we compare the achievements of children who have been assigned to the contract teacher hired through the ETP program to that of children who stayed with the civil service teachers in ETP schools. Because children were randomly assigned to either group, the comparison is a direct measure of the impact of being taught by a contract teacher rather than the civil service teacher in a school where there is a contract teacher. It is important to note that because the effort of the civil service teacher was likely affected by the presence of the contract teacher, this comparison cannot be interpreted as the effect of hiring contract teachers rather than civil service teachers to achieve pupil-teacher ratio reductions.

The regression results in Table 5 show that students assigned to a contract teacher in ETP schools score 0.21 standard deviations higher than their schoolmates assigned to civil service teachers (Row 3), and 0.24 standard deviations higher than students in the non-ETP schools (row 14). Together rows 2 and 4 show that the SBM intervention does not affect students assigned to the contract teacher, consistent with the lack of its effect on the probability that contract teachers were found in school teaching. The positive and significant impact of the contract teacher does not disappear (if anything, it increases) when controls for demographics of the teachers are added (column 4), thus suggesting that the contract teacher effect is not a pure age effect.

The superior performance of contract teachers could potentially be due either to better choice of teachers by local school committees or to the stronger incentives faced by contract teachers. It is worth noting that good performance as a contract teacher could also have been perceived as a stepping-stone to a tenured civil service position. While the experimental program we conducted did not include provisions for giving

teachers civil service status, it did give them experience. By the end of the program 32 percent of the contract teachers hired through the experiment had been absorbed by the Kenyan Government.

These results thus suggest that teacher incentives do matter in schools, and they are relevant for assessing a policy that is probably more realistic for many developing countries, which is to hire additional teachers first on a temporary contract basis, and then regularize the ones that perform well with a civil service position. However, it cannot necessarily be assumed that the results would remain constant if the status of all teachers were changed to conditional contracts. This is because contract teachers in our experimental program may have worked hard in part because they hoped it would help them obtain civil service teaching positions.

These differences between the contract teachers and the civil service teachers may also result from the different classroom experiences that they faced. Civil service teachers often rotate across classes, so the observed test score advantage that the students of contract teachers display could also be attributed to a potential benefit from being taught by a single teacher throughout the day.

4.4. Local Monitoring

The test scores data suggest that the SBM initiative had a positive impact on learning overall. In the short-run, the SBM effect appears to be a substitute, rather than a complement, to the contract teacher effect: students assigned to the contract teacher did not benefit more from the program in SBM schools, but the SBM initiative was helpful in raising test-scores for the students of civil service teachers, just as it was successful in decreasing the classroom absence rates of these teachers. Row 13 in Table 5 suggests that students of civil-service teachers in ETP schools benefitted from the reduced pupil-teacher ratio when the school committee had been trained on teacher monitoring. The effect is large and significant for math, less so for literacy.

In the long run, however, the SBM initiative seems to have been necessary for the positive contract-teacher effects to persist. Indeed, at the long-term follow-up, only students assigned to the contract teachers in SBM schools performed significantly better than students in control schools. This could come from the fact that SBM schools were more likely to maintain the ETP program on their own after the NGO-subsidy ended. Overall, 53% of the ETP schools (75 out of 140) continued employing the contract teacher after the ETP subsidy ended. Most of them did so with community contributions (93%) while in 5% of the cases the contract teacher staid in the school has a volunteer, and in 2% of the cases the contract teacher was hired as a civil service teacher and placed in the same school. These figures are comparable

across SBM and non-SBM schools, but two differences emerge in how the SBM and non-SBM schools managed the contract teacher. First, SBM schools were more likely to hold on to the same contract teacher from start to finish: by 2007, 36% of SBM schools were employing with their own funds the contract teacher who was hired when the ETP program started in 2005, whereas only 28% of non-SBM schools were doing so. This means that non-SBM schools had a greater turnover of contract teachers, and so less continuity for the children assigned to the contract teacher. Furthermore, SBM schools were more likely to continue employing the contract teacher "in the spirit" of the ETP program: in 2007, among schools that continued employing the contract teacher with their own funds, 43% of SBM schools were using the contract teacher to teach an additional section in Grade 3 (and thus maintain the reduced class size for the study cohort for another year) whereas only 34% of non-SBM schools did so. SBM schools were particularly likely to maintain the contract teacher with the study cohort in 2007 if the same contract teacher had been employed throughout (52%), whereas non-SBM schools did not attempt to maintain continuity (only 25% of non-SBM schools who had stayed with the same contract teacher throughout assigned that teacher to grade 3 in 2007).

5. Conclusion

As we get closer to the goal of universal primary education and as governments and countries shift their attention from increasing school participation to improving the quality of education, the need for evidence on what affects education quality becomes crucial.

As did others before, this study shows that increases in resources may not be sufficient to improve school quality. Indeed, without changes in incentives or the organization of teaching, extra resources lead to less effort by teachers. Students in smaller classes in treatment schools taught by civil service teachers did not score significantly higher than students in larger classes taught by civil service teachers in comparison schools, suggesting a limited impact of reductions in pupil-teacher ratios.

On the other hand, in the same setting, we also find that two educational reforms that are often advocated by policy makers, and that are already implemented on a large scale in many developing countries, seem to have the potential to improve educational outcomes significantly.

First, we find that contract teachers tend to be present and in class teaching more frequently than tenured civil service teachers in the same type of school. Their students learn more and perform better on cumulative achievement tests; specifically, students of contract teachers performed 0.21 standard

deviations higher on achievement exams than their counterparts in classes taught by civil service teachers. This could be due to the stronger incentives faced by these teachers, to the fact that they were hired from the local community, or possibly to the fact that they were single-handedly responsible for a group of students.

Second, parental involvement in school management seems to be effective: civil service teachers were more likely to be in class and teaching during random visits in schools where the school committee was empowered to monitor teachers than in schools without the monitoring. Furthermore, we find evidence that suggests the students of civil service teachers in schools with empowered parent committees performed better (particularly in math) than their counterparts in schools without empowered committees. This last result contrasts with those in a recent study, by Banerjee et al (forthcoming), in India. There, the school committees were also given training and instruction on what they could do to improve school functioning (including asking the administration to hire an extra teacher), and more generally, parents were informed on the status of education in the village, and told about what the school committee was, and what the school committee could do to improve the quality of education. The intervention did not lead to any improvement in learning. The difference between the India study and the present study may be due to the fact that, in the present case, the school committee was given a specific agenda.[15] These two results suggest once again the importance of a significant reform of the education system: paying lip service to parents' participation is not sufficient, if they are not given concrete means of being effective.

[15] In the Indian case, there was another treatment where volunteers were asked to organize reading classes outside of schools, which was another way to provide a concrete path of action. In that treatment, many people volunteered, and there was a significant improvement in test scores.

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Table 1
School and Class Characteristics, by Treatment Group, Pre- and Post-Program Inception

	Comparison Schools		ETP schools		*P-value* ETP = Control
Panel A. Baseline School Characteristics	Mean	SD	Mean	SD	
Total enrollment in 2004	598.3	*241*	592	*241*	0.854
Number of government teachers in 2004	12.4	*4.1*	11.9	*3.4*	0.331
School pupil/teacher ratio in 2004	48.7	*14.3*	50.8	*26.0*	0.524
Performance at national exam in 2004 (out of 400)	256.0	*24.4*	258.9	*24.4*	0.418
Panel B. Class Size Prior to Program Inception (March 2005)					
Average Enrollment in first grade	95	*41*	94	*36*	0.834
Proportion of female first grade students	0.51	*0.06*	0.49	*0.06*	0.040
Average Enrollment in second grade	97	*43*	98	*40*	0.838
Panel C. Class Size 6 Months After Program Inception (October 2005)					
Average class size in first grade	82	*27*	43	*16*	0.000
Panel D. Class Size in Year 2 of Program (March 2006)					
Average class size in second grade	68	*25*	42	*18*	0.000
Number of streams in second grade	1.39	*0.60*	2.29	*0.63*	0.000
Number of streams in first grade	1.03	*0.17*	1.00	*0.00*	0.047
Number of Schools	70		70		140

	Within ETP Schools				
	Section 1 (Assigned to Contract Teacher)		Section 2 (Assigned to Civil-Service Teacher)		*P-value* 1 = 2
Panel F. Comparability of two sections within Non-Tracking Schools					
Proportion Female	0.49	*0.06*	0.49	*0.06*	0.85
Average Age at Endline	9.08	*0.53*	9.00	*0.45*	0.38
Average Standardized Baseline Score (Mean 0, SD 1 at school level)	0.003	*0.10*	0.001	*0.11*	0.92
Average Standardized Endline Score (Mean 0, SD 1 at school level)	0.081	*0.20*	-0.074	*0.22*	0.00

Notes: School averages.

Table 2
Teacher Effort

		(1)	(2)	(3)	(4)	(5)	(6)
		Dep Var: Teacher Found in school on random school day		*Dep. Var:* If found in school, found in class teaching		*Dep. Var:* Teacher found in class teaching (unconditional on presence)	
(1)	ETP School	-0.009	-0.006	-0.151	-0.147	-0.132	-0.126
		(0.024)	(0.024)	(0.042)***	(0.044)***	(0.040)***	(0.042)***
(2)	ETP School x SBM	-0.016	-0.002	0.093	0.089	0.066	0.071
		(0.030)	(0.028)	(0.053)*	(0.054)	(0.045)	(0.046)
(3)	ETP School x Contract Teacher	0.024	0.037	0.302	0.345	0.270	0.308
		(0.036)	(0.044)	(0.041)***	(0.048)***	(0.039)***	(0.047)***
(4)	ETP School x Contract Teacher x SBM	0.092	0.086	-0.078	-0.070	0.010	0.011
		(0.048)*	(0.049)*	(0.065)	(0.067)	(0.059)	(0.065)
(8)	Years of Experience Teaching		0.002		0.002		0.003
			(0.001)*		(0.002)		(0.001)*
(9)	Female		0.008		0.063		0.057
			(0.022)		(0.029)**		(0.028)**
(10)	Constant	11.631	13.113	58.786	63.371	49.082	58.799
		(8.455)	(8.576)	(15.694)***	(14.254)***	(12.248)***	(11.522)***
(11)	Observations	2067	1921	1729	1622	2067	1921
(12)	R-Squared	0.01	0.01	0.07	0.08	0.06	0.07
(13)	Mean in non-ETP schools	0.842	0.849	0.690	0.686	0.582	0.582
(14)	Total Effect: ETP School+SBM	-0.025	-0.008	-0.058	-0.058	-0.066	-0.055
	p-val (ETP School + SBM = 0)	0.376	0.757	0.258	0.263	0.118	0.216
(15)	Total Effect: ETP School+Contract Teacher	0.015	0.031	0.151	0.198	0.138	0.182
	p-val (ETP School +Contact Teacher = 0)	0.658	0.431	0.001***	0***	0.002***	0.001***
(16)	Total Effect: ETP School+SBM+Contract Teacher	0.091	0.115	0.166	0.217	0.214	0.264
	p-val (ETP School +SBM +Contract Teacher = 0)	0.001***	0.002***	0***	0***	0***	0***

Notes: Linear probability model regressions. Multiple observations per teacher. Standard errors clustered at school level. Region and date of visit dummies included in all regressions but not shown. Years of experience missing for 146 observations (64 teachers).

Table 3
Student Attendance

		(1)	(2)	(3)
		Dep Var: Present at school on day of random visit		
(1)	ETP School	-0.002	-0.003	0.002
		(0.013)	(0.013)	(0.014)
(2)	ETP School x SBM	0.015	0.015	0.026
		(0.015)	(0.015)	(0.015)*
(3)	ETP School x Assigned to Contract Teacher	0.015	0.015	0.011
		(0.007)**	(0.007)**	(0.007)
(4)	ETP School x Assigned to Contract Teacher x SBM	-0.001	-0.001	-0.002
		(0.009)	(0.009)	(0.010)
(7)	Female		-0.006	-0.008
			(0.004)*	(0.004)**
(8)	Decile in initial attainment			0.005
				(0.001)***
(9)	Observations	52350	52308	43694
(10)	R-Squared	0.01	0.01	0.01
(11)	Mean in non-ETP schools	0.860	0.860	0.863
(12)	Total Effect: ETP School+SBM	0.013	0.012	0.028
	p-val (ETP School + SBM = 0)	0.312	0.309	0.035**
(13)	Total Effect: ETP School+Contract Teacher	0.013	0.012	0.013
	p-val (ETP School +Contact Teacher = 0)	0.333	0.338	0.317
(14)	Total Effect: ETP School+SBM+Contract Teacher	0.027	0.026	0.037
	p-val (ETP School +SBM +Contract Teacher = 0)	0.03**	0.03**	0.004***

Note: Multiple observations per pupil. Standard errors clustered at school level. Region and date of visit dummies included in all regressions but not shown. Gender is missing for 42 observations. Initial attainment information is missing for 14 schools, hence the decrease in the number of observations when initial attainment is added as a control.

Table 4
Average Treatment Effects on Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Students in ETP Schools								
	Students in Comparison Schools	All ETP Schools	Schools w/o SBM	Schools with SBM	Students Assigned to Civil-Service Teacher	Students Assigned to Contract Teacher	Boys	Girls	Students in Top Half of Initial Distribution	Students in Bottom Half of Initial Distribution
Panel A: Short-Run effects: Test Scores in November 2006 (after 18 months in program)										
Total Score	0.00	0.161	0.133	0.191	0.098	0.227	0.163	0.161	0.166	0.122
		(0.083)*	(0.101)	(0.098)*	(0.088)	(0.086)***	(0.081)**	(0.093)*	(0.096)*	(0.077)
Mathematics	0.00	0.166	0.125	0.210	0.095	0.241	0.170	0.163	0.161	0.131
		(0.070)**	(0.086)	(0.078)***	(0.073)	(0.076)***	(0.072)**	(0.077)**	(0.074)**	(0.073)*
Literacy	0.00	0.125	0.114	0.136	0.081	0.170	0.125	0.127	0.137	0.091
		(0.088)	(0.109)	(0.106)	(0.093)	(0.089)*	(0.085)	(0.100)	(0.112)	(0.076)
Observations	3308	3223	1656	1567	1646	1577	1749	1474	1366	1255
Panel B: Long-Run effects: Test Scores in November 2007 (a year after program ended)										
Total Score	0.00	0.040	-0.008	0.089	0.021	0.060	0.065	0.014	0.035	0.031
		(0.076)	(0.083)	(0.096)	(0.083)	(0.076)	(0.082)	(0.082)	(0.078)	(0.079)
Mathematics	0.00	0.042	-0.013	0.099	0.029	0.056	0.093	-0.011	0.024	0.060
		(0.062)	(0.070)	(0.073)	(0.068)	(0.062)	(0.066)	(0.069)	(0.060)	(0.072)
Literacy	0.00	0.031	-0.003	0.067	0.011	0.052	0.033	0.031	0.038	0.004
		(0.082)	(0.093)	(0.105)	(0.089)	(0.083)	(0.088)	(0.088)	(0.089)	(0.081)
Observations	3134	3037	1551	1486	1556	1481	1651	1386	1284	1185

Notes: The table presents the mean test scores in the comparison group (Column 1) and various treatment groups (Columns 2 to 6). Sample: Students enrolled in Grade 1 at program inception (March 2005). Program ended at the end of November 2006. Scores are normalized such that the mean and standard deviation of the comparison group (non-ETP schools) are zero and one, respectively. Robust standard errors clustered at the school level in parenthesis. There are only 139 schools/clusters because tests could not be administered in one of the ETP schools.

Table 5

Regressions Results: Test Scores after 18 months in program

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Total Score				Math Score				Literacy Score			
(1) ETP School	0.029	0.087	0.036	0.092	0.001	0.051	-0.022	0.052	0.048	0.102	0.081	0.110
	(0.104)	(0.098)	(0.107)	(0.092)	(0.083)	(0.078)	(0.080)	(0.076)	(0.114)	(0.109)	(0.123)	(0.100)
(2) ETP School x SBM	0.140	0.128	0.180	0.133	0.191	0.178	0.268	0.178	0.067	0.058	0.068	0.067
	(0.125)	(0.119)	(0.131)	(0.108)	(0.096)**	(0.091)*	(0.096)***	(0.086)**	(0.140)	(0.133)	(0.152)	(0.120)
(3) ETP School x Assigned to Contract Teacher	0.211	0.212	0.193	0.297	0.250	0.243	0.182	0.262	0.134	0.143	0.164	0.270
	(0.069)***	(0.065)***	(0.071)***	(0.124)**	(0.061)***	(0.059)***	(0.057)***	(0.108)**	(0.075)*	(0.069)**	(0.081)**	(0.130)**
(4) ETP School x SBM x Assigned to Contract Teacher	-0.166	-0.163	-0.144	-0.085	-0.213	-0.210	-0.182	-0.155	-0.092	-0.091	-0.083	-0.008
	(0.101)	(0.096)*	(0.099)	(0.105)	(0.098)**	(0.096)**	(0.087)**	(0.100)	(0.102)	(0.096)	(0.108)	(0.107)
(5) Teacher's Age				0.003				-0.001				0.006
				(0.007)				(0.006)				(0.007)
(6) Ratio of Female Among Teachers (*)				0.372				0.268				0.391
				(0.087)***				(0.079)***				(0.090)***
(7) School Size		0.011	0.008	-0.026		-0.162	-0.176	-0.192		0.160	0.166	0.125
		(0.094)	(0.104)	(0.087)		(0.075)**	(0.082)**	(0.070)***		(0.105)	(0.118)	(0.098)
(8) Girl		0.101	0.045	0.099		0.032	-0.029	0.030		0.141	0.100	0.139
		(0.028)***	(0.026)*	(0.028)***		(0.026)	(0.024)	(0.026)		(0.029)***	(0.029)***	(0.029)***
(9) Age		0.018	-0.054	0.017		0.055	-0.018	0.054		-0.018	-0.074	-0.018
		(0.012)	(0.013)***	(0.012)		(0.011)***	(0.011)	(0.010)***		(0.012)	(0.014)***	(0.012)
(10) Baseline score			0.503				0.501				0.405	
			(0.019)***				(0.015)***				(0.022)***	
(11) Observations	6531	6531	5569	6531	6531	6531	5569	6531	6534	6534	5572	6534
(12) R-Squared	0.010	0.040	0.260	0.050	0.010	0.040	0.270	0.050	0.000	0.030	0.170	0.050
(13) Total Effect: ETP School+SBM	0.169	0.215	0.216	0.225	0.192	0.229	0.246	0.230	0.115	0.160	0.149	0.177
p-val (ETP School + SBM = 0)	0.130	0.053*	0.064*	0.025**	0.034**	0.009***	0.007***	0.005***	0.336	0.180	0.249	0.104
(14) Total Effect: ETP School+Contract Teacher	0.240	0.299	0.229	0.389	0.251	0.294	0.160	0.314	0.182	0.245	0.245	0.380
p-val (ETP School +Contact Teacher = 0)	0.031**	0.005***	0.019**	0.011**	0.012**	0.002***	0.043**	0.021**	0.119	0.033**	0.029**	0.017**
(15) Total Effect: ETP School+SBM+Contract Teacher	0.214	0.264	0.265	0.437	0.229	0.262	0.246	0.337	0.157	0.212	0.230	0.439
p-val (ETP School +SBM +Contract Teacher = 0)	0.03**	0.007***	0.01**	0.002***	0.007***	0.001***	0.003***	0.009***	0.130	0.04**	0.047**	0.002***

Notes: OLS regressions. Scores are normalized such that the mean and standard deviation of the comparison group are zero and one, respectively. Standard errors clusters at school level There are only 139 schools/clusters because tests could not be administered in one of the ETP schools. Region and date of test dummies were included in all regressions but are not shown. Baseline score is missing for 19 schools, hence the decrease in the number of observations when basline score is controlled for in columns 3, 7 and 11.

(*) The mean of this variable is 0.67 in comparison schools and among students assigned to the civil service teacher in ETP schools. For students assigned to the contract teacher in ETP schools, the mean is 0.43.

Table 6

Regressions Results: Test Scores a year after program ended

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Total Score				Math Score				Literacy Score			
(1) ETP School	-0.062	-0.018	-0.044	-0.011	-0.060	-0.029	-0.083	-0.026	-0.053	-0.007	-0.007	0.002
	(0.090)	(0.090)	(0.091)	(0.087)	(0.079)	(0.079)	(0.079)	(0.080)	(0.098)	(0.096)	(0.099)	(0.090)
(2) ETP School x SBM	0.167	0.152	0.178	0.161	0.180	0.169	0.251	0.173	0.130	0.114	0.094	0.124
	(0.115)	(0.112)	(0.115)	(0.105)	(0.093)*	(0.090)*	(0.090)***	(0.089)*	(0.126)	(0.124)	(0.129)	(0.114)
(3) ETP School x Assigned to Contract Teacher	0.110	0.110	0.086	0.196	0.096	0.090	0.049	0.128	0.101	0.104	0.096	0.212
	(0.062)*	(0.060)*	(0.061)	(0.113)*	(0.058)	(0.059)	(0.057)	(0.100)	(0.061)	(0.058)*	(0.062)	(0.120)*
(4) ETP School x SBM x Assigned to Contract Teacher	-0.143	-0.134	-0.075	-0.076	-0.139	-0.135	-0.069	-0.094	-0.121	-0.110	-0.064	-0.049
	(0.088)	(0.086)	(0.079)	(0.095)	(0.082)*	(0.082)	(0.072)	(0.090)	(0.091)	(0.087)	(0.089)	(0.094)
(5) Teacher's Age				0.003				0.001				0.005
				(0.007)				(0.006)				(0.007)
(6) Ratio of Female Among Teachers (*)				0.308				0.222				0.322
				(0.085)***				(0.073)***				(0.089)***
(7) School Size		0.113	0.091	0.084		-0.050	-0.070	-0.073		0.216	0.196	0.187
		(0.100)	(0.104)	(0.095)		(0.074)	(0.075)	(0.072)		(0.112)*	(0.119)	(0.107)*
(8) Girl		0.151	0.113	0.148		0.077	0.042	0.075		0.181	0.147	0.178
		(0.030)***	(0.027)***	(0.030)***		(0.028)***	(0.027)	(0.028)***		(0.030)***	(0.029)***	(0.030)***
(9) Age		-0.005	-0.072	-0.006		0.026	-0.041	0.025		-0.027	-0.082	-0.028
		(0.011)	(0.013)***	(0.011)		(0.011)**	(0.012)***	(0.011)**		(0.011)**	(0.013)***	(0.011)**
(10) Baseline score			0.465				0.458				0.392	
			(0.016)***				(0.016)***				(0.016)***	
(11) Observations	6171	6171	5237	6171	6171	6171	5237	6171	6175	6175	5240	6175
(12) R-Squared	0.000	0.020	0.220	0.030	0.000	0.010	0.220	0.020	0.000	0.030	0.170	0.040
(13) Total Effect: ETP School+SBM	0.105	0.134	0.134	0.150	0.120	0.140	0.168	0.147	0.077	0.107	0.087	0.126
p-val (ETP School + SBM = 0)	0.339	0.225	0.215	0.143	0.156	0.095*	0.031**	0.061*	0.517	0.370	0.475	0.255
(14) Total Effect: ETP School+Contract Teacher	0.048	0.092	0.042	0.185	0.036	0.061	-0.034	0.102	0.048	0.097	0.089	0.214
p-val (ETP School +Contact Teacher = 0)	0.585	0.307	0.635	0.171	0.625	0.402	0.623	0.366	0.624	0.325	0.372	0.141
(15) Total Effect: ETP School+SBM+Contract Teacher	0.072	0.110	0.145	0.270	0.077	0.095	0.148	0.181	0.057	0.101	0.119	0.289
p-val (ETP School +SBM +Contract Teacher = 0)	0.431	0.238	0.134	0.049**	0.291	0.205	0.053*	0.114	0.568	0.306	0.259	0.053*

Notes: OLS regressions. Scores are normalized such that the mean and standard deviation of the comparison group are zero and one, respectively. Standard errors clusters at school level There are only 139 schools/clusters because tests could not be administered in one of the ETP schools. Region and date of test dummies were included in all regressions but are not shown. Baseline score is missing for 19 schools, hence the decrease in the number of observations when basline score is controlled for in columns 3, 7 and 11.

(*) The mean of this variable is 0.67 in comparison schools and among students assigned to the civil service teacher in ETP schools. For students assigned to the contract teacher in ETP schools, the mean is 0.43.

Appendix Table A1: Attrition

	At Endline Test (after 18 months in program)				At Long-Run Follow-up Test (a year after program ended)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Transferred to other school	Missed test	Total Attrition	Total Attrition	Total Attrition	Total Attrition
ETP School	-0.018	0.008	-0.011	-0.012	-0.012	-0.026
	(0.007)***	(0.023)	(0.025)	(0.025)	(0.025)	(0.032)
ETP School x SBM	0.003	-0.031	-0.029	-0.003	-0.003	0.014
	(0.010)	(0.024)	(0.027)	(0.033)	(0.033)	(0.037)
ETP School x Assigned to Contract Teacher	0.010	-0.049	-0.039	-0.014	-0.014	-0.026
	(0.009)	(0.020)**	(0.022)*	(0.027)	(0.027)	(0.038)
ETP School x Assigned to Contract Teacher x SBM	-0.007	0.051	0.044	-0.020	-0.020	0.025
	(0.015)	(0.029)*	(0.032)	(0.043)	(0.043)	(0.054)
Age	-0.008	-0.018	-0.026	-0.026	-0.026	-0.005
	(0.002)***	(0.003)***	(0.004)***	(0.004)***	(0.004)***	(0.004)
Girl	0.003	0.008	0.011	0.015	0.015	0.027
	(0.005)	(0.012)	(0.014)	(0.014)	(0.014)	(0.014)*
Girl x ETP School	-0.002	0.006	0.004	0.005	0.005	0.033
	(0.010)	(0.024)	(0.027)	(0.031)	(0.031)	(0.034)
Girl x ETP School x SBM	0.022	-0.003	0.019	0.003	0.003	-0.057
	(0.016)	(0.031)	(0.036)	(0.039)	(0.039)	(0.041)
Girl x ETP School x Assigned to Contract Teacher	-0.005	0.028	0.024	0.016	-0.055	-0.033
	(0.013)	(0.029)	(0.036)	(0.043)	(0.041)	(0.049)
Girl x ETP School x Assigned to Contract Teacher x SBM	-0.020	-0.029	-0.050	-0.025	0.030	0.001
	(0.024)	(0.043)	(0.050)	(0.056)	(0.053)	(0.064)
In bottom half of initial distribution				0.008		0.004
				(0.012)		(0.014)
In bottom half of initial distribution x ETP School				0.025		0.018
				(0.023)		(0.026)
In bottom half of initial distribution x ETP School x SBM				0.025		0.018
				(0.023)		(0.026)
In bottom half of initial distribution x ETP School x Contract Teacher				-0.049		0.048
				(0.029)*		(0.028)*
In bottom half of initial distribution x ETP School x Contract Teacher x SBM				0.122		-0.002
				(0.049)**		(0.047)
Constant	0.112	0.280	0.392	0.381	0.230	0.232
	(0.018)***	(0.033)***	(0.037)***	(0.041)***	(0.037)***	(0.040)***
Observations	7631	7631	7631	6776	7625	6772
Mean in Non-ETP Schools	0.052	0.132	0.184	0.182	0.228	0.231
BOYS:						
Total Effect: ETP School+SBM	-0.015	-0.023	-0.040		-0.015	
p-val (ETP School + SBM = 0)	0.052*	0.132	0.184		0.228	
Total Effect: ETP School+Contract Teacher	-0.008	-0.041	-0.050		-0.026	
p-val (ETP School +Contact Teacher = 0)	0.102	0.252	0.085*		0.342	
Total Effect: ETP School+SBM+Contract Teacher	-0.012	-0.021	-0.035		-0.049	
p-val (ETP School +SBM +Contract Teacher = 0)	0.335	0.071*	0.041**		0.445	
GIRLS:						
Total Effect: ETP School+SBM	0.005	-0.020	-0.017		-0.007	
p-val (ETP School + SBM = 0)	0.764	0.385	0.565		0.069*	
Total Effect: ETP School+Contract Teacher	-0.015	-0.007	-0.022		-0.076	
p-val (ETP School +Contact Teacher = 0)	0.132	0.745	0.359		0.143	
Total Effect: ETP School+SBM+Contract Teacher	-0.017	-0.019	-0.038		-0.066	
p-val (ETP School +SBM +Contract Teacher = 0)	0.035**	0.396	0.119		0.002***	
STUDENTS IN BOTTOM HALF OF INITIAL DISTRIBUTION:						
Total Effect: ETP School+SBM				0.019		-0.090
p-val (ETP School + SBM = 0)				0.049**		0.102
Total Effect: ETP School+Contract Teacher				-0.022		-0.056
p-val (ETP School +Contact Teacher = 0)				0.071*		0.664
Total Effect: ETP School+SBM+Contract Teacher				0.002		-0.085
p-val (ETP School +SBM +Contract Teacher = 0)				0.646		0.940

Notes: OLS Regressions; standard errors clustered at school level.