

# School Governance, Teacher Incentives, and Pupil-Teacher Ratios: Experimental Evidence from Kenyan Primary Schools

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## Abstract

We examine a program that enabled Parent-Teacher Associations (PTAs) in Kenya to hire novice teachers on short-term contracts, reducing class sizes in grade one from 82 to 44 on average. PTA teachers earned approximately one-quarter as much as teachers operating under central government civil-service institutions but were absent one day per week less and their students learned more. In the weak institutional environment we study, civil-service teachers responded to the program along two margins: first, they reduced their effort in response to the drop in the pupil-teacher ratio, and second, they influenced PTA committees to hire their relatives. Both effects reduced the educational impact of the program. A governance program that empowered parents within PTAs mitigated both effects. Better performing contract teachers are more likely to transition into civil-service positions and we estimate large potential dynamic benefits of contract teacher programs on the teacher workforce.

*JEL Codes:* I21, M51, O15

*Keywords:* contract teachers, PTA, School-Based Management, teacher effort, test score gain, nepotism, corruption, decentralization, institutions.

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## 1. Introduction

Social service delivery in developing countries is often of abysmally low quality. Patients frequently do not receive correct diagnosis and treatment (Amin, Das and Goldstein, 2007), and students score very low on internationally comparable tests (Hanushek and Woessman, 2010). Policymakers seeking to address this problem have focused on two leading explanations: limited resources and poorly functioning governance institutions, leading to weak provider incentives (World Bank, 2004).

Efforts to increase education resources typically focus on hiring more teachers to bring down pupil-teacher ratios. While several studies in developed country contexts have found that reducing class size can increase test scores, at least in the short run,<sup>1</sup> studies in lower-income countries consistently fail to find an effect of pupil-teacher ratio on test scores.<sup>2</sup> One hypothesis is that weak governance institutions in many developing countries distort teacher incentives in ways that undermine the impact of hiring additional teachers.

Whereas social service delivery in the United States and some other developed countries historically grew out of a system of locally-governed institutions (Goldin, 1999), many developing countries adopted centralized state control over social service delivery at independence. The appropriate governance roles of formal state institutions and local informal institutions in those countries have been hotly debated (Mamdani, 1996). High teacher and health care worker absence rates (Chaudhury et al., 2006) can be seen as evidence of limited central state capacity or capture by providers. Recently, many developing countries, including Kenya, India, and Indonesia, have changed their constitutions to decentralize authority and incorporate a role for locally elected bodies in addition to professionals from central ministries in service delivery.

In education, two widely-advocated reforms are hiring teachers locally, on short-term contracts, rather than centrally through a civil-service system, and School-Based Management (SBM). Both reforms have been implemented in many countries over the past decade.<sup>3</sup> While

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<sup>1</sup> For positive effects see Krueger and Whitmore (2002) in the US, Angrist and Lavy (1999) in Israel, Piketty (2004) in France. Hoxby (2000) finds no significant effect in the US.

<sup>2</sup> The lack of effect in lower-income countries has been observed across a range of geographic contexts, starting with very different initial levels of class size, and examining both civil service and NGO teacher. See Banerjee, Jacob, and Kremer (2005), Banerjee et al. (2007), and Urquiola and Verhoogen (2009).

<sup>3</sup> Contract teachers have been used in Benin, Burkina Faso, Cambodia, Cameroon, Chad, Congo, Côte d'Ivoire, Guinea, many Indian states, Indonesia, Madagascar, Mali, Nicaragua, Niger, Senegal, Togo, Mali, Niger, Togo, and

some applaud these trends, others are concerned that the abandonment of civil-service hiring rules may create the potential for capture by local elites and favoritism in hiring.

We examine these reforms in a Kenyan context. We find that locally hired contract teachers are much more likely to be present in class and teaching than civil-service teachers, and that their students learn more. Yet the potential learning impact of adding contract teachers to schools is not fully realized due to partial capture – not by traditional local elites, but rather by local agents of the state (civil-service teachers). Specifically, we find that civil-service teachers respond to the introduction of contract teacher programs by (1) reducing their own effort and (2) securing contract teacher positions for relatives. Both forms of capture can be mitigated and learning can improve, however, when contract teacher programs are combined with SBM reforms that promote parental involvement and control at the local level.

We examine a program in Kenya which provided funds for Parent-Teacher Association (PTA) committees to hire additional teachers and to create an additional section in grade one, reducing class size from 82 to 44 on average. The locally-hired contract teachers earned only about one quarter as much as existing teachers. Whereas existing teachers were civil servants employed by the central government, represented by a strong union, and under the authority of other professional educators, the new teachers were on short-term contracts and under the authority of PTA committees composed primarily of parents. The contract teachers were responsible for a particular class and stayed with that class over two years, rather than rotating in and out of classes to teach a particular subject. Students were randomly assigned to the class taught by existing civil-service teachers or that taught by the contract teacher.

In a (randomly selected) subset of schools participating in the contract teacher program, parents in the PTA committee received School-Based Management (SBM) training on how to interview and select job applicants, monitor and assess teachers' effort and performance, and perform a formal review of the contract teacher's performance to decide whether to renew her contract.

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other African countries (see Duthilleul, 2005, for a review, Kumar et al., 2005, and Chaudhury et al., 2006). For evaluations of such programs, see DeLaat and Vegas (2003) and Muralidharan and Sundararaman (2010). School-Based Management programs have been implemented in Argentina, Australia, Bangladesh, Canada, Guatemala, Honduras, Hong Kong, India, Lebanon, Lesotho, Macedonia, Madagascar, Mexico, Nicaragua, the Philippines, Senegal, Serbia, Sri Lanka, The Gambia, the United Kingdom, and the United States. See Blimpo and Evans (2011), Gertler et al. (2011), Banerjee et al. (2010), King and Ozler (1998), Ozler (2001), Kaestner and Gershberg (2002) and Bruns et al. (2011) for a review.

Overall, test scores after 18 months were 0.16 standard deviations higher in schools with contract teacher programs than in comparison schools. Locally-hired contract teachers were present in class far more often than civil-service teachers and students randomly assigned to contract teachers learned substantially more than those assigned to civil-service teachers.

In the absence of the SBM training program, existing civil-service teachers responded to the contract teacher program and the resulting change in pupil-teacher ratio in ways that reduced its effectiveness. First, they substantially reduced the rate at which they were in class. Despite the tremendous reduction in class size, pupils who remained with civil-service teachers in schools without SBM training saw only a small, insignificant increase in test scores. Second, there is evidence of rent-seeking on behalf of relatives. In the absence of SBM, 31% of those hired as contract teachers were relatives of existing civil-service teachers. Hiring of relatives was particularly common in schools where more existing civil-service teachers were male (and thus more likely to have relatives nearby, since Kenya is a patrilocal society).

SBM training for PTA committees mitigated both effects. Civil-service teachers in SBM schools were more likely to be present and teaching, and student test scores were correspondingly higher in their classes. Only 16% of those hired as contract teachers in SBM schools were teacher relatives, and there is evidence that teacher relatives hired in SBM schools were more likely to be present in class and that their students learned more.

Under some assumptions, we can use an instrumental variables approach based on experimental variation to separately estimate the roles of class size and teacher effort. We estimate that teacher effort has a large effect on test scores, and holding teacher effort constant, smaller classes moderately increase test scores. The class size effect we estimate is statistically significant, but five to ten times smaller than has been sometimes observed in developed countries.

These results suggest that endogenous behavioral responses under weak governance institutions may help explain why increases in resources alone have generally had disappointing effects on test scores in developing countries (see Kremer and Holla, 2009, for a review). If some of the additional resources are diverted by teachers, as we observe, or by parents as observed in Das et al. (2004, 2011), 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.

Our finding that resources have greater impact in a better governance environment is consistent with Lavy (2010), who finds greater productivity of instructional time in countries with greater school accountability and school autonomy in hiring and firing teachers. More generally, recent randomized evaluations of education find substantial similarity in consumer behavior across contexts but much more heterogeneity in program impacts on provider behavior (Kremer and Holla, 2009). Our results suggest that this may be due to interactions across different elements of the governance and provider incentive system. Complementarities between different components of the governance and incentive system and between this system and resources could help reconcile the very large differences in test scores across countries (on the order of a standard deviation) with the typically smaller estimated impacts of specific educational interventions.

Almost half of the contract teachers were eventually hired as permanent teachers by the civil service, and contract teachers whose students learned more were more likely to obtain positions as civil-service teachers. Our results should therefore be interpreted as shedding light on the potential impact of a system in which teachers are initially hired as contract teachers by local PTA committees before becoming eligible to be hired as civil servants.<sup>4</sup> Such a system has the potential not only to reduce salary costs and improve performance during the period of contract-teacher service, but also to improve learning in the long run, by improving the teacher workforce. With relatively conservative assumptions we estimate that if teachers started their careers by working for three years on contract for PTAs that received SBM training and then were hired into the civil-service according to the process observed over the duration of our study, the steady state gain in test scores for the education system as a whole would be 0.18 standard deviations.

The remainder of this paper is organized as follows: Section 2 provides background on central government and local community institutions for school governance in Kenya. Section 3 describes the Extra Teacher Program, the experimental design, and the data. Section 4 presents the impact of the program on test scores and teacher behavior, and uses the variation introduced by the program to separately estimate the impact of pupil-teacher ratios and teacher effort on test scores. Section 5 discusses the potential dynamic impact of contract teacher hiring on the teacher workforce, based on evidence about how contract teachers' performance affects their likelihood of being hired as civil-service teachers and how school committees select contract teachers. Section 6 draws conclusions and discusses evidence on related programs.

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<sup>4</sup> Murgai and Pritchett (2006) recommended that such a system should be adopted in India.

## 2. Education Governance in Kenya

As noted in the introduction, many developing countries adopted highly centralized education systems at much lower levels of development than did some of today's developed countries. Newly independent states adopted central control in part to rapidly expand education levels from a low base and in part as an element in nation-building, designed to unify diverse segments of society. Civil-service rules were seen as an important bulwark against politicization, ethnic favoritism, and nepotism in hiring and in favor of professionalism.<sup>5</sup>

Kenya has had an evolving mixture of local, informal education governance and governance by the central state, but over time the role of the central state has grown, while that of local institutions has shrunk. During the colonial period, churches and local communities started many independent schools. While the Harambee movement in the late 60s and 70s encouraged local communities to start schools and retained representation of parents, the local community, and founding churches on school committees, overall the post-independence government adopted a fairly centralized education system. Civil-service headmasters and teachers were sent to take over successful Harambee schools and school committees were *de facto* relegated to fundraising rather than teacher governance. The Ministry of Education sought to professionalize and standardize teacher training, hiring, and discipline. A strong teacher union strengthened the emphasis on formal educational qualifications and on accountability to professional norms rather than to local parents.

### 2.1 TSC and PTA teachers

Historically, Kenyan schools have had two types of teachers –those hired as civil servants through the Teachers Service Commission (TSC) of the Ministry of Education and Parent-Teacher Association (PTA) teachers hired locally and informally by local school committees. For civil-service teachers, who have long constituted the vast majority of teachers, promotion, transfers, and disciplinary measures are decided through the TSC, rather than by more locally accountable bodies. Hiring and promotions are based heavily on formal, objective criteria, such as educational qualifications and experience. These teachers are represented by a strong union,

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<sup>5</sup> Debates over community vs. central control of education have had an important ethnic dimension in the United States as well, for example with the racially-charged contention between community activists and the teachers union after New York State experimented with community control in the 1960s (Schiff, 1976).

have civil-service protection, and receive wages and benefits considerably above market-clearing levels. Disciplinary issues are addressed by the TSC through formal legalistic procedures with adjudication by panels dominated by professional educators. Graduates of teacher training colleges typically have to queue for civil-service jobs, often undergoing many years of unemployment before they are hired.

PTA teachers are hired by PTA committees at each school. PTA committees are primarily composed of parent representatives, elected for each class. Thus a particular parent member is responsible for representing the interest of parents of children in grade 1, another represents parents of children in grade 2, etc. School committees also include some additional *ex officio* members, including the headmaster, or a member appointed by the founding church. There is considerable variance in the *de facto* relative strength of the headmaster and parents in the PTA. Some school committees are dominated by the headmaster, but in others parents or other community members play a very influential role. Hiring and supervision of PTA teachers is quite informal, with much left to the discretion of the school committee or headmaster.

PTA teachers are typically paid much less than their TSC counterparts. In the area of study, in 2004, PTA teachers received compensation in the range of 2,000 Kenyan shillings, or US\$ 25 per month. In comparison, the average civil service teacher received around US\$ 120 per month plus benefits, including housing allowances, provisions for retirement, and medical coverage (Glewwe et al., 2010). PTA teachers are effectively at will employees of the school committee. Despite the low pay and lack of job security, PTA positions are actively sought after by unemployed teachers, in part because teaching experience helps them obtain formal civil service teaching position.

## **2.2 National Politics, Free Primary Education, and Local Governance**

In the Kenyan political context, national politics has been about assembling coalitions of local ethnic leaders, with politicians competing largely on their ability to deliver resources to their constituencies. National political leaders placed in charge of a Ministry therefore had more incentive to focus their energies on delivering resources to their home areas than on transferring value from teachers nationally to parents nationally, for example through enforcement of rules prohibiting unauthorized teacher absence. Their constituents would gain only a small fraction of any benefits from tougher enforcement. The shifting pattern of coalitions among local leaders

meant there was little discipline from a national political party interested in building a reputation for delivering services.

School committees were a means for a local role in school governance, but, as argued elsewhere (Kremer et al., 2003) their structure tilted towards representation of local elites, so they typically favored higher levels of fees and enforcement of rules requiring uniforms, for example than the median voter in Kenya would have preferred. There were also sometimes tensions between parent representatives and the often more educated teachers and headmasters, who sometimes resented having lower-status, less-educated PTA members.

With Kenya's first fully competitive election in 2002, politicians with national ambitions were incentivized to appeal directly to the typical voter rather than to rely as exclusively on the previous pattern of assembling coalitions of local leaders. They promised free primary education, thus appealing to voters of lower socio-economic status than typically serve on school committees.

After primary school fees were abolished in 2003, enrollment rose nearly 30 percent. Since parents were no longer required to pay fees, local school committees were generally unable to raise the funds necessary to hire PTA teachers. They received grants from the central government, but the grants had to be spent on physical inputs, not hiring teachers locally, so *de facto* the introduction of truly competitive national elections and free primary education strengthened the role of the central ministries in teacher governance at the expense of parents and local elites. Many parents exited the public system, turning to informal private schools, which have grown dramatically since the introduction of free primary education (Lucas and Mbiti, 2011). A survey we conducted with over 300 primary schools in 2004 showed that 80 percent had no locally-hired teachers. Average class size in first grade in 2005 was 83, and median class size was 74; 28 percent of grade 1 classes had more than 100 pupils.

### **3. The Extra Teacher Program and Study Design**

#### **3.1 Program Description**

##### ***Extra Teacher Program***

More than three-quarters of education spending in low-income countries goes to teachers, and many efforts to increase resources for education focus on hiring extra teachers to bring down class size or accommodate additional pupils. The Extra Teacher Program (ETP), implemented in



2005 and 2006, provided funds to school committees to hire PTA teachers to supplement TSC teachers teaching in lower grades. The program was implemented by the non-governmental organization International Child Support (ICS), with funding from the World Bank, in a random subset of schools in three districts of Western Kenya. ICS staff met with the headmaster, the civil-service teachers assigned to the lower grades, and the parents of students in grade 1 to explain how the program worked and ask if they wanted to participate. To qualify for ICS funding the PTA teachers hired under the ETP contract had to have the same academic qualifications as civil-service teachers, and schools participating in the program had to create an additional section in first grade and to randomly assign students and the ETP contract teacher across sections. (The randomization was carried out by ICS staff with support from the research team.) Other inputs such as classroom facilities, were also supposed to be equally allocated across sections.<sup>6</sup>

PTA teachers hired under ETP focused on a specific class (one of the sections in first grade), in contrast to TSC teachers who most often focused on a particular subject. This mirrors the organization of school committees and made it easier for parents to hold teachers accountable for absenteeism. For example, if a parent knows that their child was unsupervised during the day, they know who is responsible if a single teacher is assigned to the class. When the program continued the following school year, school committees were free to replace or keep the ETP contract teacher, but they were asked to move the teacher to second grade with the same group of students.<sup>7</sup>

The monthly allowance for teachers under the ETP program was 2,500 Kenyan shillings (around US\$35), putting it at the top of the range of what is typically paid to locally-hired contract teachers by school committees in Kenya, but at only one quarter of the typical TSC teachers' salaries, and a smaller fraction of their compensation, since civil-service teachers also receive benefits including not only pensions but also housing allowances.

The ETP program represents a shift in the balance between local parent control and formal professional control, central standards, and the role of the state, but it should be seen as augmenting, rather than replacing, the role of the central state. Eligibility for the program was

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<sup>6</sup> In practice, established civil-service teachers may have pulled rank and obtained better physical classroom infrastructure. We do not have data to estimate how common this was, but in any case, it would tend to bias downwards estimates of the contract teacher effect and bias upwards estimates of the effectiveness of class-size reductions.

<sup>7</sup> 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 teachers in 2006.

restricted to graduates of teacher training colleges, and ETP contract teachers reported to centrally appointed headmasters. As mentioned above, headmasters typically have considerable influence within school committees. Contract teachers hired under the ETP program were likely motivated in part by the prospect that they would eventually obtain civil-service jobs.

### ***School-Based Management Training (SBM)***

The School-Based Management (SBM) training, implemented by a trained ICS officer in a random subset of ETP schools, was designed to empower PTA committee members (in particular, the parents, as opposed to only the headmaster) to monitor teachers' performance. The training lasted about 90 minutes, and immediately followed the meeting that ICS held with parents to inform them of the Extra-Teacher Program. All parents were welcome to stay for the SBM training if they so wished. During the training, PTA committee members were encouraged to supervise the recruiting of the extra-teacher, taught how to conduct interviews, and taught techniques for soliciting input from other parents and checking teacher attendance. Two parents of grade 1 students were asked to volunteer to perform attendance checks on the teachers on a regular basis, and were given a notebook in which to record 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. Finally, the PTA committees were asked to hold a formal review meeting at the end of the first school year of the program to assess the contract teacher's performance and decide whether to renew the teacher's contract or replace the teacher. Although the SBM training focused on training parents to monitor the contract teacher, the advice it provided on how to measure teacher performance may have helped parents to notice, and act on, the greater absence issues among the civil-service teachers, to make sure the contract teacher was effectively used to increase resources available to grade 1 pupils, rather than to make life easier for the other teachers. The additional cost of implementing this training program was about \$100 per school (compared to around \$420 per year in salary costs for the contract teacher).

### ***Tracking***

A separate set of schools were chosen for an alternative ETP program. In those schools, the first-grade class was divided into two sections by initial achievement ("tracking"), and the ETP contract teacher was randomly assigned to one of these sections. This program is discussed in a

companion paper (Duflo et al. 2011), and data for the schools participating in that program are not part of the sample analyzed in this paper.

### 3.2 Experimental Design

Background data on enrollment, pupil-teacher ratio, and number of grade 1 sections was collected in 210 primary schools in Kenya's Western Province in 2004. Of these, 70 were randomly assigned to the tracking program and are not the focus of this paper. The remaining 140 schools were stratified by administrative division and number of grade 1 sections and randomly divided into a comparison group, and an Extra Teacher Program (ETP) group, each with 70 schools. Thirty four of the 70 ETP schools were randomly chosen to be offered the opportunity to participate in SBM training. ICS held its schools meetings to explain the program to parents and teachers in March 2005. All schools offered the opportunity to participate in the ETP and SBM programs chose to do so. School committees selected for the ETP program generally had teachers in place by early May 2005, the start of the second of the three school terms in Kenya's academic year.

Table 1 presents summary statistics. Panel A shows no significant differences in observable characteristics at baseline. Panel B suggests that the program was successful at reducing pupil-teacher ratios over two years. While average class size in grade 1 in 2005 in the comparison schools was 82, it was only 44 in ETP schools.<sup>8</sup> This class size gap was somewhat attenuated the following year in second grade: in 2006, average class size in second grade was 68 in comparison schools and 42 in ETP schools.<sup>9</sup>

Students typically attended their assigned section when both teachers were present, but when teachers were absent, sections were sometimes combined. On average across four unannounced school visits, 94% of students were found in their assigned section (conditional on the two sections being taught separately), and there was no significant difference between schools with and without SBM training. Sections were found pooled in only 10.7% of visits. The rate at which classes were pooled was not affected by the SBM training.<sup>10</sup>

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<sup>8</sup> Class size did not fall by exactly 50% since some schools already had two Grade 1 sections at baseline. In these schools, the introduction of the Extra Teacher Program reduced class size by only one third.

<sup>9</sup> The repetition rate is relatively high in Kenya. In year 2 of the program, the population in grade 2 thus included both those sampled students who had been promoted and did not transfer to another school, and grade 2 repeaters.

<sup>10</sup> Analysis of the determinants of class pooling shows that absence of TSC teachers is a main driver of pooling, but contract teacher absence is not. This suggests that contract teachers covered for TSC teachers when TSC teachers were absent, but TSC teachers did not cover for contract teachers when those were absent.

### **3.3 Study Sample**

Summary statistics about the students and teachers in the study are shown in Table 1. The student sample includes a cohort of approximately 13,500 children enrolled in first grade at the end of the first school term of 2005. Students averaged 7.7 years old at the outset of the program (with a standard deviation of 0.5 years), but ages ranged from 5 to 14. Baseline data on students' initial achievement was collected from the school records at the onset of the study. This data is not based on a common scale across schools, and thus can only be used for within-school comparisons. Random assignment of students to teachers in ETP schools led to sections with comparable average baseline scores.

A total of 754 teachers taught lessons in first grade in 2005, in second grade in 2006, or both. Of those, 653 were centrally hired civil-service teachers, while 101 contract teachers were hired through the program by the 70 ETP schools over the five school terms (corresponding to 15 months of instructions) it operated. Average age was 43 for civil-service teachers and 27 for contract teachers, and contract teachers had 13 fewer years of experience than their TSC counterparts on average. Contract teachers were also much less likely to be females than TSC teachers in our sample (48% versus 69%). This is in part due to the fact that TSC teachers in our sample are those assigned to lower grades, and female teachers are disproportionately assigned to lower grades. The share of females in the entire TSC teacher body is on average 55% in our sample of schools, statistically indistinguishable from the percentage among contract teachers.

### **3.4 Data**

#### ***Test Scores and Attrition***

Standardized tests covering math and literacy questions ranging from identifying numbers and letters to subtracting two-digit numbers and writing words were administered in all schools after 5 school terms (19 months total, but only 15 months of instruction), just before the program ended (November 2006). The same tests were administered again during a follow-up one year after the program ended (November 2007). Tests were administered by trained enumerators and graded blindly by a separate team of enumerators. In each school, 60 students were randomly drawn from the initial sample to participate in the tests.

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. Not all

students were found and tested, however. Overall, the attrition rate was 18 percent for the endline test, and 23 percent for the long-run follow-up test.

Appendix Table A1 presents estimates of attrition by treatment groups. At endline, attrition was lower in ETP schools, especially for students assigned to the contract teachers. Low achievement students were significantly less likely to attrit from SBM schools, and less likely to attrit if they had been assigned to the contract teacher (although surprisingly if they were assigned to the contract teacher in an SBM school, they were not significantly less likely to attrit). This suggests that the differential attrition will bias downwards the estimates of the impact of SBM on students assigned to regular teachers, and bias downwards the estimates of the short-run impact of contract teachers in non-SBM schools. The interaction between being in an SBM school and being taught by the contract teacher led to significantly lower attrition among higher-achieving students, however. Estimates of the effect of the combination of SBM and contract teacher may thus be upward biased. The patterns of attrition are roughly similar in the long-run follow-up. Given the substantial evidence of differential attrition, when estimating impacts on test scores below we estimate lower bounds correcting for attrition.

### ***Teacher Effort***

Over the course of the program, four unannounced school visits were made by the research team to measure teacher effort. Teacher effort was measured by the teacher's presence in school and by whether she was in class teaching when the observers entered the school compound. Data on pedagogy was collected through classroom observations and structured interviews with teachers.

### ***Hiring, Retention, and Regularization of Contract Teachers***

In the school term that followed the start of the program, data was collected on the contract-teachers hired through the program, including their demographic characteristics, past experience, relationship to the school, and the hiring process. Demographic data was also collected on civil-service teachers. In the school term that followed the end of the program, headmasters of program schools were interviewed about the status of the contract-teacher, and whether the school committee had taken over the extra-teacher program after the ICS subsidy ended.

## 4. Results: Program Impacts and Interpretation

This section estimates the program impacts on pupil test scores (Subsection 4.1) and teacher behavior (Subsection 4.2). Subsection 4.3 reports the results of an instrumental variables analysis designed to separately estimate the impact of pupil-teacher ratios and teacher effort on learning.

### 4.1 Learning Outcomes

We use the following reduced form equation to estimate the effects of each program component on student outcomes:

$$Y_{ij} = a_1ETP_j + a_2ETP_j \times SBM_j + a_3ETP_j \times CT_{ij} + a_4ETP_j \times CT_{ij} \times SBM_j + X_{ij}'a_5 + \varepsilon_{ijl} \quad (1)$$

where  $Y_{ij}$  is the endline test score of student  $i$  in school  $j$  (expressed in standard deviations of the distribution of scores in the comparison schools),<sup>11</sup>  $ETP_j$  is a dummy equal to 1 if school  $j$  participated in the Extra Teacher Program,  $SBM_j$  is a dummy equal to 1 if school  $j$  participated in the School-Based Management training,  $CT_{ij}$  is a dummy equal to 1 if student  $i$  was assigned to the contract teacher, and  $X_{ij}$  is a vector of student and teacher control variables.

The results are presented in Table 2. We present the results of three specifications that vary in the content of the  $X_{ij}$  vector. The first specification includes no controls at all. The second specification includes the following school-level controls: school size, the share of female among TSC teachers in lower grades and the average experience of TSC teachers in lower grades; and the following student-level controls: gender, standardized test score at baseline, age at the time of the test, and date of the test. Finally, the third specification further controls for whether the teacher is “novice,” that is, has less than 1 year of experience (this is the case for less than 1% of the civil service teachers and 24% of the contract teachers). Of course, this contract teacher characteristic is endogenous to the program, so for some purposes readers may prefer to focus on specifications without this control. However, for other purposes, results should account for teacher experience. For example, if one believes that novice teachers typically have lower value-added, as is consistent with a range of previous studies (Kane et al., 2008; Rockoff and Staiger, 2010), and considering that all teachers must spend a year as novices under any institutional arrangements, then estimates controlling for novice status may be appropriate. We present both sets of estimates and find little difference. Appendix Table A2 shows the estimated lower bounds for these specifications accounting for attrition, using the approach first proposed in Lee (2008).

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<sup>11</sup> An alternative specification of the endline test score for math, using item response theory, yields similar results (available from the authors). The format of the language score was not appropriate for this exercise.

In equation (1),  $a_1$  is the effect of being in a reduced pupil-teacher ratio environment, for students assigned to civil-service teachers in non-SBM schools. This effect is small (0.074 standard deviations in column 2 of Table 2, the specification with the full set of control variables) and insignificantly different from zero (standard error: 0.088). The 95% confidence interval excludes gains of even one-quarter of a standard deviation for a decline in class size of almost 50%.

The coefficient  $a_2$  is the difference between test scores of students in SBM and non-SBM schools for students assigned to the civil-service teachers in ETP schools. The difference is sizeable: students assigned to the civil-service teacher score approximately 0.13 standard deviations higher in SBM schools, relative to non-SBM schools. The difference is not significant for overall test scores, but is significant at the 10% level for math at approximately 0.18 standard deviations. This very cheap intervention more than doubles the effectiveness of the Extra Teacher Program, and makes it significantly positive, controlling for teacher experience: overall, students assigned to the civil-service teachers in schools that receive the ETP program with the SBM training do significantly better (0.204 standard deviations with a p-value of 0.036, Table 2 column 2, row 14) than students in comparison schools. This result appears relatively robust to controlling for attrition, even with strict assumptions: the lower bound estimate for this effect, presented in column 1, row 14 of Table A2, is 0.143 (with a p-value of 0.129).

Coefficient  $a_3$  in equation (1) is the difference between test scores of students assigned to civil-service teachers and those assigned to contract teachers, in schools without SBM. Students' scores are 0.228 standard deviations higher if they are assigned to the contract teacher rather than to civil-service teachers (Table 2, column 2, row 3). The lower bound of this difference estimated in Table A2 remains high and significant at conventional levels. The SBM training program partly bridges the gap between children assigned to the civil-service teachers and those assigned to the contract teacher: the difference is lower in SBM schools (coefficient  $a_4$ , Table 2, row 4).

Since all teachers will have to be novices for exactly one year of their careers, in calculating the benefit of the PTA contract teacher program, it is conceptually appropriate to net out the novice teacher effect. As noted above, roughly one-quarter of contract teachers are novices, and when one controls for this, the estimated contract teacher effect grows to 0.241 standard deviations (row 3, column 3). The coefficient estimate on the novice teacher dummy is -0.065 with a standard error of 0.091 (Table 2, column 3, row 11). Of course, the coefficient on novice

teachers in the regression is based on non-experimental variation and may not represent the causal impact of being a novice teacher, so this estimate should be interpreted with caution.<sup>12</sup>

The breakdown by subjects in columns 4 to 7 suggests that all the effects are stronger in math than in literacy. Both the SBM effect and the contract teacher effect are more than double for math than for literacy.

Appendix Table A3 presents the long-run impacts. A year after the program ended, program effects remain significant only for students in schools with SBM training. The fact that fadeout is less pronounced among this subgroup of schools is consistent with the fact that, as we will discuss below in section 5.2, schools with SBM training were significantly more likely to retain the contract teacher over the year that followed the end of the subsidized ETP program. The long-run effect on test scores in SBM schools is only significant at the 10% level for math, and it loses significance when we estimate the lower bound correcting for attrition.

## **4.2 Teacher Effort and Pedagogy**

Table 3 estimates program impact on teacher effort and on pedagogy using data from unannounced spot checks; classroom observations conducted with a subset of teachers; and surveys administered to teachers.

Contract teachers were 27.8 percentage points more likely to be found in a classroom and teaching during a random visit than civil-service teachers in comparison schools (the mean for the civil-service teachers in the comparison group was 57.9%, see Table 3, column 1, row 10). Civil-service teachers in ETP schools were 12.9 percentage points less likely to be found in class teaching during a school visit than their counterparts in comparison schools. This effect is significant at the 1% level (Table 3, Column 1) and corresponds to a 22 percent decrease in teacher presence in class compared to comparison schools. This suggests that civil-service teachers took advantage of the presence of the contract teachers to work less. Contract teachers might not have been in a position to refuse teaching combined classes when civil-service teachers were absent.

There is evidence that this reduction in effort by civil-service teachers was largely mitigated through community empowerment, however. Civil-service teachers in schools that received the ETP program with SBM training were 7.5 percentage points more likely to be found in class

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<sup>12</sup> We note however that our point estimate of -0.065 is very similar to those observed in the US. Rockoff and Staiger (2010) estimate novice effects of -0.07 in Los Angeles and -0.06 to -0.08 in New York City.



teaching than those in schools that received the ETP program alone (Table 3, Row 2, Column 1; the standard error is 0.043). It seems likely that SBM directly affected civil-service teachers, rather than influencing them by reducing the likelihood PTA teachers would cover classes for them, since we find no effect of SBM training on the likelihood that contract teachers were covering classes for civil-service teachers during unscheduled school visits (results available upon request).

The ETP program did not affect teacher presence on the school compound, but rather the likelihood that teachers were in class teaching, rather than, for example, in the staffroom drinking tea with other teachers or reading a newspaper (Table 3, Columns 2 and 3). Note that it seems easier for teachers to adjust on this margin, but on the other hand, it seems more difficult for parents to observe and enforce along this margin.

We measured changes in pedagogy through direct observation of lessons; and structured interviews with teachers about classroom processes. Students in ETP schools were more likely to be called to the blackboard and to be called on to answer questions. Teachers in those schools (whether civil servants or contract teachers) also report having more time to provide individual attention to children. However, SBM training did not seem to affect our measures of pedagogy (Table 3, Columns 4 to 9.) A smaller share of students of novice teachers report answering questions and novice teachers report difficulty paying attention to slow learners.

### **4.3 Interpreting the test scores gains: the role of class size and teacher effort**

While the estimates in section 4.1 and 4.2 are interesting in their own right, under certain assumptions, they can also be combined to disentangle the possible gains of decreasing class size (keeping effort constant) and the impact of effort (proxied by teacher's time in class). To do this, we take advantage of the difference between weakly incentivized civil-service teachers and more strongly incentivized PTA teachers, as well as the SBM program, which changed teacher effort but not the pupil-teacher ratio.

We are interested in estimating the following equation:

$$Y_{ij} = b_1 PTR_{ij} + b_2 E_{ij} + X_{ij}' b_3 + S_j' b_4 + \varepsilon_{ij3} \quad (3)$$

in which  $PTR_{ij}$  is the pupil-teacher ratio faced by student  $i$  in school  $j$  and  $E_{ij}$  is the average effort level of student  $i$ 's teachers in school  $j$ .  $X_{ij}$  is a vector of students' characteristics and  $S_j$  is a vector of school characteristics.

We consider two different IV estimates, with different exclusion restrictions. None of these exclusion restrictions are perfect, so the exercise is more illustrative than absolutely definitive.

First, we assume that, conditional on teacher's sex and experience (which are observed), the only difference between contract teachers and civil-service teachers is that contract teachers have stronger incentives. Under this assumption, the design provides four instruments for  $PTR$  and  $E$ : whether the student is in an ETP school, whether the student is in an SBM school, whether the student was assigned to a contract teacher, and the interaction between being assigned to a contract teacher and being in an SBM school. The first stage equations are:<sup>13</sup>

$$E_{ij} = c_1ETP_j + c_2ETP_j \times SBM_j + c_3ETP_j \times CT_{ij} + c_4ETP_j \times CT_{ij} \times SBM_j + \varepsilon_{ij4} \quad (4)$$

$$PTR_{ij} = d_1ETP_j + d_2ETP_j \times SBM_j + d_3ETP_j \times CT_{ij} + d_4ETP_j \times CT_{ij} \times SBM_j + \varepsilon_{ij5} \quad (5)$$

The second IV estimate relaxes the assumption that contract teachers and civil-service teachers affect students differently only due to their differences in experience, sex, and effort. Relaxing this assumption is important since contract teachers were instructed to spend all day with one specific section/class, and to teach all subjects to this class, while in most schools civil-service teachers rotate across sections/classes, teaching one subject to many classes. This could change teacher effort, for example if teachers focused on a single grade are more accountable or develop stronger bonds with their pupils. This is not a problem for the IV estimation. However, if there are pedagogical effects of being taught by a single teacher throughout the day, holding teacher effort constant, this would violate the exclusion restriction for “assigned to contract teacher” as an instrument for teacher effort.<sup>14</sup>

Due to the variation in teacher effort induced by the SBM program and its interaction with other variables, equation (3) is still identified if we control in addition for a dummy for whether the teacher is a contract teacher: we still have three instruments for two endogenous variables. We thus also estimate a version of equation (3) which controls for whether the teacher is a contract teacher:

$$Y_{ij} = b_1PTR_j + b_2E_j + X_{ij}'b_3 + S_j'b_4 + b_5CT_{ij} + \varepsilon_{ij7} \quad (6)$$

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<sup>13</sup> These equations are estimated in Appendix Table A4. The results on teacher's effort were already discussed in the previous section. The class size results are unsurprising: by design, class size is much lower in ETP schools, by 39 students on average. Students in schools without the School-Based Management program who were assigned to contract teachers ended up in slightly larger classes than students assigned to the civil-service teachers (this is due to chance, as teachers were randomly assigned to each section).

<sup>14</sup> Evidence shown in Table 3 and discussed earlier suggests that contract teachers exert more effort than civil service teachers along other dimensions besides class attendance. In particular, contract teachers were more likely to have a lesson plan prepared and to help students individually. This will not violate the exclusion restriction as long as class attendance serves as a good overall proxy for teacher effort.

In practice, power is lower since the effort differences due to SBM are smaller than those due to the teacher's contract structure. In particular, for contract teachers, SBM only impacted effort if the teacher was related to a TSC teacher.

The exclusion restriction for SBM as an instrument for effort would itself be violated if SBM affected test scores through channels other than teacher effort, for example by leading teachers to adopt a more interactive approach with pupils, or through a different selection process for those teachers (we discuss this possibility in more details below). The data on pedagogy presented in Table 3 and discussed earlier provides little evidence that the program led to such effects. SBM (alone) had no significant effect on any of the classroom dynamics variables. Specifically, teachers in SBM schools were not more likely to call students to the blackboard or to encourage students to ask questions than teachers in other ETP schools. Thus, while SBM significantly increased teachers' presence in the classroom, it did not affect what teachers did once they were in the classroom.

Table 4 presents estimates of the first instrumental variable equation, equation (3) for overall scores (Columns 1, 2 and 3), math (Columns 5, 6, 7) and language (Columns 9, 10 and 11), each using different control variables included in the vector  $S$ . The results are similar across specifications. Overall, test scores would increase by 0.042 to 0.064 standard deviations with a 10-student reduction in class size. Since the class size reduction we observe was close to 40 pupils, that means that, if civil-service teachers had not changed their effort in response to the program, the ETP Program would have increased test scores by 0.17 to 0.26 standard deviations. While significant, these results are fairly modest: they suggest a per-pupil effect that is about 3 times smaller than the effect found for Israeli 4<sup>th</sup> graders by Angrist and Lavy (1999), and about 8 times smaller than the per-pupil effect implied by the 8-student reduction in the Tennessee Star experiment (Krueger and Whitmore, 2002). However, our results are consistent with other results suggesting fairly modest (or no) impact of class size reductions in developing countries. This may be in part because classes, in our context as in those, remained fairly large (the reduced class size still had 44 students on average), or because weak governance hindered changes in pedagogy that lower class size may enable in developed countries.<sup>15</sup>

Results in columns 5-7 and 9-11 of Table 4 also suggest that teacher effort has a strong effect on learning: a 10 percentage point decrease in teacher absence from class would increase

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<sup>15</sup> In appendix Table A5, we present the coefficients for equation (3) estimated separately for schools with classes below and above the median size (76 pupils). The coefficient estimates for the effect of class size is twice as large in small classes as in big classes, but the standard errors are large and we cannot reject the null of equality.

test scores by 0.09 to 0.12 standard deviations in math, and 0.05 to 0.14 standard deviations in language (0.08 to 0.14 overall, columns 1-3). All these estimates are significant. Interestingly, these estimates are virtually identical to the estimated effect of a reduction in absence reported by Duflo, Hanna, and Ryan (2009).

These results are overall unchanged when we use our second IV strategy, which does not use contract teacher assignment as an excluded instrument. The point estimate for the effect of a 10-pupil reduction in class size is 0.068, very close to the 0.067 in column 3. The estimate of the impact of effort becomes noisier, however. This is not because the estimate drops in magnitude (in fact, it increases somewhat), but because the standard error increases. The point estimate suggests that a 10 percentage point increase in teacher presence would increase overall test score by 0.16 standard deviations (standard error: 0.16). We cannot reject equality between the parameter estimates with and without controlling for assignment to the ETP teacher (e.g., between columns 3 and 4), although given the noise in the estimate, this test has low power.<sup>16</sup> Another way to state this result is that the over-identification tests for the specification in the columns that are actually over-identified never rejects the validity of the joint set of instruments. Not surprisingly given these results, the point estimate of the direct contract teacher effect conditional on effort is actually negative (although small and insignificant). Overall, this specification, combined with the over-identification test in columns 1-3, gives us some confidence that ignoring other effects of the ETP contract teacher dummy does not lead us to over-estimate the importance of either teacher effort or class size. We find that both matter, although class size itself has a lower impact than estimated in other contexts. Going back to the main results in section 4.1, the reason the class size effect was muted for students assigned to civil-service teachers seems to be that teachers took advantage of the presence of the contract teacher to work less.

#### **4.4 Discussion**

The results discussed above imply that under reasonable assumptions, providing funds to PTAs to hire contract teachers is likely to be much more cost effective than hiring of additional civil-service teachers by the central government. In particular, given the difference in compensation costs between civil-service and PTA contract teachers, and under the assumption

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<sup>16</sup> We reproduce this analysis in Table A6, adding observations from the schools where the Extra Teacher Program was implemented along with tracking of students by initial achievement. While this increases the sample size by about 30%, it unfortunately does not increase the precision of the estimates.

that civil-service teachers would react to an increase in the pupil-teacher ratio from the hiring of an additional civil-service teacher in the same way that they react to the increase in the pupil-teacher ratio from adding a contract teacher, our point estimates imply that the government would need to spend ten times as much on hiring civil-service teachers as on contract teachers to achieve the same test score results. Combining these results with those of a companion paper (Duflo et al. 2011) suggests that if the government spent \$1 on hiring additional contract teachers, implementing SBM training, and tracking students by initial achievement, it would generate as much of a test score gain as spending \$13 on hiring additional teachers under existing institutions. (Section 5 argues that there may also be large dynamic gains from improving the long-run quality of the teacher workforce, but the calculations above exclude those potential dynamic gains.)

Several caveats are worth noting, however. First, while we see relatively large learning effects in the years contract teachers are present, these effects appear to fade out once students are reassigned to regular classes. One year after the program ended and students had gone back to being taught by civil service teachers in large classes, most of the effects were no longer statistically significant. Such fade-out is not out of line with the decay in test scores observed for other early interventions, including India (e.g. Banerjee et al, 2007) and the US (see Cascio and Staiger, 2011, for a review), and with rates of fade out estimated in developing countries (Andrabi et al. (2011) find that only fifth to a half of learning persists between grades), but contrasts with the more persistent effects of tracking that we observed in the same context (Duflo et al, 2011). Also note that decay in test score effects does not necessarily imply a comparable decay in non-cognitive impacts (see Chetty et al. (2010) for evidence of long-run effects on earnings of an early intervention that had only short-run effects on test scores). Note that if there were no long-run effect, hiring teachers on a contract basis would still be attractive on cost grounds relative to the alternative of hiring an equivalent number of civil-service teachers.

It should also be noted that, while we find positive and significant effects of SBM training on test scores in the context of the main ETP program, the estimated effects were smaller and not statistically significant in the tracking program (results are available upon request). This may merely reflect the more limited scope for improvement under that program: the tracking program already increases teacher effort (at least in the top track), probably leaving little scope to further increase effort and test scores.

Finally, our results should be interpreted as the impact of supplementing a civil service system with locally-hired contract teachers, brought in on a probationary basis, with the expectation of eventually obtaining a civil-service position, not as the impact of replacing a civil service teacher system with a system based entirely on local hiring of contract teachers. The contract teachers we study likely worked hard in part because they believed this would help them obtain a civil service position, and, as discussed in the next section, about half of the contract teachers in our study eventually obtained civil-service positions by the end of our study period, with better-performing teachers more likely to obtain these positions.

## **5. Dynamic Effects of Local Contract Teacher Hiring**

The results in section 4 suggest that in the short run, moving from a system of education in which teachers are immediately given tenure and managed solely through centralized state-run institutions, to a system in which teachers are initially hired locally on temporary contracts before becoming eligible for TSC positions and local PTAs are trained in how to recruit and monitor teachers, would not only save roughly 75% on teacher salaries during this initial phase but also significantly strengthen incentives for teachers and produce better learning outcomes.

However, the long-run impact of adding a new phase to the career of teachers in which they are hired locally on short-run contracts before obtaining a civil-service positions depends not only on the relative performance of civil-service teachers and contract teachers at a given point in time, but also on how local hiring of contract teachers affects the teacher workforce over time. Understanding this requires understanding which contract teachers transition to civil-service jobs and how PTA committees select and retain contract teachers. One concern is that decentralization may lead to nepotism – for example, Durante et al. (2011) find that a 1998 reform that increased autonomy by local university officials in Italy resulted, in some areas, in a significant increase in the incidence of nepotism towards relatives.

In this section, we first present evidence that contract teachers whose students performed well were more likely to be hired as civil-service teachers, suggesting potential dynamic benefits from a local contract teacher hiring program. However, maximizing these benefits requires selecting the best contract teachers. Teachers on PTA hiring committees may wish to transfer rents toward relatives by offering them contract teacher positions, which may lead to civil-service positions paid above market-clearing wages; those relative may perform less well once

they have obtained a position, both because of weaker selection and because of weaker incentives. Providing these rents is relatively cheap. Since the central government typically posts new civil-service hires to schools other than the school where they worked as contract teachers, schools hiring contract teachers do not bear the long-run consequences of hiring relatives with low motivation or talent for teaching. In fact, many PTA committees hired relatives of existing teachers, particularly in schools with a high proportion of male teachers. These relatives were present and teaching less often than other contract teachers, and their students scored lower on tests. Point estimates suggest that SBM training, which was designed to empower parents, reduced hiring of teachers' relatives, and that those relatives hired under SBM were present and teaching in class more often and their students learned more, compared to teacher relatives hired without SBM.

### **5.1 Potential Dynamic Impact of Contract Teachers on Teacher Workforce**

To see the potential of contract teacher programs to channel good teachers into civil-service positions, note that by the beginning of year 3, the year after the program ended, 47% of the initial contract teachers had been hired by the TSC (Table 5, Column 7).<sup>17</sup> Experience (a proxy for cohort) was an important driver of TSC hiring. But holding experience and school quality constant, contract teachers whose students had good scores were more likely to be hired by the TSC. A one standard deviation increase in student performance increases the likelihood that the ETP teacher was absorbed by the TSC by 34 percentage points (Table 5, column 7). Students of contract teachers hired by the TSC scored 0.32 standard deviations greater than their counterparts who had teachers who were not hired by the TSC.<sup>18</sup> This 0.32 standard deviations can plausibly be considered a lower bound on the difference in teacher value added between those hired by the TSC and those not hired, since students of teachers hired by the TSC presumably experienced some disruption from changing teachers and the replacement teachers were presumably typically of lower skill than those hired by the TSC.<sup>19</sup>

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<sup>17</sup> Data on the contract teacher are missing for 2 of 70 program schools.

<sup>18</sup> This estimate was obtained through an OLS regression run on the subset of students assigned to the ETP contact teacher. We regress students' endline score on a dummy equal to 1 if the ETP teacher was hired by TSC within 2 years of the ETP program start, controlling for the same student and school level controls as in column 3 of Table 2. The coefficient is 0.32 and the standard error is 0.11.

<sup>19</sup> Among contract teachers hired by the TSC, 40% were hired in the course of their first year as ETP teachers, 35% were hired in the course of their second ETP year, and 25% were hired right after they completed their second ETP year. This means that we have little power to examine the difference in test scores between students of teachers who

It seems likely that benchmark competition between contract teachers and existing civil service teachers was responsible for the tendency for teachers with better scores to obtain TSC positions. Indeed, there was no relationship between student test scores and teacher hiring by the TSC in the schools where students were tracked by initial performance, which presumably made it more difficult to measure contract teacher performance.

Gordon, Kane, and Staiger (2006) estimate that in Los Angeles, dropping the bottom quartile of teachers after their first year of teaching would increase the average impact of retained teachers by 1.5 percentile points. This would be partially offset by the need to increase the flow of new teachers into the system and thus the increased proportion of novice teachers, so the net increase in student test score gains would be 1.2 percentile points per year. In our context, the potential benefits of a system in which new teachers initially work as contract teachers are far greater, for two reasons. First, the gap in teacher effectiveness appears larger – as discussed above, the gap we observe in our data between the students of the roughly 50% of ETP teachers that were hired into the TSC and the students of those not hired is 0.32 standard deviations, even though performance was not the only criterion used by TSC when hiring. Second, the negative novice teacher effect is counteracted by a positive contract teacher effect, as seen in Section 4.

To get a sense of how big the total effect might be, suppose teachers work for 27 years. (The standard retirement age for civil servants in Kenya is 55 and contract teachers in our sample are 27 years old on average.) If new entrants to the profession worked three years as a contract teacher and half were subsequently hired as civil service teachers, then 20% of the teaching force would be made up of contract teachers in steady state. The proportion of novices in the system would double from 1 in 30 to 2 in 30, bringing down average scores by  $0.065/30$  or 0.0022 standard deviations. Assuming that students of experienced contract teachers score 0.275 standard deviations more than students of their civil-service teacher counterparts (Column 3 of Table 4, row 16), this effect would boost average test scores by  $0.275 \times 0.2 - 0.0022 = 0.0523$  standard deviations. (Note that this includes the impact of any class size reduction due to the additional contract teachers.) The 80% of the teacher workforce made up of civil service teachers would have scored, while on contract, 0.32 standard deviations more than their counterparts who were not hired into the civil service. If we assume that only half of this effect persists once they

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were hired by the TSC after the endline test and students of teachers who were never hired by the TSC, but we cannot reject the hypothesis that this difference is also 0.32 standard deviations.



become civil-service teachers,<sup>20</sup> students of teachers hired into the civil service would in the long run score 0.16 standard deviations more than those who were not hired into the TSC. Since 80% of teachers would be TSC teachers, the gain in tests scores among students overall from improved quality of the TSC workforce is  $0.16 \times 0.8 = 0.13$  standard deviations. Overall, the gain would be approximately  $0.052 + 0.13 = 0.182$  standard deviations.<sup>21</sup>

The analysis above assumes that the entire gain in student performance documented in section 4 for contract teachers is due to their exerting higher effort in response to stronger incentives and therefore does not persist once they become civil servants. However, it could also reflect positive selection among those initially hired as contract teachers compared to civil servants, which would mean some of the effect would persist, in which case our 0.182 standard deviation estimate above should be interpreted as a lower bound. To obtain an upper bound of the possible effect, if the entire test score gain in section 4 were due to selection rather than incentives, then in steady state rather than only roughly 20% of the teaching workforce generating the test score gain we observe among students of ETP teachers, 100% would, increasing the estimated effect by an additional 0.22 standard deviations, for a total gain of approximately 0.4 standard deviation.<sup>22</sup>

Of course we cannot measure all potential channels of dynamic impact. Incentives to become a teacher could potentially either increase or decrease under a system in which teachers initially were hired on a contract basis by local PTAs. As discussed earlier, under the current system, civil-service teacher wages are held far above market clearing levels by the politically powerful teachers union. This motivates many to train as teachers. Jobs are rationed by queuing. Those entering teacher training college currently can expect several years of unemployment. Replacing a period of unemployment with a period of contract teaching at low wages could potentially increase the net present value of becoming a teacher. On the other hand, entering teacher training college will be less attractive to the extent prospective teacher training students

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<sup>20</sup> The literature on the inter-temporal stability of teacher effectiveness is mostly based on US data. Raw correlations are around 0.3-0.4. See McCaffrey et al. (2009) and Goldhaber and Hansen (2008). Using multiple years of data to reduce the noise coming from variation in students, McCaffrey et al. (2009) estimate within-teacher correlation in value-added ranging from 0.5 in elementary grades to 0.8 in middle grades.

<sup>21</sup> Note that this calculation interprets the novice teacher effect as causal. The true novice teacher effect could be different but this would have a relatively minor effect on the overall calculation.

<sup>22</sup> Note that another reason this figure should be considered an upper bound is that it is based on comparing contract teachers to civil-service teachers who teach the lower grades and thus may not be representative of civil-service teachers as a whole.

fear that they will perform badly as contract teachers and therefore will not eventually obtain civil-service positions.<sup>23</sup>

It is also possible that there is complementarity or substitutability between teacher value added and the incentive system, so the gap in performance between teachers under civil-service contracts could be either smaller or larger than under short-term contracts.

## **5.2 Selection and Retention of Contract Teachers: Teacher Rent Seeking and Parents' Empowerment**

Realizing the full dynamic benefit of contract teacher programs requires hiring and retaining good contract teachers and getting rid of bad teachers. As noted above, when civil-service positions open up, contract teachers typically have to switch schools to obtain them. To the extent that it reveals information, hiring and retaining a good contract teacher thus potentially generates positive externalities for other schools. However, since contract teachers often become civil-service teachers, hiring contract teachers may provide opportunities to allocate rents.

As background, in Kenya, those with formal sector jobs often face strong pressure from members of their extended families to help them obtain such jobs as well. This pressure may be particularly strong for males, because Kenya is a patrilocal society and contract teachers are overwhelmingly hired from the local area, so a greater proportion of male teachers will be local to the area of the school and will have relatives from the area interested in applying for the job. Moreover, women are considered to have fewer obligations towards their birth family when they marry into a new family.

To understand the factors influencing hiring and retention of contract teachers it is useful to consider the following framework. Suppose PTA hiring committees can be motivated both by a desire to obtain rents for relatives and a desire to improve student performance. We assume that among the members of the PTA, parents put relatively more weight on maximizing student performance, while teachers (particularly male teachers) put more weight on delivering rents to their relatives. Insofar as SBM training empowers parents within the PTA committee, it will reduce the weight PTA committees place on delivering rents to relatives of teachers.

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<sup>23</sup> While in general equilibrium if programs to fund PTAs to hire locally on temporary contracts were expanded to cover all of the schools in the area, the pool of potential contract teachers would be somewhat reduced, this effect is likely small as two thirds of schools in the area were provided funds to hire contract teachers either through this program or the tracking program described in Duflo et al. (2011), and contract teachers in the program were drawn from the local area.

Suppose that PTA hiring committees receive applications and can obtain observable information such as sex, education, and experience along with an imperfect signal of teacher quality. Suppose also that teachers can supply additional information on their relatives creating a force leading to a potentially positive impact of hiring relatives. Once teachers have been hired, school committees obtain additional information on their performance, and decide whether to renew their contract and more generally how to manage them, and civil-service teachers may continue to act to deliver rents to relatives. Relatives set effort knowing this.

The combined effects of information and rent seeking on aggregate hiring of relatives and on the relative test scores of students of relatives of existing civil-service teachers are ambiguous, but this framework suggests that SBM should unambiguously reduce hiring of relatives, particularly those hired due to rent seeking motives as opposed to those hired based on positive information provided by teachers working at the school. Moreover, if it is politically more difficult for teachers on PTA committees to fire teacher relatives, then in equilibrium we might see lower effort among those relatives (due to lower incentives), but may increase equilibrium effort among teacher relatives working as contract teachers. Test scores of students of relatives should be higher under SBM than without it. We have limited power to test these predictions due to the small sample size, but the evidence we do have is consistent with the predictions.

### *Hiring of Relatives*

Overall, 26% of contract teachers hired through the ETP program were relatives of existing teachers in the school. Relatives of civil-service teachers were much less likely to be hired in schools with SBM: the share of relatives among contract teachers fell from 31% in non-SBM schools to about half that (16%) in SBM schools (Table 5, column 5; the t-statistic on the SBM effect is only 1.5 due to the limited sample size, but a similar SBM effect was observed in tracking schools, suggesting this is not coincidence.)

One frequent concern with empowerment program is the risk of local capture: empowering parents within PTA might merely lead to a bias towards locals, perhaps with a strong ethnic component. However, there is no evidence that the SBM program led to a shift in favoritism from relatives of teachers to locals. On the contrary, point estimates suggest SBM schools were 12 percentage points less likely to hire contract teachers from the local area, although the

difference is not significant given the small sample size (see Column 4, Table 5).<sup>24</sup> More generally, it seems that SBM training increased the transparency of the contract-teacher recruiting process. SBM schools advertised for the position more broadly and interviewed more candidates (results available upon request).

### *Performance-based Retention*

As we noted (and is clear from Table 5, column 7), teachers who performed well were more likely to be absorbed by the TSC. We also see that PTAs were more likely to retain more effective teachers (among those who were not hired by the TSC).<sup>25</sup> Overall, 19% of contract teachers left their school (for reasons other than having obtained a TSC position) before the end of the program and had to be replaced. Teachers whose students' had poor endline performance were more likely to be replaced, with a one standard deviation reduction in test scores associated with a 24 percentage point increase in the chance of early departure (Table 5, column 6).

Even more telling (since the low performance of the students may have been caused by the early departure, rather than the other way around) is what happened at the end of the program. The funds for the program stopped, but PTA could raise their own funds to continue it. Schools with the SBM program (which increased the overall learning impact of the ETP program) were more likely to retain contract teachers into year 3. While 25% of the non-SBM schools in which the contract teacher was not absorbed by the TSC kept the contract teacher on board after the ICS subsidy ended, this figure was 48% among SBM schools (see Table 5, column 8). The finding that parents were twice as likely to pay to continue the ETP program when they had stronger governance rights is dramatic. We cannot distinguish the direct channel of parents being more willing to raise funds for a program because they felt they had a greater role in governance from the indirect channel of governance improving program performance and this in turn increasing willingness to pay.

Finally, among teachers not hired by the TSC, PTAs were more likely to retain good teachers: a one standard deviation increase in student scores is associated with a 43 percentage point higher likelihood of retention as a PTA teacher for teachers who were not hired by the TSC in year 3 (Table 5, column 9). Interestingly, this correlation entirely disappears for teachers who are relative of the teachers in the schools, consistent with the lower incentives they face

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<sup>24</sup> SBM schools were also less likely to hire someone related to a pupil or parent (data not shown).

<sup>25</sup> School committees never explicitly voted against renewing a contract.

(results available upon request). Column 9 in Table 5 shows that relatives are not less likely to be retained, despite worse performance, so conditional on performance, relatives are more likely to be retained.

### *Teacher effort and student performance*

The framework above suggests that teachers' relatives hired under SBM should perform better than teacher relatives hired in the absence of SBM, both because SBM should reduce rent seeking while continuing to allow information flows and thus should improve selection of teachers, and because SBM schools may be better able to monitor and incentivize those relatives of existing civil-service teachers who are hired. Looking at students' tests scores in column 1 of Table 6, we estimate both the main effect of being taught by an ETP teacher who is a relative of an existing teacher, and an interaction between SBM and this variable. We do find that relatives perform less well than non-relatives: the point estimate of the "teacher relative" effect on test scores is  $-0.339$  in non-SBM schools (significant at the 10% level). This indicates that hiring relatives must to some extent be a way to share rents, and is inefficient from society's point of view. The SBM program entirely undoes this effect however: the interaction is  $+0.385$  standard deviations (significant at the 5% level). Taken together, these numbers imply that in SBM schools the total "teacher relative effect" is statistically undistinguishable from zero, consistent with hypothesis that SBM undoes the tendency either to select weak relatives or to let relatives get away with low effort.

Similarly, the point estimates in column 4 of Table 6 suggest that contract teachers who are relatives of civil-service teachers are less likely to be in class teaching, but that this tendency is mitigated by the SBM program. Coefficients are large in magnitude and are significant at the 10% level.

The framework also suggests that if existing civil-service teachers are female, rent seeking will be lower but that information channels should still operate, leading to better performance of relatives of females. The data is consistent with this: fewer teacher relatives were hired in schools with a higher fraction of females among civil-service teachers in lower grades (Table 5, column 5), and students of relatives hired as ETP teachers learned more in those schools (Table 6, column 1, significant at the 10% level). Point estimates suggest they were present and teaching in class more often (Table 6, column 4).

Combining these estimates with the IV estimate in Table 4 can help us determine whether the SBM effect on test score for students taught by teacher relatives can mainly be attributed to higher effort, or to higher skills conditional on effort. Absence from class among ETP teachers who are relatives in non-SBM schools is 24 percentage points higher than among relatives in SBM schools (Table 6, column 4). Taking the estimated effect of teacher effort from Table 4, column 4 (which is entirely identified from the difference in the performance of the students of the TSC teachers), this implies a  $0.24 \times 1.60 = 0.38$  standard deviation gap in test scores between students of relatives in non-SBM schools and students of relatives in SBM schools. This line up quite well with the 0.32 standard deviation in test scores observed in Table 6, column 1 (students of relatives in non-SBM Schools scores  $-0.339$  and students of relatives in SBM schools scored  $-0.065 - 0.339 + 0.385 = -0.019$ , for a difference of 0.32). Although the standard errors around these estimates are large, their comparable order of magnitudes suggests that the SBM effect on contract teachers related to TSC teachers is primarily mediated by effort (whether due to monitoring or to selection of harder working relatives).

### **5.3 Discussion**

Hiring new teachers by local school committees on temporary contracts and then promoting them to civil service positions based on performance has the potential not only to reduce salary costs, but also to improve learning both in the short run, and in the long run by improving the teacher workforce. However, to achieve the full benefits of such a program, it will be important to combine it with SBM training, to empower parents and reduce rent seeking on behalf both of existing teachers (who, in our experiments, reduced the effect of the program by reducing their own effort in the schools where there was no SBM training), and to limit the extent to which (possibly) weak relatives of existing (male) teachers are hired and promoted regardless of their performance. From a methodological standpoint, our analysis suggests a way to measure disproportionate hiring of relatives. While there are legitimate reasons to hire relatives, comparing hiring and performance of relatives hired under different institutional environments provides a way to measure this form of corruption.

## 6. Conclusion

Efforts to improve education in developing countries often focus either on providing additional resources, typically by hiring more teachers to bring down class size, or on governance reform. We examine two popular approaches: decentralized hiring of contract teachers, meant to decrease pupil-teacher ratios at low cost while at the same time strengthening teacher incentives through local control; and School-Based Management training programs, designed to empower parents. We examine these two types of programs in Kenya, and find that they can work as complements. In the absence of parents' empowerment, the additional resources brought about by a contract teacher program are partly captured by the existing civil-servants teachers in two ways. First, civil-service teachers reduce effort, which undoes the positive impact of class size reduction for their students. Second, they seek to capture rents by hiring relatives, whose students perform less well than students of other contract teachers. Empowering parents through a short training session mitigates both these negative effects. First, in schools with SBM training, civil-service teachers were more likely to be present in class and teaching; second, in those schools, relatives of civil-service teachers were less likely to be hired as contract teachers; third, those relatives who were hired anyway performed as well as non-relatives (which could come from better selection of the remaining relatives, or stronger incentives).

Our results suggest that in the presence of weak institutions, increases in resources may be undermined by the behavioral responses of existing providers. However, local governance offers the potential to translate increased resources into better outcomes. In particular, programs devolving authority to hire teachers on short-term contracts have potential both to improve test scores in the short run and to improve the quality of the teaching labor force in the long run. But details matter: small differences in program design (e.g., a few hours of SBM training) can substantially affect outcomes.

In this light it is worth considering two related studies. A large-scale randomized study in India contemporary to ours (Muralidharan and Sundararaman, 2010) finds that adding contract teachers (hired without a promise of regularization) to government-run primary schools leads to average test score gains very similar to those we observe.

On the other hand, preliminary results from a study following up on ours (Bold et al., 2012), also in Kenya, suggest that the impact of contract teacher hiring is indeed sensitive to the institutional context and whether the program is executed as designed. A contract teacher

program in which administration was contracted out to an NGO which made payments to school committees to hire teachers yielded learning gains similar to those we found. But in a variant in which payments to schools were made through the district offices of the Ministry of Education (a separate institution from the TSC, which normally handles payments to civil-service teachers), contract teacher positions were less likely to be filled, monthly payments to teachers were often seriously late (the average delay was 2.33 months and 10% of teachers waited 10 months to be paid) and the program did not significantly improve test scores. Bold et al (2012) argue that part but not all of the difference is accounted for by the unfilled vacancies and the late salary payments. As discussed below, the Kenyan government eventually established a contract teacher program, and under its agreement with the teachers' union, contract teachers were promised civil-service positions. One possible explanation for the differing program effects found by Bold et al (2012) is that some teachers in the government program may have expected to be hired as civil servants independent of their job performance.<sup>26</sup>

A classic objection to decentralization and local control is that it may lead to capture by local elites. We indeed find evidence of such capture, but in our context, capture is not by local gentry or high caste landowners, but rather by the local representatives of the central state itself. Kenya has long had efforts to devolve control, for example through district-based planning. However, these have taken place within the context of a centralized state without local elected legislatures. Our analysis suggests that unless such initiatives are combined with institutional reforms to allow democratic control at the local level, local agents of the state may use devolution to capture rents. Kenya's new Constitution, overwhelmingly approved by voters, combines decentralization with the creation of new elected legislatures at the local level.

Methodologically, our results also have implications for the literature on corruption, governance, and ethnic conflict. Much of the recent literature in economics on corruption focuses on financial corruption – taking bribes or stealing state funds, for example. Yet we would argue that obligations to relatives often conflict with obligations to the state, or to employers more generally, and that favoring relatives is a widespread form of corruption.

It is also a form of corruption that interacts closely with ethnic politics. When civil servants receive rents in the form of above market wages, enjoy civil-service protection and thus long-term employment, and can in turn influence the hiring of other civil servants, favoritism toward

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<sup>26</sup> Our paper differs from both these studies in the randomized allocation of students to either contract or civil-service teachers; the SBM program; and the separate IV estimation of pupil-teacher ratio and teacher effort effects based on randomized allocation of students and of SBM.



relatives can launch a dynamic process of growth of a network of coethnics within the state. Our analysis suggests that altruism towards those family members (or greater ability to enforce reciprocity within the family) could be one reason why politics in much of Africa is often defined by ethnic cleavage, rather than any other arbitrary variable (such as religion or class). Since marriage in the society we study is primarily within ethnic group, relatives will overwhelmingly be coethnics, and since the society is patrilineal, relatives of males will be coethnics. In societies in which only a minority have the educational qualifications required for most civil-service jobs, networks of co-ethnics with sufficient education to qualify for these jobs may be tight enough that individuals benefit indirectly from hiring of unrelated coethnics.

What are the political prospects for local hiring of contract teachers? While teacher unions are likely to resist efforts to reduce their members' wages, they may be willing to compromise on a system in which existing members are protected, but new entrants to the teaching profession are first hired on short-term contracts at lower pay, and then are eventually brought in as civil-service employees. Under some standard models of union bargaining in which unions can set or negotiate wages and benefits but the government controls total hiring of new civil-service teachers, some teacher training college graduates who are willing to work at less than the government's reservation value but will not be hired. One possible solution would be for the government to hire these teachers outside union rules at a lower wage. Ex post, this would be statically efficient. One problem, from the union's point of view, is that the government may not be able to commit not to use the fact that it expects to hire teachers on this basis in its wage negotiations with the teachers union. Moreover, the government could simply replace TSC teachers with PTA teachers over time. To avoid this, the government would have to commit to limit the number of contract teachers hired over time. One way to do this would be through an "up or out" system in which the workers outside the union bargaining agreement have to be either hired as civil servants or fired after a certain time. A key issue would be to ensure that the selection process maintains the positive correlation with student performance found in our sample.

"Up or Out" systems are of course common in the higher education labor market in the United States. Short-term contracts prior to long-term jobs have also been a feature of some European labor market reforms. It is possible that such arrangements could be part of reforms of education markets in developing countries with strong unions. Murgai and Pritchett (2006) proposed essentially such a system for India.

Subsequent to our study, the Kenyan government, which had long had a freeze on hiring of new civil-service teachers, hired 18,000 contract teachers. Initial plans included no guarantee of civil-service employment afterwards. However, the Kenyan National Union of Teachers opposed the initial plans and under the eventual agreement, contract teachers were hired at much higher salaries than in the program we study, hiring was done under civil-service rules heavily weighting the cohort in which applicants graduated from teacher training college rather than the judgment of local school committees, and contract teachers hired under the program were promised civil-service positions. Our analysis suggests that these features could potentially undermine both the incentive and selection effects of local hiring of contract teachers by PTAs. Understanding the selection and incentive impacts of this program, and of potential program variants, is an important topic for future research.

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Table 1. Schools, Teachers and Students Characteristics, by Treatment Group, Pre- and Post-Program

	(1)		(2)		(3)
	Comparison Schools		ETP Treatment Schools		<i>p-value</i> (1) = (2)
<i>Panel A. School Characteristics Prior to Program Inception</i>					
	Mean	SD	Mean	SD	
Points on 2004 national exam (out of 400)	256	24	258	23	0.676
Total student enrollment, 2004	598	241	611	246	0.759
Number of TSC teachers, 2004	12.4	4.1	12.0	3.8	0.537
Proportion of female teachers among TSC teachers, 2004	0.71	0.46	0.67	0.47	0.428
Years of experience among TSC teachers, 2004	17.04	9.26	15.84	9.63	0.197
Average age among TSC teachers, 2004	43.17	8.49	42.18	9.11	0.258
School-level pupil/teacher ratio, 2004	48.7	14.3	53.8	33.4	0.245
Average enrollment in grade 1, March 2005	95	41	94	37	0.902
Proportion of female grade 1 students, March 2005	0.51	0.06	0.49	0.06	0.165
Average enrollment in grade 2, March 2005	97	43	99	40	0.798
Class-based (rather than subject-based) assignment of TSC teachers <sup>a</sup>	0.03	0.18	0.05	0.21	0.703
<i>Panel B. After Program Inception</i>					
Average class size in grade 1, October 2005	81.5	27	43.6	18	0.000
Class-based (rather than subject-based) assignment of TSC teachers	0.05	0.22	0.18	0.39	0.019
If subject-based assignment: number of TSC teachers for grade 1	3.53	1.07	3.54	1.13	0.955
Average class size in grade 2, March 2006	68	25	42	18	0.000
Number of streams in grade 2, March 2006	1.39	0.60	2.29	0.63	0.000
Number of streams in grade 1, March 2006	1.03	0.17	1.00	0.00	0.145
<i>Panel D. Comparability of Grade 1 Sections (ETP schools only)</i>					
	Taught by ETP Contract Teachers		Taught by TSC Teachers		<i>p-value</i> (1) = (2)
Proportion of female teachers	0.48	0.50	0.66	0.47	0.010
Teachers' Years of experience	1.50	0.96	15.54	9.63	0.000
Teachers' Age	27.28	3.60	42.02	9.11	0.000
Proportion of female grade 1 students	0.49	0.07	0.49	0.08	0.841
Students' age	7.68	0.50	7.64	0.48	0.639
Standardized baseline score (Mean 0, SD 1 at school level)	0.01	0.11	0.03	0.10	0.355
Standardized endline score (Mean 0, SD 1 at school level)	0.08	0.22	-0.07	0.20	0.000

Notes: Data from 70 comparison schools and 70 ETP treatment schools. Standard deviations in italics.

<sup>a</sup>Under "class-based assignment" of teachers, each teacher is assigned a specific class for which she teaches all subjects (math, reading, etc.). Under "subject-based assignment" of teachers, each teacher is assigned a specific subset of subjects and teaches those subjects in multiple classes and/or grades.

Table 2. Endline Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total Score			Math Score		Literacy Score	
(1) ETP school	0.087 (0.098)	0.074 (0.088)	0.073 (0.088)	0.051 (0.078)	0.015 (0.073)	0.102 (0.111)	0.111 (0.096)
(2) ETP school x SBM	0.125 (0.118)	0.130 (0.106)	0.130 (0.106)	0.182 (0.093)*	0.186 (0.086)**	0.050 (0.133)	0.055 (0.117)
(3) ETP school x assigned to ETP contract teacher	0.207 (0.064)***	0.228 (0.058)***	0.241 (0.066)***	0.251 (0.058)***	0.260 (0.062)***	0.128 (0.070)*	0.178 (0.069)**
(4) ETP school x SBM x assigned to ETP contract teacher	-0.156 (0.095)	-0.174 (0.085)**	-0.169 (0.083)**	-0.213 (0.095)**	-0.224 (0.087)**	-0.076 (0.095)	-0.088 (0.085)
(5) School size (/100)		-0.055 (0.088)	-0.051 (0.088)		-0.201 (0.075)**		0.089 (0.096)
(6) Share female among TSC teachers		0.502 (0.104)***	0.497 (0.104)***		0.307 (0.093)***		0.567 (0.107)***
(7) Average years of experience (/10) among TSC teachers		0.021 (0.067)	0.020 (0.068)		-0.017 (0.058)		0.049 (0.070)
(8) Girl		0.056 (0.023)**	0.056 (0.023)**		-0.013 (0.021)		0.106 (0.026)***
(9) Age		-0.043 (0.012)***	-0.043 (0.012)***		-0.007 (0.011)		-0.065 (0.013)***
(10) Baseline score		0.498 (0.018)***	0.498 (0.018)***		0.497 (0.015)***		0.401 (0.022)***
(11) Teacher has <1 year of experience			-0.065 (0.091)		-0.012 (0.085)		-0.100 (0.107)
(12) Observations	6531	6531	6531	6531	6531	6534	6534
(13) R-squared	0.030	0.250	0.250	0.030	0.250	0.030	0.180
(14) Total effect: ETP school+SBM <i>p-val (ETP School + SBM = 0 )</i>	0.212 <i>0.056*</i>	0.204 <i>0.036**</i>	0.203 <i>0.036**</i>	0.233 <i>0.01**</i>	0.201 <i>0.016**</i>	0.152 <i>0.204</i>	0.166 <i>0.110</i>
(15) Total effect: ETP school+contract teacher <i>p-val (ETP school +contract teacher = 0 )</i>	0.294 <i>0.006***</i>	0.302 <i>0.002***</i>	0.314 <i>0.003***</i>	0.302 <i>0.001***</i>	0.275 <i>0.003***</i>	0.230 <i>0.045**</i>	0.289 <i>0.009***</i>
(16) Total effect: ETP school+SBM+contract teacher <i>p-val (ETP school +SBM +contract teacher = 0 )</i>	0.263 <i>0.007***</i>	0.258 <i>0.005***</i>	0.275 <i>0.007***</i>	0.271 <i>0.001***</i>	0.237 <i>0.007***</i>	0.204 <i>0.048**</i>	0.256 <i>0.019**</i>

Notes: OLS regressions. Endline test was administered after the program had been in place for five school terms. Scores are normalized such that the mean and standard deviation of the comparison group are zero and one, respectively. Robust standard errors clustered at the school level in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively. 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.

Table 3. Teacher Effort and Pedagogy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Data Source:</i>	Unannounced Spot Checks			Classroom Observations			Teacher Interviews			
	Teacher Found in Class Teaching	Teacher Present at School	If Present at School: Found in Class Teaching	Share of Students Who Went to Blackboard to Write an Answer	Share of Students Who Answered a Question	Share of Students Who Asked a Question	Teacher had a lesson plan prepared for the class	Teacher did not have time to mark individual work for children	Teacher reports not having time to help children individually	Teacher reports not having time to pay attention to slow learners
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) ETP school	-0.129 (0.040)***	-0.009 (0.025)	-0.145 (0.042)***	0.033 (0.015)**	0.155 (0.081)*	-0.025 (0.018)	0.073 (0.123)	-0.238 (0.128)*	-0.224 (0.124)*	-0.145 (0.132)
(2) ETP school x SBM	0.075 (0.043)*	0.001 (0.029)	0.093 (0.050)*	-0.001 (0.029)	0.016 (0.087)	-0.013 (0.012)	-0.089 (0.141)	0.098 (0.143)	-0.008 (0.141)	-0.033 (0.149)
(3) ETP school x ETP contract teacher	0.278 (0.047)***	0.057 (0.043)	0.294 (0.050)***	-0.032 (0.028)	-0.015 (0.080)	0.001 (0.016)	0.165 (0.131)	-0.078 (0.133)	-0.356 (0.111)***	-0.165 (0.146)
(4) ETP school x ETP contract teacher x SBM	0.029 (0.065)	0.092 (0.049)*	-0.063 (0.073)	-0.006 (0.042)	-0.035 (0.113)	-0.001 (0.017)	-0.024 (0.165)	-0.032 (0.188)	0.157 (0.149)	0.149 (0.196)
(5) Years of experience teaching	0.002 (0.001)	0.002 (0.001)*	0.001 (0.002)	-0.001 (0.001)	0.002 (0.002)	-0.001 (0.001)	-0.002 (0.006)	0.000 (0.005)	0.001 (0.005)	-0.002 (0.005)
(6) < 1 year of experience teaching	0.001 (0.074)	-0.067 (0.046)	0.065 (0.064)	0.117 (0.091)	-0.050 (0.081)	-0.009 (0.014)	-0.187 (0.157)	-0.009 (0.170)	-0.059 (0.179)	0.109 (0.170)
(7) Female	0.054 (0.028)*	0.004 (0.021)	0.066 (0.029)**	-0.037 (0.026)	0.004 (0.045)	0.016 (0.010)	0.093 (0.079)	0.043 (0.089)	0.127 (0.079)	0.096 (0.086)
(8) Log # of teachers involved in the grade	-0.016 (0.040)	-0.136 (0.041)***	0.105 (0.046)**							
(9) Observations	1957	1957	1636	168	168	168	165	166	165	165
(10) Mean in comparison schools	0.579	0.840	0.688	0.012	0.154	0.044	0.597	0.742	0.726	0.677
(11) Total effect: ETP school+SBM <i>p-val (ETP School + SBM = 0)</i>	-0.054 0.182	-0.008 0.772	-0.052 0.283	0.032 0.241	0.171 0.002***	-0.038 0.127	-0.016 0.894	-0.140 0.203	-0.232 0.041**	-0.178 0.126
(12) Total effect: ETP school+contract teacher <i>p-val (ETP school +contact teacher = 0)</i>	0.149 0.007***	0.048 0.234	0.149 0.008***	0.001 0.966	0.140 0.014**	-0.024 0.364	0.238 0.064*	-0.316 0.024**	-0.580 0***	-0.310 0.015**
(13) Total effect: ETP school+SBM+contract tea <i>p-val (ETP school +SBM +contract teach</i>	0.253 0***	0.141 0***	0.179 0.002***	-0.006 0.819	0.121 0.048**	-0.038 0.136	0.125 0.395	-0.250 0.135	-0.431 0.002***	-0.194 0.189

Notes: Standard errors clustered at school level. Region and date of visit dummies included in all regressions but not shown. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively.

Columns 1-3: Linear probability model regressions. Multiple observations per teacher.

Columns 4-10: In each school, two or three grade 1 teachers (including the ETP teacher in ETP schools) were selected for classroom observation and for an interview. OLS regressions in columns 4-6 and LPM regressions in columns 7-10.



Table 4. Instrumental Variable Estimates of Effect of Class Size and Teacher Effort on Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Total Score				Math Score				Literacy Score			
Class Size (/100)	-0.42 (0.20)**	-0.60 (0.26)**	-0.64 (0.24)***	-0.68 (0.42)	-0.43 (0.18)**	-0.50 (0.23)**	-0.51 (0.22)**	-0.69 (0.45)	-0.33 (0.22)	-0.58 (0.27)**	-0.62 (0.24)**	-0.51 (0.44)
Prob. teacher teaching	0.80 (0.30)***	1.31 (0.66)**	1.42 (0.67)**	1.60 (1.56)	0.93 (0.28)***	1.01 (0.59)*	1.19 (0.61)*	1.95 (1.68)	0.52 (0.29)*	1.31 (0.64)**	1.35 (0.64)**	0.89 (1.71)
ETP teacher				-0.06 (0.49)				-0.27 (0.50)				0.16 (0.53)
Average experience of teachers (/10)		0.14 (0.24)	0.20 (0.25)	0.16 (0.38)		0.06 (0.22)	0.14 (0.23)	0.00 (0.39)		0.18 (0.23)	0.20 (0.24)	0.29 (0.35)
Square (Average experience of teachers /10)		-0.06 (0.08)	-0.08 (0.09)	-0.07 (0.11)		-0.04 (0.08)	-0.07 (0.08)	-0.03 (0.12)		-0.06 (0.08)	-0.07 (0.09)	-0.10 (0.10)
Share of female teachers		0.31 (0.09)***	0.31 (0.09)***	0.30 (0.10)***		0.22 (0.08)**	0.21 (0.08)***	0.18 (0.12)		0.33 (0.09)***	0.33 (0.09)***	0.35 (0.10)***
Girl			0.06 (0.02)***	0.06 (0.02)***			0.21 (0.08)***	0.18 (0.12)			0.33 (0.09)***	0.35 (0.10)***
Age			-0.04 (0.01)***	-0.04 (0.01)***			-0.01 (0.02)	-0.01 (0.02)			0.11 (0.03)***	0.11 (0.03)***
Baseline score			0.49 (0.02)***	0.49 (0.02)***			-0.46 (0.20)**	-0.38 (0.23)			-0.35 (0.11)***	-0.40 (0.21)*
Observations	6531	6505	6505	6505	6531	6505	6505	6505	6531	6505	6505	6505
Hansen overid P-value	0.57	0.97	0.97		0.36	0.47	0.50		0.88	0.87	0.86	

Notes: 2SLS estimation where Class size and Probability that Teacher Teaches are endogeneous regressors. First-Stage estimations are presented in Table A4. Robust standard errors clustered at the school level are presented in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively.

Table 5. Contract Teacher Selection, Retention and Promotion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Characteristics of ETP Contract Teacher hired by the PTA					What happened to the ETP Contract Teacher?			
	Female	<1 year of experience	Previously PTA teacher at the school	Born or married locally	Relative of a teacher in the school	Left (not for TSC) and had to be replaced	Hired by TSC during or shortly after ETP program	Employed by school beginning of year 3	If not hired by TSC: Employed by school beginning of year 3
SBM	-0.16 (0.13)	0.15 (0.10)	-0.02 (0.10)	-0.12 (0.12)	-0.15 (0.10)	0.02 (0.10)	-0.18 (0.11)	0.23 (0.12)*	0.07 (0.19)
Female						0.17 (0.09)*	0.04 (0.11)	-0.06 (0.12)	-0.05 (0.18)
Years of experience						0.05 (0.05)	0.11 (0.06)*	-0.19 (0.06)***	-0.16 (0.09)*
Relative of a teacher in the school						-0.08 (0.13)	-0.07 (0.15)	0.18 (0.16)	0.05 (0.25)
Average presence						-0.51 (0.19)***	0.24 (0.22)	-0.1 (0.24)	-0.11 (0.34)
Mean score of students at endline						-0.24 (0.11)**	0.34 (0.12)***	0.03 (0.14)	0.43 (0.25)*
Prop. TSC teachers female	-0.14 (0.20)	-0.06 (0.17)	-0.01 (0.16)	-0.25 (0.19)	-0.48 (0.15)***	-0.16 (0.16)	-0.11 (0.19)	0.11 (0.20)	0.1 (0.33)
Average experience among TSC teachers	0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.02 (0.01)*	0.02 (0.01)*	-0.01 (0.01)	0.03 (0.01)***	-0.02 (0.01)*	-0.02 (0.02)
School performance on 2004 national exam	0.02 (0.08)	0.03 (0.07)	-0.02 (0.07)	-0.01 (0.08)	-0.02 (0.06)	0.11 (0.06)*	-0.07 (0.07)	-0.05 (0.08)	-0.06 (0.12)
School Size (/100)	-0.04 (0.04)	0.02 (0.04)	0.03 (0.03)	0.00 (0.04)	0.08 (0.03)**	0.02 (0.03)	0.01 (0.04)	-0.04 (0.04)	-0.02 (0.06)
Number of teachers assigned to lower grades	0.02 (0.04)	-0.04 (0.03)	-0.01 (0.03)	0.03 (0.04)	0.03 (0.03)	-0.02 (0.03)	0.01 (0.03)	0.03 (0.04)	0.03 (0.05)
School had at least one PTA teacher in 2004	-0.02 (0.15)	-0.26 (0.12)**	-0.04 (0.12)	-0.06 (0.14)	-0.17 (0.11)	0.03 (0.11)	0.13 (0.13)	0.18 (0.14)	0.25 (0.20)
Observations	70	70	68	68	68	68	68	68	42
R-Squared	0.09	0.17	0.06	0.10	0.07	0.35	0.44	0.28	0.31
Mean in schools without SBM	0.53	0.17	0.20	0.74	0.31	0.19	0.47	0.25	0.47

Notes: Sample restricted to the 70 schools sampled for the Extra-Teacher Program. Linear probability model regressions with standard errors presented in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively.

Table 6. "Relative" effect: Performance of ETP Contract Teachers related to existing TSC teachers

	(1)	(2)	(3)	(4)
	Student Performance	ETP Contract Teacher's Effort		
	Total Endline Score for Students assigned to ETP teacher	Teacher Present at School	If Present at School: Found in Class Teaching	Teacher Found in Class Teaching
School based management (SBM)	-0.065 (0.131)	0.019 (0.042)	0.000 (0.063)	0.020 (0.067)
(a) ETP contract teacher is the relative of a TSC teacher	-0.339 (0.175)*	-0.071 (0.103)	-0.285 (0.203)	-0.277 (0.143)*
(b) SBM x ETP contract teacher is the relative of a TSC teacher	0.385 (0.165)**	0.199 (0.096)**	0.124 (0.173)	0.242 (0.131)*
Proportion of females among TSC teachers	0.141 (0.173)	0.152 (0.070)**	-0.166 (0.093)*	-0.019 (0.098)
Proportion of females among TSC teachers x ETP contract teacher is the relative of a TSC teacher	0.507 (0.261)*	-0.035 (0.117)	0.325 (0.192)*	0.237 (0.178)
ETP contract teacher has < 1 year of experience	-0.159 (0.092)*	-0.055 (0.051)	0.039 (0.061)	-0.017 (0.076)
Observations	1548	217	192	217
<i>p-val a+b=0</i>	0.770	0.181	0.204	0.794
Mean in schools without SBM		0.845	0.816	0.690
Unit of Observation	Student	Teacher-Day	Teacher-Day	Teacher-Day

Notes: Standard errors in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively.

Column 1: OLS. Sample restricted to students assigned to ETP contract teachers in ETP schools. Data source: endline test administered by research team.

Columns 2-4: LPM. Sample restricted to ETP contract teachers in ETP schools. Up to 5 observations per teacher. Data source: Five unannounced spot checks.

Appendix Table A1: Attrition

	(1)	(2)	(3)	(4)
	Attrited at Endline Test (after 19 Months in Program)		Attrited at Long-Run Follow-up Test (a Year after Program Ended)	
Extra-teacher program school	-0.024 (0.021)	-0.005 (0.021)	0.003 (0.021)	0.018 (0.027)
Extra-teacher program school x school based management	-0.016 (0.023)	-0.017 (0.024)	-0.034 (0.023)	-0.035 (0.031)
Extra-teacher program school x assigned to contract teacher	-0.018 (0.013)	-0.014 (0.017)	-0.029 (0.018)	-0.065 (0.028)**
Extra-teacher program school x assigned to contract teacher x SBM	0.016 (0.019)	-0.021 (0.028)	0.030 (0.024)	0.051 (0.041)
Age	-0.022 (0.004)***	-0.022 (0.004)***	-0.006 (0.004)*	-0.006 (0.004)
Girl	0.012 (0.009)	0.012 (0.010)	0.025 (0.010)***	0.025 (0.011)**
In bottom half of initial distribution		0.009 (0.012)		0.013 (0.015)
In bottom half of initial distribution x ETP school		-0.015 (0.026)		-0.030 (0.029)
In bottom half of initial distribution x ETP school x SBM		-0.029 (0.034)		-0.016 (0.031)
In bottom half of initial distribution x ETP school x contract teacher		-0.018 (0.034)		0.089 (0.032)***
In bottom half of initial distribution x ETP school x contract teacher x SBM		0.099 (0.051)*		-0.042 (0.051)
Observations	6628	6628	6628	6628
Mean in Comparison Schools	0.184	0.184	0.228	0.228
<i>Total effects</i>				
Extra-teacher program school+school based management <i>p-val (Extra-teacher program school + school based management = 0 )</i>	-0.040 0.062*		-0.031 0.083*	
Extra-teacher program school+contract teacher <i>p-val (Extra-teacher program school +contract teacher = 0 )</i>	-0.042 0.036**		-0.026 0.174	
ETP school+SBM+contract teacher <i>p-val (ETP school +SBM +contract teacher = 0 )</i>	-0.042 0.028**		-0.030 0.034**	
<i>Total effects for students in top half of initial distribution</i>				
Extra-teacher program school+school based management <i>p-val (Extra-teacher program school + school based management = 0 )</i>		-0.022 0.291		-0.017 0.390
Extra-teacher program school+contract teacher <i>p-val (Extra-teacher program school +contract teacher = 0 )</i>		-0.019 0.339		-0.047 0.037**
ETP school+SBM+contract teacher <i>p-val (ETP school +SBM +contract teacher = 0 )</i>		-0.057 0.004***		-0.031 0.182
<i>Total effects for students in bottom half of initial distribution:</i>				
Extra-teacher program school+school based management <i>p-val (Extra-teacher program school + school based management = 0 )</i>		-0.066 0.009***		-0.063 0.009***
Extra-teacher program school+contract teacher <i>p-val (Extra-teacher program school +contract teacher = 0 )</i>		-0.052 0.036**		0.012 0.641
ETP school+SBM+contract teacher <i>p-val (ETP school +SBM +contract teacher = 0 )</i>		-0.020 0.433		-0.030 0.183

Notes: LPM regressions. Robust standard errors clustered at the school level are presented in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively.

Table A2. Lower Bounds for Treatment Effects at Endline

	(1)	(2)	(3)	(4)	(5)	(6)
	Total score		Math score		Literacy Score	
(1) ETP school	0.036 (0.086)	0.036 (0.086)	-0.012 (0.072)	-0.012 (0.072)	0.071 (0.094)	0.071 (0.094)
(2) ETP school x SBM	0.107 (0.100)	0.107 (0.100)	0.177 (0.085)**	0.177 (0.085)**	0.024 (0.109)	0.024 (0.109)
(3) ETP school x assigned to ETP contract teacher	0.176 (0.054)***	0.178 (0.061)***	0.231 (0.051)***	0.225 (0.057)***	0.093 (0.061)	0.101 (0.066)
(4) ETP school x SBM x assigned to ETP contract teacher	-0.144 (0.088)	-0.143 (0.087)	-0.214 (0.088)**	-0.216 (0.088)**	-0.054 (0.093)	-0.051 (0.092)
(5) School size (/100)	-0.029 (0.084)	-0.029 (0.085)	-0.184 (0.074)**	-0.186 (0.074)**	0.112 (0.091)	0.114 (0.092)
(6) Share female among TSC teachers	0.458 (0.101)***	0.458 (0.101)***	0.283 (0.092)***	0.285 (0.092)***	0.522 (0.103)***	0.519 (0.103)***
(7) Average years of experience (/10) among TSC teachers	0.020 (0.064)	0.020 (0.064)	-0.017 (0.056)	-0.017 (0.056)	0.049 (0.065)	0.049 (0.066)
(8) Girl	0.067 (0.022)***	0.067 (0.022)***	-0.007 (0.021)	-0.007 (0.021)	0.119 (0.025)***	0.119 (0.025)***
(9) Age	-0.035 (0.012)***	-0.035 (0.011)***	-0.002 (0.010)	-0.002 (0.010)	-0.057 (0.012)***	-0.057 (0.012)***
(10) Baseline score	0.479 (0.017)***	0.479 (0.017)***	0.487 (0.015)***	0.487 (0.015)***	0.377 (0.020)***	0.377 (0.020)***
(11) Teacher has <1 year of experience		-0.007 (0.084)		0.031 (0.083)		-0.041 (0.098)
(12) Observations	6429	6429	6429	6429	6432	6432
(13) R-squared	0.240	0.240	0.240	0.240	0.170	0.170
(14) Total effect: ETP school+SBM <i>p-val (ETP School + SBM = 0 )</i>	0.143 <i>0.129</i>	0.143 <i>0.128</i>	0.165 <i>0.048**</i>	0.165 <i>0.047**</i>	0.095 <i>0.331</i>	0.095 <i>0.332</i>
(15) Total effect: ETP school+contract teacher <i>p-val (ETP school +contract teacher = 0 )</i>	0.212 <i>0.015**</i>	0.214 <i>0.024**</i>	0.219 <i>0.007***</i>	0.213 <i>0.014**</i>	0.164 <i>0.076*</i>	0.172 <i>0.083*</i>
(16) Total effect: ETP school+SBM+contract teacher <i>p-val (ETP school +SBM +contract teacher = 0 )</i>	0.175 <i>0.043**</i>	0.178 <i>0.063*</i>	0.182 <i>0.019**</i>	0.174 <i>0.042**</i>	0.134 <i>0.144</i>	0.145 <i>0.154</i>

Notes: OLS regressions. Lower bounds à la Lee obtained by trimming upper tail in treatment group (see text for details). Scores are normalized such that the mean and standard deviation of the comparison group are zero and one, respectively. Robust standard errors clustered at the school level in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively. 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.

Table A3. Regressions Results: Test Scores at Long-run Follow-up (1 year after program ended)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS Regressions			Lee Lower Bounds		
	Total Score	Math Score	Literacy Score	Total Score	Math Score	Literacy Score
(1) ETP school	-0.015 (0.085)	-0.038 (0.080)	0.006 (0.087)	-0.032 (0.084)	-0.054 (0.079)	-0.009 (0.086)
(2) ETP school x SBM	0.155 (0.104)	0.176 (0.089)**	0.113 (0.111)	0.126 (0.099)	0.153 (0.087)*	0.084 (0.107)
(3) ETP school x assigned to ETP contract teacher	0.105 (0.058)*	0.080 (0.055)	0.103 (0.058)*	0.074 (0.057)	0.055 (0.054)	0.074 (0.058)
(4) ETP school x SBM x assigned to ETP contract teacher	-0.116 (0.074)	-0.124 (0.077)	-0.089 (0.073)	-0.098 (0.073)	-0.107 (0.077)	-0.072 (0.073)
(5) School size (/100)	0.063 (0.094)	-0.082 (0.073)	0.160 (0.105)	0.073 (0.092)	-0.074 (0.071)	0.171 (0.103)
(6) Share female among TSC teachers	0.419 (0.100)***	0.253 (0.086)***	0.473 (0.106)***	0.399 (0.098)***	0.236 (0.085)***	0.454 (0.104)***
(7) Average years of experience (/10) among TSC teachers	-0.013 (0.061)	-0.023 (0.049)	-0.003 (0.068)	-0.013 (0.058)	-0.024 (0.048)	-0.003 (0.065)
(8) Girl	0.111 (0.025)***	0.037 (0.024)	0.147 (0.027)***	0.102 (0.024)***	0.032 (0.023)	0.137 (0.026)***
(9) Age	-0.064 (0.012)***	-0.033 (0.011)***	-0.077 (0.012)***	-0.068 (0.011)***	-0.037 (0.010)***	-0.080 (0.011)***
(10) Baseline score	0.462 (0.016)***	0.454 (0.016)***	0.389 (0.016)***	0.460 (0.015)***	0.454 (0.015)***	0.386 (0.015)***
(11) Teacher has <1 year of experience	-0.029 (0.096)	-0.009 (0.091)	-0.038 (0.107)	-0.027 (0.090)	-0.003 (0.090)	-0.039 (0.101)
(12) Observations	6171	6171	6175	6094	6094	6098
(13) R-squared	0.210	0.190	0.170	0.210	0.190	0.170
(14) Total effect: ETP school+SBM <i>p-val (ETP School + SBM = 0)</i>	0.140 <i>0.154</i>	0.138 <i>0.082*</i>	0.119 <i>0.262</i>	0.094 <i>0.328</i>	0.099 <i>0.206</i>	0.075 <i>0.473</i>
(15) Total effect: ETP school+contract teacher <i>p-val (ETP school +contract teacher = 0)</i>	0.090 <i>0.310</i>	0.042 <i>0.564</i>	0.109 <i>0.253</i>	0.042 <i>0.616</i>	0.001 <i>0.996</i>	0.065 <i>0.481</i>
(16) Total effect: ETP school+SBM+contract teacher <i>p-val (ETP school +SBM +contract teacher = 0)</i>	0.129 <i>0.173</i>	0.094 <i>0.248</i>	0.133 <i>0.183</i>	0.070 <i>0.4370</i>	0.047 <i>0.556</i>	0.077 <i>0.430</i>

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. Region and date of test dummies were included in all regressions but are not shown. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively.

Appendix Table A4. First-stage for IV estimates: Predicting Teacher Effort and Class Size with Experimental Variation

	(1)	(2)
	Class Size	Prob. Teacher Teaching <sup>a</sup>
(a) Extra-teacher program (ETP) school	-39.43 (3.93)***	-0.13 (0.04)***
(b) ETP school x School based management (SBM)	2.60 (4.11)	0.09 (0.04)**
(c) ETP school x Contract teacher	2.40 (0.77)***	0.22 (0.04)***
(d) ETP school x SBM x Contract Teacher	-3.67 (1.46)**	-0.06 (0.060)
Observations	6531	6531
R-squared	0.44	0.12
Mean in Comparison Schools	83.40	0.558
p-value (a + b = 0)	0.000	0.226
F-Stat	33.5	10.4

<sup>a</sup>Prob. Teacher Teaching = Average across 5 unannounced spot checks by research team. Robust standard errors clustered at the school level in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels.

Table A5. IV Estimates of Effect of Class Size and Teacher's Effort on Test Scores, by Initial Class Size Category

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Score							
	Students in Schools with Initial Class Size ≤ Median				Students in Schools with Initial Class Size > Median			
Class Size (/100)	-0.82 (0.56)	-0.77 (0.40)*	-0.84 (0.58)	-0.80 (0.82)	-0.35 (0.22)	-0.42 (0.20)**	-0.43 (0.19)**	-0.66 (0.48)
Prob. teacher teaching <sup>a</sup>	0.55 (0.90)	0.40 (1.08)	-0.49 (1.29)	-0.57 (1.52)	0.91 (0.29)***	0.95 (0.41)**	0.99 (0.38)***	2.09 (1.90)
Extra-teacher program teacher				0.04 (0.39)				-0.32 (0.52)
Average experience of teachers		-0.07 (0.13)	-0.25 (0.17)	-0.24 (0.23)		-0.03 (0.05)	-0.03 (0.05)	-0.08 (0.12)
Share of female teachers		0.64 (0.16)***	0.55 (0.17)***	0.55 (0.17)***		0.24 (0.10)**	0.23 (0.09)**	0.16 (0.15)
Girl			0.04 (0.05)	0.04 (0.05)			0.07 (0.03)***	0.07 (0.03)***
Age			-0.08 (0.02)***	-0.08 (0.02)***			-0.03 (0.01)**	-0.03 (0.02)*
Baseline score			0.62 (0.03)***	0.62 (0.03)***			0.47 (0.02)***	0.46 (0.02)***
Observations	1288	1288	1288	1288	5243	5217	5217	5217
Hansen overid p-value	0.91	0.32	0.97		0.51	0.64	0.62	

Notes: 2SLS estimation where Class size and Probability that Teacher Teaches are endogeneous regressors. The median class size at baseline was 76 students per class (grade 1). Robust standard errors clustered at the school level are presented in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively.



Table A6. IV Estimates of Effect of Class Size and Teacher's Effort on Test Scores, including Tracking Schools

	(1)	(2)	(3)	(4)
	Total Score			
Class Size (/100)	-0.33 (0.20)	-0.46 (0.21)**	-0.49 (0.21)**	-0.58 (0.32)*
Prob. teacher teaching <sup>a</sup>	1.19 (0.32)***	1.64 (0.60)***	1.67 (0.61)***	1.16 (1.14)
ETP teacher				0.08 (0.25)
Average age of teachers		0.02 (0.06)	0.01 (0.06)	0.02 (0.05)
Share of female teachers		0.19 (0.08)**	0.21 (0.08)***	0.24 (0.08)***
Girl			0.06 (0.02)***	0.07 (0.02)***
Age			-0.05 (0.01)***	-0.05 (0.01)***
Baseline score			0.50 (0.02)***	0.51 (0.02)***
Observations	9279	9254	9254	9254
Hansen overid p-value	0.55	0.33	0.39	

Notes: 2SLS estimation where Class size and Probability that Teacher Teaches are endogenous regressors. Robust standard errors clustered at the school level are presented in parentheses. \*\*\*, \*\*, \* indicate significance at the 1%, 5%, and 10% levels respectively. All regressions include controls for Tracking status, startin in the Bottom Half of the test score distribution as baseline, as well as the following interactions: Tracking \* ETP teacher, Tracking \* Bottom Half, Tracking \* ETP Teacher \* Bottom Half.