

## Contract Teachers: Experimental Evidence from India

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**Abstract:** The large-scale expansion of primary education in developing countries has led to the increasing use of teachers on fixed-term renewable contracts who are not professionally trained and who are paid much lower salaries than regular civil service teachers. This has been a very controversial policy, but there is limited evidence on the effectiveness of contract teachers in improving student learning (with identification being a key constraint). We present experimental evidence from a program that provided an extra contract teacher to a randomly-chosen subset of a representative sample of government-run primary schools in the Indian state of Andhra Pradesh. At the end of two years, students in schools with an extra contract teacher performed significantly better than those in comparison schools by 0.16 and 0.10 standard deviations in math and language tests respectively. We find strong evidence for heterogeneous treatment effects, with the largest impact of the extra contract teacher found in remote schools and for students in the first grade. Contract teachers were significantly less likely to be absent than civil service teachers, and more likely to be engaging in teaching activity when observed during unannounced visits to schools. Finally, we collect data on teachers in rural private schools in the same districts and find that private school teacher salaries are much closer to that of contract teachers than civil-service teachers, suggesting that expanded use of contract teachers in government schools can move education production closer to the efficient frontier.

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## 1. Introduction

The large scale expansion of primary education in developing countries over the past two decades under attempts to achieve Education for All (EFA) goals, has led to significant improvements in school access and enrollment, but has also created difficulties with regards to maintaining and improving school quality.<sup>1</sup> A particularly challenging problem has been one of recruiting enough teachers and posting them in areas where they are needed. The challenge is both fiscal (since teacher salaries account for the largest component of education spending<sup>2</sup>) and logistical (since teachers are less willing to be deployed to underserved and remote areas where their need is the greatest).

Governments in several developing countries have responded to this challenge by staffing unfilled teaching positions with locally-hired contract teachers who are not civil service employees.<sup>3</sup> Common characteristics of contract teachers are that they are: (1) appointed on fixed-term (typically annual) renewable contracts, with no guarantee of renewed employment (unlike regular civil-service teachers); (2) appointed and renewed by school committees as opposed to higher-level education bureaucrats; (3) often less qualified than regular teachers and much less likely to have a formal teacher training certificate or degree; (4) more likely to be from the local area where the school is located; and (5) paid much lower salaries than those of regular teachers (Table 1). Contract teacher systems in different countries feature varying combinations of the characteristics above, while in India, all of these features are typical.<sup>4</sup>

While contract teacher programs in India started out as a way to staff remote and underserved areas, their use has continued and expanded (especially in poorer states) due to fiscal pressures.<sup>5</sup> The increasing use of contract teachers has been one of the most

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<sup>1</sup> A recent all-India survey shows that nearly 60% of children aged 6-14 cannot read at the second grade level even though over 95% of them are enrolled in school (Pratham, 2008).

<sup>2</sup> Typically over 80% and often over 90% of education spending in many developing countries is on teacher salaries (education spending data by country available at <http://www.uis.unesco.org/en/stats/stats0.htm>)

<sup>3</sup> Contract teacher schemes have been widely employed in several states of India (under different names such as Shiksha Karmi in Madhya Pradesh and Rajasthan, Shiksha Mitra in Uttar Pradesh, Vidya Sahayak in Gujarat and Himachal Pradesh, and Vidya Volunteers in Andhra Pradesh), and many other developing countries including Cambodia, Indonesia, Kenya, Mali, Nicaragua, Niger, Togo, and several other African countries (see Duthilleul (2005) for a review of contract teacher programs in several countries).

<sup>4</sup> In Kenya, for instance, the contract teachers are as qualified as regular teachers but they share features the other features on this list (Duflo et al 2007).

<sup>5</sup> For example, over 25% of the primary school teachers in the large Indian states of Uttar Pradesh, Bihar, and Madhya Pradesh are contract teachers (as calculated from the State Report Cards issued by the

significant policy innovations in providing primary education in developing countries over the last two decades, but it has also been highly controversial. Supporters say that using contract teachers is an efficient way of expanding education access and quality to large number of first-generation learners, and that contract teachers face superior incentives compared to tenured civil service teachers. Opponents argue that using under-qualified and untrained teachers may staff classrooms but will not produce learning outcomes, and that the use of contract teachers de-professionalizes teaching, reduces the prestige of the entire profession, and reduces motivation of all teachers.<sup>6</sup> However, while the use of contract teachers has generated much controversy, there is very little rigorous evidence regarding the effectiveness of contract teachers in improving student learning outcomes. In particular, identification of the impact of contract teachers is challenging because they are more likely to be deployed in backward and remote areas.

We present experimental evidence on the impact of contract teachers from a program that was designed to mimic an expansion of the current contract teacher policy of the government of the Indian state of Andhra Pradesh (AP).<sup>7</sup> The study was conducted across a representative sample of 200 government-run schools in rural AP with 100 of these schools being selected by lottery to receive an extra contract teacher over and above their allocation of regular and contract teachers.<sup>8</sup> This paper presents the first experimental evidence from an “as is” expansion of a contract teacher policy in a representative sample of schools anywhere in the world.

This paper answers the following questions in the context of rural education in a developing country: (i) What is the impact on student learning outcomes of providing an

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Ministry of Human Resource Development in India – see Mehta (2007)). While this refers to the total *stock* of teachers, the share of contract teachers in the annual *flow* of new teachers has been significantly higher in the past 15 years.

<sup>6</sup> See Kumar et al (2005) for an example of these criticisms.

<sup>7</sup> The study was conducted as part of a larger education research initiative known as the Andhra Pradesh Randomized Evaluation Study (AP RESt), which is a partnership between the government of AP, the Azim Premji Foundation (a leading non-profit organization working to improve primary education in India), and the World Bank to rigorously evaluate the effectiveness of several policy options to improve the quality of primary education in developing countries. The Azim Premji Foundation (APF) was the main implementing agency for the study. We have served as technical consultants and have overseen the design, and evaluation of the various interventions.

<sup>8</sup> The provision of an extra contract teacher was one of 4 policy options studied under AP RESt. The overall study was conducted across 500 schools, with 100 schools randomly assigned to each of the 4 policies and 100 schools serving as the comparison group. The other 3 policies studied include the provision of cash block grants to schools, and the provision of group and individual-level performance pay to teachers. Details are presented in companion papers.

extra contract teacher to schools? (ii) How does the impact of an extra contract teacher vary by school and student characteristics? (iii) How does the behavior of contract teachers compare with those of regular teachers? and (iv) Would expanding public spending on primary education by hiring more contract teachers move schools closer to the efficient frontier of education production?

At the end of two years of the program, we find that students in schools with an extra contract teacher perform significantly better than those in comparison schools by 0.16 and 0.10 standard deviations (SD) in math and language tests respectively, suggesting that even untrained teachers with less education than regular civil-service teachers were able to improve student learning levels. The mean treatment effect of 0.14 SD across subjects is equal to 5.5 percentile points at the median of a normal distribution. The magnitude of the program effect was very similar in the first and second year of the program, which provides confidence in the stability of our estimates.

We find strong evidence of heterogeneous treatment effects along two dimensions (which hold after both one year and two years of the program). First, students in remote schools benefit more from the extra contract teacher suggesting that the marginal benefit of an additional contract teacher is higher in these areas. Second, we also find that the largest gains in test scores in treatment schools are for students in the first grade (averaging 0.23 and 0.25 SD in math and language). One of the mechanisms for program impact is likely to have been the reduction in class sizes made possible by the presence of the extra contract teacher in the program schools (though the differential incentives faced by the contract teachers means that we are not evaluating a typical class-size reduction). Using the random assignment of an extra contract teacher to the school as an instrument for class size, we find that IV estimates of the impact of class size on learning outcome decline steadily as the grades increase, with no significant impact in grades four, and five (the OLS estimates show the same pattern of declining effect of class size by grade). To the extent that the program impact operates through reduced class sizes, our results suggests that class size reductions enabled by hiring additional contract teachers have the greatest benefit for the youngest children.

We also find evidence to support the idea that contract teachers face superior incentives than regular civil-service teachers. Contract teachers were significantly less

likely to be absent than regular teachers (16% versus 27%) and more likely to be engaging in active teaching (46% versus 39%). We also find that contract teachers with higher rates of absence are less likely to have their contracts renewed, suggesting that the contract renewal decision is correlated with measures of performance and that this could be an important source of incentives for higher effort on the part of contract teachers.

While the experiment studied here shows positive effects of hiring an extra contract teacher on learning outcomes (which is not obvious, given the lack of training and the lower qualifications of the contract teachers)<sup>9</sup>, it does not by itself answer the question of whether increasing the use of contract teachers would improve productive efficiency in education. Economic theory suggests that optimal production would use expensive highly-qualified regular teachers and inexpensive less-qualified contract teachers in the proportion where the ratio of marginal costs equals the ratio of marginal productivity. However, since the public sector typically does not have the incentives to optimize productive efficiency (Hanushek, 2004), looking at the production function of private schools may provide such a benchmark.<sup>10</sup> We collect data on rural private school teachers in the same districts where the contract teacher experiment was carried out and find teacher salaries in private schools are much lower than those of regular civil-service teachers and we cannot reject the null that mean private school teacher salaries are equal to contract teacher salaries. Expanding the use of contract teachers may therefore move education production in public schools closer to the efficient frontier.

Our results contribute to an emerging literature on understanding the impact of contract teachers in developing countries. In addition to a few descriptive studies regarding the use of contract teachers,<sup>11</sup> recent papers that use observational data to study the effect of contract teachers include De Laat and Vegas (2005) in Togo, Bourdon et al

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<sup>9</sup> For instance, the well-known Tennessee STAR experiment found a positive effect on test scores of reducing class sizes with a regular teacher, but not with less-qualified teacher-aides.

<sup>10</sup> Since private schools have to compete for fee-paying parents (who choose private schools over free public schools), they are likely to have better incentives to discover the efficient frontier of education production since efficiency gains allow them to be more competitive.

<sup>11</sup> Notable among these are Duthilleul (2005) describing experiences with contract teachers in Cambodia, India, and Nicaragua, and Govinda and Josephine (2004) who conduct a detailed review of contract teachers (also known as para-teachers) in India and summarize the key arguments for and against the use of contract teachers in India. The 3 case studies in Pritchett and Pande (2006) also provide a good discussion on locally-hired contract teachers in India

(2006) in Niger, and Bourdon et al (2007) in Niger, Mali, and Togo.<sup>12</sup> Duflo et al (2007) conduct an experimental evaluation of a contract teacher program in Kenya and find that students randomly assigned to contract teachers (and whose class size was halved) do significantly better than students in comparison schools, while students assigned to regular teachers in program schools (where the class size was also halved) do no better than those in the comparison schools. They conclude therefore that class-size reductions don't by themselves improve learning outcomes unless accompanied by an improvement in teacher incentives such as a renewable contract.

One important difference between our paper and Duflo et al (2007) is that they consider the case of equally qualified teachers who operate under a different contract than regular civil-service teachers. In the Indian context, however, the contract teachers are less educated on average (Table 1 – Panel A), and typically have very little formal training as teachers.<sup>13</sup> This lack of training has been an important reason for criticism of the use of contract teachers (Kumar et al, 2005). Our paper provides the first experimental evaluation of an “as is” expansion of the use of untrained low-cost contract teachers), and is conducted in a representative sample of schools across a large Indian state.<sup>14</sup>

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<sup>12</sup> Using a data set from Togo, De Laat and Vegas (2005) control for observable differences in student and teacher characteristics and find that students of regular teachers perform better than those of contract teachers. Bourdon et al (2006) use data from Niger and conclude that after controlling for confounding factors, contract teachers do not perform much worse than regular teachers. Bourdon et al (2007) use data from Togo, Niger, and Mali and find differential effects across these countries (positive effects in Mali, mixed effects in Togo, and negative effects in Niger) and suggest that these may be explained by differences in how contract teacher programs were implemented in these countries, with positive effects found where the contract teachers were managed through local communities and negative effects where contract teacher hiring was centralized.

<sup>13</sup> A paper that is close to this context is Banerjee et al (2007), who conduct an experimental evaluation of a remedial education program staffed by informal teachers in the Indian cities of Mumbai and Vadodara and find that the program was highly effective in improving learning outcomes. However, this program was a targeted intervention run by an NGO that combined informal teachers with a specific focus on remedial education for the weakest children in the class and featured the pedagogic innovation of removing these children from the classroom for a few hours each day, and is thus quite different from the typical contract teacher policies implemented in several Indian states over the past two decades.

<sup>14</sup> The random assignment of treatment provides high internal validity to the estimates of program impact, while the random sampling of schools into the universe of the study provides greater external validity than typical experiments by avoiding the “randomization bias”, whereby entities that are in the experiment are atypical relative to the population that the result is sought to be extrapolated to (Heckman and Smith (1995)).

The results in this paper are also relevant to the literature on service delivery in remote areas (see Jacob, Kochar, and Reddy (2008) on the impact of sub-scale schools on learning outcomes in India, and Muralidharan et al (2009) on the impact of remote area allowances for teachers in Indonesia), the literature on decentralization and accountability in the provision of public services (see Bardhan (2002) for a theoretical discussion, and Sawada and Ragatz (2005) on decentralization in education delivery in the EDUCO program in El Salvador), and the literature on the relative efficiency of public and private production of education (Hanushek (2002), Pritchett (2004)).

Finally, we also contribute to the extensive class-size literature in developed and developing countries, and add to the small set of class size experiments.<sup>15</sup> While not strictly a class-size intervention (since the contract teachers have different incentives) our results provide empirical support to the theory put forward in Lazear (2001) that small class sizes matter most when the probability of disruptive children is higher – which is likely to be the case when children are younger and especially when they are new to school. The results are also consistent with Krueger (1999) who finds that the largest gains from class-size reductions accrue to children in grade one.

The rest of this paper is organized as follows: section 2 describes the experimental intervention and data collection, section 3 presents the results of the extra contract teacher program, section 4 compares regular and contract teachers to private school teachers, and section 5 discusses policy implications and concludes.

## **2. Experimental Design**

### **2.1. Context**

Andhra Pradesh (AP) is the 5<sup>th</sup> largest state in India, with a population of over 80 million, 73% of whom live in rural areas. AP is close to the all-India average on various measures of human development such as gross enrollment in primary school, literacy, and infant mortality, as well as on measures of service delivery such as teacher absence (Figure 1a). The state consists of three historically distinct socio-cultural regions (Figure 1b) and a total of 23 districts. Each district is divided into three to five divisions, and

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<sup>15</sup> References based on US evidence include Krueger (1999, 2003) and Hanushek (1999, 2003). Angrist and Lavy (1999) and Urquiola (2006) provide international evidence. Krueger (1999) reports results from the Tennessee STAR project, which is probably the most well known class-size reduction experiment.

each division is composed of ten to fifteen mandals which are the lowest administrative tier of the government of AP. A typical mandal has around 25 villages and 40 to 60 government primary schools. There are a total of over 60,000 such schools in AP and over 80% of children in rural AP attend government-run schools (Pratham, 2008).

The average rural government primary school is quite small, with total enrollment of around 80 to 100 students and an average of 3 teachers across grades one through five.<sup>16</sup> One teacher typically teaches all subjects for a given grade (and often teaches more than one grade simultaneously). All regular teachers are employed by the state, and their salary is mostly determined by experience and rank, with minor adjustments based on postings, but no component based on any measure of performance. The average salary of regular teachers is over Rs. 8,000/month and total compensation (including benefits) is over Rs. 10,000/month (per capita income in AP is around Rs. 2,000/month). Regular teachers' salaries and benefits comprise over 90% of non-capital expenditure on primary education in AP. Incentives for attendance and performance are weak, with teacher absence rates of over 25% (Kremer et al (2005)). Teacher unions are strong and disciplinary action for non-performance is rare.<sup>17</sup>

## **2.2 The Extra Contract Teacher Intervention**

Contract teachers (also known as para-teachers) are generally hired at the school level by school committees and have usually completed either high school or college but typically have no formal teacher training. Their contracts are renewed annually and they are not protected by any civil-service rules. Their typical salary of around Rs. 1000 - 1500/month is less than one fifth of the average salary of regular government teachers.<sup>18</sup> They are also much more likely to be female, to be from the same village, and live closer to the school they teach in (Table 1 – Panel A). Contract teachers usually teach their own classes and are not 'teacher-aides' who support a regular teacher in the same classroom.

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<sup>16</sup> This is a consequence of the priority placed on providing all children with access to a primary school within a distance of 1 kilometer from their homes.

<sup>17</sup> Kremer et al (2005) find that on any given working day, 25% of teachers are absent from schools across India, but only 1 head teacher in their sample of 3000 government schools had ever fired a teacher for repeated absence. The teacher absence rate in AP is almost exactly equal to the all-India average.

<sup>18</sup> The salary of contract teachers was Rs. 1,000/month in the first year of the project (2005 – 06) and was raised to Rs. 1,500/month in the second year (2006 – 07).

The process by which contract teachers are typically hired in Andhra Pradesh is that schools apply to the district education administration for permission to hire a contract teacher based on their enrolment and teacher strength at the start of the school year. Thus contract teachers can be appointed both against vacant posts (that may have been filled by a regular teacher) and as additional resources to meet the needs of growing enrollment. If the permission (and fiscal allotment) is given, a contract teacher will be hired by the school committee. The authorization of the position is not guaranteed for subsequent years, but once a position is approved, it is usually continued unless there are significant changes in enrollment patterns. But since renewal is not guaranteed, the appointment of contract teachers is typically for a 10-month period.<sup>19</sup>

The extra contract teacher intervention studied in this paper was designed to resemble the typical process of contract teacher hiring and use as closely as possible. Schools that were selected for the program by a lottery were informed in a letter from the District administration that they had been authorized to hire an additional contract teacher, and that they were expected to follow the same procedures and guidelines for hiring a contract teacher as they would normally do. Also, the teachers were allocated to the school and not to a specific grade or pre-specified role, which is also how teachers (regular and contract) are typically allocated to primary schools.

Most schools (~80%) reported starting the process of hiring the extra contract teacher within a week of receiving the notification and the modal selection committee consisted of three members (the head teacher, a member of the local elected body, and another teacher). The most important stated criterion for hiring the contract teacher was qualification (62%), followed by experience and distance from the school (20% each). The additional contract teachers hired under this program had the same average characteristics as typical contract teachers in the comparison schools (Table 1 – Panel B), and so the intervention mimicked an expansion of the existing contract teacher program in AP to 100 randomly selected schools.

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<sup>19</sup> See Govinda and Yazali (2004) for a more detailed description of contract teacher appointment procedures across Indian states.

### 2.3. Sampling and Randomization

We sampled 5 districts across each of the 3 socio-cultural regions of AP in proportion to population (Figure 1b).<sup>20</sup> In each of the 5 districts, we randomly selected one division and then randomly sampled 10 mandals in the selected division. In each of the 50 mandals, we randomly sampled 10 schools using probability proportional to enrollment. Thus, the universe of 500 schools in the study was representative of the schooling conditions of the typical child attending a government-run primary school in rural AP. Experimental results in this sample can therefore be credibly extrapolated to the full state of Andhra Pradesh.

As mentioned earlier, the extra contract teacher program was one of four policy options evaluated under the AP RESt project, with 100 schools being randomly assigned to each of the treatments. The school year in AP starts in mid June, and baseline tests were conducted in the 500 sampled schools during late June and early July, 2005.<sup>21</sup> After the baseline tests were evaluated, the Azim Premji Foundation randomly allocated 2 out of the 10 project schools in each mandal to one of 5 cells (four treatments and one control). Since 50 mandals were chosen across 5 districts, there were a total of 100 schools (spread out across the state) in each cell. The geographic stratification allows us to estimate the treatment impact with mandal-level fixed effects and thereby net out any common factors at the lowest administrative level of government.

Since no school received more than one treatment, we can analyze the impact of each program independently with respect to the control schools without worrying about any confounding interactions. This analysis in this paper is based on the 200 schools that comprise the 100 schools randomly chosen for the extra contract teacher (ECT) program and the 100 that were randomly assigned to the comparison group. Table 2 (Panel A) shows summary statistics of baseline school and student characteristics for both treatment

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<sup>20</sup> Subject to the selected districts within a region being contiguous for ease of logistics and supervision.

<sup>21</sup> The selected schools were informed by the government that an external assessment of learning would take place in this period, but there was no communication to any school about any of the treatments at this time.

and comparison schools, and we see that the null of equality across treatment groups cannot be rejected for any of the variables.<sup>22</sup>

#### **2.4. Data Collection**

The data used in this paper comprise of independent learning assessments in math and language (Telugu) conducted at the beginning of the study, and at the end of each of the two years of the experiment. We also use data from regular unannounced “tracking surveys” made by staff of the Azim Premji Foundation to measure process variables such as teacher attendance and teaching activity.<sup>23</sup> The treatment and comparison schools operated under identical conditions of information and monitoring and only differed in the treatment that they received. This ensures that Hawthorne effects are minimized and that a comparison between treatment and control schools can accurately isolate the treatment effect.

The tests used for this study were designed by India’s leading education testing firm and the difficulty level of questions was calibrated in a pilot exercise to ensure adequate statistical discrimination on the tests. The baseline test (June-July, 2005) covered competencies up to that of the previous school year. At the end of each project school year (March-April, 2006 and 2007), schools had two rounds of tests with a gap of two weeks between them. The first test (the 'lower endline') covered competencies up to that of the previous school year, while the second test (the 'higher endline') tested skills from the current school year's syllabus. Doing two rounds of testing at the end of each year allows the testing of more materials, and improves power by allowing the smoothing of measurement errors specific to the day of testing.

For the rest of this paper, Year 0 (Y0) refers to the baseline tests in June-July 2005; Year 1 (Y1) refers to both rounds of tests conducted at the end of the first year of the program in March-April, 2006; and Year 2 (Y2) refers to both rounds of tests conducted at the end of the first year of the program in March-April, 2007.

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<sup>22</sup> Table 2 shows sample balance between the comparison schools and those that received an extra contract teacher, which is the focus of the analysis in this paper. The randomization was done jointly across all 5 treatments shown in Table 3.1, and the sample was also balanced on observables across the other treatments.

<sup>23</sup> Six visits were made to each school in the first year (2005 – 06), while four visits were made in the second year (2006 – 07)

### 3. Results

#### 3.1. Teacher and Student Turnover and Attrition

Regular civil-service teachers in AP are transferred once every three years on average. While this could potentially bias our results if more teachers chose to stay in or tried to transfer into the ECT schools, it is unlikely that this was the case since the treatments were announced in August '05, while the transfer process typically starts earlier in the year. There was no statistically significant difference between the treatment and comparison groups in the extent of teacher turnover, and the transfer rate was close to 33%, which is consistent with rotation of teachers once every 3 years (Table 2 – Panel B).

As part of the agreement between the Government of AP and the Azim Premji Foundation, the Government agreed to minimize transfers into and out of the sample schools for the duration of the study. As a result, the average teacher attrition in the second year was only 6%,<sup>24</sup> but it is slightly higher in the treatment schools (9%) than in the comparison schools (4%). It does not look like *all* these teachers were replaced since the fraction of teachers in the schools at the end of the school year who were also there in the beginning is identical in both groups (95%), and so it is possible that the existence of the “extra” contract teachers may in a few cases have led to non-replacement of departing regular teachers in the second year. The magnitude of this bias is likely to be small, and any bias caused by this would lower the estimates of the second year program impact, and so the estimates presented here are likely to be a lower bound on the program impact.

The average student attrition rate in the sample (defined as the fraction of students in the baseline tests who did not take a test at the end of each year) was 7.7% and 24.5% in year 1 and year 2 respectively, but there is no significant difference in attrition across the groups. Attrition is higher among students with lower baseline scores in both treatment and comparison groups, but we find no difference in mean baseline test scores across the groups among the students who drop out from the test.

#### 3.2. Specification

Our default specification uses the form:

$$T_{ijkm}(Y_n) = \alpha + \gamma_j \cdot T_{ijkm}(Y_0) + \delta \cdot ECT + \beta \cdot Z_m + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (3.1)$$

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<sup>24</sup> There was also a court order to restrict teacher transfers in response to complaints that teacher transfers during the school year were disruptive to students. This may have also helped to reduce teacher transfers during the second year of the project.

The main dependent variable of interest is  $T_{ijkm}$ , which is the normalized test score on the specific test (normalized with respect to the score distribution of the comparison schools), where  $i, j, k, m$  denote the student, grade, school, and mandal respectively.  $Y_0$  indicates the baseline tests, while  $Y_n$  indicates a test at the end of  $n$  years of the treatment. Including the normalized baseline test score improves efficiency due to the autocorrelation between test-scores across multiple periods.<sup>25</sup> All regressions include a set of mandal-level dummies ( $Z_m$ ) and the standard errors are clustered at the school level. Since the treatments are stratified by mandal, including mandal fixed effects increases the efficiency of the estimate. We also run the regressions with and without controls for household and school variables.

The 'ECT' variable is a dummy at the school level indicating if it was selected to receive the extra contract teacher (ECT) program, and the parameter of interest is  $\delta$ , which is the effect on the normalized test scores of being in an ECT school. The random assignment of treatment ensures that the 'ECT' variable in the equation above is not correlated with the error term, and the estimate of the one-year and two-year treatment effects are therefore unbiased.

Note that specification in (3.1) can be used to consistently estimate the one-year and two-year effect of the program, but not the second year effect alone (with Y2 scores as the dependent variable controlling for Y1 scores) because Y1 scores are a post-treatment outcome that are correlated with the treatment. Thus, specifications with Y2 scores as the dependent variable controlling for Y1 scores will not provide consistent estimates of  $\delta$  for the second year of the program.

### 3.3. Impact of ECT program on Test Scores

Averaging across both math and language, students in program schools scored 0.09 standard deviations (SD) higher than those in comparison schools at the end of the first year of the program, and 0.14 SD higher at the end of the second year (Table 3 – Panel A, columns 1 and 5). The impact of the program at the end of two years is greater in math (0.16 SD) than in language (0.10 SD) as seen in Panels B and C of Table 3. The addition

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<sup>25</sup> Since grade 1 children did not have a baseline test, we set the normalized baseline score to zero for these children (similarly for children in grade 2 at the end of two years of the treatment).

of school and household controls does not significantly change the estimated value of  $\delta$ , confirming the validity of the randomization (columns 2 and 6).

Column 3 of Table 3 shows the results of estimating equation (3.1) with Y2 scores on Y1 scores. This estimate is biased as discussed above and is not the main focus of our discussion, but taking these numbers as *illustrative*<sup>26</sup> suggests that the treatment effect of the ECT program was comparable across both years (0.09 SD in both years). However, the two-year treatment effect of 0.14 SD is not the sum of these two effects because of depreciation of prior gains. A more detailed discussion of depreciation of learning is beyond the scope of this paper, but the important point to note is that calculating the average treatment effect by dividing the “n” year treatment effect by “n” years, will typically underestimate the impact of the treatment beyond the first year relative to the counterfactual of discontinuation of the treatment. On the other hand, if the effects of most educational interventions exhibit fade out, then it is likely that extrapolating one-year treatment effects will typically overstate the long-term impact of programs, which highlights the importance of carrying out long-term follow ups of even experimental evaluations in order to do better cost-benefit calculations.<sup>27</sup>

### **3.4. Heterogeneous treatment effects by grade**

Disaggregating the treatment effects by grade, we find that children in the first grade in treatment schools show the largest gains, scoring 0.20 SD and 0.27 SD better in the first and second year respectively (Table 4 – columns 1 and 2). Given sampling variation, we must exercise caution in inferring heterogeneous treatment effects, unless the same pattern is repeated over multiple years. Finding the same results (of highest treatment effect for grade one) in both years, therefore gives us confidence in the inference that the program had the greatest benefits for students in grade 1.

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<sup>26</sup> Andrabi et al (2008) show that the downward bias from measurement error and the upward bias from unobserved ability in the estimation of  $\gamma$  roughly cancel out in another South Asian context (primary education in Pakistan), which suggests that the estimates in column 3 are unlikely to change much even after making the adjustments they do. In any case, we consider the estimate of the second year treatment effect alone to be illustrative only.

<sup>27</sup> The issue of persistence/depreciation of learning has only recently received attention in the literature on the effects of education interventions on test scores over multiple years. See Andrabi et al (2008) and Jacob et al (2008) for a more detailed discussion of issues involved with estimating the extent of persistence of interventions, and the implications for cost-benefit analysis.

Note, however, that the extra contract teacher is assigned to the school as opposed to a specific class to exactly mimic the actual contract teacher assignment procedures. Thus, the choice of how to assign the teacher is made at the school level and it could have been possible that schools chose to reduce class sizes the most in grade 1. Table 5 shows the effective class size<sup>28</sup> experienced by students in each grade in both treatment and comparison schools. We see that in most cases, there was a significant reduction in effective class size for all grades in both years of the program, with the largest reductions being achieved in grade 3. Also, in the second year, we cannot reject the null hypothesis that the effective class size reduction was the same in all grades, though in the first year, we do reject the null and it appears that most of the effective class size reductions were in grades 1 to 3 and not 4 and 5.

However, even though the assignment of a teacher to specific grades/classes is endogenous to the school, we can use the random assignment of the extra contract teacher as an instrument for class size and estimate the impact of class size on learning outcomes across different grades. Table 6 shows the OLS and IV estimates of the log of class size on learning outcomes (for each year and combining both years of data), and we see that the impact of class size steadily declines as the grades go up.

Note that the estimates in Table 6 are not precise estimates of the impact of a typical class-size reduction caused by hiring more regular teachers, because the class-size reduction is confounded with the different incentives faced by the contract teachers and those induced among regular teachers by the presence of the contract teacher. However, the point we emphasize is the sharp declining trend of the impact of class size (brought about by any combination of regular and contract teachers) on learning outcomes across the grades of primary school.

Thus, the reported treatment effects by grade show that providing schools with an extra contract teacher leads to greatest benefits for children in the first grade. The figures in Table 5 suggest that this was not simply a consequence of schools choosing to make

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<sup>28</sup> We use the term “effective class size” because of the common prevalence of multi-grade teaching whereby a single teacher simultaneously teaches more than one grade. Thus ECS in any school-grade combination is defined as the number of other students that a student in that school-grade simultaneously shares his/her teacher with. For example, consider a school with enrollment of 15, 20, 25, 15, and 15 in the five grades and with three teachers, with one teacher teaching grades 1 and 2, one teaching grade 3, and the last one teaching grades 4 and 5. In this case, the ECS in this school would be 35 in grades 1 and 2, 25 in grade 3, and 30 in grades 4 and 5.

larger class size reductions in grade 1 relative to other grades. Finally, the estimates in Table 6 suggest that the most likely mechanism for this result is the possibility that the class-size reductions brought about by having an extra contract teacher matter most for younger students. This is consistent with the education production function proposed in Lazear (2001), where the key insight is that classroom production of education is a public good where having a disruptive child produces a negative spillover effect for the rest of the class. Thus, small classes have greater benefits when the probability of having a disruptive child is higher. This is likely to be the case for younger children – especially those who are coming to school for the first time, as is the case with first grade students in Andhra Pradesh. Our results therefore provide empirical support for the theory of education production proposed in Lazear (2001) if we are willing to assume that the youngest children in primary school are likely to be most disruptive relative to their older peers who have been acclimatized to the schooling environment. Our findings are also consistent with Krueger (1999), who finds the largest benefits from small classes for students in grade 1 in the Tennessee STAR class-size reduction experiment.

### 3.5. Heterogeneous treatment effects by other school/student characteristics

We test for heterogeneity of the ECT program effect across student, and school characteristics by testing if  $\delta_3$  is significantly different from zero in:

$$T_{ijkm}(EL) = \alpha + \gamma \cdot T_{ijkm}(BL) + \delta_1 \cdot ECT + \delta_2 \cdot Characteristic + \delta_3 \cdot (ECT \times Characteristic) + \beta \cdot Z_m + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (3.2)$$

Table 7 shows the results of these regressions on several school and household characteristics, and each column represents one regression testing for heterogeneous treatment effects along the characteristic mentioned (the key row to pay attention to is the third one that reports the coefficients on the interactions).<sup>29</sup> Given the presence of several covariates in Table 7, caution should be exercised to avoid data mining for differential treatment effects since a few significant coefficients are likely simply due to

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<sup>29</sup> We also estimate the heterogeneity non parametrically, grouping each characteristic into quartiles and then quintiles, and test if the interaction of the incentive treatment and the top or bottom quartile (and quintile) is significantly different from the omitted category (the middle 2 quartiles and 3 quintiles) and if the interaction with the top and bottom quintiles are significantly different from each other. The results are unchanged and so the table only reports the linear specification in (3.2).

sampling variability. Thus, we only infer evidence of heterogeneous treatment effects from consistent evidence across multiple years.

The main result is that schools in more remote areas consistently benefit more from the addition of an extra contract teacher. The proximity index aggregates 8 variables (coded from 1-3)<sup>30</sup> indicating proximity to a paved road, a bus stop, a public health clinic, a private health clinic, public telephone, bank, post office, and the mandal educational resource center. Thus an index of 8 means the school is close to all 8 of the facilities, while a score of 24 indicates a school being far away from all of them. The strong and significant positive coefficient on this interaction in both years shows that the marginal benefit of the extra contract teacher was highest in the most remote areas. A related (but weaker) result is that schools with poorer infrastructure and with fewer students also benefit more from the extra contract teacher (the interactions with infrastructure and number of students are negative and significant after two years, and negative though not significant after the first year).

The other interesting result is the *lack* of heterogeneous treatment effects by several household and child-level characteristics. In particular, if we consider the baseline test score to be a summary statistic of all prior inputs into the child's education, then the lack of any significance on the interaction of the program with baseline scores suggests that all children benefited equally from the program regardless of their initial level of learning and that the gains from the program were quite broad. Similarly, there was no difference in program effectiveness based on household affluence, parental literacy, caste, and gender of the child.

### **3.6. Differences in Teacher Behavior by Contract Status**

One limitation of the current experimental design is that unlike Duflo et al (2007), we cannot experimentally compare the impact of a contract teacher versus that of a regular teacher.<sup>31</sup> However, we can compare the incentives for accountability faced by the two

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<sup>30</sup> The coding roughly corresponds to the nearest third, middle third, and furthest third of the schools on each metric. Converting to a common code based on the distribution of the raw distance allows the units to be standardized.

<sup>31</sup> Since our evaluation is designed to evaluate an 'as is' expansion of the contract teacher program, it was important to adhere to the key feature of the actual policy, which is that teachers are assigned to schools

types of teachers using our measures of teacher absence and teaching activity that are collected by direct physical observation during unannounced visits to the schools.

Table 8 – Panel A shows that contract teachers had significantly lower levels of absence compared to regular teachers (16% versus 27% on average over two years), with the difference being higher in the second year (12.6%) compared to the first year (8.9%). Contract teachers also had higher rates of teaching activity compared to regular teachers (46% versus 39%), though these numbers are less reliable than the absence figures, because it is easier for an idle teacher to start teaching when he/she sees an enumerator coming to the school than for an absent teacher to materialize during a surprise visit to the school.

These differences in rates of absence and teaching activity are even higher with school fixed effects, suggesting that the presence of the contract teachers may have induced regular teachers to shirk a little more. We can test this directly by comparing the absence rates of regular teachers in comparison schools with those in program schools and we see that regular teachers in program schools do have higher rates of absence and lower rates of teaching activity than their counterparts in comparison schools (Table 8 – Panel B), and that these differences are significant when aggregated across both years of the program.

Thus, we do see that contract teachers show significantly superior performance on measures such as attendance and teaching activity. This could be due to a combination of being from the local area and feeling more connected to the community, living much closer to the school and therefore having lower marginal costs of attendance, or the superior incentives from being on annually renewable contracts without the job security of civil-service tenure.

We find some suggestive evidence of the last point by looking at the correlates of renewal of the contract teachers. On average, less than 50% of contract teachers are renewed from one year to the next (Table 9A), and thus, contract renewal is by no means certain. We correlate the probability of contract renewal with both measures of teacher performance (attendance and teaching activity) and measures of teacher qualifications

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and not to grades. In any case, even if a teacher were assigned to a specific grade, schools would be likely to reassign duties to balance the workload among teachers.

(education and training) and find that teachers with lower absence rates are more likely to have their contracts renewed (Table 9B). However, both qualifications and teacher training (which are stated as important criteria for hiring) are not associated with the renewal decision (and the point estimates are negative), suggesting that the qualifications were not correlated with the school committee's perception of actual teacher quality (to the extent that the renewal decision is influenced by this). The regressions in Table 9B are correlations and should not be interpreted causally (for instance, teachers who know they plan to leave may be more absent), but they are suggestive that performance-contingent contract renewal may be a source of superior incentives for contract teachers.

#### **4. Public and Private Production of Education**

The results presented so far show that providing an extra contract teacher to schools led to significant improvements in student learning, but this experiment does not tell us if expanding the use of contract teachers would be efficient relative to hiring additional regular teachers. However, given the large cost difference (regular teachers are paid over five times more than contract teachers)<sup>32</sup>, the policy-relevant question is not the comparison of a regular teacher to a contract teacher (which is what the literature has focused on), but rather the comparison of one regular teacher to several contract teachers.

Economic theory suggests that optimal production would use expensive highly-qualified regular teachers and inexpensive less-qualified contract teachers in the proportion where the ratio of marginal costs equals the ratio of marginal productivity. However, as several studies have pointed out (Hanushek (2002) and Pritchett (2004) are good examples), public production of education rarely uses inputs in the most cost effective way to achieve a desired level of education outputs. A useful benchmark for efficient production of education may be to look at the teacher profiles of private schools in the same districts.

A prominent feature of primary education in India over the past ten years has been the rapid increase in the number of private schools (Muralidharan and Kremer, 2005)

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<sup>32</sup> Reasons for this wage premium are likely to include higher education (and corresponding outside opportunities), a compensating differential to locate to remote areas (since most regular teachers live in cities) and a union/civil-service premium. We don't aim to decompose the wage premium in this discussion, but to focus on the optimal ratio of expensive highly-qualified and inexpensive less-qualified teachers.

catering to an increasing number of students with nearly 20% of primary school students in rural Andhra Pradesh attending a fee-charging private school (Pratham, 2008). Since private schools need to compete against free public schools as well as other fee-charging schools for students and also need to compete for teachers (and their characteristics), they face better incentives than public schools to operate close to the efficient frontier of education production, where the desired quality of education is produced at the lowest possible cost.

Table 10 compares regular teachers, contract teachers, and private school teachers on a range of characteristics and shows that private school teachers are typically less qualified and trained than regular teachers, but more so than contract teachers. But, the main result is that the salary levels of private school teachers are much closer to the contract teachers than the regular teachers and we cannot reject the null that private school teacher salaries are not different from those of the contract teachers. Figure 2 plots the salary distribution of teachers in government and private schools,<sup>33</sup> and we see that the distribution of salaries in private schools is around the range of the contract teachers' salaries, and there is almost support in the private school salary distribution corresponding to the top half of the government school salary distribution.

While private schools pay much lower teacher salaries on average, they also hire many more teachers per student and have student teacher ratios that are around a third that of the public schools with village fixed effects (Muralidharan and Kremer, 2005).<sup>34</sup> Thus to a first approximation, it appears that private schools pay salaries that are close to that of contract teachers, but hire many teachers.<sup>35</sup> To the extent that the input combination used by private schools is likely to be closer to the efficient frontier of education production, expanding the use of contract teachers in government-run schools may be a way of moving public production of education closer to the efficient frontier.

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<sup>33</sup> Figure 2 and Table 10 are preliminary and based on a limited sample (<20%) of data available from private schools that is being collected at present as part of a separate study on private schools.

<sup>34</sup> These figures from prior work by one of the authors will be replaced with actual student teacher ratios in private schools in the districts where the contract teacher experiment was carried out (the data is still being collected).

<sup>35</sup> One caveat is that equally qualified teachers may be willing to accept a lower salary in private schools if there are other compensating differentials like being able to teach more motivated students. However, private schools also compete with other private schools for teachers and so the market wage for private school teachers reflects competition even among the non-monetary dimensions of the job.

## 5. Conclusion

Regular teachers in India are highly qualified, but command a substantial wage premium (greater than a factor of five) over the market clearing wage of private school teachers that is explained partly by their better education and outside opportunities, partly by a compensating differential to locate to rural and remote areas, and partly by a union and civil-service premium. The hiring of contract teachers can be a much more cost-efficient way of adding teachers to schools because none of these three sources of wage premiums are applicable for them. However, since local contract teachers are not as highly qualified as regular civil-service teachers hired via a statewide selection process, opponents of the use of the contract teachers have posited that the use of contract teachers will not lead to improved learning.

Despite these concerns, the increased use of contract teachers has been one of the major education policy innovations in developing countries over the past two decades, as governments have struggled to cope with the fiscal and logistical demands on the existing education system posed by rapid increases in school enrolment and the need to provide universal access to education. While this has been a highly controversial policy (especially in India), there has been very little rigorous evidence on the impact of contract teachers on learning outcomes.

We present experimental evidence from an “as is” expansion of the existing contract teacher policy of the government of Andhra Pradesh, implemented in a randomly selected subset of 100 schools among a representative sample of schools in rural AP. We find that adding a contract teacher significantly improved average learning outcomes in treatment schools, and especially benefited the children in the first grade (the first year of formal schooling since there is no kindergarten) and those in more remote areas. We also find that contract teachers show superior performance on measures such as attendance and teaching activity and that measures of performance such as attendance are positively correlated with the probability of contract renewal.

The combination of low cost, superior performance measures than regular teachers on attendance and teaching activity, and positive program impact suggest that expanding the use of contract teachers could be a highly cost effective way of improving primary

education outcomes in developing countries. We do not claim that a contract teacher is more effective than a regular teacher (and have not addressed the question of relative effectiveness in this paper), but highlight that the large cost differences imply that the correct policy comparison ought to be not of a regular teacher versus a contract teacher, but rather between an additional regular teacher and *several* additional contract teachers. In particular, expensive policy initiatives to get highly qualified teachers to remote areas may be less cost effective than hiring several local contract teachers to provide much more attention to students at a similar cost. Observing the input choices of private schools suggests that there may be an extent to which quantity can more than make up for the lower qualifications of contract teachers (especially for primary schooling) and do so in a cost-effective way.

One caveat to keep in mind is that there may be complementarities between regular and contract teachers and that the effectiveness of contract teachers may depend on the availability of regular teachers to guide them and monitor their performance. However, since an expansion of the use of contract teachers would take place on top of the current stock of regular teachers, the experiment studied in this paper provides exactly the estimate needed to measure the impact of allocating additional education spending on the margin to expanding the use of contract teachers.

Opponents of the use of contract teachers worry that their expanded use may lead to a permanent second-class citizenry of contract teachers, which in the long-run will erode the professional spirit of teaching and shift the composition of the teacher stock away from trained teachers towards untrained teachers. Thus, even if expanding the use of contract teachers is beneficial in the short run, it might be difficult to sustain a two-tier system of teachers in the long run. Finally, the political economy concern is that hiring larger numbers of contract teachers will lead to demands to be regularized which may be politically difficult to resist given the strengths of teacher unions and if such regularization were to happen, it would defeat the purpose of hiring a large number of contract teachers in the first place.

One possible course of action is to hire all new teachers as contract teachers at the school-level, and create a system to measure their performance over a period of time (six to eight years for example) that would include inputs from parents, senior teachers, and

measures of value addition using independent data on student performance.<sup>36</sup> These measures of performance could be used in the contract-renewal decision at the end of each fixed-term contract (of say two years), and consistently high-performing contract teachers could be promoted to regular civil-service rank at the end of a fixed period of time. In other words, contract teachers need not be permanent adjunct faculty, but can be part of a performance-linked tenure track.<sup>37</sup> Teacher training and professional development during the summer breaks could be a natural component of this career progression.<sup>38</sup> Integrating contract and regular teachers into a career progression should help to address most of the concerns above, including the political economy ones.

Recent literature in the US suggests that there are large differences in teacher effectiveness (measured as teacher fixed effects in student value added regressions over time), but that these differences are only weakly predicted by observable characteristics of teachers including their level of education and training (Hanushek et al, 2005, Rockoff, 2004). In related work, the results from our parallel experimental study on teacher performance pay (Muralidharan and Sundararaman, 2008) suggest that even small amounts of performance-linked performance linked bonuses for teachers led to significant improvements in learning outcomes without negative consequences. Combining these results suggests that there could be significant gains from moving to a system of hiring teachers on fixed-term contracts and then using performance measures to pay bonuses on an ongoing basis and to inform the tenure decision after a longer period of performance measurement. Such a system could both move the entire distribution of teacher effectiveness to the right (by increasing effort) and further increase average teacher effectiveness by not renewing the contracts of ineffective teachers.

As governments in developing countries face the challenge of improving education quality, it is not clear that expanding spending along existing patterns is the most effective way of doing so. With over 25% of teachers in India absent and less than half of them engaged in teaching activity at any point in time, improving teacher incentives is

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<sup>36</sup> Gordon et al (2006) provide a similar recommendation for the US (as part of the Hamilton Project) on identifying effective teachers through measuring their on the job performance.

<sup>37</sup> A similar proposal is made by Pritchett and Pande (2006).

<sup>38</sup> Under the status quo, government officials who administer the education department typically don't even *want* the contract teachers to get trained, since that increases the risk of legal action on the part of contract teachers to be treated on par with regular civil-service teachers. Harmonizing the two cadre of teachers by making them both of a career progression would mitigate this issue.

an imperative for effective use of public funds. The use of teachers on fixed-term renewable contracts can be a highly cost effective policy for improving student learning outcomes, especially if placed in the context of a long-term professional career path that rewards effort and effective teaching at all stages of the teaching career.

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**Table 1: Characteristics By Teacher Type**

<b>Panel A: Regular versus Contract Teachers (Comparison Schools)</b>			
	Regular Teachers	Contract Teachers	P-value (H0: Diff=0)
<b>Male</b>	63.1%	31.8%	0.004***
<b>Age</b>	40.35	25.81	0.000***
<b>College Degree or Higher</b>	84.3%	45.5%	0.000***
<b>Formal Teacher Training Degree or Certificate</b>	98.3%	9.1%	0.000***
<b>Received any Training in last twelve months</b>	93.5%	54.5%	0.000***
<b>From the same village</b>	7.2%	81.8%	0.000***
<b>Distance from home to school (km)</b>	11.9	1.1	0.000***
<b>Teacher Salary (Rs./month)</b>	8698	1000 (1500)	0.000***

<b>Panel B: Comparison of Contract teacher characteristics in Comparison and Treatment schools</b>			
	Contract Teachers in Comparison Schools	Contract Teachers in Treatment Schools	P-value (H0: Diff=0)
<b>Male</b>	31.8%	35.3%	0.793
<b>Age</b>	25.81	28.09	0.197
<b>College Degree or Higher</b>	45.5%	47.1%	0.909
<b>Formal Teacher Training Degree or Certificate</b>	9.1%	14.7%	0.543
<b>Received any Training in last twelve months</b>	54.5%	38.2%	0.238
<b>From the same village</b>	81.8%	97.1%	0.052 *
<b>Distance from home to school (km)</b>	1.1	0.6	0.270
<b>Teacher Salary (Rs./month)</b>	1000 (1500)	1000 (1500)	0.219

**Notes:**

1. Table reports summary statistics from the first year of the project (2005 - 06). The teacher characteristics were similar in the second year as well. The only difference was that contract teacher salary was Rs. 1000/month in the first year, but increased to Rs. 1,500 across the entire state in the second year

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 2: Sample Balance Across Treatment and Comparison Groups**

				<b>Panel A</b>		
				<b>(Mean Pre-program Characteristics)</b>		
				[1]	[2]	[3]
				Comparison Schools	Extra Contract Teacher Schools	P-value (H0: Diff=0)
<b>School-level Variables</b>						
1	Total Enrollment (Baseline: Grades 1-5)			113.2	104.6	0.41
2	Total Test-takers (Baseline: Grades 2-5)			64.9	62.0	0.59
3	Number of Teachers			3.07	2.83	0.24
4	Pupil-Teacher Ratio			39.5	39.8	0.94
5	Infrastructure Index (0-6)			3.19	3.13	0.84
6	Proximity to Facilities Index (8-24)			14.65	14.97	0.55
<b>Baseline Test Performance</b>						
7	Math (Raw %)			18.4	17.3	0.33
8	Telugu (Raw %)			35.0	34.1	0.63
				<b>Panel B</b>		
				<b>(Mean Turnover/Attrition During Program)</b>		
				[1]	[2]	[3]
				Comparison Schools	Extra Contract Teacher Schools	P-value (H0: Diff=0)
<b>Teacher Turnover and Attrition</b>						
<b>Year 1 on Year 0</b>						
9	Teachers Who Stayed the Full Year/ Total in School Beginning of School Year (%)			0.70	0.69	0.80
10	Teachers Who Stayed the Full Year/ Total in School at End of School Year (%)			0.66	0.67	0.84
<b>Year 2 on Year 1</b>						
11	Teachers Who Stayed the Full Year/ Total in School Beginning of School Year (%)			0.96	0.91	0.04**
12	Teachers Who Stayed the Full Year/ Total in School at End of School Year (%)			0.95	0.95	0.96
<b>Year 2 on Year 0</b>						
13	Teachers Who Stayed the Full Year/ Total in School Beginning of School Year (%)			0.68	0.63	0.24
14	Teachers Who Stayed the Full Year/ Total in School at End of School Year (%)			0.63	0.63	0.98
<b>Student Turnover and Attrition</b>						
<b>Year 1 on Year 0</b>						
15	Student Attrition (Students who did not take an endline test as a fraction of those who took a baseline test)			0.082	0.072	0.26
16	Baseline Maths test score of attritors			-0.15	-0.12	0.79
17	Baseline Telugu test score of attritors			-0.27	-0.27	0.97
<b>Year 2 on Year 0</b>						
18	Student Attrition (Students who did not take an endline test as a fraction of those who took a baseline test)			0.25	0.24	0.52
19	Baseline Maths test score of attritors			-0.13	-0.07	0.53
20	Baseline Telugu test score of attritors			-0.21	-0.17	0.67

**Notes:**

1. The infrastructure index sums binary variables showing the existence of a brick building, a playground, a compound wall, a functioning source of water, a functional toilet, and functioning electricity.

2. The proximity index sums 8 variables (coded from 1-3) indicating proximity to a paved road, a bus stop, a public health clinic, a private health clinic, public telephone, bank, post office, and the mandal educational resource center.

3. The t-statistics for the baseline test scores and attrition are computed by treating each student/teacher as an observation and clustering the standard errors at the school level (Grade 1 did not have a baseline test). The other t-statistics are computed treating each school as one observation

**Table 3: Impact of Extra Contract Teacher on Student Test Scores**

<b>Panel A: Combined</b>						
Dependent Variable = Normalized End of Year Test Score						
	<b>Year 1 on Year 0</b>		<b>Year 2 on Year 1</b>		<b>Year 2 on Year 0</b>	
	[1]	[2]	[3]	[4]	[5]	[6]
Extra Contract Teacher School	0.09 (0.034)**	0.09 (0.034)**	0.09 (0.036)**	0.07 (0.037)*	0.14 (0.043)***	0.12 (0.044)***
School and Household Controls	No	Yes	No	Yes	No	Yes
Observations	44506	41658	41624	35875	39030	34618
R-squared	0.33	0.35	0.32	0.33	0.21	0.23
<b>Panel B: Maths</b>						
Dependent Variable = Normalized End of Year Test Score						
	<b>Year 1 on Year 0</b>		<b>Year 2 on Year 1</b>		<b>Year 2 on Year 0</b>	
	[1]	[2]	[3]	[4]	[5]	[6]
Extra Contract Teacher School	0.10 (0.039)***	0.10 (0.040)**	0.09 (0.044)**	0.08 (0.045)*	0.16 (0.049)***	0.14 (0.050)***
School and Household Controls	No	Yes	No	Yes	No	Yes
Observations	22124	20710	20781	17918	19431	17236
R-squared	0.32	0.33	0.28	0.29	0.20	0.22
<b>Panel C: Telugu</b>						
Dependent Variable = Normalized End of Year Test Score						
	<b>Year 1 on Year 0</b>		<b>Year 2 on Year 1</b>		<b>Year 2 on Year 0</b>	
	[1]	[2]	[3]	[4]	[5]	[6]
Extra Contract Teacher School	0.07 (0.034)*	0.07 (0.034)**	0.08 (0.032)**	0.06 (0.033)*	0.10 (0.040)**	0.10 (0.041)**
School and Household Controls	No	Yes	No	Yes	No	Yes
Observations	22382	20948	20843	17957	19599	17382
R-squared	0.37	0.39	0.37	0.39	0.25	0.26

**Notes:**

1. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level. They also include normalized lagged test scores, which are set to 0 if the lagged test score does not exist (such as for students in grade 1 or for students in grade 2 in the 2-year regressions)
  2. The two year treatment effect regressions (Year 2 on Year 0) include students who entered grade 1 in the second year of the program and who were there in the schools at end of two years of the program, but who have only been exposed to the program for one year at the end of two years of the program.
  3. Constants are insignificant in all specifications and are not shown.
- \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 4: Impact of Extra Contract Teacher (ECT) by Grade**

Dependent Variable = Normalized Endline Test Score						
	Combined		Math		Telugu (Language)	
	Y1 on Y0	Y2 on Y0	Y1 on Y0	Y2 on Y0	Y1 on Y0	Y2 on Y0
	[1]	[3]	[4]	[6]	[7]	[9]
ECT * Grade 1	0.204 (0.078)***	0.267 (0.070)***	0.239 (0.082)***	0.213 (0.071)***	0.172 (0.079)**	0.323 (0.078)***
ECT * Grade 2	0.18 (0.058)***	0.117 (0.07)	0.188 (0.063)***	0.102 (0.08)	0.164 (0.060)***	0.134 (0.069)*
ECT * Grade 3	0.04 (0.05)	0.196 (0.071)***	0.074 (0.06)	0.238 (0.082)***	0.004 (0.05)	0.148 (0.071)**
ECT * Grade 4	0.10 (0.045)**	0.065 (0.06)	0.156 (0.055)***	0.098 (0.07)	0.047 (0.05)	0.028 (0.05)
ECT * Grade 5	(0.02) (0.05)	0.107 (0.050)**	-0.056 (0.05)	0.159 (0.065)**	0.012 (0.06)	0.061 (0.05)
Observations	44506	39030	22124	19431	22382	19599
F-Test (Equality Across Grades)	0.04	0.05	0.01	0.24	0.17	0.01
R-squared	0.34	0.22	0.32	0.2	0.37	0.24

**Notes:**

1. The two year treatment effect regressions (Year 2 on Year 0) include students who entered grade 1 in the second year of the program and who were there in the schools at end of two years of the program, but who have only been exposed to the program for one year at the end of two years of the program.

2. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level. They also include normalized lagged test scores, which are set to 0 if the lagged test score does not exist (such as for students in grade 1 or for students in grade 2 in the 2-year regressions).

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 5: Effective Class Size in ECT Schools versus Comparison Schools**

	Year 1			Year 2		
	Control	Treatment	Difference	Control	Treatment	Difference
<b>Class 1</b>	41.01	36.06	4.95*	42.51	32.70	9.82***
<b>Class 2</b>	40.43	33.75	6.68**	46.15	36.93	9.22***
<b>Class 3</b>	36.56	29.02	7.54***	41.72	30.46	11.26***
<b>Class 4</b>	34.88	33.84	1.04	38.46	31.59	6.87***
<b>Class 5</b>	35.93	32.57	3.36	38.77	33.89	4.88*
<b>p-value of F-test testing equality of ECS reduction across grades</b>	<b>0.04**</b>			<b>0.2</b>		

**Notes:**

1. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level.

2. ECS stands for Effective Class Size, and ECT stands for Extra Contract Teacher

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 6: Effect of Class-level ECS on Learning Outcomes Year 1**

<b>Year 1</b>										
Dependent Variable = Normalized Endline Test Score										
	Class 1		Class 2		Class 3		Class 4		Class 5	
	[1] OLS	[2] IV	[3] OLS	[4] IV	[5] OLS	[6] IV	[7] OLS	[8] IV	[9] OLS	[10] IV
(Log) School ECS	-0.352 (0.095)***	-1.459 (0.437)**	-0.261 (0.078)***	-0.691 (0.255)**	-0.222 (0.059)**	-0.101 (0.22)	-0.074 (0.07)	-0.844 (0.84)	-0.079 (0.08)	-0.066 (0.07)
Observations	5969	5945	7626	7590	8975	8899	10677	10585	11259	11169
R-squared	0.29	0.29	0.40	0.40	0.41	0.40	0.48	0.48	0.37	0.37
<b>Year 2</b>										
Dependent Variable = Normalized Endline Test Score										
	Class 1		Class 2		Class 3		Class 4		Class 5	
	[1] OLS	[2] IV	[3] OLS	[4] IV	[5] OLS	[6] IV	[7] OLS	[8] IV	[9] OLS	[10] IV
(Log) School ECS	-0.261 (0.084)***	-1.310 (0.448)**	-0.174 (0.073)**	-1.327 (0.603)**	-0.143 (0.050)**	-0.563 (0.238)**	-0.021 (0.050)	-0.735 (0.700)	-0.071 (0.046)	-0.029 (0.757)
Observations	8410	8410	8954	8954	11798	11798	12955	12955	15068	15068
R-squared	0.14	0.14	0.26	0.26	0.38	0.38	0.41	0.41	0.46	0.46
<b>Year 1 and Year 2 Stacked</b>										
Dependent Variable = Normalized Endline Test Score										
	Class 1		Class 2		Class 3		Class 4		Class 5	
	[1] <b>OLS</b>	[2] <b>IV</b>	[3] <b>OLS</b>	[4] <b>IV</b>	[5] <b>OLS</b>	[6] <b>IV</b>	[7] <b>OLS</b>	[8] <b>IV</b>	[9] <b>OLS</b>	[10] <b>IV</b>
(Log) School ECS	-0.310 (0.074)***	-1.411 (.355)***	-0.239 (0.053)***	-0.907 (0.235)***	-0.184 (.047)**	-0.414 (.178)**	-0.084 (.045)*	-0.357 (0.444)	-0.043 (0.046)	-0.184 (0.385)
Observations	12105	12081	14035	13999	17588	17512	20168	20076	22234	22144
R-squared	0.13	0.13	0.29	0.29	0.33	0.33	0.42	0.41	0.40	0.40

**Notes**

1. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level.

2. ECS stands for Effective Class Size

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 7: Heterogenous Treatment Effects**

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Log Number of Students	Proximity (8 - 24)	Infrastructure (0 - 6)	Household Affluence (0 - 7)	Parental Literacy	SC or ST (lower caste)	Gender	Baseline Score
<b>Year 2 on Year 0</b>								
Extra Contract Teacher	0.853 (0.414)**	-0.863 (.227)***	0.501 (.152)***	0.099 (0.072)	0.111 (.061)*	0.111 (0.05)**	0.121 (.050)**	0.121 (.045)***
Covariate	-0.051 (0.05)	-0.010 (0.01)	0.007 (0.034)	0.026 (.011)**	0.021 (.006)***	-0.057 (0.038)	0.013 (0.026)	0.447 (.023)***
<b>Interaction</b>	<b>-0.149</b> <b>(0.084)*</b>	<b>0.072</b> <b>(017)***</b>	<b>-0.120</b> <b>(.045)***</b>	<b>0.005</b> <b>(0.017)</b>	<b>0.001</b> <b>(0.010)</b>	<b>0.044</b> <b>(0.06)</b>	<b>(0.003)</b> <b>(0.039)</b>	<b>0.021</b> <b>(0.037)</b>
Observations	32894	32894	32894	31121	31121	31114	31121	32894
R-squared	0.24	0.25	0.24	0.24	0.24	0.24	0.24	0.23
<b>Year 1 on Year 0</b>								
Extra Contract Teacher	0.198 (0.33)	-0.211 (0.15)	0.202 (.117)*	0.013 (0.059)	0.054 (0.05)	0.089 (.037)**	0.078 (0.039)**	0.086 (.035)**
Covariate	-0.067 (0.04)	-0.008 (0.01)	0.018 (0.021)	0.019 (.009)**	0.023 (.005)***	-0.042 (0.030)	0.007 (0.017)	0.497 (.023)***
<b>Interaction</b>	<b>-0.023</b> <b>(0.07)</b>	<b>0.021</b> <b>(.011)**</b>	<b>-0.038</b> <b>(0.035)</b>	<b>0.018</b> <b>(0.014)</b>	<b>0.006</b> <b>(0.007)</b>	<b>-0.019</b> <b>(0.05)</b>	<b>0.006</b> <b>(0.030)</b>	<b>0.019</b> <b>(0.029)</b>
Observations	44506	43209	43209	42860	42860	42848	42858	44506
R-squared	0.33	0.34	0.34	0.34	0.34	0.34	0.34	0.33

**Notes:**

1. The infrastructure index sums binary variables showing the existence of a brick building, a playground, a compound wall, a functioning source of water, a functional toilet, and functioning electricity.

2. The proximity index sums 8 variables (coded from 1-3) indicating proximity to a paved road, a bus stop, a public health clinic, a private health clinic, public telephone, bank, post office, and the mandal educational resource center.

3. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 8: Behavior of Contract Teachers versus Regular Teachers**

<b>Panel A : Contract Teachers versus Regular Teachers</b>					
<b>Teacher Absence</b>					
	Contract Teachers (%)	Regular Teachers (%)	Difference (%)	Difference with mandal fixed effects	Difference with School Fixed Effects
Year 1	15.2%	24.1%	-8.9%***	-9.8%***	-10.6%***
Year 2	16.7%	29.4%	-12.6%***	-13.4%***	-17.0%***
Combined	16.0%	26.8%	-10.8%***	-11.8%***	-13.6%***
<b>Teacher Activity</b>					
	Contract Teachers (%)	Regular Teachers (%)	Difference (%)	Difference with mandal fixed effects	Difference with School Fixed Effects
Year 1	49.4%	45.2%	4.2%***	6.3%**	7.3%***
Year 2	43.8%	35.4%	8.4%***	9.0%***	9.8%***
Combined	45.9%	39.0%	6.9%**	7.8%***	8.4%***

**Panel B : Regular Teachers in ECT Schools versus Regular Teachers in non-ECT Schools**

<b>Teacher Absence</b>				
	Regular teachers in ECT schools	Regular teachers in non-ECT schools	Difference (%)	Difference with mandal fixed effects
Year 1	24.5%	23.8%	0.7%	0.1%
Year 2	31.2%	25.8%	5.4%**	5.5%***
Combined	27.4%	24.8%	2.7%	2.4%*
<b>Teacher Activity (Active Teaching)</b>				
	Regular teachers in ECT schools	Regular teachers in non-ECT school	Difference (%)	Difference with mandal fixed effects
Year 1	41.7%	48.6%	-6.9%*	-6.4%***
Year 2	35.7%	36.5%	-0.8%	-0.8%
Combined	38.5%	41.9%	-3.4%	-3.2%**

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 9A: Probability of Contract being Renewed**

<b>Year 1 to Year 2</b>	<b>Year 2 to Year 3</b>	<b>Year 1 to Year 3</b>
47%	46%	25%

**Table 9B: Correlates of Contract Renewal**

	[1]	[2]	[3]	[4]
	Mean Teacher Absence in Previous Year	Mean Teaching Activity in Previous Year	College Degree	Teacher Training
	<b>Combined over 2 years</b>			
Probability of Contract Renewal	-0.226 (0.123)*	0.175 (0.12)	-0.041 (0.100)	-0.068 (0.105)
Observations	284	284	171	171

**Table 10: Private School Teacher Characteristics (PRELIMINARY)**

	Regular Teachers	Contract Teachers	Private School Teachers	P-Value (H0: Contract Teacher = Pvt School teacher)
Male	63.14%	31.82%	17.42%	0.218
Age	40.35	25.81	28.36	0.052*
Passed College	84.30%	45.45%	73.48%	0.034**
Training	98.29%	9.09%	33.91%	0.006***
Trained in last twelve months	93.52%	54.55%	2.29%	0.000***
From the same village	7.17%	81.82%	46.67%	0.001***
Distance from home to school (km)	11.92	1.09	3.27	0.01***
Teacher Salary	8698.12	1500.00	1699.47	0.331

**Notes**

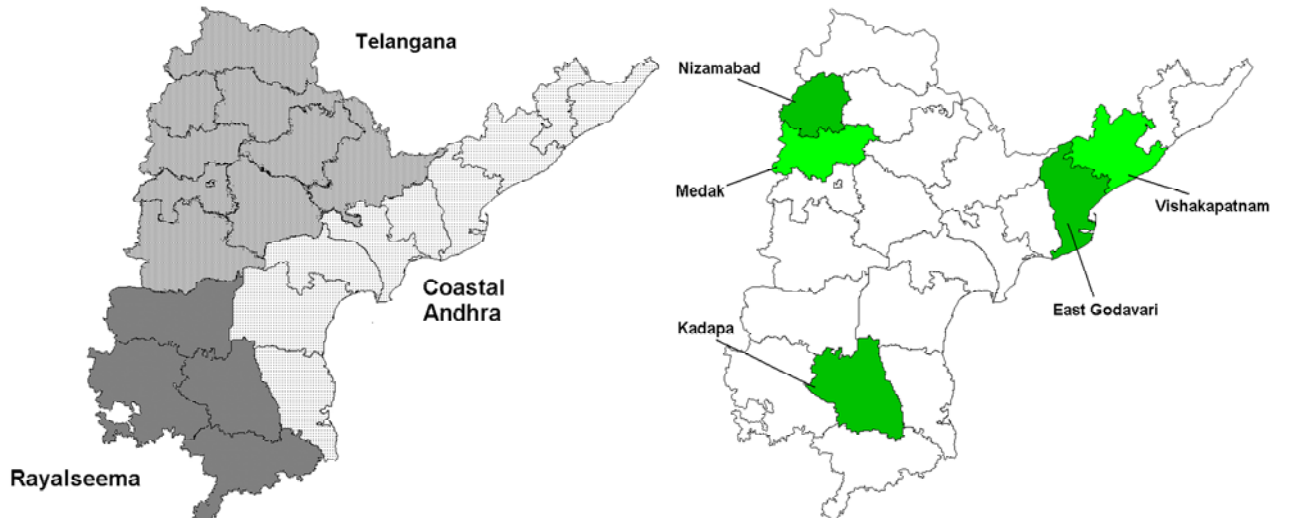
1. Preliminary Table based on a limited sample of private schools (<20% of total to be collected)

**Figure 1a: Andhra Pradesh (AP)**



	India	AP
<b>Gross Enrollment (Ages 6-11) (%)</b>	95.9	95.3
<b>Literacy (%)</b>	64.8	60.5
<b>Teacher Absence (%)</b>	25.2	25.3
<b>Infant Mortality (per 1000)</b>	63	62

**Figure 1b: District Sampling (Stratified by Socio-cultural Region of AP)**



**Figure 2: Salary Distribution by School and Teacher Type**

