# Monitoring Works:
# Getting Teachers to Come to School

Esther Duflo
Department of Economics and Poverty Action Lab, Massachusetts Institute of Technology

Rema Hanna
Wagner School of Public Service, New York University and Poverty Action Lab

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**Abstract**

In the rural areas of developing countries, teacher absence is a widespread problem, both in formal and informal schools. This paper tests whether incentives based on teacher presence can reduce teacher absence, and whether they can lead to more teaching activities and more learning. In 60 informal one-teacher schools in rural India, randomly chosen out of 120 (the treatment schools), a financial incentive program was initiated to reduce absenteeism. Teachers were given a camera with a tamper-proof date and time function, along with instructions to have one of the children photograph the teacher and other students at the beginning and end of the school day. The time and date stamp on the photographs were used to track teacher attendance. A teacher's salary was a direct function of his attendance. The remaining 60 schools served as comparison schools. The introduction of the program resulted in an immediate decline in teacher absence. The absence rate (measured using unannounced visits both in treatment and comparison schools) changed from an average of 43 percent in the comparison schools to 24 percent in the treatment schools. When the schools were open, teachers were as likely to be teaching in both types of schools, and the number of students present was roughly the same. The program positively affected child achievement levels: a year after the start of the program, test scores in program schools were 0.17 standard deviations higher than in the comparison schools and children were 43 percent more likely to be admitted into regular schools. This suggests that a high absence rate contribute to low school quality: instrumental variable estimates suggest that reducing absence rate by 10 percentage point would increase test score by 0.10 standard deviation.

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## I. INTRODUCTION

The United Nations Millennium Development Goals call for achieving universal primary education by 2015. To fulfill this goal, UNESCO estimates that, worldwide, 15 to 35 million new teachers will be needed. In response, many developing countries, including India, are rapidly trying to improve access to schooling, while at the same time not exhausting their already stretched budgets. To do so, many have opted to hire para-teachers on short contract, with lower salaries, to work in primary schools, or to finance NGOs and local government to run non-formal education (NFE) centers. In some countries, informal teacher account for most of the growth in the teaching staff over the last few years. In India alone, 21 million children, mainly poor children in rural areas, attend NFEs.[1] In Gujarat, one of India's largest states, para-workers comprise 43% of the teaching staff in rural areas (Duthilleul, 2004).

However, improved access often is not matched by improvements in school quality. As a result, while more children start primary school, many leave after just a few years, after having learned very little in the process. In India, a nationwide survey found that 65% of the children enrolled in grade 2 to 5 in government primary schools could not read a simple paragraph, and 50% cannot do simple subtraction or division (Pratham, 2006). Such poor learning outcomes may be, in part, due to high absence among teachers. Using unannounced visits to measure teacher attendance, a nationally representative survey found that 24 percent of teachers in India were absent from the classroom during normal school hours (Chaudhury, et al., 2005a, b).[2] The situation in India is particularly bleak. In terms of absence rates, India ranked seventh among the eight countries for which comparable data was collected. Getting teachers to attend school may help India achieve the improvements in school quality needed to make "universal primary education" a meaningful term.

One solution to the absence problem—championed by many, including the 2004 World Development Report—is to expand community control by improving community-based monitoring,

[1] This is a large number of children: for comparison, in the US, enrollment in *all* public schools from kindergarten to grade 8 was just 33.6 million in 2004.
[2] Although teachers do have some official non-teaching duties, this absence rate is much too high to be fully explained by this particular story.

strengthening the flow of information between the community and the government, involving the community in decisions to both hire and fire teachers, or transferring wholesome control of teachers to the community. Hiring of para-teachers is one way to provide this control. Since para-teachers are on short contracts, they are more likely—at least in theory—to be at risk of being fired than regular teachers. They are also usually from the local community, and often under the control of the community. The official government documents of India show that, since the early 1990s, the Indian Government has viewed hiring para-teachers as a probable solution to curb the absence problem (Duthilleul, 2004). Publicly funded para-teachers and informal schools are therefore viewed by some as a way, not only to improve access while keeping budget in check, but also potentially as a way to improve school quality. Others, however, fear that para-teachers are as likely to be absent as regular teachers, and that they lower school quality since the qualification requirements are lower than that of teachers.

Chaudhury et al. (2005b) found that locally hired teachers, contract teachers and teachers at non-formal schools run by NGOs all had absence rates significantly *higher* than those of regular government school teachers.[3] More generally, evidence from a variety of contexts suggests that community control interventions have not been particularly effective at reducing absence (Banerjee and Duflo, 2005). Banerjee, Deaton, and Duflo (2004) found that community-based monitoring, even when robustly structured, did not reduce absenteeism among service providers at government health facilities in rural India. Kremer and Vermeersch (2005) found no effect for a program in rural Kenya that empowered school committees to monitor teachers, share performance information with officers in the Ministry of Education, and to give prizes to the best-performing teachers. Finally, Olken (2004) found that increasing community participation in meetings where public officials accounted for the expenditures of public funds did not reduce corruption in local development projects in Indonesia.

In contrast, there is limited, but encouraging, evidence, that external control, coupled with a clear and credible threat of punishment, may be more effective at inducing "good" behavior. Contrary to his

[3] It may, however, be said that the difficult conditions (remote areas, part-time teachers and students, etc.) under which these schools operate may counterbalance the effects of community pressure.

findings on community participation, Olken (2004) found that the threat of a top-down audit resulted in a significant decline in corruption. Likewise, Chaudhury et al. (2005b) report that teachers at schools that were inspected more often tended to have lower absentee rates. Many believe that providing high-powered incentives to government teachers would be politically difficult, since teachers are often a powerful political force, and teacher unions would presumably oppose such a plan. It is however feasible to implement such incentives for contract teachers (either in regular schools or in NFEs) since they do not form a entrenched constituency, they are already subject to yearly renewal of their contract, and there is a long queue of qualified individuals for these jobs. In fact, this ability to discipline para-teachers is another reason why the Indian government has favored hiring contract teachers over regular teachers, even though in practice, there has been little effort to put such systems in place (Duthilleul, 2004).

In this paper, we empirically test whether the direct monitoring of para-teacher attendance (we refer to them as teachers for the rest of the paper), coupled with high-powered incentives based on their presence, results in higher quality schooling. We ask three main questions: If teachers are given high-powered incentives to attend school based on external monitoring, will they attend school more? If teachers attend school more, will they teach more? Finally, if teacher absenteeism is reduced, will children learn more as a result?

Although there are many good reasons to believe that high-powered incentives based on presence may reduce absenteeism, incentives may fail if teachers face constraints that prevent them from taking advantage of them. For example, some argue that teachers skip school because they must participate in meetings, training sessions, election or census duty, or help villagers with other tasks. These pressures may be particularly high on para-teachers, who are placed under the control of the community: if the demand for schooling is not particularly high in the community, they may require the teachers to perform other tasks where an educated teacher is required (such as maintaining accounts or chairing meetings), threatening them to give them a bad report if they do not comply. Moreover, para-teachers sometimes live far from school or might have many other things to do that attending school regularly is not possible.

Even if these incentives increase teacher attendance, it is unclear whether or not they actually increase child-learning levels. Teachers may be subject to *multitasking*, where the agent concentrates on the easiest way to increase the rewarded measure with little or no gains in the measure the principal ultimately wants to improve (Holstrom and Milgrom, 1991). Under this particular type of incentive scheme, teachers may focus on being present (or even on being present just during the few minutes when they are to be monitored), but reduce their efforts in other dimensions. This may be a legitimate concern as there is evidence that other incentive programs have been prone to multitasking. Glewwe, Ilias and Kremer (2003) estimated the effect of a teacher incentive program based on child test scores in Kenya. They found that the program did increase test scores in the short run, but that the gains in learning were only temporary and were not accompanied by increases in teacher attendance or effort. Teachers, they concluded, may have just gamed the system by teaching to the test.[4] Studies conducted in the United States provide further evidence of similar gaming behavior among educators facing high-powered incentives, including altering what was served at lunch on the day of the test (Figlio and Winicki, 2002), manipulating who took the test (Figlio and Getzler, 2002), and outright cheating (Jacob and Levitt, 2003).

Other theories also suggest that incentives schemes based on attendance may cause teachers to teach less even as they attend school more.[5] First, such schemes may demoralize teachers, resulting in less effort. In laboratory experiments, Fehr and Schmidt (2004) found that individuals under high-powered incentive systems may lose their motivation and, thus, work less than under a flat wage regime. Second, financial incentives may harm a teacher's intrinsic motivation, that is, the sense of duty or enjoyment of the job that motivates them to come to work (Kreps, 1997). This threat is particularly real for teachers, who as a group may have strong intrinsic motivation because of the value they place on interacting with the children and in seeing them succeed.[6] If provided incentives based on presence, teachers may come to believe that just attending class is enough and that their classroom behavior is not important. Finally,

[4] Lavy (2004) provides a more optimistic assessment of a teacher incentive program in Israel.
[5] Chaudhuury (2005a), for example, found that only 45 percent of teachers in India were actually teaching at the time of the unannounced visits.
[6] Despite the high absence rate, this can be said of teachers in developing countries. After all, despite difficult circumstances and the lack of any sanctions for absence, many of them attend school and do teach.

some teachers, who previously believed that they were required to work every day, might decide to stop working once they have reached their target income for the month (Fehr and Gotte, 2002).

On the other hand, incentives can improve child learning levels if the main cost of working is the opportunity cost of attending school, rather than carrying out other income generating activities. Once in school, the marginal cost of actually teaching may be quite low. Under these circumstances, an incentive system that directly rewards presence would have a good chance of increasing child learning. Thus, whether or not an incentive program based on absence can improve teaching effectiveness and child learning is ultimately an empirical question.

In this study, we examine the impact of a teacher incentive program run by the NGO Seva Mandir on teacher presence, teaching activities, and child learning. Seva Mandir runs single-teacher non-formal education centers (NFEs) in the rural villages of Udaipur, India. As is typical in many rural areas, teacher absenteeism is high, despite the threat of dismissal for repeated absence. In the baseline study (August 2003), we find an absence rate of 44 percent.

Faced with such high absenteeism, Seva Mandir implemented an innovative monitoring and incentive program in September 2003. In 57 randomly selected program schools, Seva Mandir gave teachers a camera, along with instructions to have one of the students take a picture of the teacher and the other students at the start and close of each school day. The cameras had tamper-proof date and time functions, allowing for the collection of precise data on teacher attendance that could be used to calculate teachers' salaries. Each teacher was, then, paid according to the number of valid school days for which they were actually present, where a "valid" day was defined as one for which the opening and closing photos were separated by at least five hours and both photos showed a minimum number of children. In the 56 comparison schools, teachers were paid a fixed rate for the month, and were told (as usual) that they could be dismissed for repeated, unexcused absences.

The introduction of the program resulted in an immediate and long lasting improvement in teacher attendance rates in treatment schools (as measured through one unannounced visit per month in both treatment and comparison schools). Over the 27 months in which attendance was tracked, teachers at program schools had an absence rate of 24 percent, roughly half of the 44 percent baseline and the 43 percent at comparison schools. That absence rates stayed low after the end of the proper evaluation phase (the first ten months of the program) implies that teachers did not change their behavior simply for the evaluation.

We see no evidence that effort declined in other dimensions. When school was open, teachers were as likely to be teaching in treatment as in comparison schools, suggesting that the marginal costs of teaching are low once the teacher is present. Since they had better attendance records than their comparison school counterparts, teachers at treatment schools taught for the equivalent of 54 more child days (or a third more) per month. Student attendance was the same in both groups, but more teaching meant more learning for children in treatment schools. A year into the program, their test scores were 0.17 standard deviations higher than children in comparison schools. Children were also 6 percentage points (or 44%) more likely to transfer to formal primary school, which requires passing a competency test. The program impact and cost are similar to other successful education programs.

The findings clearly demonstrate the link between simple, straightforward, well-enforced incentives and teacher presence, as well as the link between teacher presence and student achievement. In theory, this type of incentive scheme is already in place. Teachers are paid to come to work every day, and most school systems, both private and public, have provisions to penalize unexplained absences. In developing countries, however, teachers are rarely punished (much less dismissed) for poor attendance.[7] The political clout of government schoolteachers may make it difficult to enforce these attendance rules. However, this paper shows that establishing a clear link between presence and pay among para-teachers appears to be both feasible and beneficial. These findings also imply that para-teachers can be, at least in some circumstances, effective teachers. This confirms earlier findings, showing very large impact of para-teachers when used as remedial teachers in primary schools (Banerjee et al, 2005).

[7] Chaudhury et al. (2005a) report that, the 25 percent absence rate notwithstanding, only one principal in their sample of 3,000 government schools reported a case in which a teacher was fired for repeated absence.

The remainder of the paper is organized as follows. Section II provides a detailed description of the incentive program and the evaluation techniques. The results are presented in Section III. Section IV concludes.

## II. THE PROGRAM AND THE EVALUATION

### 1. Non-formal Education Centers

Non-formal education centers (NFEs) are an integral component of India's education system. Since the National Policy on Education of 1986, they have played an increasingly important role in India's drive towards universal primary education. The NFEs serve two main purposes. First, since they are easier to establish and cheaper to run, they have been the primary instrument for rapidly expanding access to schooling to children in poor, remote rural areas where there are no government schools or where schools are far away. The government of Madhya Pradesh, for example, mandated that NFEs be established for all communities where there were no schools within a kilometer. Second, the NFEs have been used to ease children, who may otherwise not attend school, to join a government school at the age-appropriate grade level. In particular, since NFEs are subject to fewer regulations than government schools, they can tailor their hours and curricula to meet the diverse needs of the children. As of 1997, 21 million children were enrolled in NFEs across India (Education for All Forum, 2000), and similar informal schools operate throughout most of the developing world (Bangladesh, Togo, Kenya, etc.).

Children of all ages may attend, though most were between 7-10 years in our sample. Nearly all the children are illiterate when they first join the centers. In the setting of our project, the NFEs are open six hours a day and have 20 students, all taught in one classroom by one teacher, who is recruited from the local community and has, on average, completed up to a 10th grade education. Instruction focuses on teaching children basic Hindi and math skills. As the schools studied in this paper have only one teacher, when the teacher is absent, the school is closed.

## 2. The Program

Seva Mandir administers about 150 non-formal primary education centers (NFEs) in the tribal villages of Udaipur, Rajasthan. Udaipur is a sparsely populated, arid and hilly region, where villages are remote and access is difficult. As a result, it is often difficult for Seva Mandir to regularly monitor the NFEs. Absenteeism is high, despite the organization's policy calling for dismissal of absent teachers. A 1995 study (Banerjee et al., 2005) found that the absence rate was 40 percent, while the baseline of this study (in August 2003) found that the rate was 44 percent.

Seva Mandir was, therefore, motivated to identify ways to reduce absenteeism among its teachers. To this end, they implemented an innovative external monitoring program in September 2003. They chose 120 schools to participate in the study, with 60 randomly selected schools for the program serving as the treatment group and the remaining 60 as the comparison group.[8] Prior to the announcement of the program, 7 of these schools closed or failed to open; these were equally distributed amount the treatment and controls schools, and were not due to the program.

In the 57 treatment schools, Seva Mandir gave each teacher a camera, along with instructions for one of the students to take a photograph of the teacher and the other students at the start and end of each school day. The cameras had a tamper-proof date and time function, which made it possible to precisely track each school's openings and closings.[9] As Figure 1 demonstrates, the day of the month and the time of day appear in the right corner (the month does not appear, but there is no ambiguity about that since the rolls were changed every month). Camera upkeep (replacing batteries, changing and collecting film) was done monthly at regularly scheduled teacher meetings. If a camera malfunctioned, teachers were instructed to call the program hotline within 48 hours. Someone was then dispatched to replace the camera, and teachers were credited for the day in which the camera was broken.[10]

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[8] Seva Mandir operates in five blocks in the Udaipur district. Stratified random sample was conducted within block.
[9]The time and data buttons on the cameras were covered with heavy tape, and each had a seal that would indicate if it had been tampered with. Fines would have been imposed if cameras had been tampered with (this did not happen) or if they had been used for another purpose (this happened in one case, when a teacher photographed his family).
[10]Teachers were given the 48-hour leeway to report malfunctioning cameras because not all villages have a working phone and phone services are not always reliable.

Seva Mandir's monthly base salary for teachers was is Rs1000 ($23) for at least 20 days of work per month. In the treatment schools, teachers received a Rs50 ($1.15) bonus for each additional day they attended in excess of the 20 days, and they received a Rs50 fine for each day of the 20 days they skipped work. A "valid" day was defined as a day in which the opening and closing photographs were separated by at least five hours and enough children (at least eight) were present in both photos to indicate that the school was actually functioning. Due to ethical and political concerns, Seva Mandir capped the fine at Rs500; hence, a teacher's salary ranged from Rs 500 to Rs 1,300 (or $11.50 to $29.50). In the 56 comparison schools, teachers were paid the flat rate of Rs 1,000, and were informed that they could be dismissed for poor attendance (though this happens very rarely, and did not happen during the span of the evaluation).[11]

Seva Mandir pays its teachers every two months. In each two-month period, they collected the last roll of film a few days before the salary payment, so that payment was made immediately after the end of the relevant time period. To reinforce the understanding of the program, Seva Mandir showed treatment teachers a detailed breakdown of how their salary was calculated after the first payment.

## 3. Evaluation

In this paper, we test whether incentives based on attendance can improve school quality. An independent evaluation team led by Vidhya Bhawan (a Udaipur-based consortium of schools and teacher training institutes) and MIT's Poverty Action Lab collected regular data on the functioning of the program to answer three basic questions: If teachers are provided with high-powered incentives to attend school that are based on external monitoring, will they attend more? If they do attend school more, will teaching time increase? Finally, will children learn more as a result?

The Poverty Action Lab collected data on teacher attendance through one random unannounced visit per month in both treatment and comparison schools. By comparing the absence rates obtained from

[11]Teachers in the control schools knew that the camera program was occurring, and that some teachers were randomly selected to be part of the pilot program.

the random checks across the two types of schools, we can determine the incentive program's effect on absenteeism.[12] In addition, Seva Mandir provided access to all of the camera and payment data for the treatment schools, allowing us to compare absence rates measured by the random checks against those measured by the cameras. In addition to verifying whether the random checks provide a good estimate of actual attendance rates, this comparison also allows us to verify whether teachers were simply coming to school in the mornings and afternoons for the photos, rather than attending the entire school day.

Data collected on teacher and student activity during the random check allow us to determine whether teachers taught more as a result of the program. For schools that were open during the visit, the enumerator noted what the teachers and students were doing: how many children were sitting in class when the enumerator arrived, if anything was written on the blackboard, and if the teacher was talking to the children. While these are, of course, crude measures of teacher performance, they were chosen specifically because each could be easily observed *before* the teacher and students could adjust their behavior: for example, the enumerator could see if anything was written on the blackboard the instant he walked in the door. Moreover, since the schools have only one teacher and one classroom, teachers could not be warned that the enumerator was in the building, and therefore, change their behavior.

Since teaching time is also a function of child attendance, the enumerator also collected student attendance data at the time of the random check. After completing the observation sheet, the enumerator conducted a roll call to document which children on the evaluation roster were present.[13] They also noted whether any of the absent children had dropped out of school or had enrolled in a government school, and then updated the evaluation roster to include new children.

To determine whether child learning increased as a result of the incentive program, in collaboration with Seva Mandir, the evaluation team administered three basic competency exams to all children enrolled in the NFEs in August 2003: a pre-test in August 2003, a mid-test in April 2004, and a

[12] The random checks were not linked with any incentives, and teachers were aware of that fact. We cannot rule out the fact that the random check could have increased attendance in comparison schools. However, we have no reason to believe the random checks would differentially affect the attendance of comparison and treatment teachers.

[13] Evaluation rosters were different from the school roster in that they included all children enrolled at the beginning of the experiment and all children enrolled subsequently.

post-test in September 2004. The pre-test followed Seva Mandir's usual testing protocol. Children were given *either* a written exam (for those who could write) *or* an oral exam (for those who could not). For the mid-test and post-test, all children were given the oral exam and an opportunity to try the written exam. Those unable to write got a zero on the written section. The oral exam tested simple math skills (counting, one-digit addition, simple division) and basic Hindi vocabulary skills, while the written exam tested for these competencies plus more complex math skills (two-digit addition and subtraction, multiplication and division), the ability to construct sentences, and reading comprehension. Thus, the written exam tested both a child's ability to write and his ability to handle material requiring higher levels of competency relative to the oral exam.

Finally, detailed data were collected on teachers' characteristics to determine the extent to which the program impact on child learning varied with teacher characteristics. First, to determine whether the effect on learning depended upon a teacher's academic ability, Seva Mandir administered a competency exam to each teacher prior to the program. Second, after the program had been in place for two months, the evaluation team observed each school for a whole day, in order to assess whether the program impact depended on the pedagogy employed by the teachers.[14]

## III. Results

In this section, we begin by reporting the results of the baseline survey and assessing the integrity of the randomized framework (Section 1). Then, we discuss the impact of the program on teacher attendance (Section 2), child attendance (Section 3) and child learning (Section 4). Finally, in Section 5, we provide a cost-benefit analysis of the program.

### 1. Baseline and Experiment Integrity

[14] Note that unlike the crude measures of teacher performance collected at the random checks, teachers may have changed their behavior as a result of the observations.

Given that schools were randomly allocated to the treatment and comparison groups, we expected the quality of schooling measures before the program onset to be similar across the groups. Before the program was announced in August 2003, the evaluators were able to randomly visit 44 schools in the treatment group and 41 in the comparison.[15] Panel A of Table 1 shows that the attendance rates were 66 percent and 63 percent, respectively. This difference is not significant. Other measures of school quality were also similar prior to the program: in all dimensions shown in Table 1 (number of students present in school at the time of visit, infrastructure, teacher qualification and performance), the treatment schools appear to be slightly better than comparison schools, but the differences are always small and never significant. Finally, to determine the joint significance of the treatment variable on all the outcomes listed in Panel B through Panel E, we estimated a SUR model. The results are listed in the final row of Table 1: The F-statistic is 1.13, with a p-value of 0.25, implying that the comparison and treatment schools were similar to one another at the program's inception.

Baseline academic achievement and preparedness were the same for students across the two types of schools. Table 2 presents the results of the pre-test (administered in August 2003). Panel A shows the percentage of children who could write. In Panels B and C, we report the results from the oral and written tests, respectively. On average, students in both groups were at the same level of preparedness before the program. Seventeen percent of children in the treatment schools and 19 percent in the comparison schools took the written exam. This difference is not significant. Those who took the oral exam were somewhat worse in treatment schools, and those who took the written exam were somewhat better in treatment schools. Again, the differences are not significant.

## 2. Teacher Absence

[15] Unfortunately, due to time constraints, all schools could not be checked before the program was announced. Thus, 85 randomly selected schools of the 113 were visited prior to the program. We have checked that there was no significant (or perceivable) difference in the characteristics of the schools that were not observed before the program. Moreover, the conclusion of the paper remains unchanged when we restrict all the subsequent analysis to the 85 schools that could be observed before the program was started.

The effect on teacher absence was both immediate and long lasting. Figure 2 shows the fraction of schools found open on the day of the random visit, by month. Between August and September, teacher attendance increased in treatment schools relative to the comparison schools. For the remainder of the program, the attendance rates in treatment and comparison schools followed similar seasonal fluctuations, with treatment school attendance systematically higher than comparison school attendance.

As Figure 2 shows, the effect of the program remained strong even after the administration of the post-test, which marked the end of the evaluation. Since the program had been so effective, Seva Mandir maintained it, but only had enough resources to keep the program running at the 57 treatment schools (expansion to all the schools is planned in the coming months). Random checks conducted after the post-test showed that higher attendance rates persisted at treatment schools even after the teachers knew that the experiment was over and that the program had become permanent. This suggests that teachers did not change their behavior simply for the duration of the evaluation.

Table 3 presents a detailed breakdown of the effect of the program on absentee rates. Columns 1 and 2 report the means for the treatment and comparison schools, respectively, over the entire period for which random checks were conducted (September 2003 to October 2005). Column 3 presents the difference between the treatment and comparison schools for this period, while Columns 4 through 6 respectively present the difference until the mid-test, between the mid-test and post-test, and after the post-test. On average, teacher absence was 20 percentage points lower in the treatment than in the comparison schools (Panel A). Thus, the program almost halved absence rates in treatment schools. The treatment effect was smaller for the period between the mid-test and post-test, largely because comparison school teachers attended class more often, and then rose 22 percentage points after the post-test.[16]

The program effects on teacher attendance were pervasive—teacher attendance increased for both low and high quality teachers. In Panel B, we report the impact on absenteeism for teachers with above

[16]This reduction in school closures was comparable to that of a previous Seva Mandir program which tried to reduce school closures by hiring a second teacher for the NFEs. In that program, only fell by 15 percentage points (Banerjee, Jacob and Kremer, 2005), both because individual teacher absenteeism remained high and because teachers coordinated to come on different days.

median test scores on the teacher skills exam conducted prior to the program, while Panel C shows the impact for teachers with below median scores.[17] The program impact on attendance was larger for below median teachers (a 25 percentage point increase versus a 15 percentage point). However, this was due to the fact that the program brought below median teachers to the same level of attendance as above median teachers (77%).

Given that both high and low skill teachers were affected by the program (and the correlation between teacher skills and attendance), it was unsurprising that teachers with both low and high attendance were also affected. Figure 3A plots the observed density of absence rates in treatment and comparison schools for the 25 random checks conducted during the program, while Figure 3B graphs the estimated cumulative density function of the frequency of attendance assuming that the distribution of absence follows a beta-binomial distribution The actual and estimated distributions are very similar, indicating that the assumption of a binomial distribution is quite accurate. Both figures clearly show that the incentive program shifted the entire distribution of absence for treatment teachers. Of the 25 days, not one of teachers in the comparison schools was present on all days. Almost 25 percent of teachers were absent more than half the time. In contrast, 5 of the program teachers were present for all days, 47% of teachers were present for 21 days or more, and all teachers were present at least half the time. Therefore, the camera program was effective on two margins: it eliminated extremely delinquent behavior (less than 50 percent presence), and increased the number of teachers with perfect or very high attendance records.

A comparison of the random check data and the camera data suggests that, for the most part, teachers did not "game" of the system. The fact that treatment teachers had a lower absence rate at the random checks, which were conducted in the middle of the day, suggests that teachers did not just to come for the photographs, and then leave in the intervening period. A comparison of the random check data and the camera data provides direct proof of this. Table 4 shows that for the treatment schools, the camera data tends to match the random check data quite closely. Out of the 1337 cases, 80 percent

[17] Teacher test scores and teacher attendance are correlated: In the control group, below median teachers came to school 53% of the time, while above median teachers came to school 63% of the time.

matched perfectly, that is, the school was open and the photos were valid or the school was closed and the photos were not valid. In 13 percent of the cases, the school was found open at the random check, but the photos indicated that the day was not considered "valid" (which is not an instance of "gaming"). There are 88 cases (7 percent) where the school was closed and the photos were valid, but only 54 (4 percent of the total) of these were due to teachers being absent in the middle of the day during the random check and shown as present both before and after. In the other cases, the data did not match because the random check was completed after the school had closed for the day, or there was missing data on the time of the random check or photo (Table 4, Panel C). Overall, while there were some instances of gaming, the fact that it occurred in only 4% of the time suggests the program was quite robust.

Of the 179 cases (13 percent) where the school was open but the photos were invalid, it was primarily because there was only one photo (90 cases) or because the school was open for less than the full five hours (43 cases). This suggests that for a small number of cases, the random check may have designated a comparison school as open for the day, even though it was open for only part of the school day. Therefore, since the program may also have affected the length of each school day, the random check data may, if anything, underestimate the effect of the program on total teaching time a child received. Figure 4 provides some support for this hypothesis. The figure plots the difference in average teacher attendance for treatment and comparison schools at the time of the random check. The difference in the attendance rate was larger at the start and end of the school day, suggesting that teachers in treatment schools not only attended more often, but also kept the schools open for more hours.

The program had two components: daily monitoring of teacher presence; and the fact that an incentive was linked to the teacher presence. As such, these results do not tell us whether the monitoring *per se* or the incentive reduced absence. While we cannot answer this question definitively, several factors suggest that the incentives are likely to be an important factor. First, both comparison and treatment schools received a monthly random check visit (not linked to an incentive), in addition to the Seva Mandir regular visit: all the schools were therefore under observation. Second, Banerjee, Deaton and Duflo (2004) suggest that, for health workers at least, weekly monitoring alone has no impact on presence.

## 2. Teacher Behavior

Though the program increased teacher attendance and the length of the school day, the program could still be considered ineffective if the teachers compensated for increased presence by teaching less. We looked at the activity data collected at the time of the random check to determine what the teachers were doing in the classroom. Since we can only measure the impact of the program on teacher performance for schools that were open, the fact that treatment schools were open more may introduce selection bias. That is, if teachers who tended to be absent also tended to teach less when present, the treatment effect may be biased downward since more observations would be drawn from among such low-effort teachers in the treatment group than in the comparison group. This bias is likely to be in the direction of finding reduced effort. Nevertheless, Table 5 shows that there was no significant difference in teacher activities: in the comparison schools, as in the treatment schools, teachers were as likely to be in the classroom, to have used the blackboard, and to be addressing students when the enumerator arrived. This does not appear to have changed during the duration of the program.

The fact that, as opposed to just showing up to class more, teachers did not reduce their effort in school suggests that the fears of multitasking and loss of intrinsic motivation were perhaps unfounded. Instead, our findings suggest that once teachers were forced to attend (and therefore to forgo the additional earnings they could get by working elsewhere, or their leisure time), the marginal cost of teaching must not have been that large. This belief was supported during in-depth conversations with 15 randomly selected NFE teachers regarding their teaching habits in November to December 2005. We found that teachers spent little time preparing for class. Teaching in the NFE follows an established routine, with the teacher conducting the same type of literacy and numeracy activities every day. One teacher stated that he decides on the activities of the day as he is walking to school in the morning. Other teachers stated that, once they left the center, they were occupied with household and field duties, and, thus, had little time to prepare for class outside of mandatory Seva Mandir training meetings. Furthermore, despite the poor attendance rates, many teachers displayed a motivation to teach. All of the teachers stated that they felt good when the student learned, and liked the fact they were helping the most disadvantaged students get an education. Plus, most teachers stated that they did like teaching, once they were in the classroom: "The day is all teaching, so I just try to enjoy the teaching."

The teachers' general acceptance of the incentive system may be an additional reason why multitasking appeared not to be a problem: Several months into the program, teachers were asked to fill out feedback forms, which gave us a qualitative impression of the program's perception among teachers. Seva Mandir also conducted a feedback session at their bi-annual sessions, which were attended by members of the research team. No teachers complained about the principle of the program, though many teachers had some specific complaints about the inflexibility of the rules. For example, many did not like the fact that a day was not valid even if a teacher was present 4 hours and 55 minutes (the normal school day is six hours, but an hour's slack was given to the teachers). Others stated that assembling eight children on time at the beginning of the day is difficult, or that they disliked the fact that the program did not plan for sick leave or leave for extenuating circumstances, such as a funeral. On the other hand, many felt empowered by the fact that the onus of performing better (and being better paid as a result) was actually in their hands: "Our payments have increased, so my interest in running the center has gone up." Others described how the payment system had made others in the community less likely to burden the teacher with other responsibilities once town members knew that a teacher would be penalized if he did not attend school on a given day. This suggests that the program may actually have stronger effects in the long run, as it signals a change in the norms of what teachers are expected to do.

### 3. Child Attendance

On the feedback forms, many teachers said that the program increased children attendance: "This program has instilled a sense of discipline among us as well as the students. Since we come on time, the students have to come on time as well." Unfortunately, conditional on whether a school was open, the effect of the program on child attendance cannot be directly estimated without bias, because of selection of the observations where the school was open. For example, if schools that were typically open also attracted more children, and the program induced the "worst" school (with fewer children attending regularly) to be open more often in the treatment schools than in the comparison schools, then this selection bias will tend to bias the effect of the program on child attendance downwards. The selection bias could also be positive, for example if the good schools generally attract students with better earning opportunities, who are more likely to be absent, and the "marginal" day is due to a weak schools catering to students with little outside opportunities. Selection bias is a realistic concern (and likely to be negative) since, for the comparison schools, there is a positive correlation between the number of times a school is found open and the number of children found in school. Moreover, we found that the effect of the program was higher for schools with originally weak teachers, who may attract fewer children.

Keeping this caveat in mind, child attendance was actually not significantly different in treatment and comparison schools. In Table 6, we present the participation rates of a child in an open school, by treatment status (Panel A).[18] While an average child's participation rate was slightly higher in treatment schools (48 percent) than in comparison schools (47 percent), this difference is not significant. Excluding children who left the NFE, child attendance is higher overall (63 percent for treatment and 60 percent for comparison schools), and the difference is also insignificant. Higher skilled children were no more likely to attend than lower skilled children. In Panel B, we disaggregate the data by whether or not the child could write at the program inception. Of those who could not write at the start of the program, the attendance rate was 49% in the treatment and 46% in the control schools, while the child attendance rate of those who could write was 41% in the treatment and 48% in the control schools.

Treatment schools had more teaching days. Even if the program did not increase child attendance on a particular day, the increase in the number of days the school was open should result in more days of

[18] "Participation" subsumes both attendance and enrollment. It is the correct concept to use in an environment when being enrolled does not necessarily indicate that the child actually attends school. The participation dummy is defined for every day a random check is conducted, and is equal to 1 if the child is present on that day and 0 otherwise.

teaching per child. The impact of the program on child instruction time is reported in Panel C of Table 6. Taking into account days in which the schools were closed, a child in a treatment school received 9 percentage points (or 30 percent) more days of instruction than a child in a comparison school. Assuming 27 days of work in a month (schools are open six days a week), a child obtained 2.7 more days of instruction time a month at treatment schools. Since there are roughly 20 children per classroom, this figure translates into 54 more child-days of instruction per month in program schools than in comparison schools. This effect is larger than that of successful interventions that have been shown to increase child participation, such as the PROGRESA program of conditional cash transfers, which increased enrollment by 3.4 percent in primary schools and had no impact on attendance (Schultz, 2004); de-worming, which increased participation by 7.5 percentage points (Miguel and Kremer, 2004); a child incentive program (Kremer, Miguel, and Thornton, 2004), which increased participation by 5 percentage points; and a child scholarship program, which increased participation by 8 percentage points (Kremer et al., 2004). The effect is comparable to that of adding a second teacher in Seva Mandir NFEs (Banerjee, Jacob and Kremer, 2005), which increased the number of days of instruction per month by 3.1.

In summary, since children were as likely to attend class on a given day in treatment schools as in comparison schools, and because the school was open much more often, children received significantly more days of instruction in the treatment schools. This finding suggests that the high teacher absence we observed is not likely to be the efficient response to a lack of interest by the children: if it were the case that children came to school 55 percent of the time because they could afford to attend more than a certain number of days, then we would see a sharp reduction in children presence in treatment schools on days where the school was open. On the other hand, we do not see a sharp increase in the presence of children in treatment schools despite the increased presence of the teachers. This suggests that either teacher absence is not the main cause of the irregular child presence, or that the children have not yet had time to adjust to this new pattern. The latter explanation is not entirely plausible, however, since the program has now been in place for a long time, and we do not see a larger increase in the presence of children in the later periods than in the earlier period.

## 4. **Effects on Learning**

Children in treatment schools, on average, received 30 percent more instruction time than children in comparison schools. Over the course of a year, this resulted in 34 more days of instruction per child. Apparently, there was no decline in the teachers' effort level. Some, however, are questioning the effectiveness of para-teachers, arguing that, because they are less qualified than other teachers, it is not clear that they are teaching much to their students despite the support and in-service training they get from NGOs like Seva Mandir. If para-teachers are not effective teachers, the fact that it is possible to provide them incentives to come to school more is not particularly relevant for policy. Evaluating the effect of the program on learning is therefore critical.

### *4.1 Attrition and Means of Mid- and Post-Test*

Before comparing test scores in the treatment and comparison schools, we must first ensure that selective attrition does not invalidate the comparison. There are two possible sources of attrition.[19] First, some children leave the NFEs, either because they drop out of school altogether or because they start attending regular primary schools. Second, some children were absent on testing days. To minimize the impact of attrition on the study, we made considerable attempts to track down children who did not show for the last two tests (even if they had left the NFE to attend a formal school or had been absent on the testing day) and administered the post-test to them. Consequently, attrition was fairly limited. Of the 2,230 students who took the pre-test, 1,893 also took the mid-test, and 1,760 also took the post-test. Table 7 shows the attrition in the treatment and comparison groups as well as the characteristics of the attriters. At the time of the mid-test, attrition was higher in the comparison group than in the treatment group. At the time of the post-test, attrition was similar in both groups, and children who dropped out of the treatment group were similar to those that dropped out of the comparison group.

[19] As mentioned earlier, 7 centers closed down or failed to open prior to the start of the program. These closures were unrelated to the program, and equally distributed among treatment and comparison schools. We made no attempt to test the children from these centers in the pre-test.

Table 7 also provides some simple descriptive statistics, comparing the test scores of treatment and comparison children. The first row presents the percentage of children who were able to take the written exam, while subsequent rows provide the mean exam score (normalized by the mid-test comparison group). Relative to the pre-test and mid-test, many more children, in both the treatment and comparison schools, were able to write by the post-test. On the post-test, students did slightly worse in math relative to the mid-test comparison, but they performed much better in language.

Table 7 also shows the simple differences between treatment and comparison at the mid- and post-tests. On both tests, in both language and math, the treatment students did better than the comparison students (a 0.16 standard deviation increase and 0.11 standard deviations in language at the post-test score), even though the differences are not significant at a 95 percent confidence level. Since children test scores are strongly auto-correlated, we obtain more precision below by controlling for the child's pre-test score level.

*4.2. Test Results*

In Table 8, we report the impact of the program on the mid-test (conducted in April) and the post-test (conducted in October). We compare the average test scores of students in the treatment and comparison schools, conditional on a child's pre-program competency and preparedness level. In a regression framework, we model the effect of being in a school *j* that is being treated ($Treat_j$) on child *i*'s test score ($Score_{ikj}$) on test *k* (where k denotes either the mid- or post-test exam):

$$Score_{ikj}=\beta_1+\beta_2 Treat_j+\beta_3 Pre\_Writ_{ij}+\beta_4 Oral\_Score_{ij}+\beta_5 Written\_Score_{ij}+\varepsilon_{ijk}. \qquad [1]$$

Because test scores are highly autocorrelated, controlling for a child's test scores before the program increases the precision of our estimate. However, the specific structure of the pre-test (i.e. the fact that children either took the written or the oral test in the pre-test, so that there is not one "score" on a comparable scale for each child) does not allow for a traditional difference-in-difference (DD) or "value added" (child fixed effect) strategy. Instead, we include a variable containing the child's pre-test score for the oral test if he took the oral pre-test and 0 otherwise (*Oral_Score*$_{ij}$), the child's pre-test score on the written test if he took the written test and 0 otherwise (*Written_Score*$_{ij}$), and an indicator variable for whether he took the written test at the pre-test (*Pre_Writ*$_{ij}$).[20] This fully controls for the child pre-test achievement, and is thus similar in spirit to a DD strategy. Standard errors are clustered by school. Each cell in Table 8 represents the treatment effect ($\beta_2$) obtained in a separate regression. For ease of interpretation, the mid-test results (Columns 1 to 4) and post-test results (Columns 5 to 8) are expressed in the standard deviation of the distribution of the mid-test score in the comparison schools.[21]

The tables reveal that the program had a significant impact on learning, even as early as the mid-test. Children in treatment schools gained 0.16 standard deviations of the test score distribution in language, 0.15 standard deviations in math, and 0.17 overall (Panel A). Including controls for school characteristics—location, teacher test scores, and the infrastructure index of school—does not significantly change our findings (Panel B). Children with higher initial test scores gained the most from the program: those able to write at the pre-test had mid-test test scores 0.25 standard deviations higher in treatment schools than in comparison schools (Panel D).

The differences between students in the treatment and comparison schools persisted in the post-test (Columns 5 to 8). Children in treatment schools gained 0.21 standard deviations in language, 0.16 in math, and 0.17 overall (Panel A). Similar to the mid-test, much of the gains came from children with higher initial learning levels. The treatment effect of 0.17 standard deviations is similar to other successful educational interventions, such as the Tennessee Star experiment in the United States (Kruger

[20] At the pre-test, children were given either the oral or the written score. At the mid- and post-test, every child took the oral part, and every child who could write took the written exam (all children were given a chance to try the written exam; if they could not read, they were given a zero for the written test).

[21] Scores are normalized such that the mean and standard deviation of the comparison group at the time of the mid-test exam is zero and one, respectively. (Specifically, we subtract the mean of the comparison group in the pre-test, and divide by the standard deviation.) This allows for comparison across samples, as well as with results from other studies. We could not normalize with respect to the pre-test score distribution since not every child took the same test at the pre-test.

and Whitmore, 2001) , the Balsakhi Remedial Education Program in India during its first year (Banerjee, et al., 2005), and a girls' incentive program in Kenya (Kremer, Miguel and Thornton, 2004).

Finally, we compare the impact of the program on girls versus boys in Panel E and F of Table 8. The data shows that girls gained as much, if not more, from the program as boys. On the mid-test, 7 percentage-points more of girls in the treatment schools were able to write relative to the comparison schools, compared to only 2 percentage-points of boys (this 5-percentage point difference is significant). The post-test also suggests that girls gained slightly more from the program than the boys, but these differences are not significant.

### *4.3. Leaving the NFE*

NFEs prepare children, who might not otherwise attend school, to join government schools at the age-appropriate grade level. To join a government school, children must demonstrate proficiency for a grade, either by passing an exam or through vetting by a government teacher. The ability to join schools is therefore a strong signal of the success of an NFE in fulfilling its mission. The program increased the number of children graduating to the government schools. As shown in Table 9, 20 percent of students in the treatment schools graduated to the government schools, compared to only 14 percent in the comparison schools. This 6-percentage point difference implies a 43% increase in the graduation rate.

In the final row of Table 9, we present the dropout rates for children who left school entirely (i.e. left the NFE and did not join a government school). The dropout rate is slightly lower for the treatment schools, but we cannot reject the hypothesis that the difference between treatment and comparison schools is zero.

### *4.4 Teacher Presence on Learning*

The previous sections presented the reduced form analysis of the effect of the incentives program on child learning. Table 10 interprets what these estimates can tell us about the impact of teacher presence.[22] Columns 1 to 3 report simple correlations between teacher presence and test scores. Specifically, they report the coefficient estimate of the number of times a school was found open ($Open_j$) on a regression of the mid-test or post-test scores:

$$Score_{ikj}=\beta_1+\beta_2 Open_j+\beta_3 Pre\_Writ_{ij}+\beta_4 Oral\_Score_{ij}+\beta_5 Written\_Score_{ij}+\varepsilon_{ijk}. \qquad [2]$$

As in the previous tables, we continue to control for the child's pre-test score and to cluster standard errors by school.

Column 1 reports OLS estimation of Equation 2 for comparison schools in order to obtain the correlation between presence and child achievement levels. In this case, the random check data is used to estimate the number of times a school is found open. The coefficient is 0.20, indicating that the test scores of children in centers open 100 percent of the time would be 0.10 standard deviations higher than those of children in a center open 50 percent of the time. The coefficient is also insignificant.

This point estimate is similar to those reported in other studies (Chaudhury, et al., 2005a) and, taken at face value, would imply that the effect of teacher attendance on learning is not that large. Chaudhury et al. (2005a) conjectures that the measurement of absence rates based on a few random visits per school have considerable error, and may thus bias the results downwards. Consistent with this theory, the effect on the post-test scores, where having more months of random check data allows us to better estimate the absence rate per school, becomes larger (0.58 standard deviations). Our study provides a much more direct test of this hypothesis, since, for treatment teachers, the photograph data gives us the actual attendance. We present the OLS estimate of the effect of presence for treatment teachers using the random check data (Column 2) and camera data (Column 3). Overall, the effect of teacher presence is larger in the treatment schools than the comparison schools (compare 0.39 in Column 2 to 0.20 in Column 1, both obtained with random check data). More interestingly, consistent with the measurement error hypothesis, the effect of teacher presence is larger and much more significant when using the more accurate measure of presence, especially for the mid-test scores (the estimate is 0.87 standard deviations

[22] This estimate are the effect of being present at a random check, which cumulates the effect of having come at all, and having come for a longer time.

in the Column 3, compared to 0.39 in Column 2). For the post-test, where we have a much more accurate measure of presence from the random check data, the results from the two methods are instead similar (0.98 in Column 3 versus 1.17 in Column 2).

Finally, in Column 4, we pool both samples and instrument $Open_j$ (as measured by the random check) by the treatment status of the school to obtain exogenous variation in the percentage of time the school was found open in the random check. Since we have shown that the program had a direct effect on the length of the school day, as well as whether or not the school opened at all, the 2SLS estimate captures the joint effect of outright absence and of a longer school day. The 2SLS estimates are higher than the OLS results found in Column 1, and they are indistinguishable from the OLS results in Column 3, obtained with the precisely measured absence. This suggests that the relatively low correlation between teacher absence and test scores that was observed in previous studies is indeed likely to be due to measurement error in the teacher absence data, and that reducing absence would have the potential to greatly increase test scores. Even a 10-percentage point reduction in the absence rate would result in a 0.10 standard deviation increase in test scores.

Extrapolating these estimates (which must be done with caution, since the local effect may be different form the overall effect), we would conclude that the effect of being enrolled in an NFE for a year with a teacher present every day is about one standard deviation. This point estimate is very similar to that of the effect on children enrolled in regular primary school, but not having achieved basic numeracy or literacy of attending remedial education classes with a para-teacher for one year in urban India (Banerjee et al. 2005): the point estimate there was 1.12 standard deviation. Both of these suggest that, at least when and NGO provides them some guidance on what to teach, para-teachers can be extremely effective teachers.

### *4.5. Teacher and Child Characteristics*

In Table 11, we examine whether the treatment effect varies based on teacher and student characteristics. Each cell in Table 11 reports the coefficient estimate ($\beta_4$) of the interaction of being in a treated school and a school's characteristic ($Char_j$) on a regression of the test score:

$$Score_{ikj}=\beta_1+\beta_2 Treat_j+\beta_3\, Char_j+\beta_4\, Treat_j * Char_j+\beta_5 X_{ij}+\varepsilon_{ijk} \quad [3]$$

$X_{ij}$ includes controls for pre-test scores and controls for the interaction of the pre-test scores with the school characteristic. In Columns 1 and 2, we interact the treatment effect with a teacher's academic abilities at the start of the program; the treatment effect is slightly larger for teachers with higher test scores and for teachers with more years of schooling, but this effect is small and not always significant. Since we have seen that the effect has a larger impact on the attendance of the less qualified teachers, this nevertheless suggests that teachers with high test scores or more education do teach more effectively when they are there. The treatment effect does not vary based on the infrastructure level of the school (Column 3), and does not vary much based on teacher pedagogy (Column 4) or student behavior (Column 5) at the time of the school observations in October 2003. This suggests that regardless of the level of school infrastructure or teaching competency, initiating the incentive program can result in positive gains to learning. These results are similar to other studies (Hanushek, Rivkin and Kain, 2005; Lavy 2004) that find no relationship between school characteristics and student performance.

## 5. Costs-Benefit Analysis

The evaluation presented in this paper shows that a straightforward monitoring and incentive program can effectively reduce teacher truancy. The benefits (in terms of child learning) of running such a program, relative to costs, are high, and comparable to other successful education programs in developing countries (evaluated with randomized evaluations).

Table 12 presents an estimate of the administrative costs of the program for one year. For the treatment schools, the average teacher salary was nearly Rs 1,000. Since the flat salary paid to comparison teachers was also Rs1,000, the program did not increase expenditure on teacher salaries. Other program costs (administration, developing the pictures, and buying the cameras) amounted to Rs5,379 per center per year. This cost corresponds to 40 percent of a teacher's yearly salary, but to only Rs268 ($6) per child per year (assuming about 20 children per teacher).[23] Expressed in terms of cost per outcome, this program cost approximately 11 cents for each additional instruction day per child, $60 per additional school year, and $3.58 for increasing test scores by 0.10 standard deviations.

The cost per standard deviation improvement in test scores is higher than that of the Balsakhi Remedial Education Program evaluated in Banerjee et al. (2005). In the Balsakhi program, a second teacher (often a woman) was hired to provide remedial tutoring to children who had been identified as lagging behind their peers. The Balsakhi program resulted in a 0.14 increase for Rs 107 during its first year (and larger increases in its second year), which makes it over 2.5 times more cost effective. However, the Balsakhi program was evaluated in an urban setting, in the cities of Mumbai and Vadodara, where the external monitoring of teachers is cheaper. In contrast, the second teacher program evaluated in Udaipur district by Banerjee, et al. (2005), while it reduced school closures by 15 percent and increased the number of child-days, did not result in any improvement in test scores. The cost-effectiveness of the Seva Mandir camera program is comparable to that of other successful education programs in rural Africa: the cost per 0.10 standard deviations of the camera program ($3.58) is similar to that of a girl's scholarship program ($3.53) that was evaluated in Kremer, Miguel and Thornton (2004). The scholarship program is currently the only program that has been proven to *durably* improve test scores in Africa.[24]

Using the estimate in Table 6, we calculate that the cost per year of schooling is 6/0.10=$60 per additional year of schooling due to the program. This is much higher than the cost of the de-worming

[23] This estimate does take into account the opportunity cost for teachers and children. Note, however, that the effects are larger than they could be if the program was implemented on a large scale, and more cost-effective technology (such as digital cameras) could be used.
[24] The test-based teacher incentive program that was evaluated in Glewwe, Ilias and Kremer (2003) had a cost of $3.41 per 0.10 standard deviations, its gains on test scores were considered to be temporary, and it reflects gaming rather than real learning improvement.

program in Africa (evaluated to be only \$3.53 per additional year of schooling),[25] but lower than that of any other programs evaluated there, such as the child incentive program (\$90 per extra year), or a child sponsorship program which delivered uniforms to children (\$99 per extra year).[26] It is also just over half the cost of the two-teacher program, previously implemented in Seva Mandir, which, evaluated at the current teacher's salary, cost \$115 per extra year of schooling.[27] Thus, the camera program, even in its pilot form (which used an expensive way to develop photographs) is a cost-effective program compared to many others, both in terms of increasing instruction time and in terms of increasing learning.

Finally, these estimates combined with other estimates that can be found in the literature suggest that the long-run returns to the program are quite high. Duraisamy (2000) estimates an 8% return from an additional year of primary school in India. The program increased the school year by 0.09, and therefore, we expect a rate of return to wages of 0.72% from the program. In 2000, GDP per capita in India was \$2683 (Penn World Tables). To calculate the effect on the net present value of discounted wages, we assume that sixty percent of the output per worker in India is wages and that wage gains from higher school participation are earned over forty years and discounted at ten percent per year. This results in a long-run wage increase of \$125. From this, we subtract the opportunity costs of the children and the opportunity costs of the teachers. Assuming that children are half as productive as adults, the opportunity cost for the children is \$104. The opportunity cost to the teacher is approximately \$10 per child (\$209/20 children). Under these assumptions, the program increases the net present value of wages by about \$11. Given the program costs of \$6 per child, the program has a benefit-cost ratio of 1.83.

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[25] In making this comparison, it is worth noting that Kremer and Miguel (2004) use the cost of the de-worming program if implemented on a large scale, whereas we use the cost of the program as implemented in this small scale pilot. However, the cost of the program they actually evaluated was only about three times larger than what they used for the cost-benefit evaluation, which still makes the de-worming program a more cost-effective way to improve instruction time.

[26] The cost per year of the PROGRESA program in primary schools is substantially larger (\$5,902.45). However, the PROGRESA program is primarily a transfer program to families, and its cost effectiveness should probably not be based on its effect on school outcomes alone.

[27] The cost-effectiveness figure reported by Banerjee, Jacob and Kremer (2005) is \$4.82 per extra month, or \$58 per extra year, but the teachers were then paid Rs400, which was, according to the authors, untenable even then, in the face of competition for teachers, and was subsequently increased to Rs100.

## IV. Conclusion

Addressing the startlingly high rates of teacher absenteeism in developing countries is a critical step for increasing school quality. School systems have often failed to carry out their own rules regarding teacher presence, and therefore, in practice, teachers are rarely penalized for unexcused absences. Some believe that community pressure can reduce absenteeism, but several recent studies have shown that, for a variety of reasons, community monitoring often delivers disappointing results.

In contrast, we show that direct monitoring, combined with simple and credible incentives based on teacher presence leads to large increases in attendance among para-teachers in informal schools. The program evaluated in this paper cut teacher absence from an average of 42 percent in the comparison schools to 22 percent in the treatment schools. As a result, students in program schools benefited from about 30 percent more instruction time. The program had a statistically and economically significant impact on test scores: After one year, child test scores in program schools were 0.17 standard deviations higher than in comparison schools, and children were more likely to be admitted to regular primary schools. Despite being implemented on a small scale, the program is cost-effective.

Our findings show that external monitoring systems can succeed in reducing absenteeism. Quite often, monitoring systems have failed because individuals within institutions have chosen to ignore their own rules. For example, top-down monitoring systems have been shown to fail when school headmasters are in charge of implementing them (Kremer and Chen, 2001), because the headmasters have marked the teachers present even if they were absent. In contrast, mechanical systems, such as using cameras, have the advantage of not being subject to the discretion of any one individual: a commitment at a senior level would make its implementation viable.

These results suggest that extending Seva Mandir's incentive program to non-formal schools, both in India and other developing nations, has the potential to increase learning levels. Moreover it shows that informal schools can be effective teaching environments, at least in some circumstances. The question arises as to whether the program can be instituted in government schools. Teachers in government schools are often much more politically powerful than teachers in informal or private schools. Thus, it may prove difficult to institute a system where government teachers would be monitored daily using a camera or similar device such as a date-time stamps, and other methods may prove necessary (such as having more frequent inspections). However, our findings suggest that the barriers currently prevent teachers from attending school regularly (distance, other activities, lack of interest by children, etc.) are not insurmountable. Given the political will, it is therefore likely that solutions to the absence problem could be found in government schools as well.

If this turns out to be impossible , the results also tells us that the strategy of improving the access to education by increasing the role of para-teachers both within and outside the regular government schools could have, as the government of India surmised, positive side effect in terms of increasing the quality of education. However, the results also show that strong external monitoring and incentive systems might be necessary to achieve these gains.

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**Figure 1: Photographs from Program**

**Table 1: Is School Quality Similar in Treatment and Control Groups Prior to Program?**

| | Treatment (1) | Control (2) | Difference (3) |
|---|---|---|---|
| *A. Teacher Attendance* | | | |
| School Open | 0.66 | 0.63 | 0.02 |
| | | | (0.10) |
| | 44 | 41 | 85 |
| *B. Student Participation (Random Check)* | | | |
| Number of Students Present | 17.72 | 15.54 | 2.19 |
| | | | (2.23) |
| | 29 | 26 | 55 |
| *C. Teacher Qualifications* | | | |
| Teacher Test Scores | 34.99 | 33.62 | 1.37 |
| | | | (2.01) |
| | 53 | 56 | 56 |
| Teacher Highest Grade Completed | 10.21 | 9.80 | 0.41 |
| | | | (0.46) |
| | 57 | 54 | 111 |
| *D. Teacher Performance Measures (Random Check)* | | | |
| Percentage of Children Sitting Within Classroom | 0.85 | 0.84 | 0.01 |
| | | | (0.09) |
| | 29 | 26 | 55 |
| Percent of Teachers Interacting with Students | 0.79 | 0.73 | 0.06 |
| | | | (0.12) |
| | 29 | 26 | 55 |
| Blackboards Utilized | 0.86 | 0.85 | 0.01 |
| | | | (0.11) |
| | 22 | 20 | 42 |
| *E. School Infrastructure* | | | |
| Infrastructure Index | 3.39 | 3.20 | 0.19 |
| | | | (0.30) |
| | 57 | 55 | 112 |
| Fstat(1,115) | | | 1.32 |
| p-value | | | (0.25) |

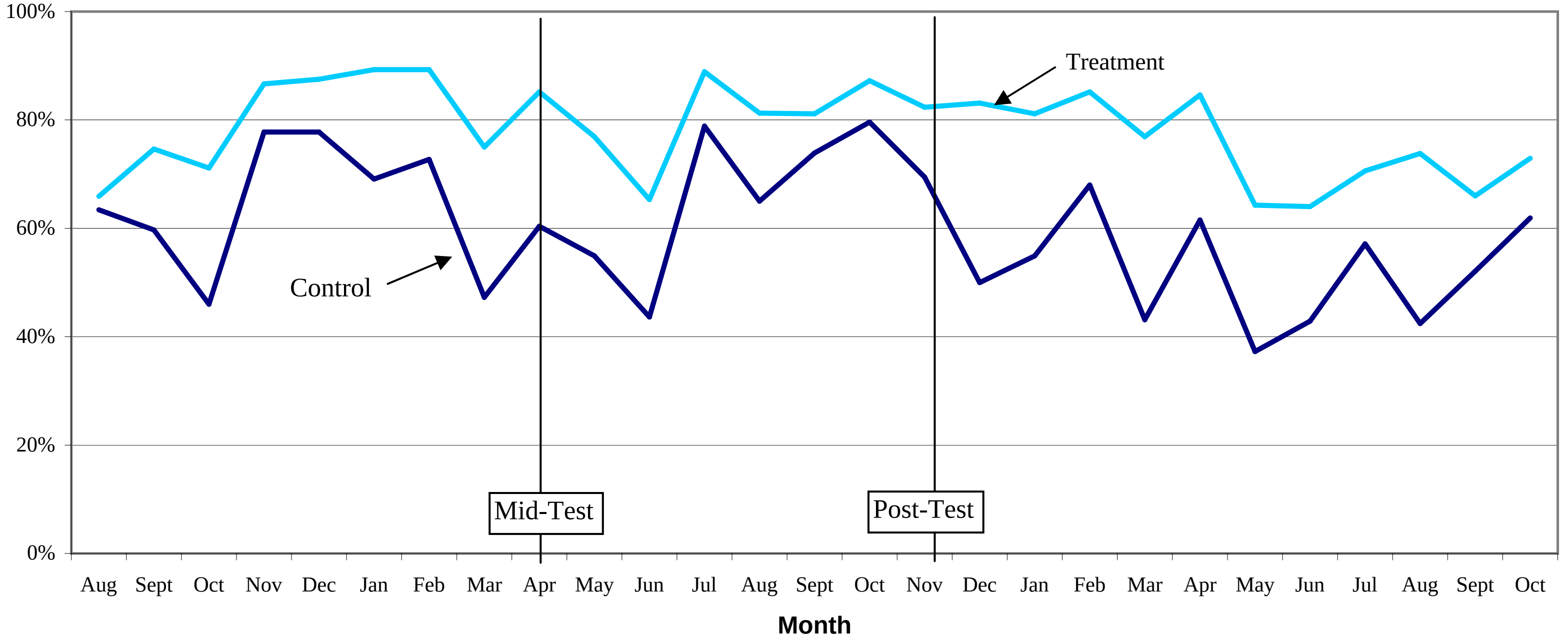
Notes: (1) Teacher Performance Measures from Random Checks only include schools that were open during the random check. (2) Infrastructure Index: 1-5 points, with one point given if the following school attribute is sufficient: Space for Children to Play, Physical Space for Children in Room, Lighting, Library, Floor Mats

**Table 2: Are Students Similar Prior To Program?**

| | Levels | | | Normalized by Control | | |
|---|---|---|---|---|---|---|
| | Treatment | Control | Difference | Treatment | Control | Difference |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | *A. Can the Child Write?* | | | | | |
| Took Written Exam | 0.17 | 0.19 | -0.02 | | | |
| | | | (0.04) | | | |
| | 1136 | 1094 | 2230 | | | |
| | *B. Oral Exam* | | | | | |
| Math Score on Oral Exam | 7.82 | 8.12 | -0.30 | -0.10 | 0.00 | -0.10 |
| | | | (0.27) | | | (0.09) |
| | 940 | 888 | 1828 | 940 | 888 | 1828 |
| Language Score on Oral Exam | 3.63 | 3.74 | -0.10 | -0.03 | 0.00 | -0.03 |
| | | | (0.30) | | | (0.08) |
| | 940 | 888 | 1828 | 940 | 888 | 1828 |
| Total Score on Oral Exam | 11.44 | 11.95 | -0.51 | -0.08 | 0.00 | -0.08 |
| | | | (0.48) | | | (0.07) |
| | 940 | 888 | 1828 | 940 | 888 | 1828 |
| | *C. Written Exam* | | | | | |
| Math Score on Written Exam | 8.62 | 7.98 | 0.64 | 0.23 | 0.00 | 0.23 |
| | | | (0.51) | | | (0.18) |
| | 196 | 206 | 402 | 196 | 206 | 402 |
| Language Score on Written Exam | 3.62 | 3.44 | 0.18 | 0.08 | 0.00 | 0.08 |
| | | | (0.46) | | | (0.20) |
| | 196 | 206 | 402 | 196 | 206 | 402 |
| Total Score on Written Exam | 12.17 | 11.41 | 0.76 | 0.16 | 0.00 | 0.16 |
| | | | (0.90) | | | (0.19) |
| | 196 | 206 | 402 | 196 | 206 | 402 |

Notes: (1) Children who could write were given a written exam. Children who could not write were given an oral exam. (2) Standard errors are clustered by school.

**Figure 2: Percentage of Schools Open during Random Checks**




Note: (1) The program began in Sept 2003. August only includes the 85 schools checked before announcement of program (August 25). September includes all random checks between August 26 through the end of September. (2) Child learning levels were assessed in a mid-test (April 2004) and a post-test (November 2004). After the post-test, the "official" evaluation period was ended. Random checks continued in both the treatment and control schools.

**Table 3: Teacher Attendance**

| Sept 2003-Oct 2005 | | | Difference Between Treatment and Control Schools | | |
|---|---|---|---|---|---|
| Treatment | Control | Diff | Until Mid-Test | Mid to Post Test | After Post Test |
| (1) | (2) | (3) | (4) | (5) | (6) |
| | | *A. All Teachers* | | | |
| 0.76 | 0.57 | 0.20 | 0.19 | 0.15 | 0.22 |
| | | (0.03) | (0.04) | (0.04) | (0.05) |
| 1461 | 1421 | 2882 | | | |
| | | *B. Teachers with Above Median Test Scores* | | | |
| 0.77 | 0.63 | 0.15 | 0.15 | 0.16 | 0.14 |
| | | (0.04) | (0.05) | (0.06) | (0.06) |
| 762 | 629 | 1391 | | | |
| | | *C. Teachers with Below Median Test Scores* | | | |
| 0.77 | 0.53 | 0.25 | 0.21 | 0.08 | 0.33 |
| | | (0.05) | (0.06) | (0.07) | (0.06) |
| 565 | 711 | 1276 | | | |

Notes: (1) Child learning levels were assessed in a mid-test (April 2004) and a post-test (November 2004). After the post-test, the "official" evaluation period was ended. Random checks continued in both the treatment and control schools. (2) Standard errors are clustered by school.

**Figure 3A: Impact of the Cameras**
**(out of at least 25 visits)**

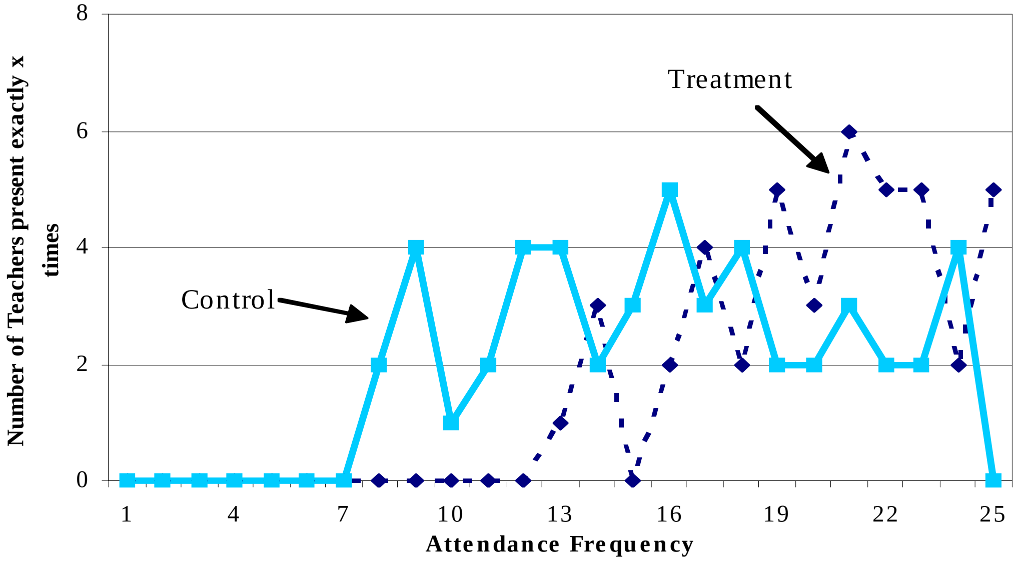
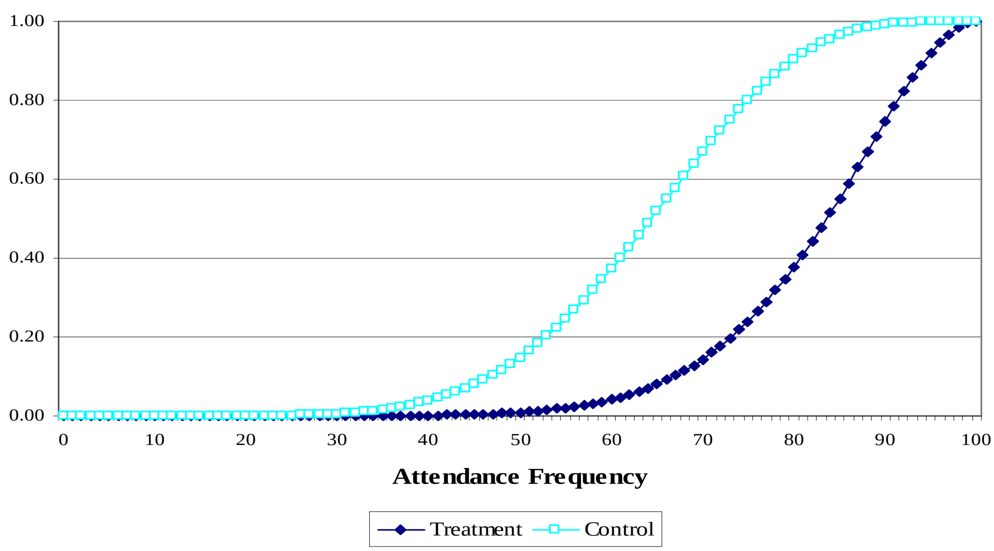



**Figure 3B: Teacher Attendence**




Note: Figure 3B is the estimated CDF of attendance, assuming that absense follows a beta-binomial distribution.

**Table 4: Comparing Random Checks to Photo Data for Treatment Schools**

| Scenario | Number | Percent of Total |
|---|---|---|
| *A. Possible Scenarios* | | |
| School Open and Valid Photos | 879 | 66% |
| School Open and Invalid Photos | 179 | 13% |
| School Closed and Valid Photos | 88 | 7% |
| School Closed and Invalid Photos | 191 | 14% |
| *B. Out of 179 where School is Open, the photos are invalid because….* | | |
| School not open for full 5 hours | 43 | 24% |
| Only one photo | 90 | 50% |
| Not enough Children | 36 | 20% |
| Instructor not in Photo | 9 | 5% |
| Don't Know | 1 | 1% |
| *C. Out of 88 where School is Closed and the photos are valid…..* | | |
| Random check completed after the school closed | 13 | 15% |
| Camera broke/excused meeting | 21 | 24% |
| Teacher left in the middle of the day | 54 | 61% |

**Figure 4: Difference in the Percent of Open Schools Between Treatment and Control, By Hour**

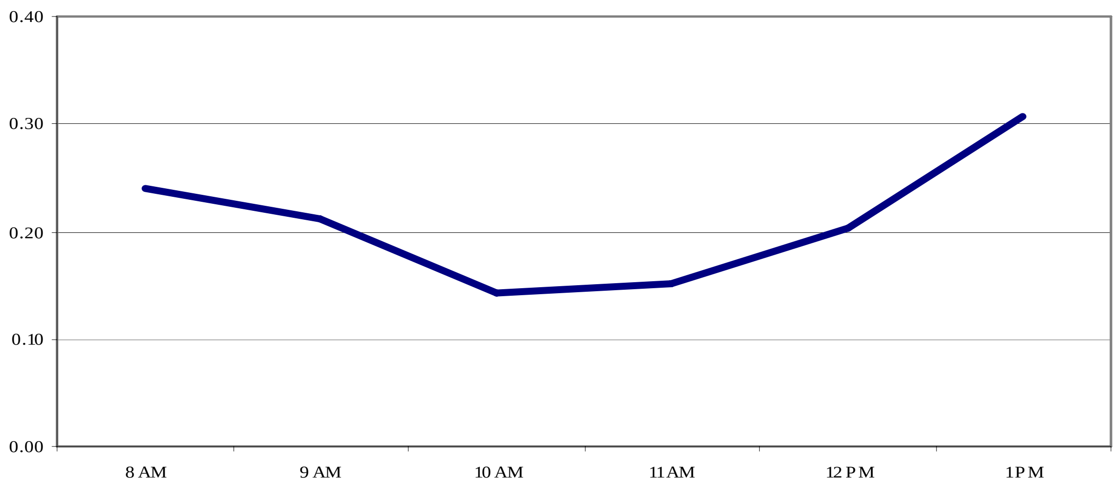


**Table 5: Teacher Performance**

| | Sept 2003-Oct 2005 | | | Difference Between Treatment and Control Schools | | |
|---|---|---|---|---|---|---|
| | Treatment | Control | Diff | Until Mid-Test | Mid to Post Test | After Post Test |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Percent of Children Sitting Within Classroom | 0.89 | 0.88 | 0.01 | 0.01 | 0.04 | -0.04 |
| | | | (0.01) | (0.02) | (0.03) | (0.03) |
| | 865 | 633 | 1498 | | | |
| Percent of Teachers Interacting with Students | 0.68 | 0.69 | -0.02 | -0.03 | 0.01 | 0.02 |
| | | | (0.03) | (0.02) | (0.02) | (0.03) |
| | 865 | 633 | 1498 | | | |
| Blackboards Utilized | 0.93 | 0.93 | 0.00 | -0.04 | 0.08 | -0.07 |
| | | | (0.01) | (0.04) | (0.05) | (0.05) |
| | 843 | 615 | 1458 | | | |

Notes: (1) Teacher Performance Measures from Random Checks only include schools that were open during the random check. (2) Standard errors are clustered by school.

**Table 6: Child Attendance**

| | Sept 03-Oct 05 | | | Difference Between Treatment and Control Schools | | |
|---|---|---|---|---|---|---|
| | Treatment | Control | Diff | Until Mid-Test | Mid to Post Test | After Post Test |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *A. Attendance Conditional on School Open* | | | | | | |
| Attendance of Students Present at Pre-Test Exam | 0.48 | 0.47 | 0.01 | 0.02 | 0.03 | 0.01 |
| | | | (0.03) | (0.03) | (0.04) | (0.03) |
| | 21495 | 14965 | 36460 | | | |
| Attendance for Children who did not leave NFE | 0.63 | 0.60 | 0.03 | 0.01 | 0.06 | 0.03 |
| | | | (0.03) | (0.03) | (0.04) | (0.03) |
| | 13475 | 10071 | 23546 | | | |
| *B. Attendance, by Student Learning Level at Program Start* | | | | | | |
| Took Oral Pre-Test | 0.49 | 0.46 | 0.03 | 0.03 | 0.06 | 0.03 |
| | (0.50) | (0.50) | (0.03) | (0.03) | (0.04) | (0.03) |
| | 17498 | 12143 | 29641 | | | |
| Took Written Pre-Test | 0.41 | 0.48 | -0.07 | -0.01 | -0.08 | -0.08 |
| | (0.49) | (0.50) | (0.05) | (0.05) | (0.06) | (0.05) |
| | 3997 | 2822 | 6819 | | | |
| *C. Instruction Time* | | | | | | |
| Presence for Students Present at Pre-Test Exam | 0.38 | 0.29 | 0.09 | 0.1 | 0.09 | 0.09 |
| | | | (0.03) | (0.03) | (0.04) | (0.03) |
| | 26906 | 24141 | 51047 | | | |
| Presence for Student who did not leave NFE | 0.51 | 0.38 | 0.13 | 0.1 | 0.13 | 0.15 |
| | | | (0.03) | (0.04) | (0.04) | (0.04) |
| | 16698 | 16020 | 32718 | | | |

Notes: (1) Standard errors are clustered at the level of the school. (2) Child attendance data collected during random check. (3) Pre-test exam determined child enrollment at the start of the program.

**Table 7: Descriptive Statistics for Mid Test and Post Test**

| | Mid Test | | | Post Test | | |
|---|---|---|---|---|---|---|
| | Treatment | Control | Difference | Treatment | Control | Difference |
| | *A. Attrition Process* | | | | | |
| Percent Attrition | 0.11 | 0.22 | -0.10 | 0.24 | 0.21 | 0.03 |
| | | | (0.05) | | | (0.04) |
| Difference in Percent Written of Pre-Test attriters-stayers | 0.01 | 0.03 | 0.02 | 0.06 | -0.03 | 0.10 |
| | | | (0.06) | | | (0.06) |
| Difference in Verbal Test of Pre-Test attriters-stayers | 0.05 | 0.08 | -0.03 | 0.02 | 0.12 | -0.10 |
| | | | (0.14) | | | (0.14) |
| Difference in Written Test of Pre-Test attriters-stayers | -0.41 | -0.23 | -0.18 | -0.19 | -0.13 | -0.06 |
| | | | (0.34) | | | (0.29) |
| | *B. Exam Score Means* | | | | | |
| Took Written | 0.36 | 0.33 | 0.03 | 0.61 | 0.57 | 0.04 |
| | | | (0.04) | | | (0.05) |
| Math | 0.14 | 0.00 | 0.14 | -0.08 | -0.24 | 0.16 |
| | | | (0.10) | | | (0.15) |
| Language | 0.14 | 0.00 | 0.14 | 1.71 | 1.60 | 0.11 |
| | | | (0.10) | | | (0.11) |
| Total | 0.14 | 0.00 | 0.14 | 0.35 | 0.24 | 0.12 |
| | | | (0.10) | | | (0.11) |

Notes: (1) Test Scores in Panel B are normalized by the mean of the mid-test control. (2) Standard Errors are clustered by school.

**Table 8: Estimation of Treatment Effects for the Mid- and Post-Test**

| Mid-Test | | | | Post-Test | | | |
|---|---|---|---|---|---|---|---|
| Took Written | Math | Lang | Total | Took Written | Math | Lang | Total |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | *A. All Children* | | | | |
| 0.04 | 0.15 | 0.16 | 0.17 | 0.06 | 0.21 | 0.16 | 0.17 |
| (0.03) | (0.07) | (0.06) | (0.06) | (0.04) | (0.12) | (0.08) | (0.09) |
| 1893 | 1893 | 1893 | 1893 | 1760 | 1760 | 1760 | 1760 |
| | | | *B. With Controls* | | | | |
| 0.04 | 0.13 | 0.14 | 0.14 | 0.06 | 0.18 | 0.14 | 0.15 |
| (0.03) | (0.07) | (0.06) | (0.06) | (0.04) | (0.13) | (0.08) | (0.09) |
| 1752 | 1752 | 1752 | 1752 | 1624 | 1624 | 1624 | 1624 |
| | | | *C. Took Pre-Test Oral* | | | | |
| | 0.14 | 0.13 | 0.15 | | 0.2 | 0.13 | 0.16 |
| | (0.08) | (0.06) | (0.07) | | (0.14) | (0.09) | (0.10) |
| | 1550 | 1550 | 1550 | | 1454 | 1454 | 1454 |
| | | | *D. Took Pre-Test Written* | | | | |
| | 0.19 | 0.28 | 0.25 | | 0.28 | 0.28 | 0.25 |
| | (0.12) | (0.11) | (0.11) | | (0.18) | (0.11) | (0.12) |
| | 343 | 343 | 343 | | 306 | 306 | 306 |
| | | | *E. Girls* | | | | |
| 0.07 | 0.18 | 0.18 | 0.2 | 0.07 | 0.22 | 0.17 | 0.18 |
| (0.03) | (0.07) | (0.07) | (0.07) | (0.05) | (0.12) | (0.09) | (0.09) |
| 891 | 891 | 891 | 891 | 821 | 821 | 821 | 821 |
| | | | *F. Boys* | | | | |
| 0.02 | 0.12 | 0.14 | 0.14 | 0.05 | 0.19 | 0.16 | 0.16 |
| (0.04) | (0.09) | (0.07) | (0.07) | (0.04) | (0.15) | (0.10) | (0.10) |
| 988 | 988 | 988 | 988 | 929 | 929 | 929 | 929 |

Notes: (1) The table presents the coefficient estimate of being in a treated school on the sum of a child's score on the oral and written exams. All regressions include controls for the child's learning levels prior to the program. (2) The mid and post test scores normalized by mid test control group. (3) Controls in Row B include Block, Teacher Test Scores, and Infrastructure Index. (4) Standard errors are clustered by school.

**Table 9: Dropouts and Movement into Government Schools**

| | Treatment | Control | Diff |
|---|---|---|---|
| | (1) | (2) | (3) |
| Child Left NFE | 0.38 | 0.34 | 0.03 |
| | | | (0.04) |
| Child Enrolled in Government School | 0.20 | 0.14 | 0.06 |
| | | | (0.03) |
| Child Dropped Out of School | 0.17 | 0.21 | -0.03 |
| | | | (0.03) |
| N | 1136 | 1061 | 2197 |

Notes: (1) Standard errors are clustered at the level of the school. (2) Dropouts are defined as being absent for the last 5 random checks in which a school was found open.

**Table 10: Does the Random Check Predict Test Scores?**

| Method: | OLS | OLS | OLS | 2SLS |
|---|---|---|---|---|
| Sample: | Control Schools | Treatment Schools | Treatment Schools | All Schools |
| Data: | Random Check | Random Check | Photographs | Random Check |
| | (1) | (2) | (3) | (4) |
| | *A. Mid-test (Sept 03-April 04)* | | | |
| Took Written | 0.02 | 0.28 | 0.36 | 0.26 |
| | (0.10) | (0.08) | (0.11) | (0.19) |
| Total Score | 0.20 | 0.39 | 0.87 | 1.07 |
| | (0.19) | (0.21) | (0.22) | (0.43) |
| N | 878 | 1015 | 1015 | 1893 |
| | *B. Post-test (Sept 03 -Oct 04)* | | | |
| Took Written | 0.24 | 0.51 | 0.59 | 0.33 |
| | (0.16) | (0.15) | (0.20) | (0.22) |
| Total Score | 0.58 | 1.17 | 0.98 | 0.97 |
| | (0.35) | (0.36) | (0.53) | (0.47) |
| N | 883 | 877 | 877 | 1760 |

Notes: (1) The table presents the coefficient estimate of the teacher's attendance on the sum of a child's score on the oral and written exams. All regressions include controls for the child's learning levels prior to the program. (2) The mid and post test scores normalized by mid test control group. (3) Standard errors are clustered by school.

**Table 11: Interactions with Teacher Skills and Performance**

| | Teacher Skills | | | | |
|---|---|---|---|---|---|
| | Test Scores | Highest Grade Completed | Infrastructure Index | Good Teacher Behavior | Good Student Behavior |
| | (1) | (2) | (3) | (4) | (5) |
| | | | *A. Mid Test Scores* | | |
| Took Written | 0.00 | 0.03 | 0.00 | 0.08 | 0.11 |
| | (0.00) | (0.01) | (0.02) | (0.06) | (0.06) |
| Total Score | 0.01 | 0.06 | 0.01 | 0.07 | 0.17 |
| | (0.01) | (0.03) | (0.04) | (0.12) | (0.12) |
| | | | *B. Post Test Scores* | | |
| Took Written | 0.00 | 0.01 | 0.04 | 0.08 | -0.08 |
| | (0.00) | (0.02) | (0.03) | (0.08) | (0.08) |
| Total Score | 0.01 | 0.02 | -0.05 | 0.14 | -0.08 |
| | (0.01) | (0.04) | (0.04) | (0.18) | (0.18) |

Notes: (1) Standard Errors are clustered by school. (2) Teacher observations were conducted in September thru October 2004. (3) Teacher Test Scores and Highest Grade Completed are in levels. The Infrastructure Index is the same as in Table 1. The Teacher and Student Behaviors are measured as being above the median in terms of each behavior.

**Table 12: Cost of Program Per Center over 12 Month Period**

| Item | Cost |
|---|---|
| *A. Camera Cost* | |
| Camera Cost[1] | 1133 |
| Film Cost | 1392 |
| Battery Cost | 552 |
| Photo Development and Printing: | 1852 |
| *B. Salaries* | |
| Teacher Salaries[2] | 0 |
| Labor Cost to Run Program[3] | 450 |
| Total Costs to Run Program | 5379 |

Notes: (1) Assumes cameras last 3 years (2) Average Teacher Salary is Rs1000 under program. In the absence of the program, it would be Rs1000. (3) It takes approximately 50 man hours to process 115 schools per month. Assume a staff worker being paid Rs 10,000 per month and works a 40 hour week. Thus, it takes 1/2 hour of labor at Rs37.5 to complete one center per month.