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Abstract

Governments of developing economies are increasingly adopting computer-assisted learning (CAL) to supplement traditional learning strategies to raise achievement levels in primary schools. In India, as in many other developing economies, the Government believes that the promotion of CAL can help reduce persistent problems of low achievement, high drop out rates and low attendance which plague the elementary school system, particularly in rural areas. However, there are few studies which do so in the context of the rural areas of developing economies, which are characterized by conditions that are likely to minimize the effectiveness of computer aided learning strategies. Yet most governments still adopt CAL in rural areas. Little is known about whether CAL strategies in rural areas are effective in enhancing student achievement. This paper provides empirical evidence on one such program, a CAL program introduced by a non-governmental organization, the Byrraju Foundation, in cooperation with the state government of the southern state of Andhra Pradesh, in India. Our evaluation is based on a two year panel of school-level data merged with child-level data on test scores, with multiple observations on test scores for each child within each year. We find that the CAL program significantly improved average test scores for mathematics, but not for language.

Keywords: Computer-assisted learning (CAL), primary school, developing economies, rural area, panel data, South India.

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Improving the quality of rural primary schools: An evaluation of a Computer Aided Learning program in South India

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1. Introduction

In their efforts to raise achievement levels in primary schools, Governments of developing economies are increasingly adopting computer-assisted learning (CAL) to supplement traditional learning strategies. In India, as in many other developing economies, the Government believes that the promotion of CAL can help reduce persistent problems of low achievement, high drop out rates and low attendance which plague the elementary school system, particularly in rural areas. While a number of studies evaluate CAL programs (Angrist and Lavy 2002, Banerjee, Cole, Duflo and Linden 2005), there are few studies which do so in the context of the rural areas of developing economies. As described later in this paper, these areas are characterized by conditions which are likely to minimize the effectiveness of computer aided learning strategies. Yet, fear about a growing gap in schooling quality across urban and rural areas has driven most governments to adopt CAL in rural areas. Little is known about whether CAL strategies in rural areas are effective in enhancing student achievement. This paper provides empirical evidence on one such program, a CAL program introduced by a non-governmental organization, the Byrraju Foundation, in cooperation with the state government of the southern state of Andhra Pradesh, in India.

Our evaluation is based on a two year panel of school-level data merged with child-level data on test scores, with multiple observations on test scores for each child within each year. Credible estimates of the effect of the program are enabled by two special features of this data set. First, we collected data on schools which received the program (“treatment” schools) as well as those that did not (“control” schools). The value of a control sample, of course, depends on how well it approximates what outcomes in the treatment schools would have been in the absence of the program exploit. In constructing our control sample, we draw on the phasing in of program benefits to the set of targeted schools over a two year period. Our treatment sample is randomly drawn from the set of schools which received program benefits in the first year, while the control sample is similarly constructed from those schools which received benefits in the second year.

Though all schools targeted for the program were drawn from a narrow geographic area and were similar in terms of initial school conditions and the socio-economic profile of the villages in which they were located, the phasing of benefits across the two years of the program was not randomly done, but instead was determined by the speed with which local communities were able to raise the matching funds required for participation in the program.[1] All targeted schools did raise the necessary funds within a relatively narrow period (one year), so it is perhaps not surprising that our analysis reveals no statistically significant difference in base-line (pre-program) average schooling performance across the set of treatment and control schools. We nevertheless further control for any bias introduced by non-random phasing through a difference-in-difference methodology, which uses data we collected on pre-program outcomes to compare the *change* in schooling performance in treatment schools to that in control schools.

A unique feature of this study is that in addition to collecting student-level data on test scores for two years of primary school (2004-05 and 2005-06), we also followed the set of students in treatment and control schools who were in fifth grade in 2005-06

[1] Details regarding the implementation of the program are deferred until the next section.

through the transition into upper primary school (grade 6), collecting data on multiple test scores in the 6^{th} grade. This allows us to do two things. First, it enables us to address the possibility that teachers in control schools artificially inflate test scores, knowing that the expectation is that the additional investment in schools should improve schooling achievement. Second, the availability of 6^{th} grade test scores enables us to assess whether programs intended to improve the quality of primary school have effects which persist through to higher levels of schooling. In many countries, including India, financial resources intended to improve school quality are disproportionately spent on higher levels of schools. If performance at these higher levels is, however, importantly influenced by outcomes in primary school, a more effective policy may require a redistribution of resources towards primary schools. Our study is one of the few that we know of which provides evidence of the dependence of performance in higher levels of schooling on the quality of primary schools.

We find that the CAL program significantly improved average test scores for mathematics, but not for language. For mathematics, these gains appear to be equally distributed across students of differing levels of ability, as measured by their place in the distribution of baseline test scores. In contrast, though the effect of CAL on average language test scores is statistically insignificant, there are significant gains for students in the top quartile of the distribution of language ability. We also find that improvements are positively correlated with cohort size: for both subjects, the benefits of CAL increase with cohort size, perhaps because smaller cohorts must necessarily be combined with other grades in computer classrooms.[2] This is likely to reduce the effectiveness of the grade-specific instructional material used for CAL. Finally, our empirical analysis finds that primary school outcomes have a significant effect on test scores in middle school, suggesting that policy efforts which improve the quality of primary schools can have long-lasting effects on schooling attainment.

The remainder of this paper is structured as follows. Section 2 briefly describes the Byrraju Program. Section 3 describes the data and our sample. Section 4 outlines empirical details, while the main results are presented in Section 5. The last section concludes.

2. The Model School Program

The Byrraju Foundation’s Model School Program was initiated in the Fall of 2004. In planning for the program, the Foundation first identified 230 schools which were targeted to receive the package of benefits. These “model” schools were located in villages in which the Foundation had a strong presence, primarily in the districts of West and East Godavari in the Southern state of Andhra Pradesh. Village populations ranged from 3,000 to 5,000, with most villages being divided into 4 to 5 sub-habitations. Because the Government of India provides a school to each sub-habitation with a population of more than 500,[3] most program villages have multiple schools. The foundation, in all cases, provided the package of inputs to the “main village” school, generally the largest government school in the village. Because of the relatively small geographical area

[2] Jacob, Kochar and Reddy (2008) provide empirical evidence on the negative effects of multigrade teaching.
[3] Scheduled caste and tribe habitations are provided a school if their population exceeds 200.

within which the program was administered, and the fact that targeted schools were all located within the main village, it is not surprising that the schooling communities in the set of treatment and control schools are comparable in terms of socio-economic backgrounds, a fact that we later verify statistically.

The program provided a computer, a TV/DVD, a water cooler and fans to selected primary schools. The cost of the intervention was Rs. 40,000 per school. Of this, the Foundation provided Rs. 10,000, requiring the village community to raise an additional Rs. 10,000. Matching funds, of Rs. 20,000, were then obtained from the Central Government. The Foundation was instrumental in keeping costs within this amount. It negotiated with suppliers, and was generally able to get the necessary equipment at a substantial discount over market rates. The Foundation also entered into agreements with other NGOs for the provision of instructional software, etc. Finally, the Foundation provided two days of computer training for teachers.

Computers were used as a learning aid. In periods of approximately 40 to 45 minutes duration, classes gathered in front of the computer and an instructional grade-specific CD in language or mathematics was played. A few students were occasionally called on to operate the computer. The Foundation assumed that each class (grade) would receive instruction for 3 to 5 computer periods in a week. In actual practice, the amount of time available for computer use depended on the availability of electricity. In most schools, electricity was only available from 11 a.m. to 3 p.m, restricting computer learning time.[4] Electricity charges are paid by the State Government (through the block office, also known as the “Mandal” office).

Each computer class comprised approximately 40 students. If a particular age cohort of students is small, it is typically combined with students from other grades. For example, in one of the smaller schools, grades 1-3 combined to generate a class size of 30 students, who then watched the computer program together. Grades 4 and 5 were combined to form a second class. Each computer period was of approximately 40-45 minutes duration.

All schools which eventually received the package of benefits were identified at the start of the program. However, for administrative purposes, the program was phased in over the set of identified schools: approximately 190 schools received the program in 2004-05 with the remaining schools receiving benefits in December 2005-06. The selection of early recipients was not randomly done; those schools which were the first to raise the necessary matching funds received the package of benefits in 2004-05. However, all targeted schools raised the required funds within the space of a year, so that the program was, indeed, initiated in remaining schools in December 2005-06.

3. The School System and Survey Data

3.1 Survey Data

Our study is based on data for 15 treatment schools and 15 controls schools, with treatment schools being randomly selected from the population of schools which were phased into the program in the first year and control schools similarly selected from

[4] School hours area generally from 9:15 to 3:25, with a one hour lunch break from 12:10 to 1:10.

schools who received the package in the second year. As previously noted, all control schools did subsequently receive program benefits. For these schools, the program was introduced between December 2005 and April 2006, whereas treatment schools received benefits between the months of August and December 2004. We therefore collected information on school tests conducted in August, December and April of the 2004-05 school year, and August and December of the 2005-06 school year. The August 2004 results serve as a baseline.

Since we intended to collect information on test scores over a two year period, we restricted our attention to students who were in grades 2 through 4 in the first survey year.[5] For these students, in addition to test scores, we have basic information such as age and gender. We also asked if students were enrolled in the school in the year *prior* to the initiation of the program, 2003-04. This is important, since the initiation of the program could well have caused an increase in enrollment in treatment schools over control schools. Indeed, field investigators reported that there was some movement from private schools back into government schools. If such students were of higher ability, then this would itself increase average school scores relative to the control sample. Our analysis is therefore restricted to students who were also enrolled in the school in 2003-04.

3.2 The primary school system

Our evaluation of the CAL program is based on the results of school tests required and administered by the state government. In Andhra Pradesh, the primary school system is administered through a multi-tier hierarchical structure, with direct responsibility for

[5] Though some schools provided computer instruction to first graders, the package was intended for grades 2 and up.

the oversight of village schools falling on an intermediate office located at the level of the block or Mandal. This office is in turn overseen by offices at the district and state level. All schools are required to administer 3 tests per year in language (Telugu) and mathematics, a pre-test in August, a half-yearly test in December and a final test in April. Each test covers the material taught (cumulatively) until the test date. The tests are written by Mandal authorities, so that the same test is administered to all village schools which come under the jurisdiction of a particular Mandal office. Though the tests are not school-specific, the grading of the tests is done by school teachers. The test results are sent back to the Mandal office, where they are recorded. The Mandal office bears the responsibility for monitoring and verifying test results. This is done by random visits by Mandal officers to schools at the time of testing, as well as through checks on the results provided by the school.

Because different tests are administered at each of the testing dates, the results are not comparable across tests (that is one cannot expect that results will improve over time) within a year, and particularly across years. That is, the level of difficulty of the test may well change from year to year. Indeed, in our sample, test scores were lower in the second survey year, relative to the first, across all schools.

3.3 The Upper Primary School System

Through a recent set of school reforms in the sate of Andhra Pradesh, grades 6-8, formerly separately taught in upper primary schools, are now, in a majority of cases, combined with higher grades in a single high school. This was the case for all schools in our sample with the exception of three instances of separate upper primary schools. Each upper level school draws in students from primary schools falling in its “catchment” area. Typically, a high school serves 4 to 5 primary schools. Unless households migrate, primary level students from government schools who continue on with schooling normally go to their assigned government high school; there is little movement from government to private schools at this level,[6] and it is extremely rare to see a student attend a different middle school from the one which serves its primary school. We identified the upper primary school associated with each of the schools in our primary school survey, and collected data for all 6th grade students in this school. In addition to test scores, we were able to collect information on the primary school attended by each student. We matched information on students and schools to our primary school survey, ensuring that we were able to correctly identify all students who had been in our original survey, as well as the school (treatment or control) which they had attended. By the time they reached 6th grade, students in our treatment group had received two years of CAL, while students in the control group had received at the most 4 months of CAL.[7]

The structure of school tests in higher grades remains the same as that in primary grades. Specifically, all students must take initial (August), half-yearly (December) and final (April) tests. We collected test scores for 6th grade students for all three tests. As in the primary school level, tests are written by a central body, this time at the district level, but graded by teachers in the school in question.

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[6] More students attend private primary schools than do private middle schools. Because of the relatively low costs of establishing a primary school, there are a large number of private primary schools, but relatively few private middle schools.

[7] In all control schools, the model school package was introduced only after December 2005-06. The school year ends in April.

3.4 Summary Statistics for the primary school sample

Table 1 provides summary statistics on treatment and control schools. The data confirm that the two sets of schools were roughly similar in important attributes such as the school size, number of teachers, etc. Average school size (based on enrollments in 2003-04) in both sets of schools is approximately equal (200 in model schools and 192 in treatment schools) and relatively small, as is the case in much of rural India. Average class size is 40 in treatment schools and 44 in control schools. Control schools reported that electricity was available for approximately 3 1/3 hours each day, while treatment schools had electricity for 4 hours a day.

4. Methodology

We evaluate the effect of CAL on schooling achievement, as measured by scores in language and mathematics tests. Though it is reasonable to expect any effect on learning to be a consequence of the introduction of computers, we cannot separately identify the distinct effects of each component of the model school package.

4.1 Difference estimates

We start our analysis by first establishing the comparability of base-line test scores in treatment and control schools. The results are reported in the first row of Table 2, separately for language (Telugu) and mathematics. They reveal that average base-line tests scores in the two subjects across treatment and control schools are not statistically different.

As a consequence, our evaluation of the program starts with a simple comparison of mean test scores across different test dates for treatment and control schools. If the schools are similar at the date of initiation of the program (as the data confirm), then the difference in test scores at any given date provides evidence on whether CAL enhanced learning.

4.2 Difference in difference estimates based on regression estimates

It, however, remains possible that schools differed in other ways which may also affect learning. The data in table 1, while confirming the approximate similarity of treatment and control schools in most aspects, also reveal differences, notably in the proportion of scheduled caste and tribe students in the school. To control for any initial differences across the set of treatment and control schools, we also implement a "difference-in-difference" methodology, comparing achievement in both sets of schools over time. We do this by pooling data over the last four rounds of testing (December 2004, April 2005, August 2005, December 2005), and testing whether students in treatment schools performed better, on average, in the last three rounds of testing relative to scores in December 2004. By utilizing December 2004 as our benchmark, we are able to implement a value added approach, which conditions on mean base-line test scores in the school in August 2004. This approach thus further conditions on any initial differences in school quality, and considerably improves the efficiency of our estimates.

Let d_t, t=2 to 5, be a set of dummy variables which take the value 1 if the test was implemented in the second to fifth round, 0 otherwise. The regression we run takes the following form:

$$(1) \qquad Y_{ist} = \alpha_o + \alpha_1 \text{mod} + \sum_{t=3}^{5} (\alpha_{2t} \text{mod} * d_t + \alpha_{3t} d_t) + \alpha_4 X_{ist} + \alpha_5 S_{st} + u_{ist}$$

The regression format allows us to introduce controls for other covariates which could influence schooling. We introduce a set of child level controls (X_{ist}), specifically, age, squared age, caste (scheduled caste and tribe, other backward caste or higher caste) and gender. We also include school level controls (S_{st}): a cubic in total school enrollment, and dummy variables which indicate the number of classrooms in the school (2 to more than 6, with 1 room schools serving as the comparison). We do not include variables, such as the number of teachers, which is determined purely on the basis of student enrollment.

Since we expect a strong correlation between test scores within a school and within different cohorts in the school, all standard errors are clustered at the school-cohort level.[8] As previously noted, all regressions are run on the sample of students who were in the school in 2003-04, the year before the initiation of the program, to control for the possibility of bias due to the movement of students into the school following the initiation of computer learning.

We report results from the specification of equation (1), but also from simpler specifications which allow for program effects only in the second year of the program. In so doing, we report two specifications – one which allows results to differ between

[8] Teachers are routinely assigned to different grades, and frequently transferred, so there is less reason to expect correlations within a fixed grade.

August 2005 and December 2005, and a simpler specification which pools results from these two test dates.

4.3 Testing effects of the program for students differentiated by initial ability

An important concern is that CAL benefits only the most able students, with students who are academically weak not being able to draw much out of the program. This is a particular concern since teaching takes the form of students following the instructional CD. If the level of instruction, even when it is grade-specific, is above the level of the weakest students in the class, they may well gain little from the program.

To test whether the ability level of the student affects the benefits from CAL, we divide students into four school-by-grade ability levels, classifying them by their test results in the baseline August 2004 tests. Our baseline specification for this regression is the simpler form of (1), when we test whether the program affects scores in year 2 relative to year 1, pooling data for the two year 2 exams, and comparing them to the test results in December and April of year 1. We re-run this regression, but additionally allow for interactions with ability level. For completeness, we interact the model school dummy (*mod*) with dummy variables for each ability level, and do the same for the year 2 dummy variable.

4.4 Testing dependence on cohort-size

Learning from CAL programs, may also be affected by cohort size. As previously noted, instructional CDs are grade-specific. However, if the size of a particular age cohort of students is small (less than 40), it is likely to be combined with students of other grades, thereby reducing the effectiveness of grade-specific instructional CDs.

To address these concerns, we report results from an additional regression which tests whether cohort-size affects the benefits of the program. For this purpose, we use data on the initial size of the cohort in question, as measured by their enrollment size in 2003, one year prior to the initiation of the model school program. As previously noted, cohort size differs from classrooms size, as several grades may be combined in one classroom in a multi-grade setting. In this setting, classroom size is endogenous, in part a function of cohort size. We focus on cohort size, asking if students are disadvantaged if there incoming cohort of students is relatively small.

As in the set of regressions described above, we use a difference-in-difference specification, which examines the change in test scores between second and first year test scores in treatment schools relative to control schools. The coefficient on *model*year2* measures the effect of the program, while the coefficient on *model*year 2*cohort size* examines if any benefits vary with cohort size.

4.5 Results from the 6th grade sample

The availability of data on 6th grade test scores for a sample of students allows us to test the medium term effects of quality improvements in primary schools, but also enables us to address an additional concern: the possibility that primary school teachers in treatment schools may artificially inflate test scores, knowing that the expectation is that the program will lead to an improvement in schooling achievement. By following our original set of students through their transition to upper primary schools, we obtain a set of test results which are free from this bias. In upper primary schools, students who attended a model primary school are combined in classes with students who did not, and there is little possibility that any positive effect of attendance in a model primary school which shows up in 6th grade test scores could be the consequence of manipulation of these scores by upper primary school teachers.

We assess the effect primary school quality on student scores in 6th grade through the following regression:

$$(2) \qquad Y_{ist} = \beta_o + \beta_1 Y_i^5 + \beta_2 X_{ist} + \beta_3 S_{st} + \sum_{t=2}^{3} \gamma_t d_t + u_{ist}$$

In (2), Y_i^5 represents student i's test scores from 5th grade (the last year of primary school), while, as before, the vector X is a set of student characteristics (age, squared age and dummy variables for caste) while S represents attributes of the high school in which the student is currently enrolled. As before, it is possible that test scores have a school-specific component, since they are graded by teachers within the school. To allow for this, our dependent variable is the individual's standardized test score, relative to the mean of other 6th grade students in the high school. We therefore test whether students from model primary schools do better relative to other students in their grade, as compared to students from our control school sample.

The regression is run on a data set which pools individual test scores for students in 6th grade over the three academic quarters (the initial test in August, the second test in December and the final test in April) of 2006-07. 6th grade students include students from

Byrraju Foundation model schools as well as from our sample of control schools, but also include students from other neighboring primary schools which fall within the "covered" area of the high school in question. Because these additional primary schools may be of different quality from those selected for the Model School program, we restrict our attention to students who were in either our treatment or control samples for the primary school survey.

The high school attributes we include in the regression are total enrollments, total enrollments in 6^{th} grade, and the proportion of scheduled caste and tribe students in the school and in 6^{th} grade. The regression also includes a set of dummy variables, d_t, which record the quarter in which the test was administered ($d_2=1$ if the test was the December test, while $d_3=1$ if the test was the April test). Because test scores are likely to be correlated within a high school, all standard errors are clustered by high school.

We treat the student's 5^{th} grade test scores as endogenous, instrumenting them with lagged values of test scores from our August 2004 benchmark survey and the indicator variable for whether the school was a Byrraju Foundation model school. The first stage regression therefore repeats our previous test of whether the model school environment improves student performance over their benchmark test scores. In conjunction with the second stage results, we can therefore address the question of whether investments in primary school improve student performance in higher levels of schooling.

A primary concern in this analysis is the relatively high drop out rate between primary and upper primary grades. Since our regression is run only on those primary school students who continue on to 6^{th} grade, the estimates of the effect of 5^{th} grade tests scores on 6^{th} grade achievement combine any direct effect with their effect through the probability of continuing school through 6^{th} grade. Since the very same variables which control achievement in any grade are likely to influence the decision to continue from one grade to the next, it is difficult to control for selection except through functional form assumptions. We follow an alternative methodology, restricting the regression sample to students in the top half of the ability distribution, as judged by their scores in the August 2004 baseline survey, and to students who are not members of either scheduled castes or other backward castes. Drop out rates are significantly lower for this sample, suggesting that any bias due to sample selection is likely to be minimal.

Table 2 reports the probability of a student continuing through from 5^{th} grade to 6^{th} grade, by caste and aptitude level. While these rates are over 50% for all groups, scheduled castes and students at the bottom of the academic distribution have a higher probability of dropping out after 5^{th} grade. For members of upper castes, continuation rates increase from approximately 62% for students in the bottom quartile of the academic distribution to 85% to students in the top quartile. For members of scheduled castes and tribes, continuation probabilities are 53% and 65% for students in the lowest quartile of the academic distr4ibution in mathematics and telugu respectively, and there is less variation by aptitude level: Continuation probabilities for scheduled castes are relatively low, even for students at the top of the academic distribution.

5. Results

5.1 Results from Simple Difference in Means

The results from a simple comparison of mean test scores across test dates are reported in table 2. As previously noted, there is no significant difference between treatment and control schools in either mathematics or language tests in August 2004, justifying the comparison of means as a method of evaluation.

Test scores for treatment scores are significantly higher for treatment scores relative to control schools commencing from December 2004, with the mean difference tending to increase over time. These results suggest a significant effect of the program on math learning.

However, the effects on language are far less. Test scores in treatment schools are statistically higher than those in control schools only for the August 2005 tests. In all other cases, there is no statistically significant difference in results.

In order to examine differences in results across the distribution of test scores, we examine the distribution function of test scores at different dates (August 2004, December 2004, August 2005 and December 2005) in figures 1 (mathematics) and figure 2 (telugu). For both sets of tests, the distribution function for treatment and control schools are roughly similar in the baseline test, August 2004. Math tests scores show a divergence in the distribution, with a reduced probability of low scores in treatment schools, starting from December 2004 and in every subsequent round of testing. In comparison, the distribution function for language test scores in treatment schools closely replicates that in control schools across all rounds of testing.

5.2 Results from regression based difference-in-difference specifications

Table 4 reports results based on equation (1), which compares the *improvement* in test scores in treatment schools relative to control schools, thereby eliminating any initial time-invariant differences in schools along characteristics which may directly affect schooling. The regression format also enables controls for observed school characteristics, including the bench-mark academic quality of students (from August 2004 tests).

The first regression for both mathematics and language compares test scores relative to those observed in December 2004. For mathematics, the results confirm that test scores are higher in the second year of the survey, with the coefficient on the interaction of the model school dummy (*model*) with the dummy variables for test 4 (August 2005) and test 5 (December 2005) being statistically significant at a 5% level of significance. The regressions also generate a positive effect of the model school program on language test scores in both quarters of the second year. However, the magnitude of these effects is small, relative to those for mathematics, and the coefficients are not statistically significant. The results confirm the evidence from a simple comparison of means: CAL in this context appears to have significantly advanced the learning of mathematics, but not language.

The second regression for both mathematics and languages pools the test scores in the second year of the survey, implementing a simpler regression which tests whether test results improved in the second year, relative to the first year. F tests confirm the validity of this simplification, and reveal, again, that test scores by the second year of the program improved significantly for mathematics, but not for language.

5.3 *Robustness check: Are the results biased by sample attrition?*

As noted above, there is a relatively high drop out rate from primary schools, and though this rate is highest in the transition from primary to upper primary schools, there is also significant drop out upon the completion of each year of primary school. This raises the possibility that the results could be a consequence of different patterns of sample attrition across model and control schools. For example, if those who drop out in model schools are amongst the worst students while drop outs from treatment schools are randomly drawn from the distribution of ability this would generate higher average achievement in the 2nd year in model schools relative to control schools.

To test whether this is the case, we compare mean baseline test scores (from the August 2004 tests) for students who remained in the schools in our second survey year. For mathematics, there is no statistical difference in these test scores: The average baseline test score is 63.7 in treatment schools and 63.1 in control schools. The F test for a difference in these scores yields a statistic of 0.81 (Probability >F=0.37). For language, baseline test scores for those who remain in schools in the 2nd survey year are 62.8 for model school students and 61.4 for students in control schools. The difference in this case is statistically significant at the 10% level (F statistic=2.85, Probability >F=0.092).

These tests suggest that the significant effects of the model school program in mathematics cannot be a consequence of differences in attrition rates across treatment and control schools. For language, since students who continue in the second year of the program in model schools appear to be of higher initial ability, this would generate a positive bias in estimates of the effect of model schools; the true effect of the program would be *less* than our estimates. But, since our estimates suggest an insignificant effect of the program on language, these results do not overturn the conclusion that the model school program significantly affects mathematics learning, but not that of language.

5.4 *Differences across aptitude groups*

Table 5 reports results from regression which allow the effects of the program in year 2 to vary across four different aptitude groups. The coefficient on the set of variables which interact the model school dummy in the second year with dummy variables for the child's location in the academic distribution (*model*year 2*aptitude level)* reveal whether the effect of the program varies by the child's initial ability level.

For mathematics, the model school program significantly affects learning by students in the lowest quartile of the baseline achievement distribution, as well as students in the top two quartiles. An F test for significant differences across the four quartiles rejects this hypothesis (F(3,87)=0.93, probability >F=0.43), suggesting that the benefits of CAL in mathematics are equally distributed across all students. The regression results for language test scores find, however, that though there are no statistically significant effects for students in the bottom three quartiles of the achievement distribution, students from the top quartile *do* benefit from the program. Thus, though the average effect of the model school program on language learning is not statistically significant, there are gains for the brightest students.

5.5 *Results by cohort size*

Table 6 reports results from regressions which allow the effects of the model school package to vary by cohort size. The results reveal that there is a significant cohort size effects. The coefficient on the interacted term *model*year 2* is now negative, while that on the term *model*year 2*class size* is positive and statistically significant at conventional levels, for both mathematics and language. This suggests that the benefits of the model school program are only realized if the cohort size exceeds a minimum level (the estimates suggest that this minimum is 33 for mathematics and 37 for language). Moreover, the benefits of the program increase with cohort sizes above this minimum level. The results thus confirm that, for small cohorts who have to be combined with other cohorts in multi-grade classrooms, computer learning has insignificant effects on schooling achievement.

5.6 Results from the 6th grade sample

Table 7 reports results from regressions on 6th grade test scores for students who were in our primary school sample, either in treatment or in control schools. As previously noted, the regression is run only on upper caste students in the top half of the academic distribution, to reduce the potential for bias due to sample selection, caused by relatively high drop out rates amongst lower caste students and those in the bottom of the academic distribution. Regression results are reported separately for mathematics and for language.

The first regression for each subject is the first stage regression on 5th grade test scores, on a set of regressors which includes the lagged (baseline) values of the individual's test score as well as the indicator of model school status as instruments. These results confirm that the model school package improved test scores for students in the top half of the academic distribution in mathematics, but not in language.

The second regression (for each subject) is an instrumental variable regression, which examines the effect of 5th grade achievement on 6th grade test scores, instrumenting the former by the individual's baseline test score in August 2004 and the indicator variable of whether the school received the model school package. The regression results confirm that 5th grade achievement positively affects learning in 6th grade, for both mathematics and language. For mathematics, while 5th grade achievement obviously reflects initial aptitude, it is also positively affected by the attributes of the primary school, specifically, the provision of the model school package. This result therefore suggests that quality improvements in primary school affect learning at higher levels of schooling, and have medium run affects which determine overall schooling achievement.

Converting our estimates into elasticities, the effect of a 1% improvement in 5th grade test scores increases 6th grade test scores by 0.7% for both language and mathematics. Though this is less than 1, the difference is not statistically significant, suggesting that differences across ability levels generated in primary school persist into middle school.[9]

6. Conclusion

In this study, we evaluate the effectiveness of a computer-aided learning program introduced in primary schools in rural Andhra Pradesh to improve school quality and enhance learning. The learning environment in rural schools differs considerably from

[9] An F test for whether the coefficients on 5th grade test scores differ from 1 generates the following results: For mathematics, F(1,21)=1.76 (Prob>F=0.21), and for language, F(1,20)=0.70 (Prob >F=0.41).

that in urban schools: Initial levels of learning are low; cohort sizes are small requiring different age cohorts to be combined in multi-grade classrooms; and the availability of electricity, required for running computers, is limited. Evidence of the value of CAL in rural environments is scant.

We find that CAL enhances learning of mathematics for students of all ability levels. For language, benefits accrue only to students in the top quartile of the language ability distribution. We also find that, in both language and mathematics, the benefits increase with cohort size.

By following 5^{th} grade students through their transition to 6^{th} grade, we were also able to assess the medium term impacts of primary school improvements. Specifically, we can address the question: do differences in the quality of primary schools persist through upper primary schools and contribute to inequality in cumulative schooling achievement? We find that primary school achievement significant enhances learning in upper primary schools. Combined with evidence that investments in primary schools, such as CAL, enhance the learning of mathematics in primary school, our results confirm the medium term impact of such investments. An important policy recommendation, then, is that investments at higher levels of schooling cannot compensate for the lack of investment in primary schools.

Table 1. Summary statistics, Treatment and Control Schools

Variable	Treatment schools	Control schools
Enrollment (2003)	200.33	192.40
	(82.20)	(49.02)
Teachers	6.23	5.52
	(2.97)	(1.65)
Proportion female teachers	0.46	0.52
	(0.23)	(0.22)
Classrooms	4.78	4.16
	(1.76)	(3.35)
Average class size	40.06	44.22
	(9.67)	(12.59)
Electricity hours per day	3.55	3.35
	(0.90)	(2.09)
Proportion SC/ST (2003)	0.12	0.18
	(0.14)	(0.22)

Note: Data are from sample survey of 15 treatment and 15 control schools. Standard deviation in brackets.

Table 2. Mean Test Scores, Treatment and Control Schools

Test date/school type	Language		Mathematics	
August 2004				
Treatment School	63.13	(0.53)	64.58	(0.51)
Control School	62.89	(0.64)	63.72	(0.60)
F test for equality* (F1,2618)	0.08	(0.77)	1.23	(0.26)
December 2004				
Treatment School	64.96	(0.52)	66.48	0.51
Control School	63.53	(0.65)	63.96	0.62
F test for equality* (F1,2254)	2.94	(0.09)	9.71	(0.002)
April 2005				
Treatment School	68.14	(0.51)	70.14	(0.49)
Control School	67.57	(0.58)	67.78	(0.59)
F test for equality* (F1,2220)	0.55	(0.46)	9.41	(0.002)
August 2005				
Treatment School	57.60	(0.62)	60.60	(0.62)
Control School	55.17	(0.69)	55.87	(0.68)
F test for equality* (F1,2031)	6.88	(0.01)	26.48	(0.00)
December 2005				
Treatment School	59.08	(0.63)	61.99	(0.58)
Control School	58.42	(0.75)	57.88	(0.70)
F test for equality* (F1,2026)	0.45	(0.50)	20.66	(0.00)

Table 3: Probability of Continuing to 6th grade, by caste and aptitude level

	Upper castes	Backward castes	Scheduled castes and tribes
Aptitude quartile – Mathematics			
Lowest	0.62	0.50	0.53
	(0.49)	(0.50)	(0.50)
2nd	0.72	0.72	0.66
	(0.45)	(0.45)	(0.48)
3rd	0.84	0.76	0.79
	(0.37)	(0.43)	(0.41)
Top	0.84	0.79	0.69
	(0.37)	(0.41)	(0.46)
Aptitude quartile – Language			
Lowest	0.63	0.55	0.65
	(0.49)	(0.50)	(0.48)
2nd	0.73	0.68	0.71
	(0.45)	(0.47)	(0.46)
3rd	0.77	0.77	0.70
	(0.46)	(0.42)	(0.46)
Top	0.86	0.77	0.64
	(0.35)	(0.42)	(0.48)

Note: Table reports the probability of students who were enrolled in primary schools in 4th grade continuing on to 6th grade. Aptitude distribution is based on test scores from the August 2004 baseline survey, for language and mathematics. Figures in brackets are standard errors.

Table 4.: Regression Estimates of Model School Program
(Improvement in test scores over base line)

	Mathematics		Language	
Model school	-1.26	-1.07	-1.30	-1.63
	(2.10)	(1.83)	(1.66)	(1.70)
Model*test-3	0.37	--	-0.65	--
	(1.84)		(1.40)	
Model*test-4	5.80*	5.62*	3.46	3.79
	(2.98)	(2.80)	(2.63)	(2.46)
Model*test-5	5.58*	5.40*	1.81	2.14
	(2.72)	(2.46)	(2.67)	(2.50)
Test 3	3.68*	3.86*	3.81*	3.49*
	(1.04)	(0.91)	(1.02)	(0.70)
Test 4	-10.91*	-10.82*	-10.78*	-10.64*
	(1.87)	(1.91)	(2.15)	(2.10)
Test 5	-9.36*	-9.27*	-7.36*	-7.52*
	(2.13)	(2.18)	(2.40)	(2.35)
Base test result	0.59*	0.59*	0.59*	0.59*
	(0.03)	(0.03)	(0.02)	(0.02)
scst	-2.19*	-2.19*	-2.36*	-2.38*
	(1.37)	(1.37)	(1.22)	(1.22)
obc	-1.53+	-1.53+	-1.36+	-1.39+
	(0.79)	(0.80)	(0.79)	(0.79)
sex	0.57	0.57	1.43*	1.43*
	(0.56)	(0.54)	(0.63)	(0.63)
Sample Size	8309	8309	8307	8307
Regression F	61.3	64.25	67.40	70.40

Note: All regressions include age, age square, cohort dummies, a cubic in total school enrollment, and dummy variables for number of rooms in the school (2 to greater than 6).
Standard errors are clustered by school-cohort
* Significant at 5% level
+ Significant at 10% level

Table 5.: Estimates by Initial Aptitude level

	Mathematics		Language	
Model school	-1.09	--	-1.64	--
	(1.84)		(1.70)	
Model*year 2	5.53*	--	2.99	--
	(2.37)		(2.33)	
Year 2	-11.99*	--	-11.00*	--
	(1.92)		(2.05)	
Model*year 2*	--	6.31+	--	2.20
lowest ability quartile		(3.72)		(2.98)
Model*year 2* 2nd	--	4.54	--	1.19
ability quartile		(2.90)		(2.92)
Model*year 3* 3rd	--	5.73*	--	3.67
ability quartile		(2.60)		(2.61)
Model*year 2 *top	--	6.10*	--	5.09*
ability quartile		(2.31)		(2.49)
Model*lowest ability	--	-0.16	--	0.23
quartile		(2.38)		(2.34)
Model*2nd ability	--	-1.06	--	-1.39
quartile		(2.15)		(2.19)
Model*3rd ability	--	-1.33	--	-3.72
quartile		(2.13)		(1.97)
Model*top ability	--	-1.60	--	-1.25
quartile		(1.82)		(1.67)
2nd quartile of ability	--	0.61		0.38*
distribution		(1.36)		(1.48)
3rd quartile of ability	--	1.78		2.97
distribution		(1.96)		(2.10)
4th (top) quartile of	--	3.38		3.73
ability distribution		(2.97)		(2.65)
Year 2*lowest ability	--	-9.64*	--	-9.66*
quartile		(2.69)		(2.43)
Year 2*2nd ability	--	-11.44*	--	-9.69*
quartile		(2.36)		(2.43)
Year 2*3rd ability	--	-13.68*	--	-12.36*
quartile		(2.14)		(2.13)
Year 2*4th ability	--	-13.25*	--	-12.57*
quartile		(1.92)		(2.23)
Sample Size	8309	8309	8307	8307
Regression F	66.49	66.49	81.02	75.60

Note: All regressions include baseline test score, age, age square, caste, sex, cohort dummies, a cubic in total school enrollment and dummy variables for number of rooms in the school (2 to greater than 6). Standard errors are clustered by school-cohort

* Significant at 5% level + Significant at 10% level

Table 6: Estimates by cohort size

Variable	Mathematics	Language
Model school	11.44*	7.31
	(5.26)	(5.00)
Model*year 2	-15.31*	-14.12*
	(6.55)	(6.69)
Model*year 2* cohort size	0.47*	0.38*
	(0.15)	(0.15)
Model*cohort size	-0.28*	-0.19
	(0.13)	(0.12)
Cohort size	0.10*	0.12*
	(0.05)	(0.04)
Year 2	-2.42	-1.54
	(3.58)	(4.20)
Year 2 * cohort size	-0.21*	-0.20*
	(0.08)	(0.08)
Sample size	8309	8307
Regression F	74.24	86.06

Note: Note: All regressions include baseline test score, age, age square, caste, sex, cohort dummies, a cubic in total school enrollment and dummy variables for number of rooms in the school (2 to greater than 6). Standard errors are clustered by school-cohort

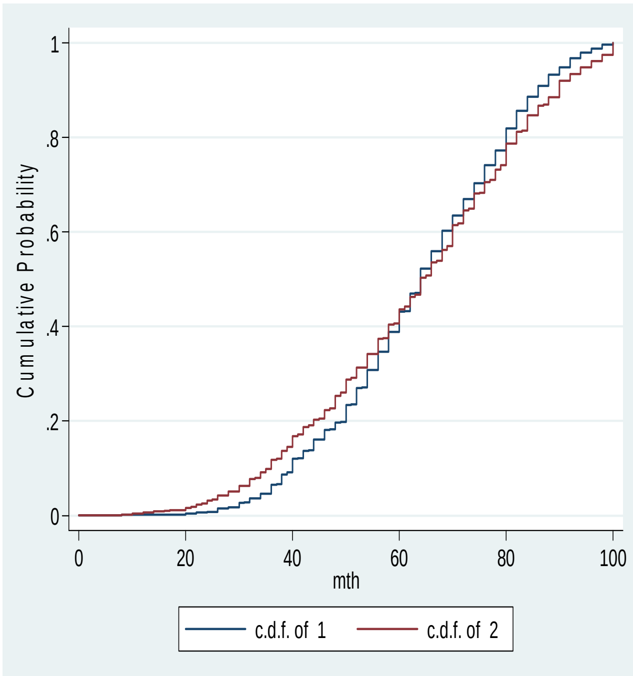
* Significant at 5% level

+ Significant at 10% level

Table 7: Effect of 5^{th} grade test scores on 6^{th} grade scores

Variable	Mathematics		Language	
	5^{th} grade	6^{th} grade	5^{th} grade	6^{th} grade
Model school	16.94*	--	2.81	--
	(2.93)		(3.05)	
Baseline test scores (Aug. 2004)	0.53*	--	0.81*	--
	(0.09)		(0.08)	
5^{th} grade test scores	--	0.02*	--	0.02*
		(0.008)		(0.009)
Age	-97.76*	-3.24+	-36.04	-3.43
	(37.89)	(1.73)	(38.47)	(2.24)
Age squared	4.46*	0.14	1.51	0.16
	(1.76)	(0.08)	(1.81)	(0.10)
Sex	2.57	-0.09	2.04	0.16
	(2.43)	(0.25)	(2.26)	(0.20)
High school total enrollment	-0.04*	0.0005	-0.06*	0.002
	(0.01)	(0.0008)	(0.02)	(0.001)
6^{th} grade enrollment	0.09	-0.004	0.43*	-0.01
	(0.09)	(0.005)	(0.09)	(0.01)
High school proportion SC/ST	40.12+	-4.29*	105.78*	-5.39*
	(22.59)	(1.07)	(20.17)	(0.91)
6^{th} grade proportion SC/ST	-47.02*	3.31*	-85.38*	4.75*
	(19.42)	(0.69)	(17.91)	(0.84)
Sample size	243	243	248	248
Regression R^2	9.08	17.34	15.31	14.18

Note: Regression sample is upper caste students in the top half of the academic distribution (based on August 2004 test scores). Additional regressors include dummy variables for the quarter in which the test was taken. 6^{th} grade test scores are standardized scores for the high school in question. Standard errors (in brackets) are clustered at the level of the high school.



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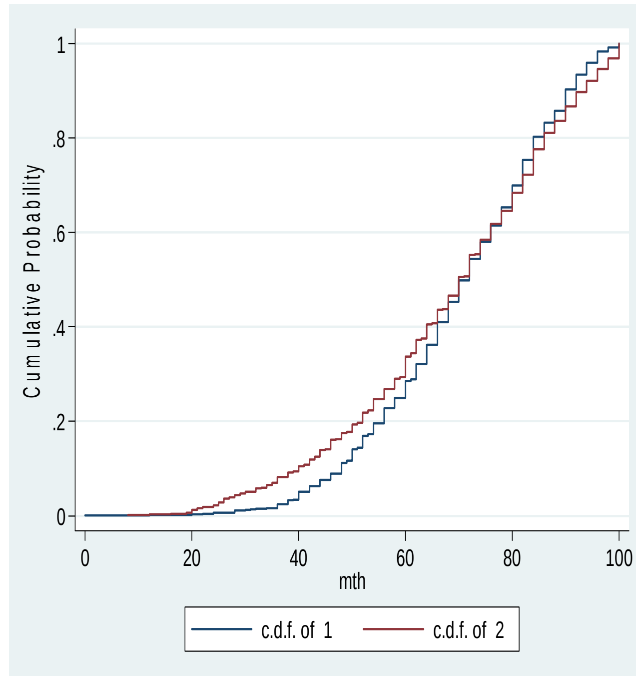

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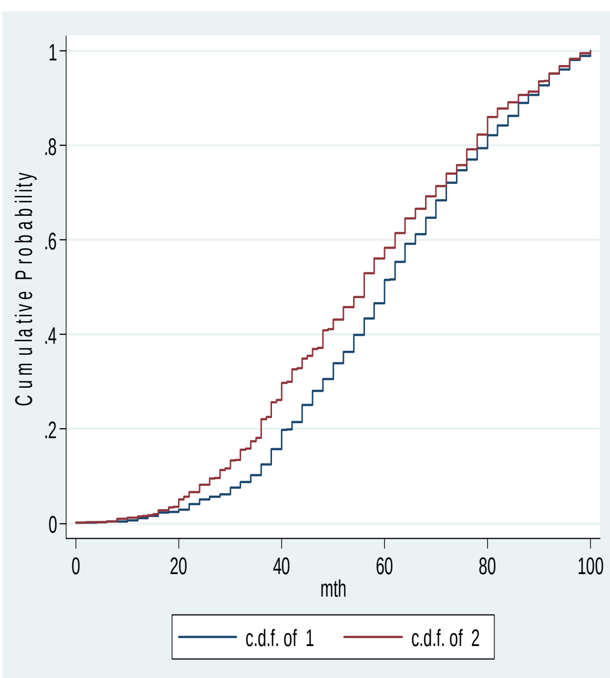

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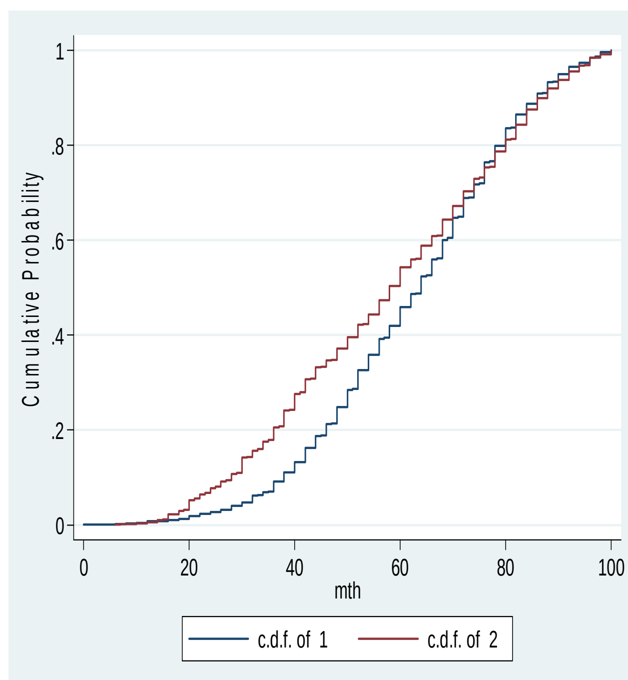
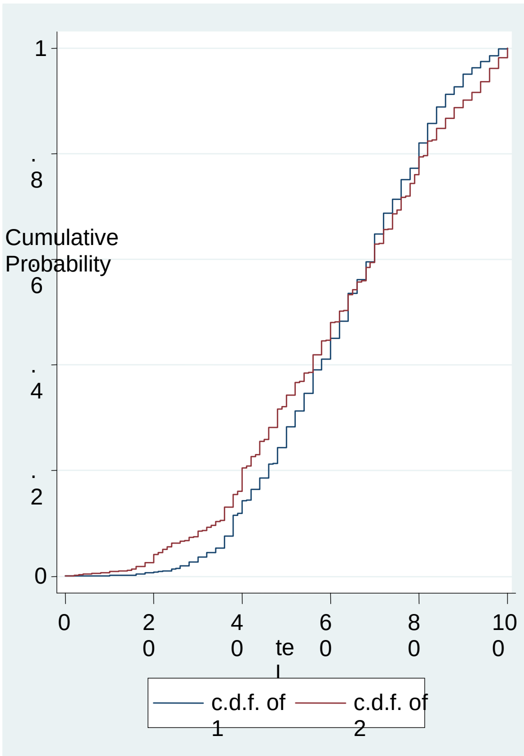


December 2005

Figure 1: Distribution of Math scores, August 2004 – December 2005



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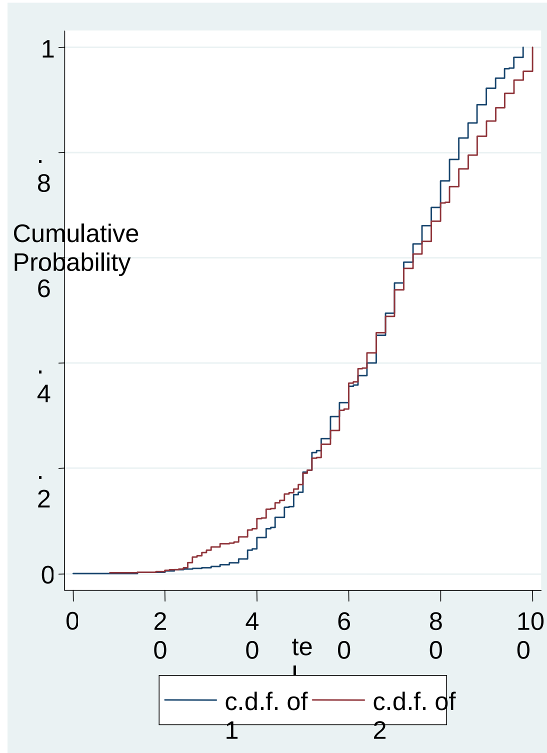

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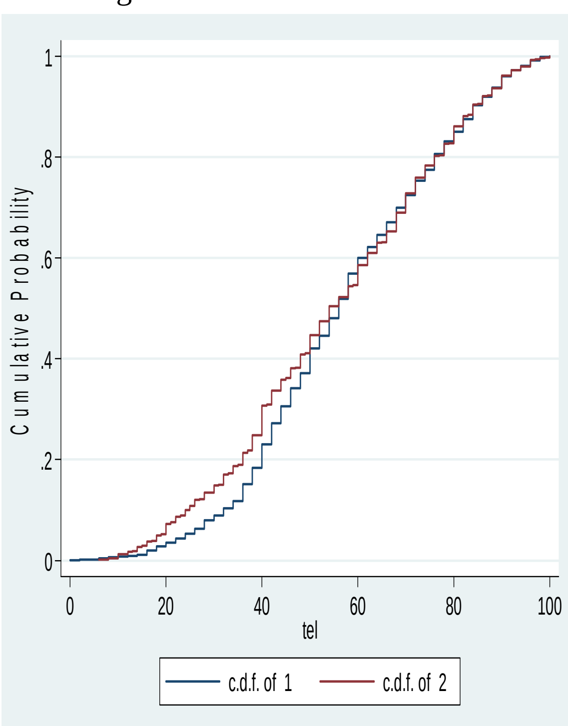

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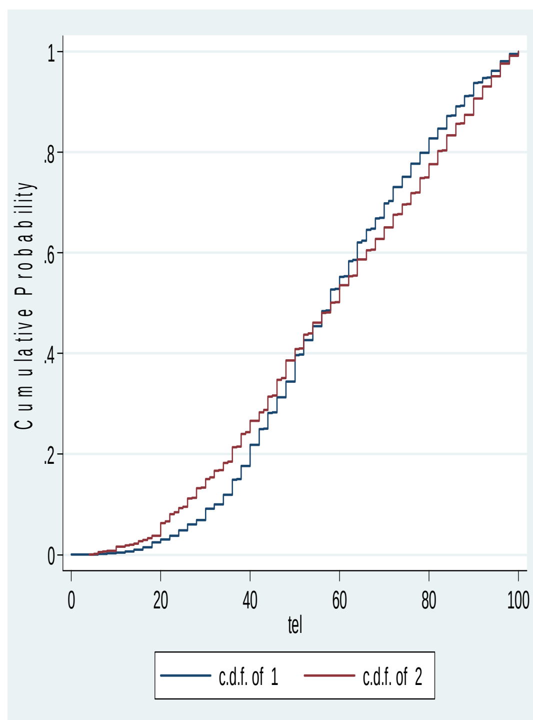

December 2005

Figure 2: Distribution of Telugu Scores, August 2004-December 2005

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