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EXPERIMENTAL EVIDENCE FROM INDIA

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Contract Teachers: Experimental Evidence from India
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ABSTRACT

The large-scale expansion of primary schooling in developing countries has led to the increasing use of non-civil-service contract teachers who are locally-hired from the same village as the school, are not professionally trained, have fixed-term renewable contracts, and are paid much lower salaries than regular civil-service teachers. This has been a controversial policy, but there is limited evidence on the effectiveness of contract teachers in improving student learning. We present experimental evidence on the impact of contract teachers using data from an 'as is' expansion of contract-teacher hiring across a representative sample of 100 randomly-selected government-run rural 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 0.16ů and 0.15ů, in math and language tests respectively. Contract teachers were also much less likely to be absent from school than civil-service teachers (18% vs. 27%). Using the experimental variation in school-level pupil-teacher ratio (PTR) induced by the provision of an extra contract teacher, we estimate that reducing PTR by 10% using a contract teacher would increase test scores by 0.03ů/year. Using high-quality panel data over five years we estimate that the corresponding gain to reducing PTR by 10% using a regular civil-service teacher would be 0.02ů/year. Thus, in addition to finding that contract teachers are effective at improving student learning outcomes, we find that they are no less effective than regular civil-service teachers who are more qualified, better trained, and paid five times higher salaries.

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1. Introduction

The large scale expansion of primary schooling in developing countries over the past two decades has led to significant improvements in school access and enrollment, but has also created difficulties in maintaining and improving school quality, with learning levels being very low in most developing countries (Pritchett 2004; Pratham 2012, Uwezo 2012). A particularly challenging problem has been that of recruiting enough teachers and staffing them where needed. The challenges include a lack of enough qualified teachers to match the needs of rapidly-expanding school systems, the high cost of hiring them, and the reluctance of qualified teachers to serve in rural areas where the needs of the expanding education system are the greatest.[1]

Governments in several developing countries have responded to this challenge by staffing teaching positions with locally-hired teachers on fixed-term renewable contracts, who are not professionally trained, and who are paid much lower salaries than those of regular teachers - often less than one fifth as much (Table 1). The growing use of contract teachers in public schools has been one of the most significant trends in providing primary education in developing countries in the last two decades.[2] Contract teachers comprise a third of public-school teachers across twelve countries in Africa (Bourdon et al. 2007) and their share among all public-school teachers in India grew from 6 percent in 2003 to 30 percent in 2010 (Muralidharan et al. 2013).

But, the use of contact teachers has been and remains highly controversial. Supporters consider the use of contract teachers to be an efficient way of improving the quality of rural schools while also providing employment to educated young people in rural areas, and argue 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, that contract teacher positions may be 'captured' by local elites, that the wide differences in pay and benefits are exploitative, and that contract teachers should be replaced with qualified, trained, and well-paid regular teachers (Kumar 2005; Raina 2006, 2009).

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[1] See Duthilleul (2005) for a discussion of how the lack of qualified teachers has been a key challenge in expanding access to schooling in several countries. Teacher salaries comprise ~70-90% of education spending in most developing countries, and the cost of salaries is typically the main fiscal constraint to education expansion (see http://www.uis.unesco.org/Education for country-level salary data). Fagernas and Pelkonen (2012) study teacher location preferences and show that more qualified teachers are less likely to desire rural postings.

[2] Contract teacher schemes have been used in several developing countries including Benin, Burkina Faso, Cambodia, Cameroon, Chad, Congo, Indonesia, Kenya, Madagascar, Mali, Nicaragua, Niger, Senegal, and Togo, (see Duthilleul (2005) and Boudon et al (2007) for reviews of contract teacher programs). They have also been widely employed in several states of India, and are also known as 'para-teachers' (see Govinda and Josephine 2004).

The perception that contract teachers are of inferior quality and that their use ought to be a temporary measure, which should be eliminated by investing in teacher training and raising education spending to hire qualified civil-service teachers, is deeply embedded in the education policy discourse in several developing countries.[3] However, while there are several descriptive and observational studies on contract teachers in developing countries, there is relatively little well-identified evidence on the impact of using contract teachers in primary schools on learning outcomes. The two main questions of interest are whether untrained contract teachers can improve learning outcomes, and how effective they are relative to qualified regular teachers.

In this paper, we answer both these questions using data from an experiment in the Indian state of Andhra Pradesh (AP) that reduced school-level pupil-teacher ratios (PTR) by allowing schools to hire an additional contract teacher. 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 usual allocation of teachers. The experiment studied in this paper represented an "as is" expansion of the existing contract teacher policy, and was implemented by the Government of AP in exactly the same way as a regular expansion. Further, the random assignment of an extra contract teacher in a *representative* sample of schools in AP provides estimates of program impact that are directly applicable to scaling up (mitigating some of the concerns of external validity discussed by Heckman and Smith 1995, and Pritchett and Sandefur 2013).

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 0.15 standard deviations (σ) in math and language tests respectively. We find no heterogeneity in impact by baseline test scores, suggesting that the gains are broadly distributed among all students. However, students in remotely-located schools benefit more from their school receiving an extra contract teacher. We also find that contract teachers were significantly less likely to be absent from school than regular teachers (18% versus 27%), and that regular teachers exerted slightly lower levels of effort in schools that received an extra contract teacher.

[3] The works cited against the use of contract teachers are by Krishna Kumar (recent head of the National Council for Education Research and Training – India's apex technical advisory body on education), and Vinod Raina (who was a core member of the team that drafted the 'Right to Education' Act in India). The recently passed 'Right to Education' (RtE) Act in India reflects this thinking and requires the use of untrained teachers to be phased out over a three-year period. In a similar vein, the Indonesian "Teacher Law" passed in 2005 required all teachers to get certified and offered a *doubling* of salary for certified teachers, and an *additional* 100% salary supplement to certified teachers who serve in remote areas (Jalal et al. 2009).

Since our experiment induced exogenous variation in school-level pupil-teacher ratio (PTR), we can construct an experimental estimate of the impact of reducing PTR with a contract teacher.[4] We find that reducing PTR by 10% using a contract teacher would improve mean test scores in the school, across subjects, by 0.03σ/year. We also estimate the impact of reducing PTR with a regular civil-service teacher, using five years of data from the control schools and estimate that reducing PTR by 10% using a regular civil-service teacher would improve mean test scores across subjects by 0.02σ/year. Identification concerns are addressed using an increasingly restrictive set of fixed effects as well as a rich set of controls including lagged test scores, and we show that these estimates are extremely robust to several alternative specifications.[5]

Thus, our two main results are that: (i) contract teachers are able to significantly improve learning outcomes in primary schools in spite of not having formal teacher training credentials and (ii) they are at least as effective at doing so as regular civil-service teachers who are more, educated, have formal teacher training credentials, and are paid over five times more. Since it is possible to hire *several* contract teachers for the cost of a regular teacher, we also test whether a regular teacher is five times (or even two times) more effective than a contract teacher, and strongly reject the hypothesis that their differences in salaries reflect differences in productivity.

To place these results in the broader context of teacher labor markets in rural India, we collect data on rural *private* school teachers in the same districts and find that private school teachers are paid even less than contract teachers (though they are more educated) and that their salaries are so much lower than regular teacher salaries, that there is *no common support* in the two *distributions* (Figure 2). The results on equal effectiveness of contract and regular teachers and the market salary benchmarks for private school teachers strongly suggest that the large wage differential between regular and contract teachers is unlikely to reflect differences in productivity and mostly represents rents accruing to unionized civil-service teachers

Our results contribute to an emerging literature on understanding the impact of contract teachers in developing countries. In addition to several descriptive and observational studies

[4] As we discuss in section 3.3, reducing PTR can help improved education outcomes through multiple channels including a reduction in class size, and multi-grade teaching. However, since the decisions of how to use teachers within a school are endogenous, we focus our analysis on the impact of reducing PTR, which is defined at the school-level, and which is also the main metric of education policy in India (as opposed to class size)

[5] The contract teacher experiment lasted for two years, but a parallel experiment on teacher performance pay was continued for five years, as a result of which the control schools were tracked for five years (Muralidharan 2012). The very rich panel data allow us to estimate the impact of PTR reductions with regular teachers using value-added estimates as well as with school fixed effects, yielding among the most reliable estimates of the impact of reducing PTR (or class-size) using a regular civil-service teacher in the literature on class size in developing countries.

regarding the use of contract teachers,[6] there are two experimental studies on contract teachers to date. Duflo et al (2012) conduct an experimental evaluation of a contract teacher program in Kenya and find that students in grade one in schools with the extra contract teacher (who was assigned to grade one) score 0.16σ higher. However, Bold et al (2013) experimentally evaluate a similar contract teacher program in Kenya and find that the program had a positive effect when implemented by a non-profit organization (as was done in Duflo et al. 2012) but had no effect when implemented by the government, suggesting that scaling up by the government may not be as effective in the Kenyan context.

As mentioned earlier, this paper analyzes an 'as is' expansion of contract teacher hiring in randomly-selected schools that was implemented by the Government of AP, and is therefore not subject to this concern. A further important advantage of our school-level randomization and analysis is that we do not require schools to have fully complied with a within-school randomization protocol or to make sure that no other resources in the school were reallocated in response to the provision of an extra teacher to one grade (these can be difficult to ensure as shown in Bold et al. 2013). Policy norms on teacher staffing and PTR are set at the school level, and our school-level experiment allows us to directly estimate the policy-relevant parameter of the impact of reducing school-level PTR with a contract teacher, without relying on any within-school comparisons that may be confounded by unobserved factors.

This paper also makes several broader contributions to the literature on service delivery in developing countries. First, we contribute to the literature on decentralization and accountability in the provision of public services, by showing that concerns that locally-hired contract teacher positions may be 'captured' by local elites (Bardhan 2002, Bardhan and Mookherjee 2000) were not first-order. Our results suggest that an 'as is' decentralization to school committees of resources to hire contract teachers led to (a) the hiring of teachers who had lower absence rates than regular teachers, (b) significantly improved primary-school learning outcomes, and (c) did so much more cost effectively than the default of state-level civil-service hiring (one rationale for which is to 'professionalize' hiring and to isolate hiring from local pressures and interests).

[6] Descriptive papers include Duthilleul (2005), Govinda and Josephine (2004), Pritchett and Pande (2006), Kingdon and Sipahimalani-Rao (2010), and Goyal and Pandey (2011). Papers that use observational data to study the effect of contract teachers include De Laat and Vegas (2005) in Togo, Bourdon et al (2007) in Niger, Mali, and Togo, and Atherton and Kingdon (2010) in India.

Second, many developing countries face challenges in delivering services in rural areas, where highly-qualified professional service providers do not want to live. A common approach to address this problem is to require qualified civil-service teachers and doctors to accept rural postings as part of their rotations, but in practice this results in a ‘market’ where substantial bribes are paid for more desirable postings (Beteille 2009), and/or a situation where service providers choose to live in further-away towns and commute to the village (Table 1), which is correlated with higher absence rates (Chaudhury et al. 2006). Our results suggest that hiring local staff from the village, even with lower levels of education and modest amounts of training, may be a more promising approach to service delivery in rural areas (especially for *primary* services). Our results are also consistent with findings in the health literature, which show that community health workers with limited training can be effective at improving health outcomes in underserved rural areas in developing countries (Bang et al. 1999, Haines et al. 2007).

Third, while set in the context of teachers and schools, this paper also illustrates the polarized two-tier labor market in many developing countries. In particular, the very large differences in pay and benefits between civil-service and contract teachers with no corresponding difference in productivity provides a striking illustration of the ‘insider-outsider’ theory of two-tier labor markets (Lindbeck and Snower 1988, 2001) and the extent to which politically powerful insiders are able to protect their rents (Kingdon and Muzammil 2001). Our results suggest that formal teaching credentials (as obtained currently) are poor predictors of teacher quality, and highlight the large potential costs of restricting entry into teaching based on these credentials (see Kleiner 2000 for a review of the economics of occupational licensing more generally).[7]

Finally, in thinking about *why* the status quo of highly inefficient choice of inputs in the production of public schooling persists on the employer side, we see close parallels to Bandiera et al (2009) who analyze public procurement in Italy and show that over 80% of wastage in public spending can be attributed to *passive waste* (attributable to inefficiencies resulting from limited incentives for public sector officials who are not residual claimants of improved efficiency or from regulatory constraints) as opposed to *active* waste (attributable to bribes and

[7] In related work on higher education in the US, Bettinger and Long (2010) show that adjunct faculty (who are less qualified and paid much less than tenure-track faculty) perform slightly better than regular faculty, and Figlio, Schapiro, and Soter (2013) show that students learn more from non-tenure track line faculty than tenure-track faculty in introductory courses. While these results are clearly related to ours, they are less stark because the job description of a tenure track faculty member also includes a primary focus on research, whereas there is no such additional expectation of civil-service teachers in our setting.

private pay-offs). Our results showing that contract and regular teachers are equally effective, even though the latter cost five times more suggest similar magnitudes of passive waste in public production of primary education in India. Since education is one of the largest components of public expenditure in many countries, our results also contribute to the broader literature on the cost effectiveness of publicly-produced services.[8]

There are large welfare implications of taking our results seriously. The Government of India is expected to spend an additional $5 billion/year to fulfill the mandate of the RtE Act to reduce pupil-teacher ratio (PTR) from 40:1 to 30:1 by recruiting additional regular teachers. Since it is possible to hire *several* contract teachers for the cost of a regular teacher, our results suggest that a substantially larger improvement in education outcomes may be obtained by allocating these funds to hiring more contract teachers. Doing so would make it possible to reduce PTR to below 15, to eliminate multi-grade teaching, and to have additional teaching resources to provide supplemental instruction to first-generation learners who are not able to keep pace with the syllabus, for the *same* cost (see Muralidharan 2013a, and 2013b for details).

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 the effectiveness of regular and contract teachers; section 5 discusses the results in the larger context of teacher labor markets in rural India, and presents caveats to our results; section 6 discusses policy implications and concludes.

2. Experimental Design

2.1. Context

While India has made substantial progress in improving access to primary schooling and primary school enrollment rates, the average levels of learning remain very low. Recent surveys show that over 60% of children aged 6 to 14 in rural India could not read at the second grade level, though over 97% of them were enrolled in school (Pratham 2012). Public spending on education has been rising as part of the "Education for All" campaign, but there are substantial inefficiencies in public delivery of education services. Kremer et al (2005) found using a

[8] On teacher personnel policies, Ballou (1996) shows that public school administrators typically do not hire the best applicants; on education spending more broadly, Hanushek (2002) reviews several studies showing the lack of a relation between public spending on education and learning outcomes; and on public sector management in general, Bloom and Van Reenen (2010) collect detailed data on management practices across countries and sectors and show that government-owned firms (especially in developing countries) are typically managed "extremely badly".

nationally representative survey (conducted in 2003) that 26% of teachers in rural public schools in India were absent on any given day. A more recent survey that revisited the same villages found substantial improvements in school quality as measured by inputs, but found that teacher absence rates in rural India were still around 24% (Muralidharan et al. 2013).

Andhra Pradesh (AP) is the 5th largest state in India, with a population of over 80 million, with around 70% living 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 (Muralidharan and Sundararaman 2011), and on measures of service delivery such as teacher absence (Kremer et al. 2005). There are a total of over 60,000 government primary schools in AP and around 70% of children in rural AP attend government-run schools (Pratham 2012).

The average rural 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. All regular teachers are employed by the state, and their salary is mostly determined by experience and rank, with minor adjustments based on assignment location, but no component based on any measure of performance. In 2006, the average salary of regular teachers was over Rs. 8,000/month and total compensation (including benefits) was over Rs. 10,000/month (per capita income in AP was around Rs. 2,000/month). Regular teachers' salaries and benefits comprise over 90% of non-capital expenditure on primary education in AP. Teacher unions are strong and disciplinary action for non-performance is rare (Kingdon and Muzzamil 2001; Kremer et al. 2005).

2.2 The Extra Contract Teacher Intervention

Contract teachers (also known as para-teachers) are 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.[9] They are more likely to be younger, to be female, to be from the same village, and live closer to the school they teach in (Table 1 – Panel A). Since the typical primary school has more grades than teachers, the default teaching arrangement is one of 'multi-grade' teaching (where one teacher simultaneously teaches multiple

[9] 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). Unless stated otherwise, all the numbers will refer to those at the time of the study (2005 - 2007 for the most part). The factor of five is almost certainly a lower bound on the ratio of the total compensation of regular and contract teachers because it does not include the value of medical and retirement benefits paid to regular teachers.

grades in the same classroom). As a result, contract teachers almost always teach their own classes and are not ‘teacher-aides’ who support a regular teacher in the same classroom.

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 enrollment and teacher strength at the start of the school year. Thus, contract teachers can be appointed both against vacant sanctioned 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, and since renewal is not guaranteed, the appointment of contract teachers is typically for a 10-month period. New hires are supposed to go through a brief accelerated training program prior to starting to teach, but this is imperfectly implemented in practice. While some states have a system of providing contract teachers with priority for being hired when openings for regular teacher positions are available, there is no such system in place in Andhra Pradesh.

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. Most schools (~80%) reported starting the process of hiring the extra contract teacher within a week of receiving the notification and reported having appointed the selected candidate within a month of receiving the notification. All schools reported having completed the full process within two months.

The modal selection committee consisted of three members (the head teacher, a member of the local elected body, and another teacher), with the head teacher typically being reported as having the most influence on candidate selection. The most important stated criteria for hiring the contract teacher was qualification (62%), followed by being from the same village and extent of teaching experience (20% each). In spite of these stated responses, it is possible that local leaders or head teachers may have exercised some favoritism towards their preferred candidates in some cases (as is also described by Srivastava 2010 in the context of para-teacher hiring in the state of Madhya Pradesh, and by Duflo et al. 2012 in Kenya). However, as per the experimental protocol, the project staff were not a part of the teacher hiring process in any way, and so the intervention mimicked an 'as is' expansion of the existing contract teacher program in AP to 100 randomly selected schools. We see in Table 1 (Panel B) that the additional contract teachers hired under this program had the same average characteristics as typical contract teachers in the comparison schools.

The additional contract 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. We chose to not try to allocate the extra contract teacher to a specific role or grade within the school,[10] because it is not possible in practice to ensure adherence to a within-school randomization protocol, and it is quite likely (as we will see later) that existing teachers would re-optimize their roles and effort in response to the extra contract teacher. It is also likely that the 'optimal' assignment of contract teachers to specific grades and tasks will vary based on unobservable school and teacher-level characteristics, and trying to randomly pre-assign the task for the extra contract teacher may be sub-optimal (and hence less likely to be adhered to). We therefore abstract away from within-school considerations and conduct all our analysis aggregated at the school level, which is also the level at which key policy decisions are made and implemented (such as PTR norms and infrastructure standards).

2.3. Sampling and Randomization

The extra contract teacher (ECT) program was evaluated as part of a larger education research initiative (across 500 schools) known as the Andhra Pradesh Randomized Evaluation Studies (AP RESt), with 100 schools being randomly assigned to each of four treatment and one control groups.[11] We sampled 5 districts across each of the 3 socio-cultural regions of AP in

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[10] The main value of such a within-school randomization protocol would have been to potentially obtain an experimental comparison of the relative effectiveness of regular and contract teachers (as attempted by Duflo, Dupas, and Kremer 2012). However, as discussed above, we chose to not do this in our setting because it would not be possible to ensure adherence to within school randomization protocols, which would then potentially confound the interpretation of the results.

[11] The AP RESt 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, that piloted and evaluated several policy options to improve learning outcomes in AP. While the Azim Premji Foundation (APF) was the main implementing agency for the teacher performance-pay, and school block-grant interventions (described in Muralidharan and Sundararaman 2011 and in Das et al. 2013), the contract teacher intervention was implemented by the Government of AP (based on the lottery conducted by the project team). However, APF was responsible for all data collection in all sampled schools, which was identical across all treatment and control schools. This ensures that Hawthorne effects are minimized and that a comparison between treatment and control schools can accurately

proportion to population. In each of the 5 districts, we randomly selected one administrative division and then randomly sampled 10 mandals (the lowest administrative tier) 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 public primary school in rural AP.

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.[12] After the baseline tests were scored, 2 out of the 10 project schools in each mandal were randomly allocated 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 analysis in this paper is based on the 200 schools that comprise the 100 schools randomly chosen for the ECT program and the 100 that were randomly assigned to the comparison group. Table 2 shows summary statistics of baseline school and student characteristics for both treatment and comparison schools and the null of equality across treatment groups cannot be rejected for any of the variables.[13]

2.4. Data

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 visits to the schools made by staff of the Azim Premji Foundation to measure process variables such as teacher attendance and teaching activity (six such visits were made to each school in the first year and four visits were made to each school in the second year). For the rest of this paper, Year 0 (Y0) refers to the baseline tests in June-July 2005; Year 1 (Y1) refers to the tests conducted at the end of the first year of the program in March-April, 2006; and Year 2 (Y2) refers to the tests conducted at the end of the second year of the program in March-April, 2007. All analysis is carried out with

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isolate the treatment effect of interest. The only difference in data collection across treatment schools was a short treatment-specific survey after a full year (that in this case documented the process of hiring the contract teacher).

[12] 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.

[13] Table 2 shows sample balance between the comparison schools and those that received the extra contract teacher, which is the focus of the analysis in this paper. The randomization was done jointly across all treatments and the sample was also balanced on observables across the other treatments. Note that we do not employ a 'cross cutting' design and our results can therefore be interpreted as the 'as is' mean effect of providing an extra contract teacher to schools without being confounded by interactions with other treatments.

normalized test scores, where individual test scores are converted to z-scores by normalizing them with respect to the distribution of scores in the control schools on the same test.

3. Experimental 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 turnover rate was close to 33%, which is consistent with rotation of teachers once every 3 years (Table 2 – Panel B, rows 11-12).

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. The average teacher turnover in the second year was only 1%, and there was no significant difference in teacher transfer rates across the treatment and control groups at the end of 2 years (Table 2 – Panel B, rows 13 - 14).[14] 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 7.3% and 20.2% in year 1 and year 2 respectively, but there is no significant difference in attrition across the treatment and control groups (rows 15 and 18). Attrition is higher among students with lower baseline scores, but this is true across all treatments, and we find no significant difference in mean baseline test score across treatment categories among the students who drop out from the test-taking sample (Table 1 – Panel B, rows 16, 17, 19, 20).

3.2. Specification

Our default specification uses the form:

$$T_{ijkm}(Y_n) = \alpha + \gamma_j \cdot T_{ijkm}(Y_0) + \delta \cdot ECT_k + \beta \cdot Z_m + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (1)$$

The 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 control schools for each test and grade separately), where *i, j, k, m* denote the student, grade, school, and mandal respectively. Y_0

[14] There was also a court order to restrict teacher transfers in response to litigation complaining 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.

indicates the baseline tests, while Y_n indicates a test at the end of *n* years of the program. Including the normalized baseline test score improves efficiency due to the high autocorrelation of test-scores over time.[15] 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 the school was selected to receive the extra contract teacher (ECT) program, and the parameter of interest is δ, 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. As discussed earlier, we focus on school-level analysis (which is the unit of randomization) and abstract away from within-school analysis because the specific ways in which the contract teacher was used would be endogenous to the school.[16]

3.3. Impact of ECT program on Test Scores

Averaging across both math and language, students in program schools scored 0.10σ and 0.16σ higher than those in comparison schools at the end of one and two years of the program respectively (Table 3 – Panel A, columns 1 and 3). The two-year effects of the extra contract teacher are similar across math (0.16σ) and language (0.15σ) (Panels B and C). The addition of school and household controls does not significantly change the estimated value of δ, confirming the validity of the randomization (columns 2 and 4).

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 δ_3 is significantly different from zero in:

[15] 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 grades 1 and 2 at the end of two years of the treatment). This will not affect the estimation of treatment effects in other grades because we allow the coefficient on the lagged test score (γ_j) to vary by grade.

[16] Note that we do collect detailed data on how the extra contract teacher was used, and in almost all cases, this teacher was given his or her own class to teach, resulting in a reduction in class size for that class (which would have typically been in a multi-grade situation) and also for other classes that may have previously been combined with this class. But we do not focus on within school analysis because the within school allocation of teaching resources is likely to be influenced by unobservable factors, which will confound the interpretation. We discuss this issue further in section 4.3.

$$T_{ijkm}(Y_n) = \alpha + \gamma_j \cdot T_{ijkm}(Y_0) + \delta_1 \cdot ECT_k + \delta_2 \cdot Characteristic_{ijkm}$$
$$+ \delta_3 \cdot (ECT_k \times Characteristic_{ijkm}) + \beta \cdot Z_m + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (2)$$

Table 4 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 (indicated by the coefficients on the interactions).

The first main 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 (column 8) 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.

We do see that schools in more remote areas derive greater benefit from the addition of an extra contract teacher (the school proximity index is defined in Table 2, with higher values representing a school that is far from basic amenities). A related (but weaker) result is that schools with poorer infrastructure and with fewer students also benefit more from the extra contract teacher (though the latter are not significant). Since remote schools also typically have fewer students and poorer infrastructure, we include all three variables (remoteness index, infrastructure index, and log of enrollment) and their interactions in an extended version of (2) and find that there is still a significant positive effect of the extra contract teacher in schools that are more remote in both years but not in schools with poorer infrastructure or fewer students (results available on request).[17] Thus, it appears that the mechanism for finding heterogeneous impacts of the extra contract teacher by school proximity is not just that the marginal reduction in school-level pupil-teacher ratio (PTR) induced by the extra contract teacher is larger in smaller schools (this would be picked up in the interaction with school enrollment) but that locally-hired contract teachers may be especially effective in remote areas where the social distance between civil-service teachers and the community would be larger, and where civil-service teachers try to avoid getting posted (Fagernas and Pelkonen 2012).

[17] Given the likelihood of significant results simply reflecting sampling variation, we are cautious to only infer heterogeneity based on observing the same patterns in multiple years of data. Further, the robustness of the result showing heterogeneity of impact by the 'Proximity Index' to the inclusion of other covariates and their interactions with the treatment dummy increases our confidence that the inference is not spurious.

3.6. Differences in Teacher Effort by Contract Status

Table 5 – Panel A shows that contract teachers had significantly lower levels of absence compared to regular teachers (17.8% versus 27.3% on average over the two years), with the difference being higher in the second year (11.3%) compared to the first year (8.1%). Contract teachers also had higher rates of teaching activity compared to regular teachers (48.4% versus 42.2%), though these numbers are easier to manipulate 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 have higher rates of absence and lower rates of teaching activity than their counterparts in comparison schools (Table 7 – Panel B), though these are not always significant. Thus, our estimate of the impact of an additional contract teacher is a composite estimate that includes the *reduction in effort* of regular teachers induced by the presence of the extra contract teacher, and is therefore likely to be a lower bound on the pure 'production function' effect of providing an additional contract teacher to schools (Das et. 2013).

4. Cost Effectiveness

4.1. Impact of Reducing PTR with a Contract Teacher

The experimental results establish that the marginal product of contract teachers is positive, and the conventional view that untrained teachers are not effective is not supported by our evidence. However, the broader policy question is that of the relative effectiveness of regular and contract teachers. In particular, since the Right to Education Act calls for reducing school-level PTR from 40:1 to 30:1 by hiring more regular teachers, it would be especially policy relevant to estimate the impact of doing so with contract teachers instead. Note that we use the term PTR as opposed to class-size throughout this paper, because policy norms in India focus on school-level resource allocations and do not make prescriptions on resource allocation within schools (including teachers). Thus, while PTR and class-size will be highly correlated, the PTR can be exogenously manipulated by policy (and by our experiment) whereas variation in class-size will typically also be determined by school and teacher-level unobservable factors that policy-makers cannot observe (and we cannot control for).

Our school-level randomization allows us to experimentally estimate the impact of reducing PTR using a contract teacher. Table 6 shows that the experiment created a significant reduction in PTR, with the treatment group having a PTR in the first year of 25.4 compared to the control group mean of 32.7. In the second year, the PTR was 28.3 and 37.8 in the treatment and control groups respectively. Thus the treatment reduced PTR by around 25% using a contract teacher. As expected, mean class size is also reduced in schools that receive an extra contract teacher, but we focus on PTR for the reasons mentioned above.

We can re-estimate the treatment effects shown in Table 3 in terms of PTR by estimating:

$$T_{ijkm}(Y_1) = \alpha + \gamma_j \cdot T_{ijkm}(Y_0) + \beta_1 \cdot \Delta \log\_PTR_k + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \tag{3}$$

where $\Delta \log\_PTR_k$ is the change in log_PTR (at the school-level) induced by the provision of the extra contract teacher (the value of $\Delta \log\_PTR_k$ will be zero for the control schools). Since the extra teacher is randomly assigned across schools, this change in log_PTR will be uncorrelated with other determinants of test scores, and β_1 provides an unbiased estimate of the average impact on test scores of reducing PTR with a contract teacher. However, the extent to which the extra contract teacher reduced the PTR in treatment schools will depend on the initial PTR in the school. So we estimate (3) both without and with a control for the initial PTR, and our preferred specification includes the control for initial PTR.

We present these results in Table 7, and see that β_1 is equal to -0.34 (averaged across subjects, and controlling for initial PTR) and is significant at the 1% level. Thus, reducing PTR by 10% using a contract teacher would improve mean test scores across schools and grades by 0.034σ per year (the estimate is similar both with and without controls for the initial PTR). Note that the specification in (3) can only be estimated experimentally with the first year of the experiment, because doing so in a value-added specification using data from the second year will require including test scores from the end of the first year on the right hand side of (3), which will not yield an experimental estimate. Thus, the number of observations in Table 7 is the same as that in column 1 of Table 3.

4.2. Impact of Reducing PTR with a Regular Civil-Service Teacher

The ideal parallel experiment would have been to provide a randomly-selected set of schools with an extra civil-service teacher that would have allowed an experimental estimate of the impact of reducing PTR with a regular teacher (and a comparison with the impact of doing so with a regular teacher). However, such an experiment was not feasible because civil-service teacher assignments have to follow administrative rules, and cannot be based on random assignment, and no such experiment has ever been conducted to the best of our knowledge.[18]

Nevertheless, as we show below, the richness of the panel data we collect allows us to construct a very robust and credible estimate of the impact of PTR reductions using a regular teacher. The first six columns of Table 8 present estimates of β_2 from estimating

$$T_{ijkm}(Y_n) = \alpha + \gamma_j \cdot T_{ijkm}(Y_{n-1}) + \beta_2 \cdot \log\_PTR_k(Y_n) + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (4)$$

where the estimation sample is restricted to control schools that *had only regular teachers* in the year concerned (during either or both of the two years of the experiment). Thus, $\log\_PTR_k(Y_n)$ is the logarithm of school-level PTR (in year *n*) in cases where *all* the teachers in the school in that year are regular teachers, and β_2 provides an estimate of the extent to which variations in PTR across schools are correlated with changes in test-score gains across schools. The main challenge in interpreting β_2 as the causal estimate of PTR on test score gains is the concern that that there are omitted variables across schools that could be correlated with both student learning trajectories as well as PTR. We therefore augment (4) with a full set of school and household controls (these are the same as in Table 3) and first with district fixed-effects, and then with mandal (sub-district) fixed effects.[19]

We estimate that β_2 ranges from -0.20 to -0.26 (Table 8 – Columns 1-6), with none of these estimates being significantly different from each other. In addition to the point estimate, it is noteworthy that these estimates are remarkably similar and not affected much by the inclusion of

[18] The Duflo et al (2012) study experimentally provides schools with an extra contract teacher, and then generates the comparison with regular teachers by further randomizing the within-school allocation of students to contract and regular teachers. Similarly, the well-known Tennessee Star experiment (Krueger 1999) also varies class size by randomly varying student assignment within schools. As discussed in sections 2.2 and 4.3, such a design would not have been appropriate or credible in our setting, given the near impossibility of ensuring that tasks were not reallocated across teachers within the school.

[19] Since there is considerable variation in the quality of administration across jurisdictions, specifications that include district (and mandal) fixed effects eliminate concerns of omitted variables across administrative jurisdictions and are identified using variation *within* the concerned administrative unit, with the mandal being the lowest level of administration in Andhra Pradesh.

controls and district and mandal fixed effects. The most likely reason for this is that the inclusion of lagged test scores in our core specification accounts for most cross-sectional omitted variables and that there are unlikely to be further omitted variables correlated with both the PTR and the gains in test scores.

Nevertheless, the identifying variation in the above results is cross-sectional and it is difficult to fully allay concerns regarding omitted variables. Our preferred estimates therefore rely on five years of panel data from the control schools, which allows us to estimate (4) using school fixed-effects, where the identifying variation in PTR comes from variation in PTR in the *same* schools over time that mainly reflect changes in cohort sizes over time and teacher transfers in and out of schools. We provide more details on the estimation sample and robustness checks in Appendix A.1 (and Appendix Tables 1-3), and the main point to highlight is that there is no correlation between changes in PTR between years within a school and the end of year test scores of the previous year, suggesting that within-school variation in PTR over time is quasi-random and highly unlikely to be correlated with omitted variables that may also be correlated with test-score gains.

Columns 7-12 of Table 8 repeat the specifications in columns 1-6, but with five years of data from the control schools (as opposed to two years), and columns 13-14 present our preferred estimates of β_2 by estimating (4) with school fixed effects with and without household controls. The main result is that the estimates are remarkably similar to those in columns 1-6 and our preferred estimate of β_2 from columns 13-14 is around -0.20, implying that a 10% reduction in PTR induced by hiring more regular teachers would improve mean test scores across schools and grades by 0.02σ per year.[20]

It is worth highlighting that the estimates presented in Columns 13 and 14 of Table 8 are probably the most credible estimates of the impact of reducing PTR (or class size) in the literature on education in developing countries. The best estimates to date are from Urquiola (2006) in Bolivia, and Angrist and Lavy (1999) in Israel, but both study the impact of class size on test score *levels* as opposed to test score gains. Thus, while both papers find that larger class

[20] Since it is possible that the range of variation in PTR used in the panel estimates in columns 13-14 is considerably narrower than the range of variation in PTR across schools, Figure 1 plots histograms of the distribution of the identifying variation in log_PTR (normalized to zero) with an increasingly restrictive set of fixed effects (none, district, mandal, and school) and we see that while there are fewer outliers in log_PTR with school fixed effects, the identifying variation in the specification with school fixed effects spans most of the range of PTR's found in the cross-sectional sample.

sizes reduce test scores, the magnitude of the estimates could also be confounded by class-size variation in previous grades and the estimates are therefore less straight-forward to use for cost effectiveness comparisons, especially relative to experimental evaluations of interventions that typically report outcomes in terms of test-score gains. Further, our estimates are based on 'as is' variation in PTR in a representative sample of schools in AP over five years, and therefore unlikely to be influenced by concerns of strategic behavior by civil-service teachers in high profile experimental evaluations.[21]

The panel-data estimates of the impact of PTR reductions using regular civil-service teachers on test score gains are an important result in themselves. However, the main use of these results in this paper is to enable a credible comparison of the impact of reducing PTR using a contract teacher versus a regular teacher. We present a formal test of the equality of the impact of reducing PTR using a contract teacher (β_1 in Table 7 – Column 2) and that of doing so with a regular teacher (each β_2 in Table 8), and we see that we can never reject the null hypothesis that contract and regular teachers are equally effective, and the point estimates suggest that contract teachers may even be *more* effective than regular teachers on the current margin (since $|\beta_1| > |\beta_2|$ for *every* estimate of $|\beta_2|$ in Table 8).

These results suggest that contract teachers are at least as effective as regular teachers, and it is extremely unlikely that any further omitted variables could reverse this result since $|\beta_1|$ in Table 7 is an experimental estimate, and including a full set of controls barely changes the estimate of $|\beta_2|$ across the columns in Table 8 (and the most credible panel-data estimates yield the *lowest* values of $|\beta_2|$). A final caveat is that $|\beta_1|$ estimates the *marginal* effect of reducing PTR with a contract teacher, whereas $|\beta_2|$ estimates the *average* effect of reducing PTR with a regular teacher. We address this concern by including a quadratic term for *log_PTR* in (4) and find that the coefficient on this term is insignificant (table available on request). Hence, the

[21] One potential downside of high-profile experimental studies such as the Tennessee STAR program is that teachers *like* smaller class sizes and teachers and teacher unions were fully aware of the study and its likely consequences for policy. Thus, it is possible that teachers in smaller classes exerted more effort than usual with a view to demonstrating the effectiveness of smaller classes, potentially biasing the resulting estimates. In contrast, the AP RESt experiments were quite low profile and the communications to schools and officials mostly emphasized that the Azim Premji Foundation was providing schools with resources for a series of programs and that the selection of schools into the overall program and to specific interventions was being done by lottery to ensure 'fairness'.

average and marginal effects of PTR reduction are unlikely to be very different for the range of PTR variation that is in our estimation sample.

Economic theory suggests that optimal production of education will use expensive better-qualified regular teachers and inexpensive less-qualified contract teachers in the proportion where the ratio of marginal costs equals the ratio of marginal productivity. Since regular teachers are paid at least five times higher salaries than contract teachers, we also test if they are five times more productive and strongly reject the hypothesis that $\beta_2 = 5 \cdot \beta_1$ (at the 1% level). In fact, we even reject $\beta_2 = 2 \cdot \beta_1$ (at the 5% level), strongly suggesting that the large salary difference between regular and contract teachers do not reflect differences in productivity.

4.3. Comparing Regular and Contract Teachers within schools

Since we can match students in each year to their teacher and know the teacher type in that year, we can also compare the relative impact of being taught by a contract teacher as opposed to a regular teacher on test score gains (using *within* school variation in the type of teacher assigned to a grade). We present these results in Appendix A.2. and find that that there is no differential effect on learning gains for students taught by contract teachers relative to those taught by regular teachers (Appendix Table 4). While these results confirm our earlier finding that there is no difference in the effectiveness of regular and contract teachers, we do not emphasize them because they are based on the *assignment* of teachers to classes, and do not reflect actual day to day teaching *practice*. So it is possible that we may overstate the effectiveness of contract teachers if regular teachers spend time out of their own classrooms in training and coaching them. Conversely, since regular teachers are more absent (as we see if Table 5) it is possible that contract teachers spend time away from their own classrooms covering for absent regular teachers, in which case we may underestimate their effectiveness.

More importantly, it is highly likely that the assignment of teachers to tasks and grades within a school is at least partly determined by factors that are not observable to the econometrician, and which cannot be controlled for (for instance, weaker teachers may be given smaller classes or easier cohorts than stronger teachers). Hence, we show these results in the Appendix for completeness, but the most credible comparison of regular and contract teachers (and the one that matters the most for policy) is the one shown in Table 8, which is based on variation in *school-level* PTR induced by adding a contract teacher and a regular teacher respectively, and abstracts away from how teaching resources are used within schools.

5. Discussion

5.1. Teacher labor markets in rural India

To provide further perspective on our results showing that contract teachers are at least as effective as regular teachers, but at much lower cost, we also briefly discuss the private sector teacher labor market in rural India. A prominent feature of primary education in India over the past ten years has been the rapid increase in the enrolment share of private schools, with recent estimates showing that around 30% of primary school students in *rural* Andhra Pradesh attend a fee-charging private school (Pratham, 2012). Since fee-charging private schools need to compete against free public schools as well as against other private schools for both students and teachers, they are likely to 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.

As part of a different study on school choice (Muralidharan and Sundararaman 2013), we collected detailed data on teachers in private schools in the same five districts where the contract teacher experiment was conducted, and Table 9 compares regular teachers, contract teachers, and private school teachers (sampled from the same villages)[22] on a range of characteristics. The age and gender profile of private school teachers are similar to those of contract teachers (younger and more likely to be female than regular teachers). Private school teachers have higher levels of general education, but even lower levels of teacher training than contract teachers. They live much closer to the school and are more likely to be from the same village relative to regular teachers (though less so than contract teachers).

But, the most relevant comparison is that the salaries of private school teachers are even lower than those of contract teachers and *around an eighth* of regular teacher salaries. Figure 2 plots the salary distribution of teachers in government and private schools, and we see that the distribution of salaries in private schools is around the range of the contract teachers' salaries, and that there is almost *no common support* between the distributions of private and regular public school teacher salaries. Finally, private school teachers and contract teachers have

[22] Note that this is a different sample from that used in Table 1. The sample in Table 1 is representative of rural government-run schools, which is the focus of this paper; the sample in Table 9 is from a sample of villages that have private schools (which tend to be larger). The data for Table 9 was also collected 3 years later than the data used for Table 1. AP government policies on contract teacher salaries now provides for some differentiation by education and experience, which accounts for the distribution in Figure 2. The lower absence rates of regular teachers in Table 9 as opposed to in Table 5 are also likely to be because the sample used for Table 9 is drawn from larger villages that are less remote.

similarly low rates of absence, which are around half that of the regular teachers in spite of being paid much lower salaries.

The private school data helps clarify the context of teacher labor markets in rural India and provides important guidance for thinking about expanding the use of contract teachers in government schools. First, the employment terms of contract teachers are not ‘exploitative’ as believed by opponents of their use, but in line with the market clearing wage paid by private schools. While their terms might seem exploitative when working side by side with regular teachers and doing the same work for a fraction of the salary, the distortion is not the ‘low’ contract teacher salaries but rather the large rents accruing to civil-service teachers.

Second, private schools in rural AP pay much lower teacher salaries, but hire more teachers and have significantly lower PTR’s than public schools (Muralidharan and Sundararaman 2013). Thus, it appears that an optimizing producer of primary education services in rural AP would hire teachers whose education, training, demographics, and pay resemble those of contract teachers rather than that of the more qualified regular teachers found in public schools, but would hire more teachers and have lower PTR’s than in public schools. 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 (especially, since it is possible to hire *many* contract teachers for the cost of a regular teacher as the private schools do).

Third, since private schools are able to fill their teacher positions with salaries that are even lower than those of contract teachers, an expansion of contract teacher hiring is unlikely to hit a supply constraint at current salary levels. Also, none of the 100 treatment schools in our experiment reported any difficulty in filling the position and the majority of positions were filled within 2 to 3 weeks from the start of the search. More broadly, the pool of educated but unemployed rural high-school and college graduates from which contract and private school teachers are hired appears to be large enough for the labor supply of contract teachers to be fairly elastic (Kingdon and Sipahimalani-Rao, 2010).[23]

Finally, it is worth highlighting that the very high salaries paid to civil-service teachers (relative to the market wage) as seen in Figure 2 can have negative consequences beyond the

[23] Another contributing factor may be that limited job opportunities within the village for educated rural women (who have cultural and family restrictions against working outside their village) may be providing a subsidy to the teaching sector by lowering the wages of female contract and private school teachers (Andrabi et al, 2012).

fiscal inefficiency of incurring five times greater costs than necessary. Kingdon (2011) suggests that the very high salaries of civil-service teachers may contribute to the large social distance between communities and teachers and make it more difficult for parents to hold teachers accountable. A potentially more disturbing consequence of the large rents in civil-service teacher jobs is that a 'market' can emerge for these jobs whereby politicians appoint candidates who are willing to pay for civil-service teacher positions.[24] Candidates appointed this way are more likely to be negatively selected on intrinsic motivation (Shleifer and Vishny 1989), and are likely to have poorer norms for effort since they have 'paid' for their jobs (Akerlof 1982).

5.2. Reasons for the effectiveness of contract teachers

As the summary statistics in Table 1 indicate, contract teachers are different from regular teachers in *many* ways, some of which may make them less effective (lower levels of education, training, and pay), and some of which may make them more effective (being from the same village with closer connections to the community, living closer to the school, and being more accountable due to the renewable nature of the job). Decomposing the relative importance of these factors is clearly important for policy, but we do not do so here because it is not possible to obtain credible exogenous variation in each of these individual teacher characteristics.

However, our results highlight that teacher quality (measured by their effectiveness in improving learning outcomes) depends on their human capital as well as their effort, and one interpretation of our results is that contract teachers make up for their lower levels of formal education and training with higher levels of effort.[25] The results also suggest that a lack of formal teacher training credentials (as currently generated by the education system in India) may not be a binding constraint to teacher effectiveness.[26] Of course, it may be possible to do even

[24] For instance, a former Chief Minister (elected *head* of government) of the Indian state of Haryana was recently prosecuted and convicted for illegally recruiting over 3000 teachers for financial considerations (Bhardwaj 2013). Note that this is probably the first time in India that a senior politician has faced legal consequences for corruption in teacher hiring, and the threat of such punishment has typically not been a deterrent to markets in the hiring and assignment of government school teachers (Beteille 2009). See Hallak and Poisson (2007) for more examples of corruption in teacher recruitment.

[25] Note that unlike some other states in India, where contract teachers are given preference for getting hired as regular teachers, AP had no history of doing so, and there was no provision for such 'regularization' of contract teachers. Thus, it is unlikely that the reason for higher effort of the contract teachers was the prospect of being rewarded for effort by being hired as a civil-service teacher.

[26] These results are consistent with other experimental studies in India showing that supplemental instruction programs delivered at the level of learning of the student (as opposed to the textbook) have been very effective at improving test scores even though these programs have typically been delivered by young women on modest stipends (usually even lower than the salary of a contract teacher) who typically have a secondary school or high school qualification and no formal training (Banerjee et al 2007, 2010, 2012; Lakshminarayana et al. 2013). These

better by combining features of contract and regular teachers, but estimating the effectiveness of such combinations is beyond the scope of this paper.[27]

5.3. Absolute Returns to Hiring an Extra Contract Teacher

In addition to the relative cost effectiveness of regular and contract teachers, it is useful to estimate the *absolute* social return to hiring more contract teachers, by comparing the costs of doing so to the present discounted value of the increased earnings that may accrue from improving human capital of students. Using the best available cross-sectional estimates of the wage returns to improved test scores in India (Aslam et al. 2011), we estimate that the internal rate of return to hiring more contract teachers would be between 1250% and 16000% (or a return ranging from 12 to 160 times the initial cost). These estimates are highly suggestive and are sensitive to assumptions (see Appendix A.3. for details), but they large enough that even if the estimates on the labor market returns to test scores were to be substantially lower, it is highly likely that hiring additional contract teachers would still have a very high social rate of return. Thus, even if *existing* education budgets are not reallocated towards many contract teachers in lieu of a single regular teacher, it is likely to be a good public investment to *expand* the education budget to hire more contract teachers over and above the current allocation of teaching resources to schools.[28]

6. Conclusion

Regular teachers in India are well qualified, but command a substantial wage premium (greater than a factor of five) over the market clearing wage of private school (and contract) teachers that can be 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/rent. The hiring of contract teachers can be a much more cost-efficient

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studies are different from ours because they also emphasize remedial pedagogy (whereas we do not modify pedagogy in any way), but they also highlight that significant learning gains can be delivered by locally-hired teachers who are much less educated and qualified than civil-service teachers.

[27] Some possibilities include hiring qualified and trained teachers on renewable contracts at the village level or hiring contract teachers and training them to focus on teaching at the level of the student as opposed to the level of the text book (Banerjee and Duflo 2011; Pritchett and Beatty 2012). These may be especially promising combinations because there is evidence to suggest that teacher knowledge and effort may be complements in this setting (Muralidharan and Sundararaman 2011, Muralidharan 2012). Another complementary possibility would be to train school committees to better manage contract teachers like in Duflo et al. (2012).

[28] This is relevant because while national and state education budgets are heavily contested by teacher unions, there are often discretionary funds for education at the district and sub-district level. These results suggest that using these funds to relax the budget constraint of schools and letting them hire an extra contract teacher is likely to be an effective use of these discretionary funds.

way of adding teachers to schools because none of these three sources of wage premiums are applicable for them. However, since locally-hired contract teachers are not as qualified or trained as civil-service teachers, opponents of the use of contract teachers have posited that the use of contract teachers will not lead to improved learning.

We present experimental evidence from an "as is" expansion of the existing contract teacher policy of the government of Andhra Pradesh, implemented by the Government of AP 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 that contract teachers are no less effective in improving student learning than regular teachers who are more qualified, better trained, and paid five times higher salaries. Thus, the strong beliefs embedded in the education policies of many developing countries that contract teachers are (a) ineffective, and (b) inferior to civil-service teachers (even if effective), are not supported by our evidence.

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. In particular, expensive policy initiatives to get highly qualified teachers to remote areas may be much less cost effective than hiring *several* local contract teachers to provide much more attention to students at a similar cost. Doing so would enable public schools to have much lower PTR's, much lower levels of multi-grade teaching, and also provide additional teaching resources to support first-generation learners who fall behind relative to the syllabus and appear to not learn much in spite of being enrolled in school for five years (Muralidharan and Zieleniak 2013).[29]

There are two caveats to the discussion above. First, our results apply to the current *margin* of the education system and speak to the relative effectiveness of hiring contract and regular teachers given the existing stock of regular and contract teachers. It is possible that the presence of regular teachers (and their support and monitoring) is required for contract teachers to be effective, and thus our results do not imply that all regular teachers should be replaced with

[29] Observing the input choices of private schools suggests that this is what a politically unconstrained, optimizing producer of rural education services would do. Note that hiring teachers with a 'contract teacher' profile allows private schools to have lower PTR's, and much lower levels of multi-grade teaching, in spite of having per-student spending that is only *one-third* of the spending in public schools (see Muralidharan and Sundararaman 2013).

contract teachers. Second, our results apply to primary school and in particular a context where a large fraction of students are first generation learners without educated parents at home and without having attended pre-school. Thus, while a high-school educated contract teacher may be highly effective at teaching these children, our results may not apply beyond primary school.

Opponents of the use of contract teachers worry that the expanded use of untrained teachers will erode the professionalism of teaching by increasing the share of untrained teachers and reducing incentives to improve teacher training.   A second concern is that hiring larger numbers of contract teachers will lead to demands that they all be regularized into civil-service status, which may be politically difficult to resist given the strengths of teacher unions - especially around the time of elections. If such regularization (and the corresponding five-fold increase in pay) were to happen, it would defeat the economic case for hiring a large number of contract teachers in the first place. Finally, there have also been cases where courts in India have ruled that contract teachers should be regularized based on principles of 'equal pay for equal work' (Robinson and Gauri 2010), and thus expansion of contract teacher hiring may not be consistent with existing labor laws.

Many of these concerns can be addressed by hiring new teachers as contract teachers at the village (or school) level, and creating a career-ladder whereby bonuses, pay raises, and promotion to regular civil-service rank are based on multiple measures of performance over time. Continuous training and professional development could be a natural component of this career progression, and integrating contract and regular teachers into a career path could address most of the concerns above. Pritchett and Murgai (2007) and Muralidharan (2013a, and 2013b) provide a practical discussion of how such a system may be implemented in practice, and are policy-focused complements to this paper.

There is also a potentially important extension based on our results. The public school system in India starts in grade one and children do not typically attend a pre-school, and have typically had no preparation in being 'school ready' when they start school (unlike private school students, who typically have two years of pre-kindergarten and kindergarten education before the first grade).   The effectiveness of contract teachers in a primary school setting, suggest that there may be high social returns to hiring teachers with similar demographic characteristics to provide pre-school instruction to first-generation learners. This may be an especially promising area for future pilots and evaluation.

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Table 1: Teacher Characteristics by Type

Panel A: Regular Teachers have Significantly Different Characteristics from Contract Teachers (Control School Sample)

	Regular Teachers	Contract Teachers	P-value (H0: Diff=0)
Male	63.1%	31.8%	0.005***
Age	38.35	23.81	0.000***
College Degree or Higher	84.3%	45.5%	0.002***
Formal Teacher Training Degree or Certificate	98.3%	9.1%	0.000***
Received any Training in last twelve months	93.5%	54.5%	0.000***
From the same village	7.2%	81.8%	0.000***
Distance from home to school (km)	11.92	1.091	0.000***
Teacher Salary (Rs./month)	8698.1	1000 (1500)	0.000***

Panel B: The Contract Teachers Provided to Treatment Schools had the Same Characteristics on Average as the Typical Contract Teacher in Control Schools

	Contract Teachers in Treatment Schools	Contract Teachers in Control Schools	P-value (H0: Diff=0)
Male	32.0%	31.8%	0.99
Age	25.56	23.81	0.18
College Degree or Higher	47.6%	45.5%	0.87
Formal Teacher Training Degree or Certificate	16.5%	9.1%	0.32
Received any Training in last twelve months	36.9%	54.5%	0.10
From the same village	89.3%	81.8%	0.42
Distance from home to school (km)	0.553	1.091	0.15
Teacher Salary (Rs./month)	1001.0	1000	0.91

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 (2006 - 07). The main 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: Treatment and Control Schools were Balanced on Observable Characteristics

		Panel A (Mean Pre-program Characteristics)		
		[1]	[2]	**[3]**
		Comparison Schools	Extra Contract Teacher Schools	**P-value (H0: Diff=0)**
	School-level Variables			
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**
	Baseline Test Performance			
7	Math (Raw %)	18.5	17.31	**0.34**
8	Math (Normalized - in Std. deviations)	0.041	-0.043	**0.29**
9	Telugu (Raw %)	35.1	34.343	**0.66**
10	Telugu (Normalized - in Std. deviations)	0.018	-0.019	**0.65**

		Panel B (Mean Turnover/Attrition During Program)		
	Teacher Turnover and Attrition	[1]	[2]	[3]
		Comparison Schools	Extra Contract Teacher Schools	**P-value (H0: Diff=0)**
	Year 1 (relative to Year 0)			
11	Teacher Attrition (%)	0.30	0.31	**0.76**
12	Teacher Turnover (%)	0.34	0.33	**0.85**
	Year 2 (relative to Year 0)			
13	Teacher Attrition (%)	0.36	0.42	**0.17**
14	Teacher Turnover (%)	0.34	0.34	**0.98**
	Student Turnover and Attrition			
	Year 1 (relative to Year 0)			
15	Student Attrition from baseline to end of year tests	0.08	0.07	**0.13**
16	Baseline Maths test score of attritors	-0.16	-0.17	**0.90**
17	Baseline Telugu test score of attritors	-0.27	-0.24	**0.76**
	Year 2 (relative to Year 0)			
18	Student Attrition from baseline to end of year tests	0.22	0.19	**0.14**
19	Baseline Maths test score of attritors	-0.13	-0.03	**0.39**
20	Baseline Telugu test score of attritors	-0.19	-0.14	**0.63**

Notes:

1. The school infrastructure index sums 6 binary variables (coded from 0 - 6) indicating the existence of a brick building, a playground, a compound wall, a functioning source of water, a functional toilet, and functioning electricity.

2. The school proximity index ranges from 8-24 and sums 8 variables (each 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. A higher value of the index indicates being further away from these facilities.

3. Teacher attrition refers to the fraction of teachers in the school who left the school during the year, while teacher turnover refers to the fraction of new teachers in the school at the end of the year (both are calculated relative to the list of teachers in the school at the start of the year)

4. The p-values 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 p-values are computed treating each school as an observation.

Table 3: Provision of an Extra Contract Teacher to Schools Led to a Significant Increase in Student Test Scores in both Math and Language

	Dependent Variable (All 3 Panels): Normalized End of Year Test Score			
	Panel A: Combined - Math and Language (Telugu)			
	After 1 Year		After 2 Years	
	[1]	[2]	[3]	[4]
Extra Contract Teacher School	**0.102*** (0.035)**	**0.094*** (0.035)**	**0.156*** (0.045)**	**0.152*** (0.048)**
Observations	27630	24930	26142	22565
R-squared	0.348	0.378	0.203	0.227
	Panel B: Math			
	After 1 Year		After 2 Years	
	[1]	[2]	[3]	[4]
Extra Contract Teacher School	**0.125*** (0.039)**	**0.112*** (0.039)**	**0.158*** (0.051)**	**0.168*** (0.055)**
Observations	13742	12399	13020	11236
R-squared	0.331	0.361	0.197	0.215
	Panel C: Language (Telugu)			
	After 1 Year		After 2 Years	
	[1]	[2]	[3]	[4]
Extra Contract Teacher School	**0.079** (0.035)**	**0.076** (0.035)**	**0.153*** (0.042)**	**0.135*** (0.045)**
School and Household Controls (All 3 Panels)	No	Yes	No	Yes
Observations	13888	12531	13122	11329
R-squared	0.379	0.409	0.218	0.247

Notes:

1. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level. They also include lagged normalized test scores (these are set to 0 for students in grade 1, and for students in grade 2 for the regressions showing 2-year effects - since these cohorts did not have a baseline test). All test scores are normalized relative to the distribution of scores in the control schools in the same grade, test, and year.

2. The two year treatment effect regressions 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. School controls include infrastructure and proximity indices as defined in Table 2. Household controls include a household asset index, parent education index (both defined as in Table 4), student gender, and an indicator for being from a scheduled (disadvantaged) caste/tribe.

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

Table 4: Heterogeneous Treatment Effects By School and Household Characteristics

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Log Number of Students	Proximity (8 - 24)	Infrastructure (0 - 6)	Household Affluence (0 - 7)	Parental Literacy (0 - 4)	SC or ST (lower caste)	Male	Baseline Score
	After 1 Year							
Covariate	-0.079**	-0.009	0.013	0.025**	0.063***	-0.007	0.014	0.501***
	(0.039)	(0.008)	(0.021)	(0.010)	(0.011)	(0.033)	(0.018)	(0.022)
Interaction	**-0.023**	**0.019***	**-0.037**	**0.013**	**0.003**	**-0.044**	**-0.012**	**0.019**
	(0.067)	**(0.011)**	**(0.035)**	**(0.016)**	**(0.017)**	**(0.047)**	**(0.030)**	**(0.030)**
Observations	27630	26788	26788	25651	25638	27630	26207	25979
R-squared	0.350	0.356	0.356	0.368	0.371	0.348	0.361	0.364
	After 2 Years							
Covariate	-0.081	-0.005	0.038	0.046***	0.072***	-0.053	0.015	0.435***
	(0.054)	(0.009)	(0.032)	(0.012)	(0.015)	(0.041)	(0.025)	(0.023)
Interaction	**-0.108**	**0.043*****	**-0.124*****	**0.004**	**-0.024**	**-0.011**	**-0.019**	**0.032**
	(0.078)	**(0.016)**	**(0.041)**	**(0.019)**	**(0.022)**	**(0.061)**	**(0.038)**	**(0.038)**
Observations	26142	26142	26142	22588	22569	26142	22966	24756
R-squared	0.209	0.209	0.209	0.221	0.221	0.204	0.215	0.203

Notes:

1. Each column in each panel reports the results of a regression that includes the covariate in the column title, a binary treatment indicator (not shown), and a linear interaction term testing for heterogeneous effects of the treatment along the covariate concerned.

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

3. The school infrastructure and proximity index are as defined in Table 2

4. The household asset index ranges from 0 to 7 and is the sum of seven binary variables indicating whether the household has an electricity connection, has a water source at home, has a toilet at home, owns any land, owns their home, has a brick home, and owns a television.

5. Parental education is scored from 0 to 4 in which a point is added for each of the following: father's literacy, mother's literacy, father having completed 10th grade, and mother having completed 10th grade

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

Table 5: Teacher Absence and Effort

Panel A: Contract Versus Regular Teachers

Teacher Absence

	Contract Teachers	Regular Teachers	Difference	Difference with School Fixed Effects
Year 1	17.6%	25.7%	-8.1%***	-9.5%***
Year 2	17.9%	29.2%	-11.3%***	-16.4%***
Combined	17.8%	27.3%	-9.5%***	-12.2%***

Teachers Observed Actively Teaching

	Contract Teachers	Regular Teachers	Difference	Difference with School Fixed Effects
Year 1	53.5%	47.7%	5.8%*	7.8%***
Year 2	42.9%	35.3%	7.6%***	7.7%***
Combined	48.4%	42.2%	6.2%***	7.3%***

Panel B : Regular Teachers in ECT Schools versus those in Control Schools

Teacher Absence

	Regular teachers in ECT schools	Regular teachers in non-ECT schools	Difference	Difference with Mandal fixed effects
Year 1	26.2%	25.3%	0.9%	0.5%
Year 2	31.2%	27.4%	3.8%	4.0%*
Combined	28.4%	26.2%	2.2%	2.0%

Teachers Observed Actively Teaching

	Regular teachers in ECT schools	Regular teachers in non-ECT school	Difference	Difference with mandal fixed effects
Year 1	44.2%	50.9%	-6.7%*	-6.6%***
Year 2	34.7%	35.8%	-1.1%	-1.0%
Combined	40.1%	44.3%	-4.2%	-4.1%**

Notes

1. All standard errors are clustered at the school level. For school fixed effects, standard errors are clustered at the teacher level.

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

Table 6: The Provision of an Extra Contract Teacher Significantly Reduced School-level Pupil Teacher Ratio (PTR) and Class-Size

	Year 1			Year 2		
	Treatment	Control	Difference	Treatment	Control	Difference
	[1]	[2]	[3]	[4]	[5]	[6]
School Pupil-Teacher Ratio (PTR)	25.409	32.663	-7.254***	28.309	37.756	-9.447***
	[11.605]	[11.930]	(1.666)	[10.952]	[13.300]	(1.722)
Log School PTR	3.13	3.404	-0.274***	3.255	3.555	-0.300***
	[0.475]	[0.442]	(0.066)	[0.450]	[0.422]	(0.062)
Class Size	28.027	33.683	-5.656***	31.724	39.104	-7.380***
	[16.485]	[15.953]	(1.803)	[16.232]	[18.263]	(1.886)
Log Class Size	3.163	3.388	-0.225***	3.305	3.54	-0.235***
	[0.597]	[0.536]	(0.064)	[0.583]	[0.532]	(0.063)

Notes

1. Numbers in brackets are the standard deviations of the underlying variable (in columns 1, 2, 4, and 5), and those in parentheses are the standard errors of the differences (in columns 3 and 6). Differences presented in columns 3 and 6 are based on regressing the concerned indicator on a treatment dummy with mandal fixed effects. Results are at the school/class level and not weighted by enrollment.

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

Table 7: Reducing School-Level Pupil-Teacher Ratio (PTR) by 10% using a <u>Contract Teacher</u> would Improve Mean Student Test Scores in the School by 0.03 σ/year

	Dependent Variable: Normalized Student Test Scores - Pooled across Math and Language (First Year Only)					
	Combined		Math		Telugu	
	[1]	[2]	[3]	[4]	[5]	[6]
Change in Log School PTR induced by additional (experimental) Contract Teacher (β1)	-0.308***	-0.337***	-0.389***	-0.421***	-0.227**	-0.253**
	(0.113)	(0.110)	(0.129)	(0.126)	(0.108)	(0.106)
Original Log School PTR		-0.232***		-0.264***		-0.201***
		(0.060)		(0.069)		(0.059)
Observations	27630	27630	13742	13742	13888	13888
R-squared	0.348	0.353	0.332	0.338	0.379	0.383

Notes:

1. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level. They also include lagged normalized test scores (these are set to 0 for students in grade 1 who did not have a baseline test). All test scores are normalized relative to the distribution of scores in the control schools in the same grade, test, and year.

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

Table 8: Reducing School-Level Pupil-Teacher Ratio (PTR) by 10% using a Regular Teacher would Improve Mean Student Test Scores in the School by 0.02 σ/year

Impact of Log School PTR on Test Scores (when taught by Regular Teachers) using 2 and 5 Years of Data														
	Estimation Sample: Control Schools with No Contract Teachers													
	Dependent Variable: Normalized Student Test Scores (Pooled across Math and Language)													
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	**[13]**	**[14]**
Log School Pupil-Teacher Ratio (PTR) (β2)	**-0.219*****	**-0.223*****	**-0.202*****	**-0.213*****	**-0.240*****	**-0.262*****	**-0.247*****	**-0.236*****	**-0.214*****	**-0.208*****	**-0.195*****	**-0.187*****	**-0.197*****	**-0.183****
	(0.067)	**(0.069)**	**(0.062)**	**(0.065)**	**(0.082)**	**(0.081)**	**(0.049)**	**(0.047)**	**(0.045)**	**(0.043)**	**(0.046)**	**(0.048)**	**(0.069)**	**(0.070)**
Years of Data	2	2	2	2	2	2	5	5	5	5	5	5	**5**	**5**
Household Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
School Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	NA
District Fixed Effects	No	No	Yes	Yes	No	No	No	No	Yes	Yes	No	No	No	No
Mandal Fixed Effects	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes	No	No
School Fixed Effects	No	No	No	No	No	No	No	No	No	No	No	No	**Yes**	**Yes**
Observations	19500	17926	19500	17926	19500	17926	45379	37232	45379	37232	45379	37232	**45379**	**37232**
R-squared	0.279	0.301	0.292	0.315	0.328	0.350	0.255	0.267	0.263	0.275	0.297	0.309	**0.311**	**0.325**
P-value (H0: β2 = β1)	**0.3747**	**0.3972**	**0.3045**	**0.3512**	**0.4951**	**0.5984**	**0.4639**	**0.4073**	**0.3065**	**0.2828**	**0.2372**	**0.2167**	**0.2824**	**0.24**
P-value (H0: β2 = 2*β1)	**0.0524**	**0.0561**	**0.0437**	**0.0502**	**0.0714**	**0.0868**	**0.0607**	**0.0539**	**0.0417**	**0.0394**	**0.0338**	**0.0317**	**0.0387**	**0.0342**
P-value (H0: β2 = 3*β1)	**0.0204**	**0.0216**	**0.0178**	**0.0198**	**0.026**	**0.0304**	**0.0229**	**0.0209**	**0.0172**	**0.0165**	**0.0146**	**0.0139**	**0.0158**	**0.0144**
P-value (H0: β2 = 5*β1)	**0.0087**	**0.0091**	**0.008**	**0.0086**	**0.0102**	**0.0114**	**0.0095**	**0.0089**	**0.0078**	**0.0076**	**0.007**	**0.0068**	**0.0073**	**0.0069**

Notes:

1. All regressions include fixed effects as indicated with standard errors clustered at the school level. They also include lagged normalized test scores (these are set to 0 for students in grade 1 who did not have a baseline test). All test scores are normalized relative to the distribution of scores in the control schools in the same grade, test, and year.

2. The results in columns 1-6 use 2 years of data, while those in columns 7-14 use 5 years of data from the control schools. Columns 13 and 14 are estimated using school-level panel data exploiting variation in school-level PTR over the 5 years that the control schools were tracked. Thus, the identifying variation in Log School PTR comes from changes in cohort size and movements of teachers over time in the same school (in the sample of control schools which did not have a contract teacher in the years included in the analysis). See Appendix A.1. for further details about the data and the estimation sample.

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

Table 9: Private School Teachers are much more similar to Contract Teachers than to Regular Teachers (and they are paid even less than contract teachers though they are more likely to be College Graduates)

	Regular Teacher	Contract Teachers	Private School Teachers	P-value (Null Hypothesis: Contract Teacher = Private School Teacher
Female =1	62.5%	80.9%	88.4%	0.047
Age of Teacher	38.39	26.95	26.57	0.626
Teacher Passed College =1	87.0%	31.3%	52.4%	0.000
Received Any Teacher Training =1	99.2%	21.2%	14.1%	0.095
Received Training Within Past Yr =1	78.1%	43.5%	2.8%	0.000
Teacher from the Same Village =1	19.4%	80.2%	54.0%	0.000
Distance to School (km)	11.73	1.01	2.48	0.000
Gross Montly Salary (Rs.)	**12,162**	**1,910**	**1,527**	**0.000**
Percentage of Absent Teachers	20.7%	11.3%	9.7%	0.487

Notes

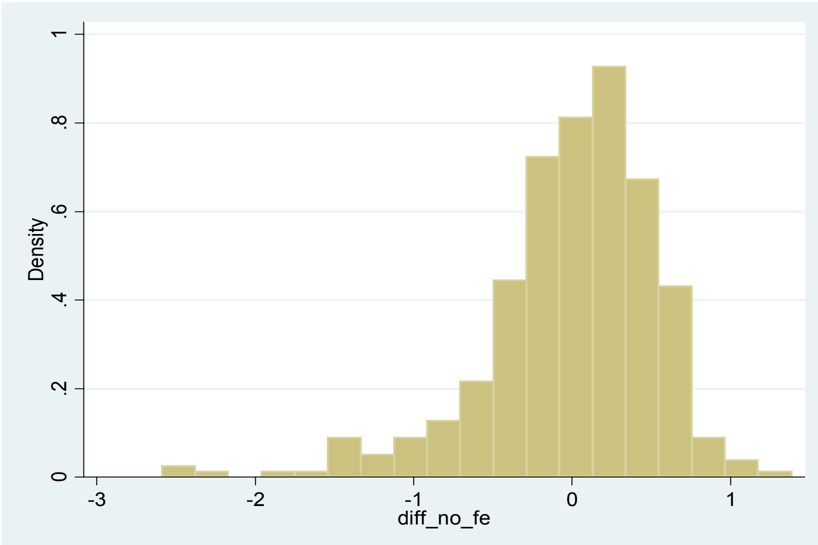
1. Robust standard errors clustered at the school level were used to obtain the p-values for the null hypothesis

2. The data used for this table comes from a different study on school vouchers and school choice in different sub-districts of the SAME districts. This data was collected based on teacher interviews in early 2009 (Muralidharan and Sundararaman 2013).

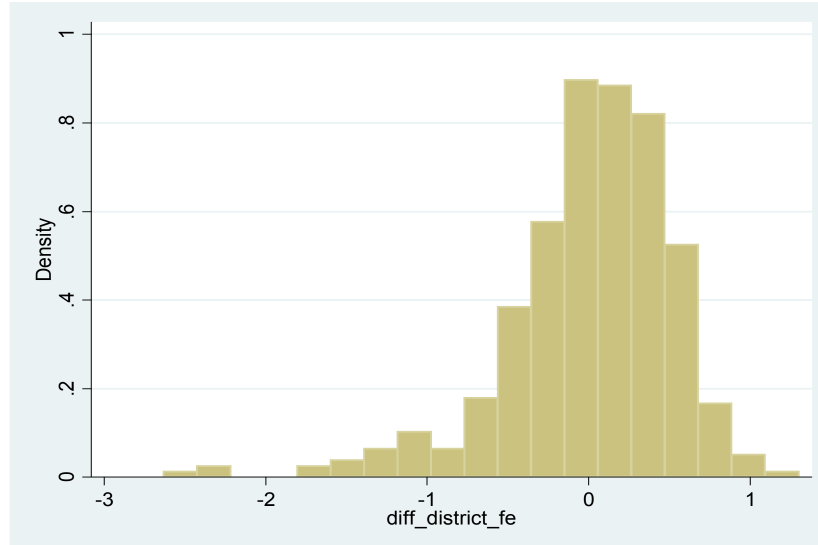
3. Differences in regular and contract teacher characteristics relative to Table 1 reflect (a) the time gap between the 2 sets of data collection of around 3 years, and (b) the fact that the data used for Table 9 comes from villages that had a private school, which tend to be larger than the typical village in AP. The sample in Table 1 is from a representative set of rural government run schools, while the sample in Table 9 is from a sample of villages that have private schools (though the public school data in Table 9 is from the same villages as the private schools in Table 9).

Figure 1: Variation in Log_School_PTR (Using only Regular Teachers) in the Different Specifications in Table 8

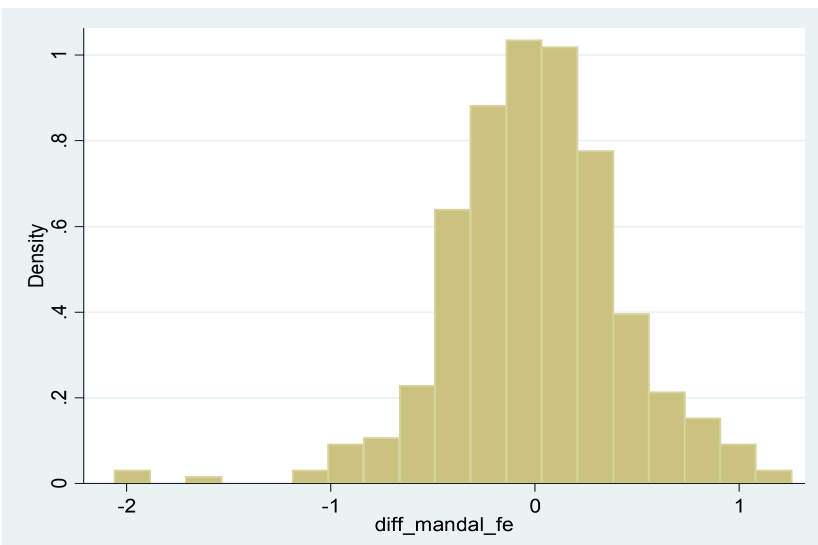
NO FIXED EFFECTS:



DISTRICT FIXED EFFECTS



MANDAL FIXED EFFECTS



SCHOOL-FIXED EFFECTS

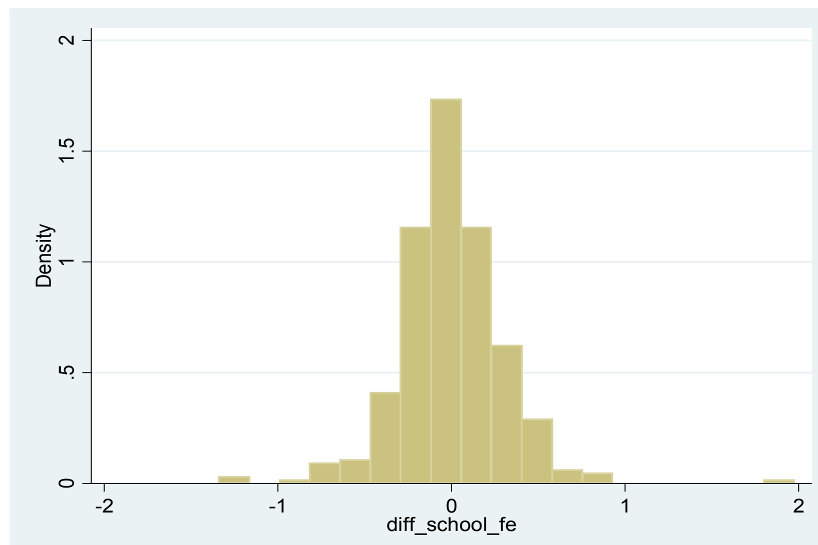
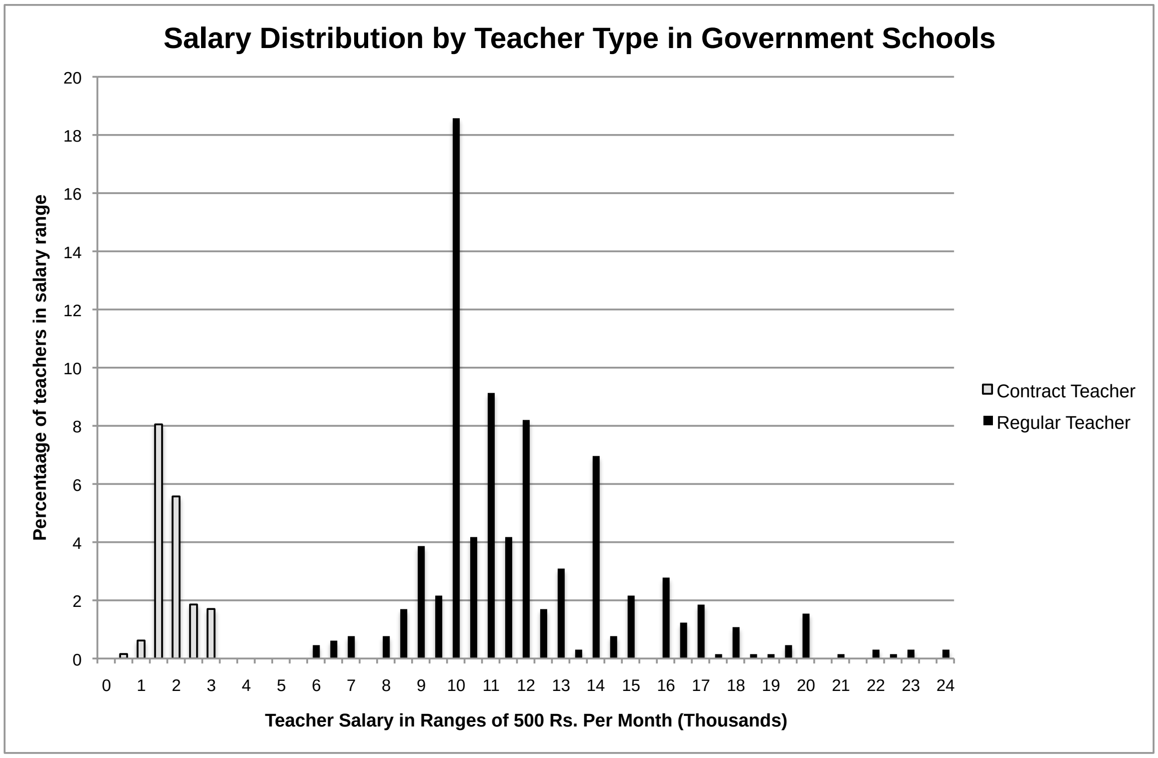
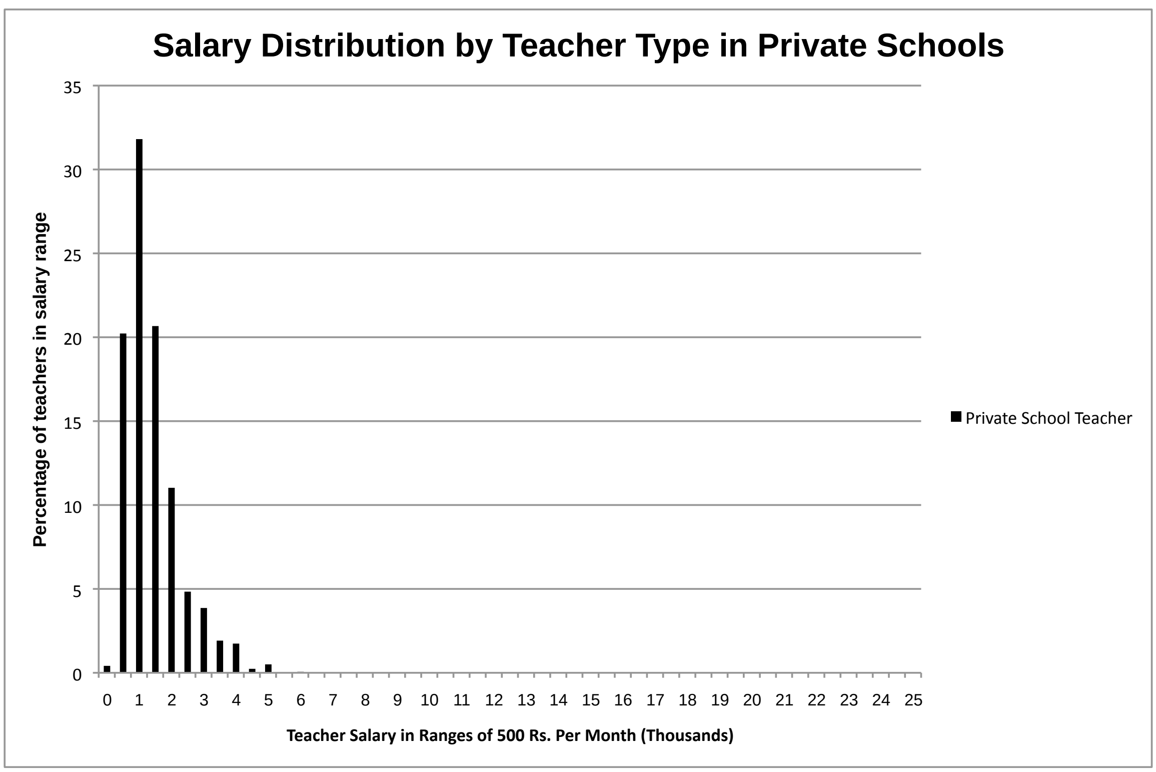


Figure 2: Salary Distribution by School and Teacher Type





Appendices (A.1. to A.3.)

A.1. Data and Identification Checks for Table 8 - Columns 13 and 14

As noted in the text, our most credible estimate of the impact of reducing school-level PTR with a regular teacher on test score gains are those in Table 8 (Columns 13 and 14). The estimating sample consists of control schools over the 5 years that we have data for, in years in which the schools had only regular teachers (thus the PTR is calculated *only* using regular teachers). There were a total of 99 control schools over the 5 year period (one of the original 100 got merged with another school during this period) and Appendix Table 1 shows the estimation sample by the number of years in which the concerned control school had no contract teachers.

Appendix Table 1: Estimation Sample for Table 8 (Columns 7-14)

1	**Number of Years with No Contract Teachers:**	**5**	**4**	**3**	**2**	**1**	**0**	**Total**
2	Number of Schools	43	22	14	12	7	1	99
3	Number of School-Year Level Observations in the Estimation Sample in Table 8 (Columns 13, 14)	215	88	42	24	0	0	369
4	Number of Adjacent School-Year Level Observations in the Estimation Sample (for Robustness checks in Appendix Tables 2 and 3)	172	51	17	5	0	0	245

We see in Row 2 that 43 control schools had no contract teacher in any of the 5 years, 22 had none in 4 years, 14 in 3 years, and 12 in 2 years. 7 schools had contract teachers in all but one year, and 1 school had a contract teacher in every year. Row 3 shows the number of school-years in the estimation sample of Table 8 (Columns 13-14) that are contributed by each of these schools. This simply multiplies rows 1 and 2 - except in the case where a school has only 1 year without a contract teacher, because there will be no within-school variation in PTR (over time) to exploit in a specification with school fixed effects. Thus out of a total possible number of 495 school-years in the estimation sample (99 x 5), the estimation sample for columns 7-12 of Table 8 has 369 school-years in it.

The variation in PTR that we use in the specification with school fixed effects comes from (a) changes in student enrollment and cohort sizes over time, and (b) teacher transfers into and out of schools over time. This specification eliminates concerns of unobserved heterogeneity across schools. The main remaining threat to identification in the specification with school fixed effects is the possibility that school-level PTR changed over time in response to lagged test scores. So for instance, if schools got extra teachers in response to poor test-score performance in the previous year, it is possible that a correlation between PTR reduction and test score gains may simply reflect mean reversion. We show in Appendix Table 2 that there is no correlation between changes in PTR and lagged test scores (with the point estimate being close to zero). This strongly suggests that the identifying variation in PTR is quasi-random, and is uncorrelated with either levels or trajectories of student test scores.

Appendix Table 2: Changes in Log PTR over time in a school are not correlated with lagged test scores

Estimation Sample: Control Schools with No Contract Teachers in Current and Previous Year		
Dependent Variable: Change in Log PTR		
	[1]	**[2]**
Lagged Test Score	-0.005	-0.003
	(0.009)	(0.008)
Observations	33153	33153
R-squared	0.000	0.241
School Fixed Effects	No	Yes

Notes:

1. All regressions have standard errors clustered at the school level. They also include lagged normalized test scores (these are set to 0 for students in grade 1 who did not have a baseline test). All test scores are normalized relative to the distribution of scores in the control schools in the same grade, test, and year.

2. The sample is limited to those observations for which the current and previous year in the school had only regular teachers.

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

Note that the estimation sample in Appendix Table 2 smaller than that in Table 8 (Columns 13 and 14) because the table above only uses the school-years where there were no contract teachers in the school in *adjacent* years (which is necessary to show that there was no correlation between changes in PTR between consecutive years and the test scores at the end of the first of these years). Thus, the test in Appendix Table 2 only uses the sample in Row 4 of Appendix Table 1, while Table 8 (Columns 13, 14) can use the full sample in Row 3 of Appendix Table 1.

We verify that our core results are not affected by this change in sample, by re-estimating Table 8 (Columns 13, 14) using the restricted sample used for Appendix Table 2, and we see below in Appendix Table 3 that the estimates of β_2 in (4) with school fixed effects are unchanged from those in Table 8 (Columns 13 and 14). The stability of the estimate of β_2 across all the columns of Table 8 and in various restricted samples strongly suggest that estimates of β_2 in Table 8 (Columns 13 and 14) can be regarded as the causal effect of changing PTR on annual student test score gains (in a value-added specification).

Appendix Table 3: The Estimates in Table 8 (Columns 13, 14) are unchanged in the restricted sample of Appendix Table 2

Estimation Sample: Control Schools with No Contract Teachers in Current and Previous Year		
Dependent Variable: Normalized Student Test Scores (Pooled across Math and Language)		
	[1]	**[2]**
Log School Pupil-Teacher Ratio (PTR) (β2)	**-0.208****	**-0.197****
	(0.092)	**(0.089)**
Years of Data	**5**	**5**
Household Controls	No	Yes
School Fixed Effects	**Yes**	**Yes**
Observations	**27544**	**22431**
R-squared	**0.312**	**0.326**
P-value (H0: β2 = β1)	**0.3587**	**0.3166**
P-value (H0: β2 = 2*β1)	**0.0477**	**0.0425**
P-value (H0: β2 = 3*β1)	**0.018**	**0.0165**
P-value (H0: β2 = 5*β1)	**0.0077**	**0.0073**

Notes:

1. The specification is identical to that in Table 8 - Columns 13, 14, but the estimation sample is restricted to that in Appendix Table 2

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

A.2. Comparing Contract and Civil-Service Teachers Within Schools

Since we can match students in each year to their teacher and know the teacher type, we can estimate the effect on gains in student learning of being taught by a contract teacher as opposed to a regular teacher using the specification:

$$T_{ijkm}(Y_n) = \alpha + \gamma_j \cdot T_{ijkm}(Y_{n-1}) + \delta_{CT} \cdot CT_{jk} + \beta \cdot X_{ijk} + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (5)$$

where the test score variables and error terms are defined as in (1), CT_{jk} is a dummy variable indicating whether the student was in a class taught by a contract teacher, X_{ijk} are a set of classroom, school and household controls, and δ_{CT} is the parameter of interest, which indicates the extent to which students taught by a contract teacher have different test score gains from those taught by a regular teacher (since the same teacher teaches all subjects within a grade, we pool test scores across math and language in our results). We estimate (5) using no fixed effects, with school fixed effects, and finally with student fixed effects (using only the sample of students who change teacher type during the course of the two years of the experiment), and find that that there is no differential effect on learning gains for students taught by contract teachers relative to those taught by regular teachers (Appendix Table 4).

These findings further support the main finding of this paper, which is that contract teachers are no less effective than regular civil-service teachers in spite of being less educated, less qualified, and being paid much lower salaries. Nevertheless, as we discuss in the text, we only present these results for completeness and do not focus on them because we cannot rule out the possibility that assignment of teachers to specific grades may be correlated with unobservable factors that are also correlated with student learning gains.

A.3 Calculating the Absolute Return to Hiring an Extra Contract Teacher

The main discussion in the paper focused on the cost effectiveness of contract teachers at increasing test scores relative to regular teachers (since reducing PTR with regular teachers is the biggest fiscal commitment under the RtE Act). However, it is also useful to briefly consider the absolute social return to hiring more contract teachers, by comparing the costs of doing so to the present discounted value of the increased earnings that may accrue from improving human capital of students. This is the ideal estimate that a social planner would want because it also helps in thinking about resource allocation across sectors in the economy, and whether the investment is worth borrowing for.

Recent cross-sectional estimates of the returns to cognitive achievement in India suggest returns of 16% for scoring one σ higher on a standardized math test and 20% for scoring one σ higher on a standardized language test (Aslam et al. 2011). Assuming that the test score gains in this program correspond to a similar long-term difference in human capital accumulation,[1] the two year treatment effect would correspond to a 5.5% increase in wages (0.16σ x 0.16 + 0.15σ x 0.20). Depending on assumptions about the rate of wage growth and discount rates, we obtain estimates of an internal rate of return ranging from 1250% to 16000% (or a return ranging from 12.5 to 160 times the initial cost).[2] These estimates are clearly a suggestive 'back of the envelope' exercise, and are highly sensitive to the assumptions (especially on the wage returns of test score gains). This is why our main discussion in the text follows the default approach to cost-effectiveness calculations in the literature on education in developing countries, which has focused on the cost per unit of test-score gains (Dhaliwal et al. 2012).

But we present the estimates on the absolute returns to hiring a contract teacher to show that they are large enough that even if the estimates on the labor market returns to test scores were to be substantially lower, or the program costs higher, the program would still have a very high rate of return. Thus, even if contract teachers are not hired instead of regular teachers, it is likely to be a good public investment to improve learning outcomes by hiring more contract teachers over and above the current allocation of teaching resources to schools.

---

[1] Chetty et al. (2011) show that there were significant long-term benefits to the class-size reductions under the Tennessee STAR program even though the test score gains faded away a few years into the program. Deming (2009) shows similar long-term gains to Head Start, though the test score gains fade away here as well. Of course, these studies are only suggestive about the long-term effects of programs that produce test-score gains, because there is no precise measure of the extent to which test-score gains in school translate into higher long-term wages.

[2] The minimum wage for in AP in 2010 was Rs. 112/day. Assuming 250 working days/year yields an annual income of Rs. 28,000 and a 5.5% increase in wage would translate into additional income of Rs. 1,540/year. We treat this as a 40-year stream of *fixed* additional earnings (which is *very conservative* since we don't assume wage growth) and discount at 10% a year to obtain a present value of Rs. 15,060 per student at the time of entering the labor market. Since the average student in our project is 8 years old, we assume that they will enter the labor market at age 20 and further discount the present value by 10% annually for another 12 years to obtain a present value of Rs. 4,800/student. The average school had 65 students who took the tests, which provides an estimate of the total present value of Rs. 312,000. The cost of the program per school for two years was Rs. 25,000, which provides an IRR estimate of 1250%. If we were to assume that wages would grow at the discount rate, the calculation yields an IRR estimate of 16000%.

Appendix Table 4: Comparing the Relative Effectivess of Regular and Contract Teachers in Increasing Test Scores (within schools)

	Dependent Variable: Normalized Student Test Scores (Pooled across Math and Language)					
	No Fixed Effects	**School Fixed Effects**		**Student Fixed Effects**		
	[1]	[2]	[3]	[4]	[5]	[6]
Taught by a Contract Teacher	0.026 (0.042)	-0.020 (0.035)	-0.015 (0.034)	0.013 (0.020)	0.023 (0.023)	0.082* (0.047)
Classroom Controls	No	Yes	Yes	No	Yes	Yes
Household Controls	No	No	Yes	N/A	N/A	N/A
Stable Teacher Sample (No Change in teacher grade assignment over the 2 years)	No	No	No	No	No	Yes
Observations	51336	51336	46889	8823	8823	3655
R-squared	0.260	0.349	0.367	0.751	0.753	0.814

Notes:

1. All Regressions include lagged normalized test scores (set to 0 for grade 1 students), with standard errors clustered at the school level

2. Household controls include a household asset index, parent education index, child gender an indicator for being from a scheduled caste/tribe. Classroom controls include class size (in logs), and an indicator for multigrade teaching.

3. Regressions with student fixed effects use the restricted sample of students who switched from being taught by a contract teacher to a regular teacher (or vice versa) over the 2 years (i.e. - the variation is coming from the natural grade progression of students, where they also happen to change teacher type as a result).

4. The Stable Sample refers to the subset of students who switched teacher type over the 2 years, and where the same teacher continues teaching the same class in both years (i.e. - these are student fixed effects estimated off a stable sample of teacher class assignments where the only variation is coming from the grade progression of students).

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