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TEACHER PERFORMANCE PAY:
EXPERIMENTAL EVIDENCE FROM INDIA

Karthik Muralidharan
Venkatesh Sundararaman

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1050 Massachusetts Avenue
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ABSTRACT

Performance pay for teachers is frequently suggested as a way of improving education outcomes in schools, but the theoretical predictions regarding its effectiveness are ambiguous and the empirical evidence to date is limited and mixed. We present results from a randomized evaluation of a teacher incentive program implemented across a large representative sample of government-run rural primary schools in the Indian state of Andhra Pradesh. The program provided bonus payments to teachers based on the average improvement of their students' test scores in independently administered learning assessments (with a mean bonus of 3% of annual pay). At the end of two years of the program, students in incentive schools performed significantly better than those in control schools by 0.28 and 0.16 standard deviations in math and language tests respectively. They scored significantly higher on "conceptual" as well as "mechanical" components of the tests, suggesting that the gains in test scores represented an actual increase in learning outcomes. Incentive schools also performed better on subjects for which there were no incentives, suggesting positive spillovers. Group and individual incentive schools performed equally well in the first year of the program, but the individual incentive schools outperformed in the second year. Incentive schools performed significantly better than other randomly-chosen schools that received additional schooling inputs of a similar value.

Karthik Muralidharan
Department of Economics, 0508
University of California, San Diego
9500 Gilman Drive
La Jolla, CA 92093-0508
and NBER
kamurali@ucsd.edu

Venkatesh Sundararaman
South Asia Human Development Unit
The World Bank
1818 H Street, NW
Washington, DC 20433
vsundararaman@worldbank.org

1. Introduction

A fundamental question in education policy around the world is that of the relative effectiveness of input-based and incentive-based policies in improving the quality of schools. While the traditional approach to improving schools has focused on providing them with more resources, there has been growing interest in directly measuring and incentivizing schools and teachers based on student learning outcomes.¹ The idea of paying teachers based on direct measures of performance has attracted particular attention since teacher salaries are the largest component of education budgets and recent research shows that teacher characteristics rewarded under the status quo in most school systems (such as experience and master's degrees in education) are poor predictors of better student outcomes.²

However, while the idea of using incentive pay schemes for teachers as a way of improving school performance is increasingly making its way into policy,³ the empirical evidence on the effectiveness of such policies is quite limited – with identification of the causal impact of teacher incentives being the main challenge. In addition, several studies have highlighted the possibility of perverse outcomes resulting from high-powered teacher incentives,⁴ suggesting the need for caution and better evidence before expanding teacher incentive programs based on test scores.

In this paper, we contribute towards filling this gap with evidence from a large-scale randomized evaluation of a teacher performance pay program implemented in the Indian state of Andhra Pradesh (AP). We studied two types of teacher performance pay (group bonuses based on school performance, and individual bonuses based on teacher performance), with the average bonus calibrated to be around 3% of a typical teacher's annual salary. The incentive program was designed to minimize the likelihood of undesired consequences (see design details later) and

¹ This shift in emphasis can be attributed at least in part to the several studies that have pointed to the low correlations between school spending and learning outcomes (see Hanushek (2006) for a review). The “No Child Left Behind” Act of 2001 (NCLB) formalized education policy focus on learning outcomes in the US. Of course, inputs and incentives are not mutually exclusive, but the distinction has policy salience in terms of the relative importance given to the two kinds of approaches, starting from the current status quo.

² See Rivkin, Hanushek, and Kain (2005), Rockoff (2004), and Gordon, Kane, and Staiger (2006)

³ Teacher performance pay is being considered and implemented in several US states including Colorado, Florida, Tennessee, and Texas, and additional resources have been dedicated to a Federal “Teacher Incentive Fund” by the US Department of Education in 2009. International examples of attempts to tie teacher pay to performance include the UK, Israel, Chile, and Australia.

⁴ Examples of sub-optimal behavior by teachers include rote 'teaching to the test' and neglecting higher-order skills (Holmstrom and Milgrom, 1991), manipulating performance by short-term strategies like boosting the caloric content of meals on the day of the test (Figlio and Winicki, 2005), excluding weak students from testing (Jacob, 2005), focusing only on some students in response to "threshold effects" embodied in the structure of the incentives (Neal and Schanzenbach, 2008) or even outright cheating (Jacob and Levitt, 2003).

the study was conducted by randomly allocating the incentive programs across a representative sample of 300 government-run schools in rural AP with 100 schools each in the group and individual incentive treatment groups and 100 schools serving as the comparison group.⁵

This large-scale experiment allows us to answer a comprehensive set of questions with regard to teacher performance pay including: (i) Can teacher performance pay based on test scores improve student achievement? (ii) What, if any, are the negative consequences of teacher incentives based on student test scores? (iii) How do school-level group incentives compare with teacher-level individual incentives? (iv) How does teacher behavior change in response to performance pay? and (v) How cost effective are teacher incentives relative to other uses for the same money?

We find that the teacher performance pay program was highly effective in improving student learning. At the end of two years of the program, students in incentive schools performed significantly better than those in comparison schools by 0.28 and 0.16 standard deviations (SD) in math and language tests respectively. The mean treatment effect of 0.22 SD is equal to 9 percentile points at the median of a normal distribution. We find a minimum average treatment effect of 0.1 SD at every percentile of baseline test scores, suggesting broad-based gains in test scores as a result of the incentive program.

We find no evidence of any adverse consequences as a result of the incentive programs. Incentive schools do significantly better on both mechanical components of the test (designed to reflect rote learning) and conceptual components of the test (designed to capture deeper understanding of the material),⁶ suggesting that the gains in test scores represent an actual increase in learning outcomes. Students in incentive schools do significantly better not only in math and language (for which there were incentives), but also in science and social studies (for which there were no incentives), suggesting positive spillover effects. There was no difference in student attrition between incentive and control schools, and no evidence of any adverse gaming of the incentive program by teachers.

⁵ The program was implemented by the Azim Premji Foundation (a leading non-profit organization working to improve primary education in India) on behalf of the Government of Andhra Pradesh, with technical support from the World Bank. These interventions were part of a larger project called the AP RESt (Andhra Pradesh Randomized Evaluation Study) that aimed to rigorously evaluate the impact of several policy options to improve the quality of primary education in AP. We have served as technical consultants and have overseen the design, and evaluation of the various interventions.

⁶ We engaged India's leading education testing firm ("Education Initiatives") to design the tests to our specifications so that we could directly test for crowding out of higher-order skills under a performance pay program for teachers.

School-level group incentives and teacher-level individual incentives perform equally well in the first year of the program, but the individual incentive schools significantly outperformed the group incentive schools in the second year. At the end of two years, the average treatment effect was 0.27 SD in the individual incentive schools compared to 0.16 SD in the group incentive schools, with this difference being nearly significant at the 10% level.

We measure changes in teacher behavior in response to the program with both teacher interviews as well as direct physical observation of teacher activity. Our results suggest that the main mechanism for the impact of the incentive program was not increased teacher attendance, but greater (and more effective) teaching effort conditional on being present.

We find that performance-based bonus payments to teachers were a significantly more cost effective way of increasing student test scores compared to spending a similar amount of money unconditionally on additional schooling inputs. In a parallel initiative, two other sets of 100 randomly-chosen schools were provided with an extra contract teacher, and with a cash grant for school materials respectively.⁷ At the end of two years, students in schools receiving the input programs scored 0.08 SD higher than those in comparison schools. However, the incentive programs had a significantly larger impact on learning outcomes (0.22 versus 0.08 SD) over the same period, even though the total cost of the bonuses was around 25% lower than the amount spent on the inputs.

There was broad-based support from teachers for the program, and we also find that the extent of teachers' ex-ante support for performance pay (over a series of mean-preserving spreads of pay) is positively correlated with their ex-post performance. This suggests that teachers are aware of their own effectiveness and that performance pay might not only increase effort among existing teachers, but systematically draw more effective teachers into the profession over time.⁸

Our results contribute to a small but growing literature on the effectiveness of performance-based pay for teachers.⁹ The best identified studies on the effect of paying teachers on the basis

⁷ The details of the input interventions and their impact on learning outcomes are in companion papers (Muralidharan and Sundararaman (2009), and Das et al (2009)), but the summary of the input program effects are discussed in this paper to enable the comparison between inputs and incentives.

⁸ Lazear (2000) shows that around half the gains from performance-pay in the company he studied were due to more productive workers being attracted to join the company under a performance-pay system. Similarly, Hoxby and Leigh (2005) argue that compression of teacher wages in the US is an important reason for the decline in teacher quality, with higher-ability teachers exiting the teacher labor market.

⁹ Previous studies include Ladd (1999) in Dallas, Atkinson et al (2004) in the UK, and Figlio and Kenny (2007) who use cross-sectional data across multiple US states. Duflo, Hanna, and Ryan (2007) present an experimental evaluation of a program that provided incentives to teachers based on attendance. See Umansky (2005) and

of student test outcomes are Lavy (2002) and (2008), and Glewwe, Ilias, and Kremer (2008), but their evidence is mixed. Lavy uses a combination of regression discontinuity, difference in differences, and matching methods to show that both group and individual incentives for high school teachers in Israel led to improvements in student outcomes (in the 2002 and 2008 papers respectively). Glewwe et al (2008) report results from a randomized evaluation that provided primary school teachers (grades 4 to 8) in Kenya with group incentives based on test scores and find that, while test scores went up in program schools in the short run, the students did not retain the gains after the incentive program ended. They interpret these results as being consistent with teachers expending effort towards short-term increases in test scores but not towards long-term learning.¹⁰

There are several unique features in the design of the field experiment presented in this paper. We conduct the first randomized evaluation of teacher performance pay in a representative sample of schools.¹¹ We take incentive theory seriously and design the incentive program to minimize the risk of perverse outcomes, and design the study to test for a wide range of possible negative outcomes. We study group (school-level) and individual (teacher-level) incentives in the same field experiment. We measure changes in teacher behavior with both direct observations and with teacher interviews. Finally, we study both input and incentive based policies in the same field experiment to enable a direct comparison of their effectiveness.

While set in the context of schools and teachers, this paper also contributes to the broader literature on performance pay in organizations in general and public organizations in particular.¹² True experiments in compensation structure with contemporaneous control groups are rare,¹³ and

Podgursky and Springer (2007) for reviews on teacher performance pay and incentives. The term "teacher incentives" is used very broadly in the literature. We use the term to refer to financial bonus payments on the basis of student test scores.

¹⁰ It is worth noting though that evidence from several contexts and interventions suggests that the effect of almost *all* education interventions appear to decay when the programs are discontinued (see Jacob et al, 2008, and Andrabi et al, 2008), and so this inference should be qualified.

¹¹ The random assignment of treatment provides high internal validity, while the random sampling of schools into the universe of the study provides greater external validity than typical experiments by avoiding the "randomization bias", whereby entities that are in the experiment are atypical relative to the population that the result is sought to be extrapolated to (Heckman and Smith (1995)).

¹² See Gibbons (1998) and Prendergast (1999) for general overviews of the theory and empirics of incentives in organizations. Dixit (2002) provides a discussion of these themes as they apply to public organizations. Chiappori and Salanié (2003) survey recent empirical work in contract theory and emphasize the identification problems in testing incentive theory.

¹³ Bandiera, Barankay, and Rasul (2007) is a recent exception that studies the impact of exogenously varied compensation schemes (though with a sequential as opposed to contemporaneous comparison group).

our results may be relevant to answering broader questions regarding performance pay in organizations.¹⁴

The rest of this paper is organized as follows: section 2 provides a theoretical framework for thinking about teacher incentives. Section 3 describes the experimental design and the treatments, while section 4 discusses the test design. Sections 5 and 6 present results on the impact of the incentive programs on test score outcomes and teacher behavior. Section 7 discusses the cost effectiveness of the performance-pay programs, while section 8 discusses teacher responsiveness to the idea of performance pay. Section 9 concludes.

2. Theoretical Framework

2.1 Incentives and intrinsic motivation

It is not obvious that paying teachers bonuses on the basis of student test scores will even raise test scores. Evidence from psychological studies suggests that monetary incentives can sometimes crowd out intrinsic motivation and lead to inferior outcomes.¹⁵ Teaching may be especially susceptible to this concern since many teachers are thought to enter the profession due to strong intrinsic motivation. The AP context, however, suggested that an equally valid concern was the lack of differentiation among high and low-performing teachers. Kremer et al (2005) show that in Indian government schools, teachers reporting high levels of job satisfaction are *more likely* to be absent. In subsequent focus group discussions with teachers, it was suggested that this was because teachers who were able to get by with low effort were quite satisfied, while hard-working teachers were dissatisfied because there was no difference in professional outcomes between them and those who shirked. Thus, it is also possible that the lack of external reinforcement for performance can erode intrinsic motivation.¹⁶

In summary, the psychological literature on incentives suggests that extrinsic incentives that are perceived by workers as a means of exercising control over them are more likely to crowd out intrinsic motivation, while those that are seen as reinforcing norms of professional behavior

¹⁴ Of course, as Dixit (2002) warns, it is important for empirical work to be cautious in making generalizations about performance-based incentives, and to focus on relating success or failure of incentive pay to context-specific characteristics such as the extent and nature of multi-tasking.

¹⁵ A classic reference in psychology is Deci and Ryan (1985). Fehr and Falk (2002) survey the psychological foundations of incentives and their relevance for economics. Chapter 5 of Baron and Kreps (1999) provides a good discussion relating intrinsic motivation to practical incentive design and communication.

¹⁶ Mullainathan (2006) describes how high initial intrinsic motivation of teachers can diminish over time if they feel that the government does not appreciate or reciprocate their efforts.

can enhance intrinsic motivation (Fehr and Falk, 2002). Thus, the way an incentive program is framed can influence its effectiveness. The program studied here was careful to frame the incentives in terms of “recognition” of excellence in teaching as opposed to framing the program in terms of “school and teacher accountability”.

2.2 Multi-task moral hazard

Even those who agree that incentives based on test scores could improve test performance worry that such incentives could lead to sub-optimal behavioral responses from teachers. Examples of such behavior include rote 'teaching to the test' and neglecting higher-order skills (Holmstrom and Milgrom, 1991), manipulating performance by short-term strategies like boosting the caloric content of meals on the day of the test (Figlio and Winicki, 2005), excluding weak students from testing (Jacob, 2005), focusing on some students to the exclusion of others in response to “threshold effects” embodied in the incentive design (Neal and Schanzenbach, 2008) or even outright cheating (Jacob and Levitt, 2003).

These are all examples of the problem of multi-task moral hazard, which is illustrated by the following formulation from Baker (2002).¹⁷ Let \mathbf{a} be an n -dimensional vector of potential agent (teacher) actions that map into a risk-neutral principal's (social planner's) value function (V) through a linear production function of the form:

$$V(\mathbf{a}, \varepsilon) = \mathbf{f} \cdot \mathbf{a} + \varepsilon$$

where \mathbf{f} is a vector of marginal products of each action on V , and ε is noise in V .

Assume the principal can observe V (but not \mathbf{a}) and offers a linear wage contract of the form $w = s + b_v \cdot V$. If the agent's expected utility is given by:

$$E(s + b_v \cdot V) - h \cdot \text{var}(s + b_v \cdot V) - \sum_{i=1}^n a_i^2 / 2$$

where h is her coefficient of absolute risk aversion and $a_i^2 / 2$ is the cost of each action, then the optimal slope on output (b_v^*) is given by:

$$b_v^* = \frac{F^2}{F^2 + 2h\sigma_\varepsilon^2} \quad (2.2.1)$$

¹⁷ The original references are Holmstrom and Milgrom (1991), and Baker (1992). The treatment here follows Baker (2002) which motivates the multi-tasking discussion by focusing on the divergence between the performance measure and the principal's objective function.

where $F = \sqrt{\sum_{i=1}^n f_i^2}$. Expression (2.2.1) reflects the standard trade-off between risk and aligning of incentives, with the optimal slope b_v^* decreasing as h and σ_ε^2 increase.

Now, consider the case where the principal cannot observe V but can only observe a performance measure (P) that is also a linear function of the action vector \mathbf{a} given by:

$$P(\mathbf{a}, \phi) = \mathbf{g} \cdot \mathbf{a} + \phi$$

Since $\mathbf{g} \neq \mathbf{f}$, P is an imperfect proxy for V (such as test scores for broader learning). However, since V is unobservable, the principal is constrained to offer a wage contract as a function of P such as $w = s + b_p \cdot P$.

The key result in Baker (2002) is that the optimal slope b_p^* on P is given by:

$$b_p^* = \frac{F \cdot G \cdot \cos \theta}{G^2 + 2h\sigma_\phi^2} \quad (2.2.2)$$

where $G = \sqrt{\sum_{i=1}^n g_i^2}$, and θ is the angle between \mathbf{f} and \mathbf{g} . The cosine of θ is a measure of how much b_p^* needs to be reduced relative to b_v^* due to the distortion arising from $\mathbf{g} \neq \mathbf{f}$.

The empirical literature in education showing that teachers sometimes respond to incentives by increasing actions on dimensions that are not valued by the principal highlights the need to be cautious in designing incentive programs. In most practical cases, $\mathbf{g} \neq \mathbf{f}$ (and $\cos \theta \neq 1$), and so it is perhaps inevitable that a wage contract with $b_p > 0$ will induce some actions that are unproductive. However, what matters for incentive design is that $b_p^* > 0$, as long as $V(\mathbf{a}(b_p > 0)) > V(\mathbf{a}(b_p = 0))$, even if there is some deviation relative to the first-best action in the absence of distortion and $V(\mathbf{a}(b_p^*)) < V(\mathbf{a}(b_v^*))$. In other words, what matters is not whether teachers engage in more or less of some activity than they would in a first-best world (with incentives on the underlying social value function), but whether the sum of their activities in a system with incentives on test scores generates more learning (broadly construed) than in a situation with no such incentives.

There are several reasons why test scores might be an adequate performance measure in the context of primary education in a developing country. First, given the extremely low levels of learning, it is likely that even an increase in routine classroom teaching of basic material will

lead to better learning outcomes.¹⁸ Second, even if some of the gains merely reflect an improvement in test-taking skills, the fact that the education system in India (and several Asian countries) is largely structured around test-taking suggests that it might be unfair to deny disadvantaged children in government-schools the benefits of test-taking skills that their more privileged counterparts in private schools develop.¹⁹ Finally, the design of tests can get more sophisticated over time, making it difficult to do well on the tests without a deeper understanding of the subject matter. So, it is possible that additional efforts taken by teachers to improve test scores for primary school children can also lead to improvements in broader educational outcomes. Whether this is true is an empirical question and is a focus of our research design (see section 4).

2.3 Group versus Individual Incentives

The theoretical prediction of the relative effectiveness of individual and group teacher incentives is ambiguous. To clarify the issues, let w = wage, P = performance measure, and $c(a)$ = cost of exerting effort a with $c'(a) > 0$, $c''(a) > 0$, $P'(a) > 0$, and $P''(a) < 0$. Unlike typical cases of team production, an individual teacher's output (test scores of his students) is observable, making contracts on individual output feasible. The optimal effort for a teacher facing individual

incentives is to choose a_i so that:
$$\frac{\partial w_i}{\partial P_i} \cdot \frac{\partial P_i}{\partial a_i} = c'(a_i) \quad (2.3.1)$$

Now, consider a group incentive program where the bonus payment is a function of the average performance of all teachers. The optimality condition for each teacher is:

$$\frac{\partial w_i}{\partial \left[\left(P_i + \sum P_{-i} \right) / n \right]} \cdot \frac{\partial \left[\left(P_i + \sum P_{-i} \right) / n \right]}{\partial a_i} = c'(a_i) \quad (2.3.2)$$

If the same bonus is paid to a teacher for a unit of performance under both group and

individual incentives then $\frac{\partial w_i}{\partial \left[\left(P_i + \sum P_{-i} \right) / n \right]} = \frac{\partial w_i}{\partial P_i}$, but $\frac{\partial \left[\left(P_i + \sum P_{-i} \right) / n \right]}{\partial a_i} = \frac{1}{n} \cdot \frac{\partial P_i}{\partial a_i}$. Since

$c''(a) > 0$, the equilibrium effort exerted by each teacher under group incentives is lower than that

¹⁸ As Lazear (2006) points out, the optimal policy regarding high-stakes tests are different for high-cost and low-cost learners, with concentrated incentives being optimal for high-cost learners. This would be analogous to saying that teaching to the test may be optimal in contexts of very low learning.

¹⁹ While the private returns to test-taking skills may be greater than the social returns, the social returns could be positive if they enable disadvantaged students to compete on a more even basis with privileged students for scarce slots in higher levels of education.

under individual incentives. Thus, in the basic theory, group (school-level) incentives induce free riding and are therefore inferior to individual (teacher-level) incentives, when the latter are feasible.²⁰

However, if the teachers jointly choose their effort levels, they will account for the externalities within the group. In the simple case where they each have the same cost and production functions and these functions do not depend on the actions of the other teachers, they will each (jointly) choose the level of effort given by (2.3.1). Of course, each teacher has an incentive to shirk relative to this first best effort level, but if teachers in the school can monitor each other at low cost, then it is possible that the same level of effort can be implemented as under individual incentives. This is especially applicable to smaller schools where peer monitoring is likely to be easier.²¹

Finally, if there are gains to cooperation or complementarities in production, then it is possible that group incentives might yield better results than individual incentives.²² Consider a case where teachers have comparative advantages in teaching different subjects or different types of students. If teachers specialize in their area of advantage and reallocate students/subjects to reflect this, they could raise $P'(a)$ ($\forall a$) relative to a situation where each teacher had to teach all students/subjects. Since $P''(a) < 0$, the equilibrium effort would also be higher and the outcomes under group incentives could be superior to those under individual incentives.²³

Lavy (2002) and (2008) report results from high-school teacher incentive programs in Israel at the individual and group level respectively. However, the two programs were implemented at different (non-overlapping) times and the schools were chosen by different (non-random) eligibility criteria, and the individual incentive program was only studied for one year. We study both group and individual incentives in the same field experiment over two full academic years.

²⁰ See Holmstrom (1982) for a solution to the problem of moral hazard in teams.

²¹ See Kandori (1992) and Kandel and Lazear (1992) for discussions of how social norms and peer pressure in groups can ensure community enforcement of the first best effort level.

²² Itoh (1991) models incentive design when cooperation is important. Hamilton, Nickerson, and Owan (2003) present empirical evidence from a garment factory showing that group incentives for workers improved productivity relative to individual incentives.

²³ The additive separability of utility between income and cost of effort implies that there is no 'income effect' of higher productivity on the cost of effort, and so effort goes up in equilibrium since $P'(a)$ is higher.

3. Experimental Design

3.1 Context

While India has made substantial progress in improving access to primary schooling and primary school enrolment rates, the average levels of learning remain very low. The most recent *Annual Status of Education Report* found that over 58% of children aged 6 to 14 in an all-India sample of over 300,000 rural households could not read at the second grade level, though over 95% of them were enrolled in school (Pratham, 2008). 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. A recent study using a nationally representative dataset of primary schools in India found that 25% of teachers were absent on any given day, and that less than half of them were engaged in any teaching activity (Kremer et al (2005)).²⁴

Andhra Pradesh (AP) is the 5th most populous state in India, with a population of over 80 million, 73% of whom live in rural areas. AP is close to the all-India average on measures of human development such as gross enrollment in primary school, literacy, and infant mortality, as well as on measures of service delivery such as teacher absence (Figure 1a). The state consists of three historically distinct socio-cultural regions and a total of 23 districts (Figure 1b). Each district is divided into three to five divisions, and each division is composed of ten to fifteen mandals, which are the lowest administrative tier of the government of AP. A typical Mandal has around 25 villages and 40 to 60 government primary schools. There are a total of over 60,000 such schools in AP and over 80% of children in rural AP attend government-run schools (Pratham, 2008).

The average rural 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.²⁵ One teacher typically teaches all subjects for a given grade (and often teaches more than one grade simultaneously). All regular teachers are employed by the state, and their salary is mostly determined by experience and rank, with minor adjustments based on assignment location, but no component based on any measure of performance. The average salary of regular teachers is over Rs. 8,000/month and total compensation including benefits is close to Rs. 10,000/month (per capita

²⁴ Spending on teacher salaries and benefits comprises over 90% of non-capital spending on education in India.

²⁵ This is a consequence of the priority placed on providing all children with access to a primary school within a distance of 1 kilometer from their homes.

income in AP is around Rs. 2,000/month; 1 US Dollar \approx 48 Indian Rupees (Rs.)). Teacher unions are strong and disciplinary action for non-performance is rare.²⁶

3.2 Sampling

We sampled 5 districts across each of the 3 socio-cultural regions of AP in proportion to population (Figure 1b).²⁷ In each of the 5 districts, we randomly selected one division and then randomly sampled 10 mandals in the selected division. In each of the 50 mandals, we randomly sampled 10 schools using probability proportional to enrollment. Thus, the universe of 500 schools in the study was representative of the schooling conditions of the typical child attending a government-run primary school in rural AP.

3.3 AP RESt Design Overview

The overall design of AP RESt is represented in the table below:

Table 3.1

		INCENTIVES (Conditional on Improvement in Student Learning)		
		NONE	GROUP BONUS	INDIVIDUAL BONUS
INPUTS (Unconditional)	NONE	CONTROL (100 Schools)	100 Schools	100 Schools
	EXTRA CONTRACT TEACHER	100 Schools		
	EXTRA BLOCK GRANT	100 Schools		

As Table 3.1 shows, the input treatments (described in section 7) were provided *unconditionally* to the selected schools at the beginning of the school year, while the incentive treatments consisted of an announcement that bonuses would be paid at the beginning of the next school year *conditional* on average improvements in test scores during the current school year. No school received more than one treatment, which allows the treatments to be analyzed independent of each other. The school year in AP starts in the middle of June, and the baseline

²⁶ See Kingdon and Muzammil (2001) for an illustrative case study of the power of teacher unions in India. Kremer et al (2005) find that 25% of teachers are absent across India, but only 1 head teacher in their sample of 3000 government schools had ever fired a teacher for repeated absence.

²⁷ The districts were chosen so that districts within a region would be contiguous for ease of logistics and program implementation.

tests were conducted in the 500 sampled schools during late June and early July, 2005.²⁸ After the baseline tests were scored, 2 out of the 10 project schools in each mandal were randomly allocated to each of 5 cells (four treatments and one control). Since 50 mandals were chosen across 5 districts, there were a total of 100 schools (spread out across the state) in each cell. The geographic stratification implies that every mandal was an exact microcosm of the overall study, which allows us to estimate the treatment impact with mandal-level fixed effects and thereby net out any common factors at the lowest administrative level of government.

Table 1 (Panel A) shows summary statistics of baseline school and student performance variables by treatment (control schools are also referred to as a 'treatment' for expositional ease). Column 4 provides the p-value of the joint test of equality, showing that the null of equality across treatment groups cannot be rejected for any of the variables and that the randomization worked properly.²⁹

After the randomization, mandal coordinators (MCs) from APF personally went to each of the schools in the first week of August 2005 to provide them with student, class, and school performance reports, and with oral and written communication about the intervention that the school was receiving. The MCs also made several rounds of unannounced tracking surveys to each of the schools during the school year to collect data on process variables including student attendance, teacher attendance and activity, and classroom observation of teaching processes.³⁰ All schools operated under identical conditions of information and monitoring and only differed in the treatment that they received. This ensures that Hawthorne effects are minimized and that a comparison between treatment and control schools can accurately isolate the treatment effect.

End of year assessments were conducted in March and April, 2006 in all project schools. The results were provided to the schools in the beginning of the next school year (July – August,

²⁸ See Appendix A for the project timeline and activities and Appendix B for details on test administration. 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 (since that could have led to gaming of the baseline test).

²⁹ Table 1 shows sample balance across control, group incentive, and individual incentive schools, which are the focus of the analysis in this paper. The randomization was done jointly across all 5 treatments shown in Table 3.1, and the sample was also balanced on observables across the other treatments.

³⁰ Six visits were made to each school in the first year (2005 – 06), while four visits were made in the second year (2006 – 07)

2006), and all schools were informed that the program would continue for another year.³¹ Bonus checks based on first year performance were sent to qualifying teachers by the end of September 2006, following which the same processes were repeated for a second year.

3.4 Description of Incentive Treatments

Teachers in incentive schools were offered bonus payments on the basis of the average improvement in test scores (in math and language) of students taught by them subject to a minimum improvement of 5%. The bonus formula was:

$$\begin{aligned} \text{Bonus} &= \text{Rs. } 500 * (\% \text{ Gain in average test scores} - 5\%) \text{ if Gain} > 5\% \\ &= 0 \text{ otherwise}^{32} \end{aligned}$$

All teachers in group incentive schools received the same bonus based on average school-level improvement in test scores, while the bonus for teachers in individual incentive schools was based on the average test score improvement of students taught by the specific teacher. We use a (piecewise) linear formula for the bonus contract, both for ease of communication and implementation and also because it is the most resistant to gaming across periods (the end of year score in any year determines the target score for the subsequent year).³³

The 'slope' of Rs. 500 per percentage point gain in average scores was set so that the expected incentive payment per school would be approximately equal to the additional spending in the input treatments (based on calibrations from the project pilot).³⁴ The threshold of 5% average improvement was introduced to account for the fact that the baseline tests were in

³¹ The communication to teachers with respect to the length of the program was that the program would continue as long as the government continued to support the project. The expectation conveyed to teachers during the first year was that the program was likely to continue but was not guaranteed to do so.

³² 1st grade students were not tested in the baseline, and so their 'target' score for a bonus (above which the linear schedule above would apply) was set to be the mean baseline score of the 2nd grade students in the school. The target for the 2nd grade students was equal to their baseline score plus the 5% threshold described above. Schools selected for the incentive programs were given detailed letters and verbal communications explaining the incentive formula. Sample communication letters are available from the authors on request.

³³ Holmstrom and Milgrom (1987) show the theoretical optimality of linear contracts in a dynamic setting (under assumptions of exponential utility for the agent and normally distributed noise). Oyer (1998) provides empirical evidence of gaming in response to non-linear incentive schemes.

³⁴ The best way to set expected incentive payments to be exactly equal to Rs. 10,000/school would have been to run a tournament with pre-determined prize amounts. Our main reason for using a contract as opposed to a tournament was that contracts were more transparent to the schools in our experiment since the universe of eligible schools was spread out across the state. Individual contracts (without relative performance measurement) also dominate tournaments for risk-averse agents when specific shocks (at the school or class level) are more salient for the outcome measure than aggregate shocks (across all schools), which is probably the case here (see Kane and Staiger, 2002). See Lazear and Rosen (1981) and Green and Stokey (1983) for a discussion of tournaments and when they dominate contracts.

June/July and the end of year tests would be in March/April, and so the baseline score might be artificially low due to students forgetting material over the summer vacation. There was no minimum threshold in the second year of the program because the first year's end of year score was used as the second year's baseline and the testing was conducted at the same time of the school year on a 12-month cycle.³⁵

We tried to minimize potentially undesirable 'threshold' effects, where teachers only focus on students near a performance target, by making the bonus payment a function of the average improvement of *all* students.³⁶ If the function transforming teacher effort into test-score gains is concave (convex) in the baseline score, teachers would have an incentive to focus on weaker (stronger) students, but no student is likely to be wholly neglected since each contributes to the class average. In order to discourage teachers from excluding students with weak gains from taking the end of year test, we assigned a zero improvement score to any child who took the baseline test but not the end of year test.³⁷ To make cheating as difficult as possible, the tests were conducted by external teams of 5 evaluators in each school (1 for each grade), the identity of the students taking the test was verified, and the grading was done at a supervised central location at the end of each day's testing (see Appendix B for details).³⁸

³⁵ The convexity in reward schedule in the first year due to the threshold could have induced some gaming, but the distribution of mean class and school-level gains at the end of the first year of the program did not have a gap below the threshold of 5%. If there is no penalty for a reduction in scores, there is convexity in the payment schedule even if there is no threshold (at a gain of zero). To reduce the incentives for gaming in subsequent years, we use the higher of the baseline and year end scores as the target for the next year and so a school/class whose performance deteriorates does *not* have its target reduced for the next year.

³⁶ Many of the negative consequences of incentives discussed in Jacob (2005) are a response to the threshold effects created by the targets in the program he studied. Neal and Schanzenbach (2008) discuss the impact of threshold effects in the No Child Left Behind act on teacher behavior and show that teachers do in fact focus more on students on the 'bubble' and relatively neglect students far above or below the thresholds. We anticipated this concern and designed the incentive schedule accordingly.

³⁷ In the second year (when there was no threshold), students who took the test at the end of year 1 but not at the end of year 2 were assigned a score of -5. Thus, the cost of a dropping out student to the teacher was always equal to a negative 5% score for the student concerned. A higher penalty would have been difficult since most cases of attrition are out of the teacher's control. The penalty of 5% was judged to be adequate to avoid explicit gaming of the test taking population. We also cap negative gains at the student-level at -5% for the calculation of teacher bonuses. Thus, putting a floor on the extent to which a poor performing student brought down the class/school average at -5% ensured that a teacher/school could never do worse than having a student drop out to eliminate any incentive to get weak students to not appear for the test.

³⁸ There were no cases of cheating in the first year, but two cases of cheating were detected in the second year (one classroom and one entire school). These cases were reported to the project management team by the field enumerators, and the concerned schools/teachers were subsequently disqualified from receiving any bonus for the second year. These cases are not included in the analysis presented in the paper.

4. Test Design

4.1 Test Construction

We engaged India's leading education testing firm, "Educational Initiatives" (EI), to design the tests to our specifications. The test design activities included mapping the syllabus from the text books into skills, creating a universe of questions to represent each skill, and calibrating question difficulty in a pilot exercise in 40 schools during the prior school year (2004-05) to ensure adequate discrimination on the tests.

The baseline test (June-July, 2005) covered competencies up to that of the previous school year. At the end of the school year (March-April, 2006), schools had two rounds of tests with a gap of two weeks between them. The first test (referred to as the "lower end line" or LEL) covered competencies up to that of the previous school year, while the second test (referred to as the "higher end line" or HEL) covered materials from the current school year's syllabus. The same procedure was repeated at the end of the second year, with two rounds of testing.³⁹ Doing two rounds of testing at the end of each year allows for the inclusion of more overlapping materials across years of testing, reduces the impact of measurement errors specific to the day of testing by having multiple tests around two weeks apart, and also reduces sample attrition due to student absence on the day of the test.⁴⁰

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

4.2 Basic versus higher-order skills

As highlighted in section 2.2, it is possible that broader educational outcomes are no better (or even worse) under a system of teacher incentives based on test scores even if the test scores improve. A key empirical question, therefore, is whether additional efforts taken by teachers to improve test scores for primary school children in response to the incentives are also likely to

³⁹ Thus in any year of testing, the materials in the LEL will overlap with those on the HEL the previous year. This makes it possible to put student achievement over time on a common "vertical scale" using the properties of item response theory (IRT), which is the standard psychometric tool used to equate different tests on a common scale (the IRT estimates are not used in this paper).

⁴⁰ Since all analysis is done with normalized test scores (relative to the control school distribution), a student can be absent on one testing day and still be included in the analysis without bias because the included score would have been normalized relative to the specific test that the student took.

lead to improvements in broader educational outcomes. We asked EI to design the tests to include both 'mechanical' and 'conceptual' questions within each skill category on the test. The distinction between these two categories is not constant, since a conceptual question that is repeatedly taught in class can become a mechanical one. Similarly a question that is conceptual in an early grade might become mechanical in a later grade, if students acclimatize to the idea over time. For this study, a mechanical question was considered to be one that conformed to the format of the standard exercises in the text book, whereas a conceptual one was defined as a question that tested the same underlying knowledge or skill in an unfamiliar way.

As an example, consider the following pair of questions (which did not appear sequentially) from the 4th grade math test under the skill of 'multiplication and division'

Question 1:
$$\begin{array}{r} 34 \\ \times 5 \\ \hline \end{array}$$

Question 2: Put the correct number in the empty box:

$$8 + 8 + 8 + 8 + 8 + 8 = 8 \times \square$$

The first question follows the standard textbook format for asking multiplication questions and would be classified as "mechanical" while the second one requires the students to understand that the concept of multiplication is that of repeated addition, and would be classified as "conceptual." Note that conceptual questions are not more difficult per se. In this example, the conceptual question is arguably easier than the mechanical one because a student only has to count that there are 6 '8's and enter the answer '6' as opposed to multiplying 2 numbers with a digit carried forward. But the conceptual question is unfamiliar and this is reflected in 43% of children getting Question 1 correct, while only 8% got Question 2 correct. Of course, the distinction is not always so stark, and the classification into mechanical and conceptual is a discrete representation of a continuous scale between familiar and unfamiliar questions.⁴¹

4.3 Incentive versus non-incentive subjects

Another dimension on which incentives can induce distortions is on the margin between incentive and non-incentive subjects. We study the extent to which this is a problem by conducting additional tests at the end of each year in science and social studies on which there

⁴¹ Koretz (2002) points out that test score gains are only meaningful if they generalize from the specific test to other indicators of mastery of the domain in question. While there is no easy solution to this problem given the impracticality of assessing every domain beyond the test, our inclusion of both mechanical and conceptual questions in each test attempts to address this concern.

was no incentive.⁴² Since these subjects are introduced only in grade 3 in the school curriculum, these additional tests were administered in grades 3 to 5.

5. Results

5.1 Teacher Turnover and Student 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 incentive 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 any of the treatment groups in the extent of teacher turnover or attrition, and the transfer rate was close to 33%, which is consistent with the rotation of teachers once every 3 years (Table 1 – Panel B, rows 11-12). A more worrying possibility was that additional teachers would try to transfer into the incentive schools in the second year of the project. 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 5%, and once again, there was no significant difference in teacher transfer rates across the various treatments (Table 1 – Panel B, rows 13 - 16).⁴³

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 25% in year 1 and year 2 respectively, but there is no significant difference in attrition across the treatments (rows 17 and 20). Beyond confirming sample balance, this is an important result in its own right because one of the concerns of teacher incentives based on test scores is that weaker children might be induced to drop out of testing in incentive schools (Jacob, 2005). Attrition is higher among students with lower baseline scores, but this is true across all treatments, and we find no

⁴² In the first year of the project, schools were not told about these additional subject tests till a week prior to the tests and were told that these tests were only for research purposes. In the second year, the schools knew that these additional tests would be conducted, but also knew from the first year that these tests would not be included in the bonus calculations.

⁴³ 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.

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 18, 19, 21, 22).

5.2 Specification

We first discuss the impact of the incentive program as a whole by pooling the group and individual incentive schools and considering this to be the 'incentive' treatment. All estimation and inference is done with the sample of 300 control and incentive schools unless stated otherwise. Our default specification uses the form:

$$T_{ijkm}(Y_n) = \alpha + \gamma \cdot T_{ijkm}(Y_0) + \delta \cdot Incentives + \beta \cdot Z_m + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (5.1)$$

The main dependent variable of interest is T_{ijkm} , which is the normalized test score on the specific test (normalized with respect to the score distribution of the control schools), where i, j, k, m denote the student, grade, school, and mandal respectively. Y_0 indicates the baseline tests, while Y_n indicates a test at the end of n years of the program. Including the normalized baseline test score improves efficiency due to the autocorrelation between test-scores across multiple periods.⁴⁴ 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 'Incentives' variable is a dummy at the school level indicating if it was in the incentive treatment, and the parameter of interest is δ , which is the effect on the normalized test scores of being in an incentive school. The random assignment of treatment ensures that the 'Incentives' 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.

5.3 Impact of Incentives on Test Scores

Averaging across both math and language, students in incentive schools scored 0.15 standard deviations (SD) higher than those in comparison schools at the end of the first year of the program, and 0.22 SD higher at the end of the second year (Table 2 – Panel A, columns 1 and 5).

⁴⁴ Since grade 1 students did not have a baseline test, we set the normalized baseline score to zero for these students (similarly for students in grade 2 at the end of two years of the treatment). All results are robust to completely excluding grade 1 students as well.

The impact of the incentives at the end of two years is greater in math (0.28 SD) than in language (0.16 SD) and this difference is significant (Panels B and C of Table 2).⁴⁵ The addition of school and household controls does not significantly change the estimated value of δ in any of the regressions, confirming the validity of the randomization (columns 2 and 6).

Column 3 of Table 2 shows the results of estimating equation (5.1) for the second-year effect (with Y1 scores on the right-hand side). This is not an experimental estimate since the Y1 scores are a post-treatment outcome, but the point estimates suggest that the effect of the incentive programs were comparable across both years (0.15 SD and 0.14 SD).⁴⁶ However, the two-year treatment effect of 0.22 SD is not the sum of these two effects because of depreciation of prior gains.⁴⁷ A more detailed discussion of depreciation (or the lack of full persistence) of test score gains is beyond the scope of this paper, but the important point to note is that calculating the average treatment effect by dividing the “n” year treatment effect by “n” years, will typically underestimate the impact of the treatment beyond the first year relative to the counterfactual of discontinuation of the treatment. On the other hand, if the effects of most educational interventions fade out, then it is likely that extrapolating one-year treatment effects will typically overstate the long-term impact of programs, which highlights the importance of carrying out long-term follow ups of even experimental evaluations in order to do better cost-benefit calculations.⁴⁸

We verify that teacher transfers do not affect the results by estimating equation (5.1) across different durations of teacher presence in the school, and there is no significant difference across

⁴⁵ This finding is consistent with several other studies on education in developing countries. One possible reason for this is that home inputs play a bigger role in the production function for language than for math. Thus, a school-level intervention is likely to have a larger impact on math than on language.

⁴⁶ Specifically the estimate of the “second year” treatment effect requires an unbiased estimate of γ , which cannot be consistently estimated in the above specification due to downward bias from measurement error and upward bias from omitted individual ability. Andrabi et al (2008) show that these biases roughly cancel out each other in their data from a similar context (primary education in Pakistan), and so we present the results of this specification as illustrative while focusing our discussion on the experimental estimates of one and two-year treatment effects.

⁴⁷ If we use analogous terms for physical and human capital, the second year treatment effect alone would be the “gross” treatment effect, while the difference between the two-year and one-year effect would be the “net” treatment effect. In the present case, the two-year net treatment effect estimated by (5.1) is the sum of the gross treatment effects over the two years less the amount of first year gains that are depreciated away. So in Table 2, the two-year treatment effect of 0.22 is equal to the sum of the “gross” treatment effects (0.15 + 0.14), less the depreciation of the first year treatment effect $((1 - 0.55) * 0.15)$.

⁴⁸ The issue of persistence/depreciation of learning has only recently received attention in the literature on the effects of education interventions on test scores over multiple years. See Andrabi et al (2008) and Jacob et al (2008) for a more detailed discussion of issues involved with estimating the extent of persistence of interventions, and the implications for cost-benefit analysis. The cost effectiveness calculations in this paper are not affected by this consideration because all the treatments being compared were part of the same experiment for the same duration.

these estimates. The testing process was externally proctored at all stages and we had no reason to believe that cheating was a problem in the first year, but there were two cases of cheating in the second year. Both these cases were dropped from the analysis and the concerned schools/teachers were declared ineligible for bonuses (see Appendix B).

The top panel of Figure 2 plots the density and CDF of the test score distribution in treatment and control schools at the baseline and the lower panel plots them after two years of the program. Figure 3 plots the quantile treatment effects of the performance pay program on student test scores (defined for each quantile τ as: $\delta(\tau) = G_n^{-1}(\tau) - F_m^{-1}(\tau)$ where G_n and F_m represent the empirical distributions of the treatment and control distributions with n and m observations respectively), with bootstrapped 95% confidence intervals, and shows that the quantile treatment effects are positive at every percentile and increasing. In other words, test scores in incentive schools are higher at every percentile, but the program also increased the variance of test scores.

5.4 Heterogeneity of Treatment Effects

We find that students in incentive schools do better than control schools for all major sub-groups including all five grades (1-5), all five project districts, both rounds of testing (lower end line and higher end line), and across all quintiles of question difficulty, with most of these differences being significant (since the sample size is large enough to precisely estimate treatment effects in various sub-groups).⁴⁹

We test for heterogeneity of the incentive treatment effect across student, school, and teacher characteristics by testing if δ_3 is significantly different from zero in:

$$T_{ijkm}(Y_n) = \alpha + \gamma \cdot T_{ijkm}(Y_0) + \delta_1 \cdot Incentives + \delta_2 \cdot Characteristic + \delta_3 \cdot (Incentives \times Characteristic) + \beta \cdot Z_m + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (5.2)$$

Table 5 (Panel A) shows the results of these regressions on several school and household characteristics.⁵⁰ We find very limited evidence of differential treatment effects by school characteristics such as total number of students, school infrastructure, or school proximity to

⁴⁹ These tables are not included in the paper, but are available from the authors on request.

⁵⁰ Each column in Table 3 represents one regression testing for heterogeneous treatment effects along the characteristic mentioned. We also estimate the heterogeneity non-parametrically for each non-binary characteristic, grouping the characteristic into quintiles, and testing if the interaction of the incentive treatment and the top or bottom quintile is significantly different from the omitted category (the middle 3 quintiles), and if the interaction of the incentive treatment with the top and bottom quintiles are significantly different from each other. The results are unchanged and so the table only reports the linear interaction specification in (5.2).

facilities.⁵¹ We also find no evidence of a significant difference in the effect of the incentives by most of the student demographic variables, including an index of household literacy, the caste of the household, the student's gender, and the student's baseline score. The only evidence of heterogeneous treatment effects is across levels of family affluence, with students from more affluent families showing a better response to the teacher incentive program.

The lack of heterogeneous treatment effects by baseline score is an important indicator of broad-based gains since the baseline score is probably the best summary statistic of prior inputs into education. To see this more clearly, Figure 4 shows the non-parametric treatment effects by baseline score,⁵² and we see that there is a minimum treatment effect of 0.1 SD for students regardless of where they were in the initial test score distribution.⁵³ The treatment effects are slightly lower for students with higher baseline scores, but this is not a significant trend as seen in Column 8 of Table 3 (Panel A).

The lack of heterogeneous treatment effects by initial scores, suggests that the increase in variance of test scores in incentive schools (Figure 3) may be reflecting variance in teacher responsiveness to the incentive program, as opposed to variance in student responsiveness to the treatment by initial learning levels. We test this by estimating teacher value addition (measured as teacher fixed effects in a regression of current test scores on lagged scores) and find that both the mean and variance of teacher value-addition are significantly higher in the incentive schools (Figure 5). Plotting the difference in teacher fixed effects at each percentile of the control and treatment distribution shows the heterogeneity in teacher responsiveness quite clearly. We see that there is no difference between treatment and control schools for the bottom 20% of teachers (as measured by their effectiveness in increasing student test scores); the difference between the 20th and 60th percentile is positive but with a 5% confidence bound that is close to zero; and finally the difference between the 60th and 100th percentile is positive, significant, and increasing.

⁵¹ Given the presence of several covariates in Table 3, we are cautious to avoid data mining for differential treatment effects since a few significant coefficients are likely simply due to sampling variability. Thus, we consider consistent evidence of heterogeneous treatment effects across multiple years to be more reliable evidence.

⁵² The figure plots a kernel-weighted local polynomial regression of end line scores (after 2 years) on baseline scores separately for the incentive and control schools, and also plots the difference at each percentile of baseline scores. The confidence intervals of the treatment effects are constructed by drawing 1000 bootstrap samples of data that preserve the within school correlation structure in the original data, and plotting the 95% range for the treatment effect at each percentile of baseline scores.

⁵³ We are thus able to test for the “bubble” student effect found in studies of NCLB such as Neal and Schanzenbach (2008) and can rule out the presence of a similar effect here.

Having established that there is variation in teacher responsiveness to the incentive program, we test for differential responsiveness by observable teacher characteristics (Table 3B). We find that the interaction of teachers' education and training with incentives is positive and significant, while education and training by themselves are not significant predictors of value addition. This suggests that teacher qualifications by themselves are not associated with better learning outcomes under the status quo, they could matter more if teachers had incentives to exert more effort (see Hanushek (2006)).

We also find that teachers with higher base pay respond less well to the incentives (Table 3 – Panel B, column 4), which suggests that the magnitude of the incentive mattered because the potential incentive amount (for which all teachers had the same conditions) would have been a larger share of base pay for lower paid teachers. However, teachers with higher base pay are typically more experienced and we see that more experienced teachers also respond less well to the incentives (column 3). So, while this evidence suggests that the magnitude of the bonus matters, it is also consistent with an interpretation that young teachers respond better to *any* new policy initiative (including performance pay), and so we cannot distinguish the impact of the incentive amount from that of other teacher characteristics that influence base pay.⁵⁴

5.5 Mechanical versus Conceptual Learning and Non-Incentive Subjects

To test the impact of incentives on these two kinds of learning, we again use specification (5.1) but run separate regressions for the mechanical and conceptual parts of the test. Incentive schools do significantly better on both the mechanical and conceptual components of the test and the estimate of δ is almost identical across both components (Table 4). Note that the coefficient on the baseline score is significantly lower for the conceptual component than for the mechanical component (in both years), indicating that these questions were more unfamiliar than the mechanical questions. The relative unfamiliarity of these questions increases our confidence that the gains in test scores represent genuine improvements in learning outcomes.

The impact of incentives on the performance in non-incentive subjects such as science and social studies is tested using a slightly modified version of specification (5.1) where lagged scores on both math and language are included to control for initial learning levels. We find that students in incentive schools also performed significantly better on non-incentive subjects at the

⁵⁴ Of course, this is a caution that applies to any interpretation of interactions in an experiment, since the covariate is not randomly assigned and could be correlated with other omitted variables.

end of each year of the program, scoring 0.11 and 0.18 SD higher than students in control schools in science and social studies at the end of two years of the program (Table 5). The coefficients on the lagged baseline math and language scores here are much lower than those in Tables 2 and 4, confirming that the domain of these tests was substantially different from that of the tests on which incentives were paid.

These results do not imply that no diversion of teacher effort away from science, social studies, or conceptual thinking took place, but rather that in the context of primary education in a developing country with very low levels of learning, teacher efforts aimed at increasing test scores in math and language are also likely to contribute to superior performance on broader educational outcomes suggesting complementarities among the measures and positive spillover effects between them (though the result could also be due to an improvement in test-taking skills that transfer across subjects).

5.6 Group versus Individual Incentives

Both the group and the individual incentive programs had significantly positive treatment effects at the end of each year of the program (Table 6, columns 1 and 7).⁵⁵ In the first year of the program, students in individual incentive schools performed slightly better than those in group incentive schools, but the difference was not significant. By the end of the second year, students in individual incentive schools scored 0.27 SD higher than those in comparison schools, while those in group incentive schools scored 0.16 SD higher, with this difference being close to significant at the 10% level (column 7). Estimates of the treatment effect in the second year alone (column 4) suggest that individual incentive schools significantly outperformed group incentive schools in the second year.

We find no significant impact of the number of teachers in the school on the relative performance of group and individual incentives (both linear and quadratic interactions of school size with the group incentive treatment are insignificant). However, the variation in school size is small with 92% of group incentive schools having between two and five teachers (the mean number of teachers across the 300 schools was 3.28, the median was 3, and the mode was 2). The limited range of school size makes it difficult to precisely estimate the impact of group size on the relative effectiveness of group incentives.

⁵⁵ Table 6 is estimated with specification (5.1) but separating out the “incentive” treatment into group and individual incentives respectively.

We repeat all the analysis presented above (in sections 5.3 – 5.5) after separating the incentive schools into the group and individual incentive categories, and Table 7 shows the disaggregated effect of group and individual incentives for each grade, for mechanical/conceptual questions, and for science and social studies. We find that the individual incentives always outperform the group incentives though the difference in point estimates are typically not significant. However, both individual and group incentives were equally cost effective, because the bonuses paid were a function of student performance (see section 7). We also find no significant difference in the patterns of heterogeneous treatment effects (discussed in the previous section) between individual and group incentive schools.

6. Teacher Behavior and Classroom Processes

A unique feature of this study is that changes in teacher behavior were measured with both direct observation as well as teacher interviews. As described in section 3.3, APF staff enumerators conducted several rounds of unannounced tracking surveys during the two school years across all schools in the project. The enumerators coded teacher activity (and absence) through direct physical observation of each teacher in the school. To code classroom processes, an enumerator typically spent between 20 and 30 minutes at the back of a classroom (during each visit) without disturbing the class and coded whether specific actions took place during the period of observation. In addition to these observations, they also interviewed teachers about their teaching practices and methods, asking identical sets of questions in both incentive and control schools. These interviews were conducted in August 2006, around 4 months after the end of year tests, but before any results were announced, and a similar set of interviews was conducted in August 2007 after the second full year of the program.

There was no difference in either student or teacher attendance between control and incentive schools. We also find no significant difference between incentive and control schools on any of the various indicators of classroom processes as measured by direct observation.⁵⁶ This is similar to the results in Glewwe et al (2003) who find no difference in teacher behavior between treatment and control schools from similar surveys and raises the question of how the outcomes

⁵⁶ These include measures of teacher activity such as using the blackboard, reading from the textbook, asking questions to students, encouraging classroom participation, assigning homework, helping students individually, and measures of student activity such as using textbooks, and asking questions.

are significantly different when there don't appear to be any differences in observed processes between the schools.

The teacher interviews provide another way of testing for differences in behavior. Teachers in both incentive and control schools were asked *unprompted* questions about what they did differently during the school year at the end of each school year, but before they knew the results of their students. The interviews indicate that teachers in incentive schools are significantly more likely to have assigned more homework and class work, conducted extra classes beyond regular school hours, given practice tests, and paid special attention to weaker children (Table 8). While self-reported measures of teacher activity might be considered less credible than observations, we find a positive and significant correlation between nearly all the reported activities of teachers and the performance of their students (Table 8 – column 4) suggesting that these self-reports were credible (especially since less than 50% of teachers in the incentive schools report doing *any* of the activities described in Table 8).

The interview responses suggest reasons for why salient dimensions of changes in teacher behavior might not have been captured in the classroom observations. An enumerator sitting in classrooms during the school day is unlikely to observe the extra classes conducted after school. Similarly, if the increase in practice tests occurred closer to the end of the school year (in March), this would not have been picked up by the tracking surveys conducted between September and February. Finally, while our survey instruments recorded if various activities took place, they did not have a way to capture the intensity of teacher efforts, which may be an important channel of impact. One way to see this is to notice that there is no difference between treatment and control schools in the fraction of teachers coded as “actively teaching” when observed by the enumerator (Table 8 – row 2), but the interaction of “active teaching” and being in an incentive school is significantly positively correlated with measures of teacher value addition (Table 3B – column 7). This suggests that teachers changed the effectiveness of their teaching in response to the incentives in ways that would not be easily captured even by observing the teacher.

Our use of both direct observations and interviews might help in reconciling the difference between the findings of Glewwe et al. (2008) and Lavy (2008) with respect to teacher behavior. Glewwe et al. use direct observation and report that there was no significant difference in teacher actions between incentive and comparison schools; Lavy uses phone interviews with teachers

and reports that teachers in incentive schools were significantly more likely to conduct extra classes, stream students by ability, and provide extra help to weak students. While both methods are imperfect, our results suggest that the difference between the studies could partly be due to the different methodologies used for measuring classroom process variables. In summary, it appears that the incentive program based on end of year test scores did not change the teachers' cost-benefit calculations on the presence/absence margin on a given day during the school year, but that it probably made them exert more effort when present (especially closer to the end of year assessments).

7. Comparison with Input Treatments & Cost-Benefit Analysis

As mentioned earlier, a parallel component of this study provided two other sets of 100 randomly chosen schools with an extra contract teacher, and with a cash block grant for school materials respectively. Contract teachers are hired at the school level 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 is less than 20% of the average salary of regular government teachers. Contract teachers usually teach their own classes and are not 'teacher-aides' who support a regular teacher in the same classroom. The use of contract teachers has increased in developing countries like India in response to fiscal pressures and to the difficulty of filling teacher positions in under-served remote areas. There is some evidence that contract teachers are more cost effective than regular teachers but their use is controversial. Proponents argue that contract teachers are a cost-effective way of reducing class size and multi-grade teaching; opponents argue that the use of untrained teachers will not improve learning.⁵⁷

The block grant intervention targeted non-teacher inputs directly used by students. The schools had the freedom to decide how to spend the block grant, subject to guidelines that required the money to be spent on inputs directly used by children. Schools receiving the block grant were given a few weeks to make a list of items they would like to procure. The list was approved by the project manager, and the materials were jointly procured by the teachers and the APF mandal coordinators, and provided to the schools by September, 2005. The majority of the

⁵⁷ See our companion paper (Muralidharan and Sundararaman, 2009) for more details on the program and its impact on student learning.

grant money was spent on notebooks, workbooks, exercise books, slates and chalk, writing materials, and other interactive materials such as charts, maps, and toys.⁵⁸

These interventions were calibrated so that the expected spending on the input and the incentive programs was roughly equal.⁵⁹ To compare the effects across treatment types, we pool the 2 incentive treatments, the 2 input treatments, and the control schools and run the regression:

$$T_{ijkn}(Y_n) = \alpha + \gamma \cdot T_{ijkn}(Y_0) + \delta_1 \cdot Incentives + \delta_2 \cdot Inputs + \beta \cdot Z_m + \varepsilon_k + \varepsilon_{jk} + \varepsilon_{ijk} \quad (7.1)$$

using the full sample of 500 schools. While both categories of treatments had a positive and significant impact on learning outcomes at the end of the first year, the incentive schools performed 0.06 standard deviations better than the input schools and this difference is significant at the 10 percent level (Table 9 - Column 1). At the end of two years, the difference is more pronounced with the incentive schools scoring 0.13 SD higher and this difference is significant at the 1% level (Table 9 – Column 7). The incentive schools perform better than input schools in both math and language and both these differences are significant at the end of two years.

The total amount spent on each intervention was calibrated to be roughly equal, but the group incentive program ended up spending significantly lower amounts per school. The average annual spending on each of the input treatments was Rs. 10,000/school, while the group and individual incentives programs cost roughly Rs. 6,000/school and Rs.10,000/school respectively. The bonus payment in the group incentive schools was lower than that in the individual incentive schools both because the treatment effect was smaller and also because classes with scores below their target brought down the average school gain in the group incentive schools, while teachers with negative gains (relative to targets) did not hurt teachers with positive gains in the individual incentive schools.⁶⁰

Both the incentive programs were more cost effective than the input programs. The individual incentive program spent the same amount per school as the input programs but produced gains in test scores that were three times larger than those in the input schools (0.27 SD

⁵⁸ See Das et al (2009), where we discuss the impact of the block grant intervention.

⁵⁹ These input programs represented 2 out of the 3 most common input-based interventions (infrastructure, teachers, and materials). We did not conduct a randomized evaluation of infrastructure both due to practical difficulties, and because the returns would have to be evaluated over the depreciation life cycle of the infrastructure. Thus, the set of interventions studied here all represent “flow” expenditures that would be incurred annually and are therefore comparable.

⁶⁰ So even conditional on the same distribution of scores, the individual incentive payout would be higher as long as there are some classes with negative gains relative to the target because of truncation of teacher-level bonuses at zero in the individual incentive calculations.

vs. 0.09 SD). The group incentive program had a smaller treatment effect than the individual incentive program (0.16 SD vs 0.27 SD), but on a cost effectiveness basis the group and individual incentive programs were almost identical in their effectiveness (0.16 SD for Rs. 6,000 in the group incentive schools and 0.27 SD for Rs. 10,000 in the individual incentive schools). Thus, both the incentive programs significantly outperformed the input programs and were roughly equal to each other in cost effectiveness.

A different way of thinking about the cost of the incentive program is to not consider the incentive payments as a cost at all, because it is simply a way of reallocating salary spending. For instance, if salaries were increased by 3% every year for inflation, then it might be possible to introduce a performance-based component with an expected payout of 3% of base pay in lieu of a standard increase across the board. Under this scenario, the 'incentive cost' would only be the risk premium needed to keep expected utility constant compared to the guaranteed increase of 3%. This is a very small number with an upper bound of 0.1% of base pay if teachers' coefficient of absolute risk aversion (CARA) is 2 and 0.22% of base pay even if the CARA is as high as 5.⁶¹ This is less than 10% of the mean incentive payment (3% of base pay) and thus, the long-run cost of the incentive program can be substantially lower than the full cost of the bonuses paid in the short run.⁶² Finally, if performance-pay programs are designed on the basis of multiple years of performance, differences in compensation across teachers would be less due to random variation (which would need to be compensated for by paying a risk-premium), and more due to heterogeneity in ability, which would attract higher-ability teachers into the profession, and reduce the rents paid to less effective teachers (see next section).

A full discussion of cost effectiveness should include an estimate of the cost of administering the program. The main cost outside the incentive payments is that of independently administering and grading the tests. The approximate cost of each annual round of testing was Rs. 5,000 per school, which includes the cost of two rounds of independent testing and data entry

⁶¹ The risk premium here is the value of ε such that $0.5[u(0.97w + \varepsilon) + u(1.03w + \varepsilon)] = u(w)$, and is easily estimated for various values of CARA using a Taylor expansion around w . This is a conservative upper bound since the incentive program is modeled as an even lottery between the extreme outcomes of a bonus of 0% and 6%. In practice, the support of the incentive distribution would be non-zero everywhere on $[0, 6]$ and the risk premium would be considerably lower.

⁶² This would not be true for the current teachers in the system who are used to low levels of effort, and who would need to be compensated not only for the risk of variable pay but for the extra effort that they may need to exert under a performance-pay system. However, new teachers who are not accustomed to the rents of the civil-service job would only need to be compensated for the risk premium.

but not the additional costs borne for research purposes. The incentive program would be more cost effective than the input programs even after adding these costs and even more so if we take the long-run view that the fiscal cost of performance pay can be lower than the amount of the bonus, if implemented in lieu of a scheduled across the board increase in pay.

8. Teacher Opinions on Performance Pay

Nearly 75% of teachers in incentive schools report that their motivation levels went up as a result of the program (with the other 25% reporting no change); over 95% had a favorable opinion about the program; over 85% had a favorable opinion regarding the idea of providing bonus payments to teachers on the basis of performance; and over two thirds of teachers felt that the government should consider implementing a system of bonus payments on the basis of performance.

Of course, it is easy to support a program when it only offers rewards and no penalties, and so we also asked the teachers their opinion regarding performance-pay in an *expected wage-neutral* way. Teachers were asked their preference regarding how they would allocate a hypothetical budgetary allocation for a 15% pay increase between an across-the-board increase for all teachers, and a performance-based component. Over 75% of teachers supported the idea of at least some performance-based pay, with over 20% in favor of having 20% or more of annual pay determined by performance.⁶³

The longer-term benefits to performance pay include not only greater teacher effort, but also potentially the entry of better teachers into the profession.⁶⁴ We regress the extent of teachers' preference for performance pay holding expected pay constant (reported before they knew their outcomes) on the average test score gains of their students and find a positive and significant correlation between teacher performance and the extent of performance pay they desire. This suggests that effective teachers know who they are and that there are likely to be sorting benefits

⁶³ If teachers are risk-averse and have rational expectations about the distribution of their abilities, we would expect less than 50% to support expected-wage-neutral performance pay since there is no risk premium being offered in the set of options. The 75% positive response could reflect several factors including over optimism about their own abilities, a belief that it will be politically more feasible to secure funds for salary increases if these are linked to performance, or a sense that such a system could bring more professional respect to teachers and enhance motivation across the board.

⁶⁴ See Lazear (2000) and (2003), and Hoxby and Leigh (2005)

from performance pay. If the teaching community is interested in improving the quality of teachers entering the profession, this might be another reason to support performance pay.⁶⁵

9. Conclusion

Performance pay for teachers is an idea with strong proponents, as well as opponents, and the empirical evidence to date on its effectiveness has been mixed. In this paper, we present evidence from a randomized evaluation of a teacher incentive program in a representative sample of government-run rural primary schools in the Indian state of Andhra Pradesh, and show that teacher performance pay led to significant improvements in student test scores, with no evidence of any adverse consequences of the program. Additional schooling inputs were also effective in raising test scores, but the teacher incentive programs were three times as cost effective in raising test scores.

The significant effect of teacher performance pay on learning outcomes over both the one-year and two-year horizon of the program suggests that the program effects are unlikely to be due to its novelty. The continued gains on both mechanical and conceptual test questions as well as on non-incentive subjects indicate that the distortions from multi-tasking are less of a concern at low levels of learning. Finally, the finding that more educated and better trained teachers responded better to the incentives (while teacher education and training were not correlated with learning outcomes in comparison schools), highlights the potential for incentives to be a productivity-enhancing measure that can improve the effectiveness of other school inputs (including teacher human capital).

While certain features of our experiment may be difficult to replicate in other settings, and certain aspects of the Indian context (like low average levels of learning), may be most relevant to developing countries, our results suggest that performance pay for teachers could be an effective policy tool in India, and perhaps in other settings of low levels of learning as well. Input and incentive-based policies for improving school quality are not mutually exclusive, but our results suggest that conditional on the status quo patterns of spending in India, the marginal

⁶⁵ Ballou and Podgursky (1993) show that teachers' attitude towards merit pay in the US is more positive than is supported by conventional wisdom and argue that the dichotomy may be due to divergence between the interests of union leadership and members. There is some evidence that this might be the case here as well. Older teachers are significantly less likely to support the idea of performance pay in our data, but they are also much more likely to be active in teacher unions.

returns to spending additional resources on performance-linked incentives for teachers may be higher than additional spending on unconditionally-provided school inputs.

However, there are several unresolved issues and challenges that need to be addressed before scaling up teacher performance pay programs. One area of uncertainty is the optimal ratio of base and bonus pay. Setting the bonus too low might not provide adequate incentives to induce higher effort, while setting it too high increases both the risk premium and the probability of undesirable distortions.

We have also not devised or tested the optimal long-term formula for teacher incentive payments. While the formula used in this project avoided the most common pitfalls of performance pay from an incentive design perspective, its accuracy was limited by the need for the bonus formula to be transparent to all teachers (most of whom were encountering a performance-based bonus for the first time in their careers). A better formula for teacher bonuses would net out home inputs to estimate a more precise measure of teachers' value addition. It would also try and account for the fact that the transformation function from teacher effort into student outcomes is likely to be different at various points in the achievement distribution. A related concern is measurement error and the potential lack of reliability of test scores at the class and school levels.⁶⁶

The incentive formula can be improved with teacher data over multiple years and by drawing on the growing literature on estimating teacher value-added models (the collection of essays in Haertel and Herman (2005) is a good starting point) as well as papers complementary to ours that focus on the theoretical properties of optimal incentive formulae for teachers (see Barlevy and Neal (2009) for a recent contribution). However, there is a practical trade-off between the accuracy and precision of the bonus formula on one hand and the transparency of the system to teachers on the other. Teachers accepted the intuitive 'average gain' formula used in the first two years of the program and trusted the procedure used and communicated by the Azim Premji Foundation. If such a program were to become policy, it is likely that teachers will start getting more sophisticated about the formula, at which point the decision regarding where to locate on the accuracy-transparency frontier can be made in consultation with teachers. At the same time,

⁶⁶ Kane and Staiger (2002) show that measurement error in class-level and school-level averages can lead to rankings based on these averages being volatile. However, as Rogosa (2005) points out, mean test-scores can be quite *precise* (in the sense of accurately estimating levels of learning) even while not being very *reliable* (in the sense of accurately ranking schools). This might be a reason to prefer contracts over tournaments.

it is possible that there may be no satisfactory resolution of the tension between accuracy and transparency.⁶⁷

While the issue of the optimal formula for teacher performance pay has not been resolved, and implementation concerns are very real, this paper presents rigorous experimental evidence (in a representative sample of schools in the Indian state of Andhra Pradesh) that even modest amounts of performance-based pay for teachers can lead to substantial improvements in student learning outcomes, with limited negative consequences (when implemented in a transparent and credible way). As school systems around the world consider adopting various forms of performance pay for teachers,⁶⁸ attempts should be made to build in rigorous impact evaluations of these programs during the phasing out of such programs. This will also allow experimentation with variations such as basing bonuses on both subjective and objective measures of performance, and putting weight on both group and individual-level performance. Programs and studies could also attempt to vary the magnitude of the incentives to estimate outcome elasticity with respect to the extent of variable pay, and thereby gain further insights not only on performance pay for teachers, but on performance pay in organizations in general.

⁶⁷ Murnane and Cohen (1986) point out that one of the main reasons why merit-pay plans fail is that it is difficult for principals to clearly explain the basis of evaluations to teachers. However, Kremer and Chen (2001) show that performance incentives, even for something as objective as teacher attendance did not work when implemented through head teachers in schools in Kenya. The head teacher marked all teachers present often enough for all of them to qualify for the prize. These results suggest that the bigger concern is not complexity, but rather human mediation, and so a sophisticated algorithm might be acceptable as long as it is clearly objective and based on transparently established ex ante criteria.

⁶⁸ Lemieux et al (2009) suggest that an important reason for the increasing prevalence of performance-based pay systems across sectors over time is the development of better measurement techniques to directly estimate individual productivity. Clearly, this is an important factor in the increasing interest in rolling out teacher performance pay systems as well.

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Appendix A: Project Timeline and Activities

The broad timeline of AP RESt was as follows:

January 2004 – October 2004:	Planning, Permissions, Partner selection, Funding
November 2004 – April 2005:	Pilot
April 2005 – June 2006:	First full year of main interventions
June 2006 – June 2007:	Second full year of interventions

Main Project (Timeline of Key Activities)

April – June 2005

- Random sampling of the 500 schools to comprise the universe of the study
- Communication of the details of baseline testing to the various district-level officials in the selected districts (*only communicated about the baseline tests and not about the inputs and incentives at this point*)

Late June – July 2005

- Baseline tests conducted in all 500 project schools in a 2-week span in early July
- Scoring of tests and preparation of school and class performance reports
- Stratified random allocation of schools to treatments groups

August 2005

- Distribution of test results, diagnostics, and announcement of relevant incentive schemes in selected schools
- Treatment status and details communicated to schools verbally and in writing

September 2005

- Placement of extra teacher in the relevant randomly selected schools
- Provision of block grants to the relevant randomly selected schools, procurement of materials and audit of procurement

September 2005 – February 2006

- Unannounced tracking surveys of all 500 schools on average once a month

March – April 2006

- Lower and higher end line assessments conducted in 500 schools

August 2006

- Interviews with teachers on teaching activities in the previous school year and on their opinion about performance pay (prior to knowledge of their outcomes)

September 2006

- Provision of school and class level performance reports
- Provision of incentive payments to qualified schools and teachers
- Communication letters about the second year of the program and repeat of above processes for the second year of the program

Appendix B: Project Team, Test Administration, and Robustness to Cheating

The project team from the Azim Premji Foundation consisted of around 30 full time staff and 250 to 300 evaluators hired for the period of the baseline and end line testing. The team was led by a project manager, and had 5 district coordinators and 25 mandal coordinators. Each mandal coordinator was responsible for project administration, supervision of independent tests, communications to schools, and conducting tracking surveys in 2 mandals (20 schools). The mandal coordinators were the 'face' of the project to the schools, while each district coordinator was responsible for overall project implementation at the district level.

Teams of evaluators were hired and trained specially for the baseline and end line assessments. Evaluators were typically college graduates who were obtaining a certificate or degree in teaching. The tests were externally administered with teams of 5 evaluators conducting the assessments in each school (1 for each grade). For the baseline there were 50 teams of 5 evaluators with each team covering a school in a day. The 500 schools were tested in 10 working days over 2 weeks. For the end of year tests, the schools were tested in 2 rounds over 4 weeks at the end of the school year. The 'lower end line' was conducted in the first 2 weeks and the 'higher end line' was conducted in the last 2 weeks. Schools were told that they could be tested anytime in a 2-week window and did not have advance notice of the precise day on which they would be tested.

Identities of children taking the test were verified by asking them for their father's name, which was verified against a master list of student data. Standard exam procedures of adequate distance between students and continuous proctoring were followed. The teachers were not allowed in the classes while the tests were being given. The tests (and all unused papers) were collected at the end of the testing session and brought back to a central location at the end of the school day. The evaluation of the papers, and the transcription to the 'top sheet' (that was used for data entry) was done in this central location under supervision and with cross checking across evaluators to ensure accuracy.

No cases of cheating were observed during the first year of the programs, but two cases of cheating were detected in the second year (one classroom and one entire school). These cases were reported to the project management team by the field enumerators, and the schools were subsequently disqualified from receiving any bonus for the second year. These cases are not included in the analysis presented in the paper.

Table 1: Sample Balance Across Treatments

Panel A (Means of Baseline Variables)					
	[1]	[2]	[3]	[4]	
	Control	Group Incentive	Individual Incentive	P-value (Equality of all groups)	
School-level Variables					
1	Total Enrollment (Baseline: Grades 1-5)	113.2	111.3	112.6	0.82
2	Total Test-takers (Baseline: Grades 2-5)	64.9	62.0	66.5	0.89
3	Number of Teachers	3.07	3.12	3.14	0.58
4	Pupil-Teacher Ratio	39.5	40.6	37.5	0.66
5	Infrastructure Index (0-6)	3.19	3.14	3.26	0.84
6	Proximity to Facilities Index (8-24)	14.65	14.66	14.72	0.98
Baseline Test Performance					
7	Math (Raw %)	18.4	17.8	17.4	0.72
8	Math (Normalized - in Std. deviations)	0.023	-0.004	-0.019	0.74
9	Telugu (Raw %)	35.0	34.8	33.4	0.54
10	Telugu (Normalized - in Std. deviations)	0.019	0.014	-0.031	0.56
Panel B (Means of Endline Variables)					
Teacher Turnover and Attrition					
Year 1					
11	Teacher Attrition (%)	0.30	0.34	0.31	0.63
12	Teacher Turnover (%)	0.34	0.33	0.32	0.90
Year 2 on Year 1					
13	Teacher Attrition (%)	0.04	0.06	0.06	0.53
14	Teacher Turnover (%)	0.05	0.04	0.03	0.37
Year 2 on Year 0					
15	Teacher Attrition (%)	0.32	0.37	0.34	0.47
16	Teacher Turnover (%)	0.37	0.36	0.33	0.82
Student Turnover and Attrition					
Year 1					
17	Student Attrition from baseline to end of year tests	0.078	0.062	0.066	0.20
18	Baseline Maths test score of attritors (Equality of all groups)	-0.16	-0.15	-0.19	0.95
19	Baseline Telugu test score of attritors (Equality of all groups)	-0.26	-0.20	-0.25	0.81
Year 2 on Year 0					
20	Student Attrition from baseline to end of year tests	0.25	0.24	0.25	0.70
21	Baseline Maths test score of attritors (Equality of all groups)	-0.14	-0.07	-0.09	0.72
22	Baseline Telugu test score of attritors (Equality of all groups)	-0.20	-0.14	-0.20	0.77

Notes:

1. The infrastructure index sums binary variables showing the existence of a brick building, a playground, a compound wall, a functioning source of water, a functional toilet, and functioning electricity.
2. The proximity index sums 8 variables (coded from 1-3) indicating proximity to a paved road, a bus stop, a public health clinic, a private health clinic, public telephone, bank, post office, and the mandal educational resource center.
3. 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 t-statistics for the baseline test scores and attrition are computed by treating each student/teacher as an observation and clustering the standard errors at the school level (Grade 1 did not have a baseline test). The other t-statistics are computed treating each school as an observation.

Table 2: Impact of Incentives on Student Test Scores

Panel A: Combined (Math and Language)						
Dependent Variable = Normalized End of Year Test Score						
	Year 1 on Year 0		Year 2 on Year 1		Year 2 on Year 0	
	[1]	[2]	[3]	[4]	[5]	[6]
Normalized Lagged Test Score	0.499 (0.013)***	0.497 (0.013)***	0.559 (0.018)***	0.568 (0.019)***	0.45 (0.015)***	0.447 (0.015)***
Incentive School	0.153 (0.042)***	0.170 (0.042)***	0.140 (0.041)***	0.130 (0.042)***	0.217 (0.047)***	0.225 (0.048)***
School and Household Controls	No	Yes	No	Yes	No	Yes
Observations	68678	62614	63004	53032	49498	44213
R-squared	0.29	0.32	0.30	0.32	0.23	0.25
Panel B: Math						
Dependent Variable = Normalized End of Year Test Score						
	Year 1 on Year 0		Year 2 on Year 1		Year 2 on Year 0	
	[1]	[2]	[3]	[4]	[5]	[6]
Normalized Lagged Test Score	0.49 (0.017)***	0.492 (0.017)***	0.505 (0.025)***	0.512 (0.025)***	0.418 (0.022)***	0.416 (0.023)***
Incentive School	0.188 (0.049)***	0.205 (0.050)***	0.184 (0.050)***	0.176 (0.050)***	0.276 (0.055)***	0.286 (0.056)***
School and Household Controls	No	Yes	No	Yes	No	Yes
Observations	34109	31105	31443	26473	24584	21953
R-squared	0.28	0.30	0.28	0.30	0.23	0.24
Panel C: Telugu (Language)						
Dependent Variable = Normalized End of Year Test Score						
	Year 1 on Year 0		Year 2 on Year 1		Year 2 on Year 0	
	[1]	[2]	[3]	[4]	[5]	[6]
Normalized Lagged Test Score	0.516 (0.014)***	0.508 (0.015)***	0.617 (0.014)***	0.627 (0.014)***	0.483 (0.014)***	0.476 (0.014)***
Incentive School	0.119 (0.038)***	0.136 (0.038)***	0.098 (0.037)***	0.086 (0.038)**	0.158 (0.043)***	0.164 (0.044)***
School and Household Controls	No	Yes	No	Yes	No	Yes
R-squared	0.319	0.341	0.341	0.366	0.246	0.269

Notes:

1. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level.
 2. Constants are insignificant in all specifications and are not shown.
 3. School controls include an infrastructure and proximity index (as defined in Table 1)
 4. Household controls include student caste, parental education, and affluence (as defined in Table 3A)
- * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Heterogenous Treatment Effects

Panel A: Household and School Characteristics

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Number of Students in School	School Proximity (8 - 24)	School Infrastructure (0 - 6)	Household Affluence (0 - 7)	Parental Literacy	Scheduled Caste/ Tribe	Male Student	Normalised Baseline Score
Year 2 on Year 0								
Incentive School	0.172 (0.057)***	-0.131 (0.216)	0.200 (0.138)	0.087 (0.074)	0.206 (0.048)***	0.221 (0.048)***	0.234 (0.049)***	0.217 (0.047)***
Covariate	-0.048 (0.031)	-0.006 (0.010)	0.016 (0.041)	0.012 (0.015)	0.084 (0.019)***	-0.054 (0.041)	0.019 (0.026)	0.453 (0.025)***
Interaction	0.033 (0.021)	0.025 (0.015)*	0.006 (0.042)	0.039 (0.019)**	0.016 (0.025)	-0.006 (0.054)	-0.013 (0.033)	-0.005 (0.031)
Observations	52756	49498	49498	45169	45169	49498	45197	49498
R-squared	0.22	0.23	0.23	0.24	0.24	0.23	0.24	0.23
Year 1 on Year 0								
Incentive School	0.133 (0.048)***	-0.032 (0.160)	0.068 (0.107)	-0.008 (0.063)	0.126 (0.044)***	0.166 (0.044)***	0.154 (0.043)***	0.150 (0.042)***
Covariate	-0.072 (0.027)***	-0.013 (0.008)	0.004 (0.024)	0.014 (0.013)	0.087 (0.016)***	-0.004 (0.035)	0.008 (0.020)	0.502 (0.021)***
Interaction	0.003 (0.016)	0.014 (0.011)	0.030 (0.030)	0.045 (0.018)**	0.024 (0.021)	-0.068 (0.047)	0.005 (0.025)	-0.005 (0.026)
Observations	70560	66656	66656	63629	63629	68251	63667	68251
R-squared	0.29	0.30	0.30	0.31	0.31	0.29	0.31	0.29

Notes:

1. The infrastructure index sums binary variables showing the existence of a brick building, a playground, a compound wall, a functioning source of water, a functional toilet, and functioning electricity.
2. The proximity index sums 8 variables (coded from 1-3) indicating proximity to a paved road, a bus stop, a public health clinic, a private health clinic, public telephone, bank, post office, and the mandal educational resource center.
3. The household affluence index sums seven binary variables coding the ownership of land, owning of current residence, residing in a "pucca" house (house with four walls and a cement and concrete roof), having each of electricity, water, toilet, and a television at home
4. The parental literacy variable is coded from 0 to 2 for how many of the child's parents are literate
5. Scheduled Caste and Schedule Tribe are the most socioeconomically backward groups in India

Panel B: Teacher Characteristics

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Education	Training	Years of experience	Salary (log)	Male	Teacher Absence	Active Teaching	Active or Passive Teaching
Stacked regression using both years of data								
Incentive School	-0.097 (0.152)	-0.148 (0.167)	0.238 (0.061)***	1.230 (0.554)**	0.205 (0.060)***	0.175 (0.044)***	0.077 (0.045)*	0.077 (0.06)
Covariate	0.012 (0.031)	-0.032 (0.040)	-0.002 (0.003)	0.001 (0.043)	0.061 (0.056)	-0.049 (0.107)	0.032 (0.066)	0.058 (0.07)
Interaction	0.080 (0.047)*	0.110 (0.058)*	-0.007 (0.004)*	-0.119 (0.061)*	-0.072 (0.068)	-0.057 (0.146)	0.202 (0.083)**	0.118 (0.09)
Observations	88026	88270	88631	89198	90932	107472	107051	124569
R-squared	0.281	0.28	0.28	0.28	0.28	0.28	0.284	0.27

Notes:

1. Teacher education is coded from 1-4 indicating 10th grade, 12th grade, College degree and Master's or higher
2. Teacher training is coded from 1-4 indicating no training, a Diploma, a bachelor's in Education, and a Master's
3. Teacher absence and active teaching are determined from direct observations 4-6 times a year

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

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

Table 4: Impact of Incentives on Mechanical Versus Conceptual Learning

Dependent Variable = End line Test Score by Mechanical/Conceptual Questions (Normalized by Mechanical/Conceptual Distribution in Control Schools)				
	Year 1 on Year 0		Year 2 on Year 0	
	[1] Mechanical	[2] Conceptual	[3] Mechanical	[4] Conceptual
Normalized Baseline Score	0.485 (0.012)***	0.339 (0.011)***	0.449 (0.013)***	0.308 (0.013)***
Incentive School	0.138 (0.038)***	0.138 (0.043)***	0.173 (0.041)***	0.183 (0.046)***
Observations	67720	67720	42554	42554
R-squared	0.28	0.17	0.24	0.15

Notes:

1. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level.
 2. The mean of the treatment effects here is not the same as in Table 1 because the normalization scale is different (the score on each component of the test is normalized by the score distribution of that component in the the control schools)
- * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Impact of Incentives on Non-Incentive Subjects

Dependent Variable: Normalized Test Score				
	Year 1 on Year 0		Year 2 on Year 0	
	[1] Science	[2] Social Studies	[3] Science	[4] Social Studies
Normalized Baseline Math Score	0.214 (0.019)***	0.222 (0.018)***	0.155 (0.023)***	0.166 (0.023)***
Normalized Baseline Language Score	0.206 (0.019)***	0.287 (0.019)***	0.214 (0.024)***	0.182 (0.024)***
Incentive School	0.107 (0.052)**	0.135 (0.047)***	0.112 (0.045)**	0.177 (0.049)***
Observations	12011	12011	9165	9165
R-squared	0.26	0.30	0.18	0.18

Notes:

1. Social Studies and Science tests were only administered to grades 3 to 5
 2. All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level.
- * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Group versus Individual Incentives

Dependent Variable = Normalized Endline Test Score									
	Year 1 on Year 0			Year 2 on Year 1			Year 2 on Year 0		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
	Combined	Maths	Telugu	Combined	Maths	Telugu	Combined	Maths	Telugu
Normalized Lagged Score	0.499 (0.013)***	0.490 (0.017)***	0.516 (0.014)***	0.559 (0.018)***	0.505 (0.025)***	0.618 (0.014)***	0.451 (0.015)***	0.417 (0.022)***	0.485 (0.014)***
Individual Incentive School (II)	0.160 (0.049)***	0.194 (0.060)***	0.128 (0.043)***	0.194 (0.049)***	0.239 (0.058)***	0.152 (0.044)***	0.271 (0.058)***	0.321 (0.068)***	0.223 (0.053)***
Group Incentive School (GI)	0.146 (0.050)***	0.183 (0.058)***	0.110 (0.046)**	0.084 (0.049)*	0.128 (0.061)**	0.042 (0.043)	0.161 (0.058)***	0.231 (0.071)***	0.091 (0.052)*
F-Stat p-value (Testing GI = II)	0.790	0.870	0.683	0.045	0.096	0.026	0.116	0.291	0.030
Observations	68678	34109	34569	63004	31443	31561	49498	24584	24914
R-squared	0.29	0.28	0.32	0.30	0.28	0.34	0.23	0.23	0.25

Notes:

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

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

Table 7: Disaggregated Treatment Effects By Individual And Group Incentives (Year 2 on Year 0)

Dependent Variable = Normalized Endline Test Score								
	Panel A: Impact of Incentive Type By Grade				Panel B: Impact of Incentive Type on Mechanical vs Conceptual Learning		Panel C : Impact of Incentive Type on Non-Incentive Subjects	
	Grade 2	Grade 3	Grade 4	Grade 5	Mechanical	Conceptual	Science	Social Studies
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Individual Incentive	0.209 (0.087)**	0.228 (0.065)***	0.203 (0.086)**	0.410 (0.081)***	0.224 (0.050)***	0.233 (0.057)***	0.186 (0.057)***	0.22 (0.060)***
Group Incentive	0.069 (0.060)	0.114 (0.080)	0.155 (0.070)**	0.276 (0.093)***	0.121 (0.051)**	0.132 (0.057)**	0.037 (0.056)	0.134 (0.061)**
F-Stat p-value (Testing GI = II)	0.153	0.223	0.631	0.250	0.082	0.138	0.032	0.220
Observations	49498	49498	49498	49498	42554	42554	9165	9165
R-squared	0.23	0.23	0.23	0.23	0.24	0.16	0.19	0.18

Notes:

1. Panels A and B combine normalized scores in both math and language

2. All effects represent two year treatment effects (Y2 on Y0)

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

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

Table 8: Teacher Behavior (Observation and Interviews)

Teacher Behavior	Incentive versus Control Schools (All figures in %)			
	Incentive Schools	Control Schools	p-Value of Difference	Correlation with student test scores
	[1]	[2]	[3]	[4]
Teacher Absence (%)	0.26	0.25	0.191	-0.069
Actively Teaching at Point of Observation (%)	0.42	0.43	0.769	0.134***
Did you do any special preparation for the end of year tests? (% Yes)	0.64	0.32	0.000***	0.094**
What kind of preparation did you do? (UNPROMPTED) (% Mentioning)				
Extra Homework	0.42	0.20	0.000***	0.068
Extra Classwork	0.47	0.23	0.000***	0.079**
Extra Classes/Teaching Beyond School Hours	0.16	0.05	0.000***	0.183***
Gave Practice Tests	0.30	0.14	0.000***	0.104***
Paid Special Attention to Weaker Children	0.20	0.07	0.000***	0.000

Notes:

- Each teacher is treated as one observation with t-tests clustered at the school level.
 - Teacher absence and active teaching in column 4 are coded as means over the year
 - All teacher response variables from the teacher interviews are binary and column 4 reports the correlation between a teacher's stated response and the test scores of students taught by that teacher (controlling for lagged test scores as in the default specifications throughout the paper)
- * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Impact of Inputs versus Incentives on Learning Outcomes

	Dependent Variable = Normalized Endline Test Score								
	Year 1 on Year 0			Year 2 on Year 1			Year 2 on Year 0		
	Combined [1]	Math [2]	Language [3]	Combined [4]	Math [5]	Language [6]	Combined [7]	Math [8]	Language [9]
Normalized Baseline Score	0.511 (0.010)***	0.493 (0.012)***	0.535 (0.011)***	0.552 (0.012)***	0.495 (0.016)***	0.614 (0.010)***	0.460 (0.012)***	0.422 (0.016)***	0.497 (0.012)***
Incentives	0.156 (0.041)***	0.189 (0.049)***	0.124 (0.038)***	0.144 (0.036)***	0.198 (0.044)***	0.091 (0.033)***	0.217 (0.048)***	0.277 (0.056)***	0.158 (0.045)***
Inputs	0.096 (0.037)***	0.11 (0.043)**	0.082 (0.036)**	0.047 (0.03)	0.047 (0.04)	0.047 (0.03)	0.084 (0.043)*	0.092 (0.049)*	0.076 (0.042)*
Difference (Incentives - Inputs)	0.06	0.08	0.04	0.10	0.15	0.04	0.13	0.19	0.08
F-Stat p-value (Inputs = Incentives)	0.091	0.061	0.199	0.006	0.000	0.170	0.002	0.000	0.042
Observations	112214	55743	56471	119788	59797	59991	82574	41043	41531
R-squared	0.29	0.27	0.32	0.29	0.26	0.33	0.21	0.21	0.24

Notes:

- These regressions pool data from all 500 schools in the study: 'Group' and 'Individual' incentive treatments are pooled together as "Incentives", and the 'extra contract teacher' and 'block grant' treatments are pooled together as "Inputs"
 - All regressions include mandal (sub-district) fixed effects and standard errors clustered at the school level.
- * significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1a: Andhra Pradesh (AP)



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

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

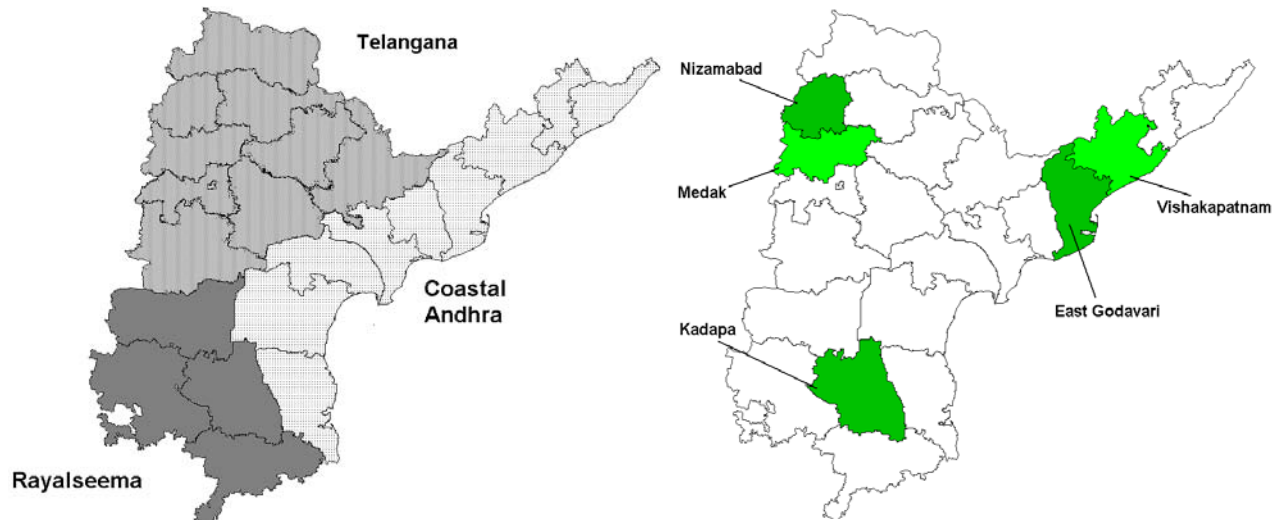


Figure 2: Density/CDF of Normalized Test Scores by Treatment

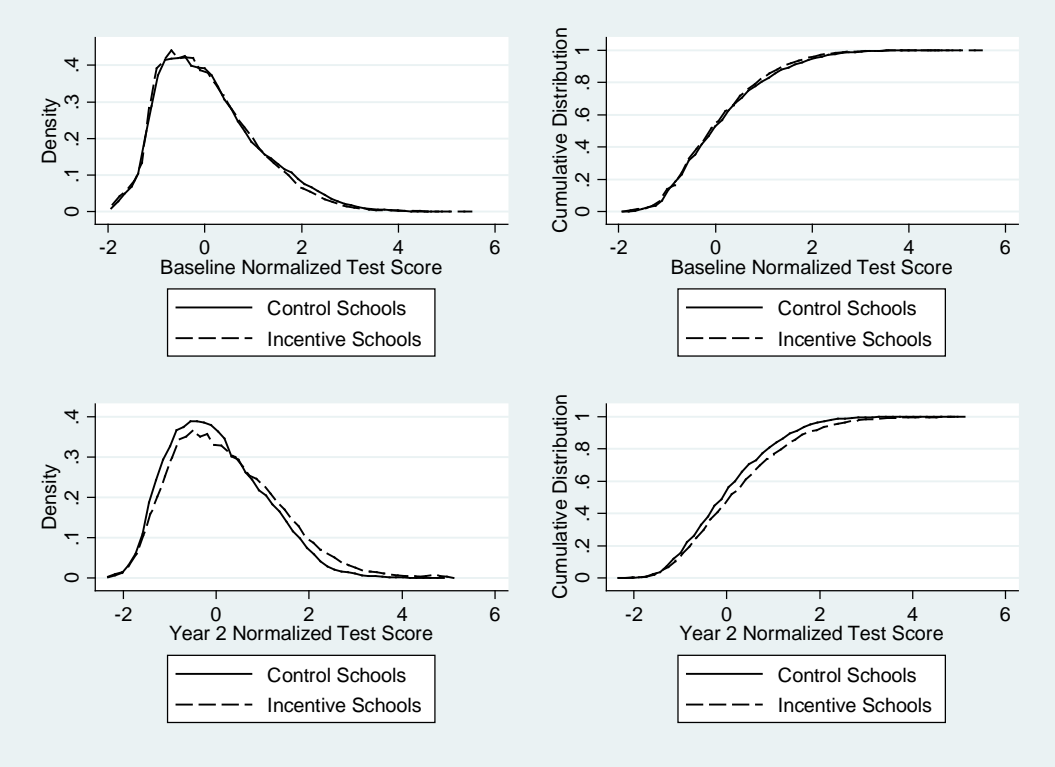


Figure 3: Quantile (Percentile) Treatment Effects

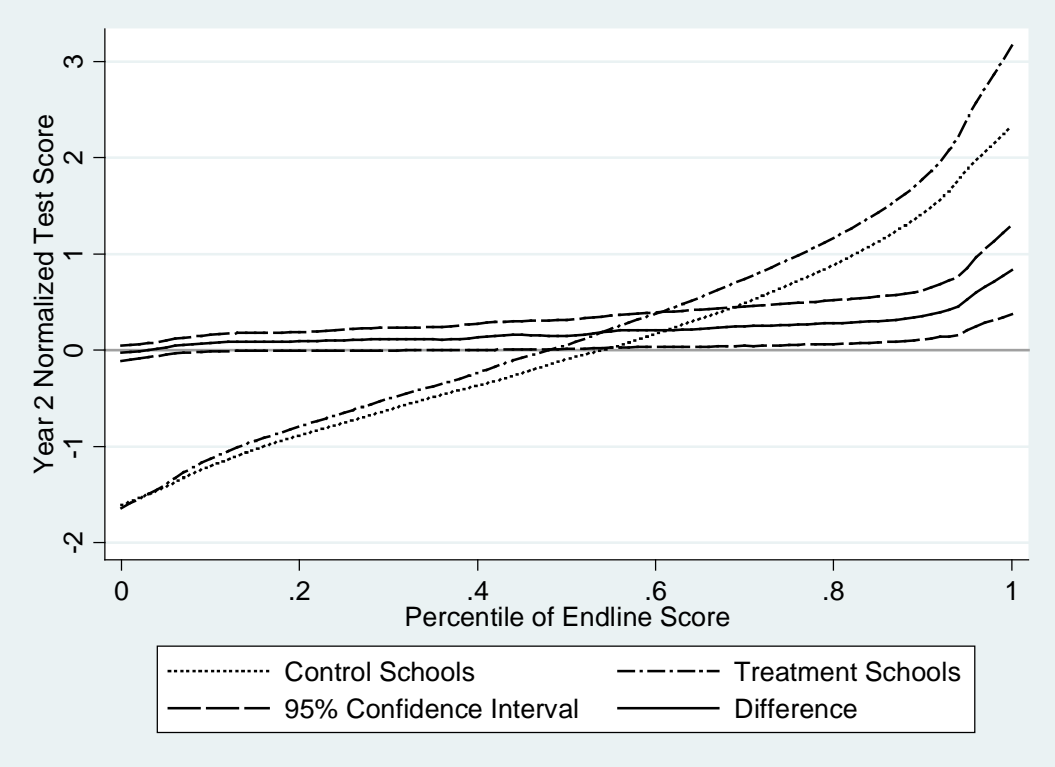


Figure 4: Heterogeneous Treatment Effects by Baseline Score Percentile

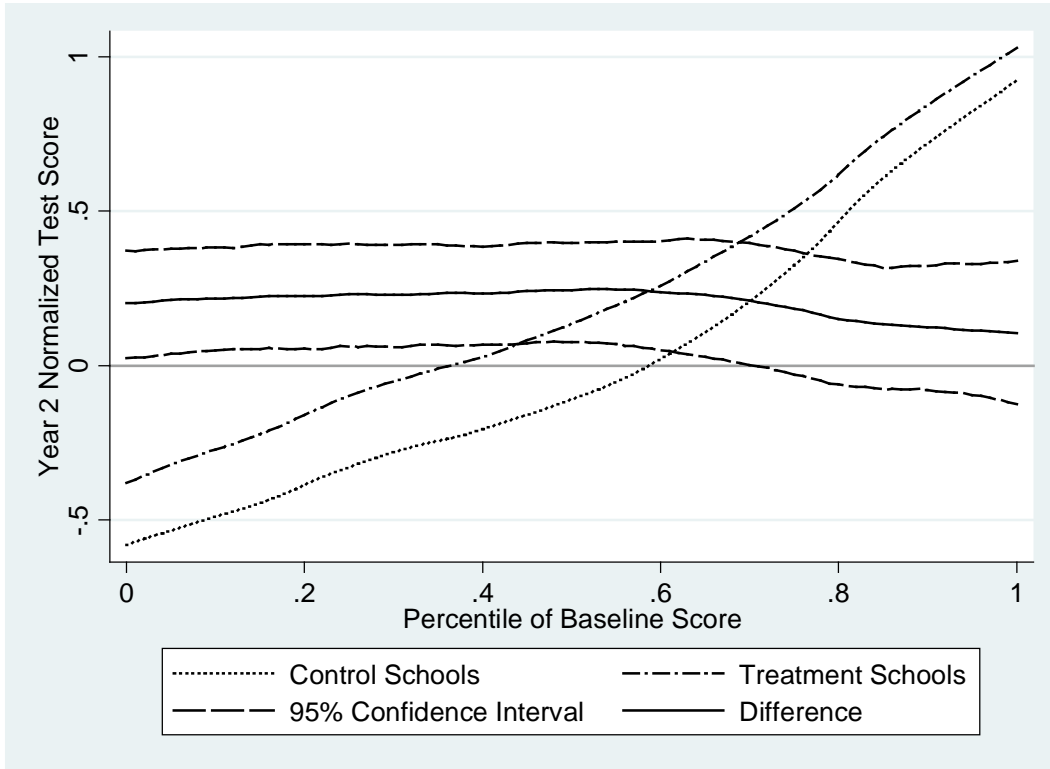


Figure 5: Teacher Fixed Effects by Treatment Status

