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# School Enrollment, Selection and Test Scores

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## Abstract

There is a strong association between schooling attained and test scores in many settings. If this association is causal, one might expect that programs that increase school enrollment and attainment would also improve test scores. However, if there is self-selection into school based on expected gains, marginal children brought into school by such programs may be drawn disproportionately from the left-hand side of the ability distribution, which could limit the extent to which additional schooling translates into more learning. To test this proposition, this paper uses data from Cambodia. The results show that a program that provides scholarships to poor students had a large effect on

school enrollment and attendance, which increased by approximately 25 percentage points. However, there is no evidence that, 18 months after the scholarships were awarded, recipient children did any better on mathematics and vocabulary tests than they would have in the absence of the program. The paper discusses results that suggest that the self-selection of lower-ability students into school in response to the program is an important part of the explanation. The analysis also shows minimal program effects on other outcomes, including knowledge of health practices, expectations about the future, and adolescent mental health.

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## **School Enrollment, Selection and Test Scores <sup>\*</sup>**

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## 1. Introduction

Despite progress in recent decades, a substantial fraction of children in developing countries attain little schooling, and many adults lack skills that are valued in the labor market. For this reason, policy-makers and academics continue to search for programs and policies that can raise educational attainment and learning in poor countries.

A number of interventions have recently been shown to increase school enrollment and attendance in some settings. These include deworming for school-aged children (Miguel and Kremer 2004); school construction (Duflo 2001); the provision of additional teachers (Banerjee et al. 2005); vouchers for private schooling (Angrist et al. 2002; Angrist, Bettinger, and Kremer 2005); and conditional cash transfers—transfers that are made to poor households, conditional on them keeping their children enrolled in school and attending regularly (see the review by Fiszbein and Schady 2009). Much less is known about how to improve learning outcomes (rather than school enrollment) for poor children in developing countries.

In principle, since there is a strong cross-sectional association between schooling attained and test scores in data from many countries (Filmer, Pritchett, and Hassan 2006), one might expect that programs that increase school enrollment and attendance would also improve learning outcomes. However, this may not occur for a variety of reasons. With the influx of new students, classrooms may become too crowded for much learning to take place. Children who are brought into school are often poorer, and education systems in developing countries may cater to the elites (as suggested by Duflo, Dupas, and Kremer 2008). There may also be selection on expected returns, so that those children who stand to benefit the most from education are already in school, while those brought into school by an intervention that increases enrollment are “marginal” children for whom the returns to schooling are low (related work includes Card 1999; Heckman, Urzua, and Vytlačil 2006; Carneiro and Lee 2008); these children may not learn much while in school. More generally, the association between schooling attained and test performance may in part reflect omitted variables, rather than primarily a causal effect of schooling on learning.

In this paper, we study the relationship between school enrollment, selection, and test scores. For this purpose, we use data from Cambodia. Specifically, we analyze the effects of a program, known as the CESSP Scholarship Program (CSP), which gives scholarships to poor children for the three years of the lower secondary school cycle.

We first show that the CSP program had a large impact on school enrollment—children offered scholarships were approximately 25 percentage points more likely to be enrolled than they would have been in the absence of the program. Data on time use shows that these children spent more hours in school; their parents paid school fees and made other education expenditures; finally, because they were promoted on time, children who were offered scholarships attained more schooling than did comparable children in the control group. Despite these increases in school enrollment and attainment, however, children who had received scholarships did no better on tests of mathematics and vocabulary than those in the control group 18 months after the beginning of the program. Moreover, we find no impacts on learning even in schools where scholarship recipients were only a small fraction of the total number of students, suggesting that over-crowding is not the main reason for the absence of program effects on test scores.

We next consider the possible role of self-selection into schooling. Our analysis is based both on tests taken by children at home, regardless of whether they were enrolled in school, and school-based tests. School enrollment data are available at numerous points in time. By combining these different sources of data we show that children who stayed in school as a result of the scholarships were drawn primarily from the left-hand side of the distribution of ability. If lower-ability children, like those whose schooling choices were affected by the CSP program, do not learn much in school, it is not surprising that there are no overall program effects on learning. We also discuss program effects on knowledge of health practices, expectations about the future (including desired age of marriage and birth of the first child, and the socioeconomic status children expect to attain in adulthood), and adolescent mental health. We find that most of these outcomes were unaffected by the scholarship program.

This paper is related to a literature on the determinants of learning outcomes and skills acquisition

in developing countries (see Hanushek 1995 and Kremer 1995 for early reviews; and Glewwe and Kremer 2006). This is an extensive literature, and we briefly discuss only a handful of recent papers that focus on the effect of demand-side incentives on test scores.

Kremer, Miguel, and Thornton (2008) evaluate the impact of a program that provided merit-based scholarships to adolescent girls who scored in the top 15 percent of the distribution of government-administered tests in Kenya; they find that the program raised school attendance rates and improved test scores (by approximately 0.1 to 0.2 standard deviations). Vermeersch and Kremer (2004) evaluate a program of school meals, also in Kenya, on participation and test scores in preschool. They find some evidence of improved test scores, although only in schools where the teacher was relatively well trained prior to the program. Angrist et al. (2002) and Angrist, Bettinger, and Kremer (2005) show that a program that provided vouchers for private school in Colombia increased the years of schooling completed, and improved test scores (by about 0.2 standard deviations); in this case, the gains appear to have been largest for students at the bottom of the latent distribution of test scores. On the other hand, Behrman, Parker, and Todd (2009) show that children who received transfers from the PROGRESA conditional cash transfer program in Mexico for two more years than other children do not score more highly on tests of mathematics and language as adults, even though they have completed approximately one-fifth more years of schooling.

Given the small number of robust evaluations of the impact of programs that increase the demand for education by households on test scores, it is hard to draw general conclusions from these studies. However, three points seem to us particularly important. First, it is not the case that programs that increase enrollment *automatically* result in greater learning—in at least some cases, the children who were brought into school do no better on tests than they would have without the additional education (as in Mexico). Second, the quality of the supply may matter—in Kenya, positive program impacts of the school breakfast program were only apparent among students who had higher-quality teachers, and the voucher program in Colombia, which also found positive impacts, allowed children to transfer to higher-quality private schools. Third, the underlying distribution of latent ability may be important in

determining outcomes—for example, the merit-based scholarship program in Kenya explicitly targeted the most able students. We return to these points in our discussion of the impacts of the CSP program in Cambodia.

The rest of the paper proceeds as follows. In section 2, we describe the CSP program and the data. Section 3 discusses our identification strategy. Section 4 presents our main results. We conclude in Section 5.

## **2. The CSP program and data<sup>1</sup>**

### **A. Scholarships in Cambodia**

Cambodia has a tradition of demand-side incentives intended to raise school enrollment and attendance rates at the secondary school level. Like many countries, it has a program of school meals, and several relatively small-scale programs that distribute bicycles, uniforms, and school materials to children in order to lower the travel and other direct costs associated with schooling. In addition, it has had a number of scholarship programs financed by non-governmental organizations (NGOs), international donors, and the government. These scholarship programs do not function as fee waivers. Rather, the families of children selected for a “scholarship” receive a small cash transfer, conditional on school enrollment, regular attendance, and satisfactory grade progress.

One recent such scholarship program was the Japan Fund for Poverty Reduction (JFPR) scholarship program, which operated in 93 lower secondary schools in Cambodia in the 2003-06 school years, with support from the Asian Development Bank and UNICEF. The JFPR offered scholarships to girls for the three years of lower secondary school.<sup>2</sup> Recipients were selected on the basis of a proxy means test, although there was room for communities to “revise” the list of recipients, which considerably complicates identification of program effects. Filmer and Schady (2008) evaluate the impact of the JFPR using propensity-score matching with data on attendance from unannounced school visits, and triple-

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<sup>1</sup> This section draws on Filmer and Schady (2009a).

<sup>2</sup> In 17 schools in 4 remote provinces ethnic minority boys and girls were eligible for scholarships under the JFPR program.

differencing with administrative data on enrollment levels. They conclude that the program increased school attendance rates by approximately 30 percentage points. The JFPR program has been replaced by the CSP, which operates in a different set of schools and which covers boys as well as girls. Finally, because many of the poorest children in Cambodia drop out of school well before lower secondary school, the government has recently launched a pilot program that provides scholarships to children in primary school. In some primary schools, children are selected purely on the basis of their socioeconomic status (SES), and in others the selection takes into account both SES and the child's performance on an academic test taken at baseline. This program is too new to have been evaluated.

The CSP scholarship program, which we analyze in this paper, works as follows. The government first selected 100 lower secondary schools throughout the country (from a total of approximately 800) to participate in the program. These CSP-eligible schools were selected because they served poor areas, as indicated by a poverty map, and because there appeared to be high levels of school non-enrollment and drop-out, as indicated by administrative data; schools covered by other scholarship programs were excluded. Next, all of the primary “feeder” schools were mapped to each CSP-eligible secondary schools. (A primary school was designated a feeder school if it had sent graduates to a given secondary school in recent years.) Within the primary feeder schools, *all* students in 6<sup>th</sup> grade, the last year of primary school, filled out an “application form” for the CSP scholarship program—regardless of whether children or their parents had expressed an interest in attending secondary school. These application forms consisted of 26 questions that were easy for 6<sup>th</sup> graders to answer, and for other students and teachers to verify. In practice, the form elicited information on household size and composition, parental education, the characteristics of the home, availability of a toilet, running water, and electricity, and ownership of a number of household durables. Forms were filled out in school, on a single day. Students and parents were not told beforehand of the content of the forms, nor were they ever told the scoring formula—both decisions designed to minimize the possibility of strategic responses (for example, by a student seeking to maximize her chances of receiving a scholarship).

Once completed, head-teachers collected the forms and sent them to Phnom Penh, the capital,

where a firm hired for this purpose digitized and scored them. Specifically, the responses were aggregated into a composite “dropout-risk score”, with the weights given by the extent to which individual characteristics predicted the likelihood that a child would fail to enroll in 7<sup>th</sup> grade after completing 6<sup>th</sup> grade, as estimated from a recent nationwide household survey, the Cambodia 2003/04 Socioeconomic Survey. The formula used, and the weights, were the same for all CSP schools. Finally, the firm produced a list of the dropout-risk score for each applicant, and ranked applicants within each CSP school by this score. In “large” CSP schools, with total enrollment above 200, 50 students with the lowest value of the score were then offered a scholarship for 7<sup>th</sup>, 8<sup>th</sup>, and 9<sup>th</sup> grade; in “small” CSP schools, with total enrollment below 200 students, 30 students with the lowest value of the score were offered the scholarship.<sup>3</sup> In total, just over 3800 scholarships were offered.<sup>4</sup> Two-thirds of the scholarships were given to girls; this is because girls are more likely than boys to drop out of school in Cambodia, so they were given a lower dropout score by the formula.

The list of students offered scholarships was posted in each CSP school, as well as in the corresponding feeder schools. The families of children who had been selected to receive scholarships received the cash award three times a year, conditional on school enrollment, attendance, and satisfactory grade progress. Payments were made at open school ceremonies, with the school principal publicly handing over the cash to parents.

The monetary value of the CSP scholarship is small—it is approximately equivalent to the direct cost of lower secondary school in Cambodia, without taking account of the opportunity cost of attending school. On average, it represents only 2 to 3 percent of the consumption of the median recipient

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<sup>3</sup> In practice, within every large school, the 25 students with the lowest dropout-risk score were offered a scholarship of \$60, and the 25 students with the next lowest scores were offered a scholarship of \$45; in small schools, the comparable numbers were 15 students with scholarships of \$60, and 15 with scholarships of \$45. We do not focus on this distinction in this paper. Rather, we compare applicants who were offered a scholarship, regardless of the amount, with others that were not. Because the identification strategy is regression-discontinuity, we are implicitly comparing applicants who were offered a \$45 scholarship, with those who were offered no scholarship at all. Students who were offered a \$60 scholarship help estimate the control function that relates enrollment to the dropout-risk score. We have elsewhere shown that there is no evidence that the effect of the CSP on enrollment is larger among students who were offered the \$60 scholarship rather than the \$45 scholarship (Filmer and Schady 2009b).

<sup>4</sup> Occasionally, there were tied scores at the cut-off. In these cases, all applicants with the tied score at the cut-off were offered the scholarships.

household, as compared to, for example, 20 percent in the case of the PROGRESA conditional cash transfer program in Mexico (Fiszbein and Schady 2009; Schultz 2004). The median age of CSP scholarship applicants was 14 at the time they completed the application survey; the 10<sup>th</sup> percentile of the distribution is 12, and the 90<sup>th</sup> percentile is 16.

## B. Data

We make use of three sources of data in this paper. We have access to the composite dropout-risk score, as well as the individual characteristics that make up the score for all 26,537 scholarship applicants. In addition, we collected both household and school-based data. The household data come from a household survey of 3225 randomly selected applicants and their families in five provinces: Battambang, Kampong Thom, Kratie, Prey Veng, and Takeo.<sup>5</sup> The household survey was collected between October and December of 2006, approximately 18 months after children filled out the application forms. For applicants who are enrolled in school and who have not repeated grades, the household survey therefore refers to the first half of 8<sup>th</sup> grade.

We also conducted four unannounced visits to the 100 CSP schools (in February, April, and June 2006, and in June 2007). The main objective of these visits was to physically verify school attendance (among CSP recipients and non-recipients). However, during one of the school visits (in June 2006) all students were also asked to complete a mathematics test—so this test refers to the end of 7<sup>th</sup> grade. Note that a simple comparison of the scores on the test taken *in school* for CSP recipients and non-recipients is unlikely to provide a satisfactory estimate of the impact of the scholarship program on learning. This is because the impacts of the program on school enrollment will introduce biases associated with selection. We discuss this in more detail below.

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<sup>5</sup> The sample was based on randomly selected schools in these five provinces. The survey was limited to applicants ranked no more than 35 places above the cutoff in these schools. This restriction was imposed to maximize the number of schools, while maintaining the density of observations “around” the cut-off—an important consideration when estimating program effects based on regression-discontinuity, as discussed below.

C. The mathematics and vocabulary tests

We use three tests for our analysis. Two of these were applied to children at home as part of the household survey—regardless of whether they were enrolled in school; the third test is a mathematics test applied in school.<sup>6</sup>

The first of the home-based tests is a mathematics test, which included 20 multiple choice items. Areas covered included basic algebra, geometry, and several questions which required using mathematical tools to answer simulated real world situations—including reading a simple graph or interpreting a bar chart. The survey protocol called for enumerators to find a quiet place where the applicant could fill out their responses; no time limit was given.<sup>7</sup>

The second home-based test we applied was a vocabulary test based on picture recognition. This test asked respondents to identify the picture corresponding to a word which the enumerator read out loud. For each word the respondent was then asked to select from a choice of four pictures. While the initial words are relatively easy to identify (“shoulder”; “arrow”; “hut”) the test is structured such that items become increasingly difficult (later words in the test we administered included, for example, “speed”; “selecting”; “adjustable”). There were in total 144 words that each applicant was asked to identify.

Last, we use a mathematics test that was administered in school to all students attending 7<sup>th</sup> grade in CSP program schools in June 2006. This was adapted from a test administered as a part of a separate national assessment exercise which tested mathematics skills at the end of 6<sup>th</sup> grade in a random subsample of primary schools (see Royal Government of Cambodia 2008). The CSP test was shorter, and focused on areas that would be reinforced during 7<sup>th</sup> grade. It consisted of 25 primarily multiple choice questions, and the areas covered were very similar to those in the mathematics test administered to

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<sup>6</sup> The tests administered as a part of the household survey were carefully piloted and adjustments were made, as necessary. (The classroom-based test had already undergone extensive validation.)

<sup>7</sup> Average time was 45 minutes; with 10 percent taking less than 26 minutes, and 10 percent taking more than 75 minutes.

children at home.<sup>8</sup> This was not coincidental: The test applied in children’s homes built directly on the school-based test, although the questions in the test applied at home were on average somewhat easier (to account for the fact that some children would have dropped out of school after completing 6<sup>th</sup> grade). The Cronbach alpha values for all of our tests are reasonably high—0.74 for the home-based mathematics test, 0.86 for the vocabulary test, and 0.79 for the in-class mathematics test.<sup>9</sup>

We begin by showing that test scores are associated with years of schooling. Specifically, we use the tests taken in the household survey and limit the sample to children who were not offered scholarships (non-recipients). Test scores are normalized so that they have mean zero and a standard deviation of one (in the sample of non-recipients). We then graph the distribution of test scores of non-recipients who dropped out before completing 7<sup>th</sup> grade (solid lines), and those who had completed 7<sup>th</sup> grade at the time of the follow-up survey (dashed lines). Figure 1 shows that, as expected, children who have completed more schooling have higher test scores—in both mathematics (left-hand panel) and vocabulary (right-hand panel). This difference in distributions does *not* have a causal interpretation. Children who dropped out of school before completing 7<sup>th</sup> grade may have lower ability than those who stayed in school, in which case the difference in the distributions could primarily reflect the impact of ability on test scores, rather than the impact of the additional schooling. Nevertheless, Figure 1 is reassuring in that it shows that our tests are not simply noise, and makes it at least plausible that CSP recipients, whose schooling levels are higher than they would have been in the absence of the program (as we show below), would also have higher test scores.

### **3. Identification strategy**

The basic identification strategy we use in this paper is based on regression discontinuity (RD). These regressions take the following form:

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<sup>8</sup> Of the 24 questions 19 were multiple choice with the remainder requiring the student to enter a single response to the question.

<sup>9</sup> Cronbach’s alpha is the average covariance across the item responses, divided by the sum of the average variance of all items plus the average covariance across all items. It is a widely used measure of the reliability of a test.

$$(1) \quad Y_{is} = \alpha_s + f(C_s) + \beta I(\mathbf{T}_{is}=1) + \varepsilon_{is}$$

where  $Y_{is}$  is an outcome (for example, enrollment, or a test score) for child  $i$  who applied to CSP school  $s$ ;  $\alpha_s$  is a set of CSP school fixed effects;  $f(C_s)$  is the control function, a flexible parametrization of the dropout-risk score. In our main results, we use a quartic in the score; we also test for the robustness of the results to this choice of functional form.  $I(\mathbf{T}_{is}=1)$  is an indicator variable that takes on the value of one if a student was offered a CSP scholarship; and  $\varepsilon_{is}$  is the regression error term. In this set-up, the coefficient  $\beta$  is a measure of the impact of receiving a scholarship. Standard errors allow for clustering at the level of the primary feeder school.

A number of things are worth noting about this specification. First, because the score perfectly predicts whether an applicant is offered a scholarship, this is a case of sharp (as opposed to fuzzy) RD. Second, because we focus on the impact of being *offered* a scholarship, rather than that of actually *taking up* a scholarship, these are Intent-to-Treat (ITT) estimates of program impact. Third, as with every approach based on RD, the estimated effect is “local”. Specifically, it is an estimate of the impact of the scholarship program around the cut-off. However, where the cut-off falls in terms of the dropout-risk score varies from school to school. This is because the number of children offered a scholarship was the same in every large and small CSP school, respectively, but both the number of 6<sup>th</sup> graders and the distribution of the underlying characteristics that make up the dropout-risk score varied.<sup>10</sup> The estimates of  $\beta$  are therefore weighted averages of the impacts for these different cut-off values.

## 4. Results

### A. Descriptive statistics

The identifying assumption in RD is that, conditional on the control function (in our case, the

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<sup>10</sup> All else being equal, in CSP schools that received more applications, and in those in which children have characteristics that make it more likely they will drop out, a child with a high dropout-risk score is more likely to be turned down for a scholarship than a similar child applying to a school that receives fewer applications or serves a population with a lower average dropout-risk score.

dropout-risk score), the average outcome for children just above the cut-off is a valid counterfactual for those who are just below. This is an untestable assumption. However, we provide evidence that it is reasonable for our data.

Table 1 summarizes the characteristics of CSP recipients and non-recipients, as reported on their application forms—separately for all applicants (left-hand panel) and applicants within ten ranks of the cut-off of the score (right-hand panel). The first four columns of each panel show that, as expected, recipients are generally poorer than non-recipients. For example, in the full sample, CSP recipients are less likely to own a bicycle (54 percent of recipients own one versus 76 percent of non-recipients); less likely to own a radio (25 versus 39 percent); and less likely to live in a dwelling whose roof is made of solid materials such as tiles, cement, concrete or iron (44 versus 65 percent). The differences between recipients and non-recipients are generally smaller when we limit the sample to children whose value of the score is closer to the cut-off.

The final two columns in each panel of Table 1 report the coefficient and p-value in a regression of each characteristic on the application form on a quartic in the dropout score, and school fixed effects. This corresponds to our basic estimation specification, and is a standard check on the validity of the RD framework (Imbens and Lemieux 2008; Lee and Lemieux 2009). This specification check suggests that differences between recipients and non-recipients are unlikely to be an important source of bias in our estimates of program impact. In the full sample, the coefficients on only two characteristics are significant—whether a child’s mother ever attended school, and the fraction of households with floors made of wood planks or bamboo. These differences between recipients and non-recipients are no longer significant when the sample is limited to children whose score places them within ten points of the cut-off. As a robustness check on our estimates of CSP program effects (and as is often done in applications of RD), we therefore also present results for this smaller sample.

## B. Program effects on school enrollment

We begin by estimating CSP program effects on school attendance (using the school-based visits)

and enrollment (using the household survey). We motivate this analysis by graphing outcomes as a function of the within-school ranking of applicants by the dropout-risk score, relative to the cut-off.<sup>11</sup> These results are presented in Figure 2, which gives the mean at each value of attendance (or enrollment) of the within-school rank; and the predicted value from a regression of attendance (or enrollment) on a quartic in the rank and an indicator variable for CSP recipients. Distinct “jumps” at the cut-off would suggest that the program affected outcomes.

Figure 2 suggests that the CSP scholarships had a substantial effect on school enrollment and attendance, about 20 to 25 percentage points. The results from the school-based data are less noisy, which is not surprising given the much larger sample sizes.

Figure 3 presents similar results for hours of schooling attended in the last week (left-hand panel), annual expenditures on schooling (middle panel), and the years of schooling attained (right-hand panel); all of these are based on the household survey. These figures suggest a CSP impact of about 7 hours on school attendance in the last week, of about US \$10 on school expenditures, and of about 0.2 years on school attainment.<sup>12</sup>

Estimates of CSP program effects on school attendance, enrollment, time in school, expenditures and school attainment are presented in Table 2. The raw differences in the first column of the table suggest that CSP recipients are 16 percentage points more likely to be enrolled in school, 17 percentage points more likely to be attending on the day of the unannounced visit, spend 5.4 more hours in school per week, had US \$9.0 higher school expenditures, and have attained about 0.21 more years of schooling than non-recipients. However, these raw differences under-estimate the CSP program effects because they do

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<sup>11</sup> Because the cut-off falls at different values of the underlying score in different schools, depending on the number of applications, the mean characteristics of applicants, and whether a school was defined as “large” or “small”, it is not informative to graph outcomes as a function of the score. Rather, for these figures we redefine an applicant’s score in terms of the distance to the school-specific cut-off, so that (for example), a value of -1 represents the “next-to last” applicant to be offered a scholarship within a school, 0 the “last” and a value of +1 represents the “first” applicant within a school who was turned down. The figures then graph outcomes as a function of this relative rank. Parametric regressions corresponding to these figures include a quartic in the relative rank, but not the vector of school fixed effects.

<sup>12</sup> School expenditures include various fees (registration fees, examination fees, allowances and fees for school events and ceremonies), school material, textbooks, uniforms, and daily expenditures (snacks, extra classes, bicycle parking, lesson copies and other daily expenses).

not take into account the fact that recipients are, on average, poorer than non-recipients.

The remaining columns of Table 2 estimate CSP program effects with different RD specifications. In the “basic” specification, with school fixed effects and a quartic in the school dropout-risk score, we estimate that children who were offered scholarships are 24 percentage points more likely to be attending school on the day of the unannounced school visit and 19 percentage points more likely to be enrolled in school, as reported in the household survey.<sup>13</sup> This specification also suggests that recipients spent 5.6 more hours in school in the last week (without conditioning on school enrollment); spent US \$10.4 more on schooling; and had completed 0.24 more years of schooling than non-recipients at the time of the survey.<sup>14</sup>

It is also possible to include school-specific quartics in the dropout-risk score, rather than a quartic which is restricted to be constant across schools. Those results are reported in the third column of the table. The impact of the CSP program is very similar to that found in the more parsimonious specification. Finally, we run regressions with a sample limited to households whose score places them within 10 ranks of the cut-off, as is often done in RD specifications (Imbens and Lemieux 2008; Lee and Lemieux 2009). This reduces the sample size by approximately two-thirds, but the results from this

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An important potential concern with these estimates of program effects is any impact that scholarships might have had on *non-recipients*. If the CSP scholarship program discouraged non-recipients from attending school, perhaps because classrooms became more crowded, the program effects we estimate could be some combination of higher school attendance among recipients and lower school attendance among non-recipients. This would be a violation of the Stable-Unit-Treatment-Value-Assumption (SUTVA)—see Imbens and Wooldridge (2009) for a discussion. We find no evidence that this is the case. Recall that, in “large” schools, the CSP offered 20 more scholarships than in “small” schools. Controlling for a polynomial in school size, “large” schools therefore had a larger influx of CSP recipients than small schools. However, in a regression of the attendance of non-recipient girls on a quartic in school size and an indicator for “large” schools the coefficient on this indicator variable is very small, and nowhere near conventional levels of significance (coefficient : -0.003, with a standard error of 0.022). We also ran a regression of attendance of girls who were turned down for scholarships on the number of CSP recipients, divided by the number of students who attended 7<sup>th</sup> grade in the year before the program. Schools in which this ratio is high are therefore those in which crowded classrooms are most likely to be an issue. The coefficient on the ratio is -0.014 (with a standard error of 0.089). (Both regressions also include a quartic in the dropout score and school fixed effects.) We conclude there is no evidence that children who did not receive scholarships decreased their enrollment in response to the program.

<sup>14</sup> This number is consistent with the impact on school enrollment: The estimates of program effects on school enrollment suggest that approximately one out of every five CSP recipients would not have been in school in the absence of the program; with on-time grade progression we would therefore expect that every fifth CSP recipient would have completed one more year of schooling than comparable non-recipients. Table 2 shows that this is approximately the case.

specification are very similar to those that include all children.

The CSP program effects we estimate are remarkable for their magnitude. One way to place them in context is by comparing them with those that have been found for similar programs elsewhere. Schultz (2004) analyzes the impact of PROGRESA, the Mexican conditional cash transfer program, on enrollment. He finds that PROGRESA reduced dropout between 6<sup>th</sup> and 7<sup>th</sup> grade by 7-9 percentage points among boys, and by 10-15 percentage points among girls, with much smaller and insignificant results on enrollment at other school grades. The enrollment effects of other CCTs are also smaller, often substantially so, than those found in Cambodia (see the review by Fiszbein and Schady 2009). The CSP program effects are particularly remarkable given the small magnitude of the transfer.

### C. Program effects on test scores

We now turn to estimates of CSP program effects on learning outcomes. For this purpose, we use data from the household survey, where children were tested regardless whether they were enrolled in school. Note that, because we do not condition on school enrollment, these estimates should be unaffected by any biases associated with selection into school. This is important, as school enrollment could be correlated with both CSP scholarships and the unobserved characteristics of applicants, including latent ability.

As before, we motivate our results with figures of the average test score at each value of the within-school ranking by the dropout score; and the corresponding values from quartic regressions in the rank and an indicator variable for CSP recipients. These results are presented in Figure 4. Unlike the results for school attendance, enrollment, time in school, education expenditures, and school attainment, we find no evidence of discrete jumps in either mathematics or vocabulary test scores at the cut-off that determines eligibility for CSP scholarships.

Estimates of CSP program effects on mathematics and vocabulary test scores from various specifications are reported in Table 3. For neither mathematics nor vocabulary, and for none of the specifications, do we find evidence of significant program effects on learning. Our estimates are

reasonably precise. Using the basic specification with a single quartic in the dropout score as a benchmark, the 95 percent confidence interval for the mathematics test is (-0.10, 0.14), and that for the vocabulary test is (-0.11, 0.11).

Conceivably, although the CSP program may not have increased learning for the average recipient, it could have improved test scores for children who attended higher quality schools. To test for heterogeneity by school quality, we use administrative data from the Education Management Information System (EMIS) for the year before students started receiving CSP scholarships. Specifically, we interacted the indicator variable for CSP recipients with variables corresponding to school-level measures of the share of teachers with upper secondary education; and the share of teachers with 5 or more years of experience.<sup>15</sup> We then ran our basic RD specification including the indicator variable for CSP recipients, as before, as well as the interaction between CSP recipients and the relevant measure of school quality. (Note that the main effect for these quality measures drops out because all of our regressions include school fixed effects.) These results are presented in the upper panel of Table 4—for the mathematics test (left-hand panel) and the vocabulary test (right-hand panel). The coefficient of interest in these regressions is that on the included interaction term. In no case is this interaction term significant. We conclude that children who were offered scholarships did not have higher test scores than they would have without the program even when they attended schools with more educated or more experienced teachers.<sup>16</sup>

The lower panel of the table focuses on various measures of classroom crowding. In the first row, the regression includes a quartic in the total number of students in a school in the year before the CSP program began, and an indicator variable for “large” schools, as defined by program administrators. Recall that 20 more scholarships were awarded by the CSP program in “large” schools, with total enrollment above 200, than in “small” schools. Controlling for a polynomial in school size, “large”

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<sup>15</sup> Mean share of teachers with upper secondary schooling across schools is 0.57 (with a standard deviation of 0.29); mean share of teachers with 5 or more years of experience is 0.47 (with a standard deviation of 0.27).

<sup>16</sup> We also estimated models in which these continuous variables were transformed into indicator variables for high and low values of quality. The results from these models were qualitatively similar to those we report.

schools therefore had a larger influx of CSP recipients than “small” schools, and would therefore be more crowded, on average. For neither the mathematics nor the vocabulary test, however, is the interaction between “large” schools and CSP recipients significant.. In the second row of this panel, the explanatory variable is the number of scholarship recipients, as a proportion of total 7<sup>th</sup> grade enrollment in the year before the program.<sup>17</sup> Here too there is no evidence that children who received CSP scholarships had lower test scores when the fraction of scholarship students was large, implying a proportionately large influx of additional students. The next three rows test for non-linearities by considering program effects on learning in schools where this fraction ratio was above the median, above the top quartile, and below the bottom quartile. In no case is the interaction term significant. We conclude from these various specifications checks that classroom crowding is not the main reason for the absence of CSP program effects on learning.<sup>18</sup>

In sum, children who were offered CSP scholarships did no better on learning tests in mathematics and vocabulary than comparable non-recipients, who on average had completed fewer years of schooling. There is no evidence that crowding of classrooms is the main reason for the absence of program effects on learning. Moreover, and with the important caveat that our measures of school quality are somewhat crude (see Hanushek and Rivkin 2006 for a discussion), we find no evidence that CSP recipients had higher test scores than non-recipients when both attended higher-quality schools.

#### D. Selection into schooling

The puzzling absence of CSP program effects on learning outcomes could plausibly be explained by differences in the latent distribution of test scores between children who were brought into school by the program and other children. (We use the terms ability and latent distribution of test scores interchangeably in this section.) Consider a case in which there is individual heterogeneity in both the

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<sup>17</sup> The median proportion of scholarship recipients was 0.20, the 10<sup>th</sup> percentile was 0.12, and the 90<sup>th</sup> percentile 0.32.

<sup>18</sup> In separate results, unreported but available from the authors upon request, we show that these findings are quite similar when we use the student-teacher ratio instead of the proportion of scholarship students as an explanatory variable.

costs and the benefits of schooling (as in Card 1999). Individuals select into schooling in part on the basis of expected gains—what Heckman, Urzua, and Vytlačil (2006) call “essential heterogeneity” (also Carneiro, Heckman, and Vytlačil 2006; Carneiro and Lee 2008). In the absence of a scholarship program, children with higher ability are more likely to stay in school than lower-ability children because for them the expected returns are high; higher-ability children also learn more while in school. Conversely, lower-ability children, for whom the expected returns to school are low, are more likely to drop out of school; these children would not have learned much had they stayed in school. The CSP program lowers the costs of education for scholarship recipients, and some of the lower-ability children who under normal circumstances would have dropped out now stay in school. However, the test performance of these children is not improved by their additional schooling.

To investigate this hypothesis we proceed in two complementary ways, both of which involve comparisons of the distribution of test scores of CSP recipient and non-recipients, conditional on attaining a certain school grade.

To begin, we restrict the sample in the household survey to children who were enrolled in 8<sup>th</sup> grade at the time they took the test. In the case of the school-based mathematics test, no such restriction is necessary, as test scores are only available for those who were enrolled in 7<sup>th</sup> grade (and were present on the day the test was applied). Also, it is useful to rescale the samples so that there is one recipient for every non-recipient “at baseline” (at the time the application for the CSP was completed). This rescaling does not affect the analysis, but allows for an easier visual inspection of patterns.<sup>19</sup>

Our first approach involves generating histograms of the number of children with a given test score, separately for CSP recipients and non-recipients. These histograms are presented in the upper panel of Figure 5, with each figure corresponding to a different test. The lower panel of the figure plots the *ratio*

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<sup>19</sup> As described above, there were about 26,537 applicants for CSP scholarships, and 3,800 scholarships were offered. We therefore rescale the sample used for the analysis of the school-based test by giving each non-recipient applicant approximately one-sixth the weight of each recipient applicant. In the household survey there are approximately twice as many recipients as non-recipients, by construction. We therefore rescale this sample by giving each recipient applicant a weight which is approximately one-half the weight of each non-recipient.

between the lines of the number of recipients and non-recipients at each test score.<sup>20</sup> Because the scholarship program affects enrollment decisions, there are in total approximately 25 percent more CSP recipients than non-recipients in 7<sup>th</sup> grade at the time of the June 2006 school visit, and 30 percent more recipients than non-recipients enrolled in 8<sup>th</sup> grade at the time of the household survey (after rescaling). The question is where these additional CSP recipients are found in the distribution of test scores.

Figure 5 clearly shows that the additional children brought into school by the CSP are not distributed proportionately at all values of the test score. For the school-based mathematics test, the ratio of recipients to non-recipients is larger than 1.2 at every value of the test 13 and below. (The median score for the sample as a whole is 11). In the household-based mathematics test, the ratio of non-recipients to recipients clearly slopes downwards from left to right. For the vocabulary test, finally, the number of CSP recipients and non-recipients is almost exactly the same at the highest values of the test score, implying that *no* children with very high test scores were kept in school by the program, while at the lowest values of the vocabulary test score there are at least twice as many recipients as non-recipients.

The second approach we take uses only data from the June 2006 in-school mathematics test. Because we visited CSP schools in both June 2006 and in June 2007, the sample of children who took the test in June 2006 can be divided into children who will continue from 7<sup>th</sup> grade to 8<sup>th</sup> grade (“stayers”) and those who will leave school over the period (“drop-outs”). We can then generate histograms for the June 2006 test scores of “stayer” and “drop-out” children, separately for recipients and non-recipients. As before, the point of the analysis is to see how the distribution of June 2006 test scores among stayers differs for recipients and non-recipients, after weighting to ensure equal numbers of recipients and non-recipients in June 2006. Figure 6 shows that virtually all of the CSP recipients who were kept in school between 7<sup>th</sup> and 8<sup>th</sup> grade had test scores in 7<sup>th</sup> grade that placed them below the median; proportionately,

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<sup>20</sup> For the school-based and household-based mathematics tests, each “bin” corresponds to a given score, ranging from 1 to 20 for the household test, and 1 to 25 for the school-based test—see above. For the vocabulary test, each “bin” corresponds to the number of children within 10-point brackets, with the number of potential correct answers on the test running from 40 to 142.

the largest number of additional children are found at the lowest test scores.<sup>21</sup>

This analysis also lends itself naturally to an RD, differences-in-differences approach.

Specifically, we run regressions of the following form:

$$(2) \quad Y_{is} = \alpha_s + f(C_s) + \beta_1 I(\mathbf{T}_{is}=1) + \beta_2(\text{Test}_{is}) + \beta_3[I(\mathbf{T}_{is}=1)*(\text{Test}_{is})] + \varepsilon_{is}$$

where the variable  $Y_{is}$  is an indicator that takes on the value of one for a child who dropped out of school between 7<sup>th</sup> and 8<sup>th</sup> grade, and zero if she remained in school;  $\alpha_s$ ,  $f(C_s)$ , and  $I(\mathbf{T}_{is}=1)$  correspond to school fixed effects, a flexible formulation of the drop-out score, and an indicator variable for CSP recipients, respectively, as before;  $\text{Test}_{is}$  is some parameterization of the score on the in-class 7<sup>th</sup> grade test; and  $[I(\mathbf{T}_{is}=1)*(\text{Test}_{is})]$  is an interaction between recipients and the test score.

Since the CSP scholarships make it more likely that children will stay in school, we would expect the coefficient  $\beta_1$  to be negative; if children with lower tests scores are more likely to drop out of school, consistent with selection on expected gains, we would expect the coefficient  $\beta_2$  to be negative. Finally, if scholarships are more likely to keep weaker children (in terms of their test scores) in school, we would expect the coefficient  $\beta_3$  on the interaction term to be positive. Note also that, these estimates of the role of selection are net of any differences in socioeconomic status between scholarship recipients and non-recipients. This is an important difference with the histograms presented in Figure 6.<sup>22</sup>

Results from these regressions are reported in Table 5. In the first row, the test score enters linearly; in the second row, the explanatory variable for the test score and the interaction with recipient status has been coded as an indicator that takes on the value of one if the score is above the median, and zero otherwise; in the third row, finally, the explanatory variable for the test score and the interaction with recipient status has been coded as an indicator that takes on the value of one if the score is above the top quartile, and zero otherwise.

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<sup>21</sup> The fact that there are *more* recipients than non-recipients among stayers at the highest values of the test is presumably a result of recipients' lower socioeconomic status. We address this question below.

<sup>22</sup> Students were not told the results of the in-school mathematics test they took in June 2006. The tests were scored centrally, in Phnom Penh, and no provision was made for the results to be sent back to the schools. It is therefore unlikely that performance on the test itself affected the probability of staying in school.

The results in Table 5 make clear that the CSP scholarships primarily keep in school children with low test scores. In all specifications, the interaction term between test scores and scholarship recipients is positive, significant, and large in magnitude. The estimates in the first row imply that a one-standard deviation higher test score decreases the probability that a child will drop out by 6.7 percentage points among children who did not receive scholarships, but only by 2.6 percentage points among children who received them. The coefficients in the second row suggest that children with above-median test scores are 9.3 percentage points less likely to drop out than those with below-median test scores if they are not scholarship recipients (from a base dropout rate of 44 percent among those with below-median test scores); this difference is only 2.8 percentage points among scholarship recipients. The estimates in the third row, finally, suggest that children with test scores in the top quartile are 12.4 percentage points less likely to drop out than those with test scores in the lower three quartiles if they were not offered scholarships (from a base dropout rate of 43 percent among those with test scores in the lower three quartiles); among those who were offered scholarships, the difference is only 2.4 percentage points. In this specification, an F-test for  $(\beta_2 + \beta_3 = 0)$  shows that we cannot reject the hypothesis that, among scholarship recipients, there is no difference in drop-out rates between children who scored in the top quartile and those who scored below (p-value: 0.341). Furthermore, an F-test for  $(\beta_1 + \beta_3 = 0)$  shows that we cannot reject the hypothesis that all of the children kept in school by scholarships were from the lower three quartiles of the distribution of test scores (p-value: 0.823), or from below the median of the distribution of test scores (p-value: 0.132).

In sum, the evidence in Table 5 is consistent with scholarships preventing the “natural” selection of weaker students out of school. Plausibly, in these schools, schooling improves learning only (or primarily) for children above a threshold in the distribution of ability.<sup>23</sup> The children who were kept in school by the CSP were drawn primarily from the left-hand side of the distribution of test scores. If these children fall largely below the threshold at which schooling translates into learning in these schools, a

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<sup>23</sup> This would be consistent with some evidence from other developing country settings—for example, Glewwe, Kremer and Moulin (2008) conclude that textbooks only improved learning outcomes among students at the top of the pre-intervention distribution of test scores in Kenya.

comparison of the average test scores of *all* scholarship recipients and non-recipients, without conditioning on school attainment (as in Table 3 above), would find small or no program effects on learning, because the marginal children kept in school did not learn much.

E. Non-learning outcomes

Education may confer benefits other than in terms of learning outcomes. In this section we discuss CSP program effects on knowledge about health practices; on expectations about the future; and on adolescent mental health. These results are presented in Table 6.

As part of the household survey, we asked children who had applied for CSP scholarships whether they agreed or disagreed with five statements about the health consequences of smoking.<sup>24</sup> For example, the first of these questions states that “smoking cigarettes can be harmful to one’s health”, and the second states that “the smoke from other people’s cigarettes is harmful to you”. The survey also asked respondents whether they had heard of HIV/AIDS and, conditional on an affirmative answer, what could be done to avoid infection. In this case, the question was open-ended, and respondents were asked to list their answers. Both the smoking and HIV/AIDS questions were asked of all applicants, regardless whether they were enrolled in school.

The first panel of Table 6 shows that, among non-recipients, schooling is associated with better knowledge of HIV transmission. CSP recipients were also more likely to provide correct responses to the HIV/AIDS questions than non-recipients: the coefficient is 0.120 (with a standard error of 0.057). On the other hand, there is no association between schooling and knowledge of the harmful effects of smoking and no effect of the program on this outcome.<sup>25</sup>

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<sup>24</sup> A small number of respondents answered “no opinion”, “don’t know” or “refuse”. The number of missing observations for each outcome is: 40 (AIDS); 0 (Cigarettes); 1 (Good home); 105 (Marriage age); 123 (Age for first child); 0 (Current economic ladder); 14 (Future economic ladder); 34 (Mental health scale 1); 6 (Mental health scale 2). The responses are no more or less likely to be missing for recipients, and the likelihood of missing status is unrelated to the dropout-risk score.

<sup>25</sup> Virtually all respondents had heard of HIV/AIDS, and there is no program effect on the likelihood of having heard of the disease. Also, because CSP recipients were more likely to answer questions about HIV/AIDS, even when the responses were not correct, we also generated a dependent variable for the share of responses that were correct

The next panel in Table 6 focuses on applicants' expectations about the future. For this purpose, we use five questions in the survey. The first of these asked respondents what they thought their chances were of "providing a good home for themselves and their families in the future". Responses were coded from "very unlikely" (coded as =1) to "very likely" (coded as =5); the second question asked respondents about the desired age of marriage; the third question asked about the desired age of their first child.<sup>26</sup> Finally, we analyzed applicant reports on their subjective well-being, using an adaptation of the "MacArthur ladders".<sup>27</sup> Applicants were shown a picture of a ladder with 10 rungs, and were told that higher rungs corresponded to better socioeconomic status. They were asked to place themselves on the ladder in relation to everyone in their communities, at the time of the survey, as well as where they expected to stand "in the future, when they have their own family".

As before, the first column of Table 6 shows that, among children who were turned down for CSP scholarships, those who have completed more schooling thought it more likely that they would be able to provide a good home; an additional year of schooling is associated with higher desired age of marriage (0.9 years) and higher desired age of first childbirth (1.4 years); on average, children with one more year of schooling place themselves about 0.1 rungs higher on the MacArthur ladder at the time of the survey, and expect to be about 0.3 rungs higher when they have their own family. The second column of the table shows, however, that there is no evidence of significant CSP program effects on any of these outcomes.

The last panel of Table 6 focuses on the associations between education levels, the CSP program, and adolescent mental health. For this purpose, we use two separate scales. The first, which we call "Mental Health Scale 1", is based on a scale used to estimate mental health among survivors of Hurricane Katrina in the United States, and includes 12 questions;<sup>28</sup> individual responses are coded from "strongly agree" (coded as =5) to "strongly disagree" (coded as =1). The second scale, which we call "Mental

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(rather than simply the number of correct responses). The CSP program effects are similar when we use this variable instead.

<sup>26</sup> Of the sample of 2273 applicants, 21 were already married and 10 already had a child. These are excluded from the analysis of desired age for marriage and first child.

<sup>27</sup> For a description and bibliography of papers that use MacArthur ladders, see the MacArthur Foundation's Network on SES and Health website: <http://www.macses.ucsf.edu/Research/Psychosocial/notebook/subjective.html>.

<sup>28</sup> We are very grateful to Christina Paxson and Cecilia Rouse for making this scale available to us.

Health Scale 2”, is an adaptation of the widely-used General Health Questionnaire (GSQ-12).<sup>29</sup> It too includes 12 questions, with individual responses coded as “never” (coded as =1), “sometimes” (coded as =2) or “always” (coded as =3).<sup>30</sup> To estimate program effects on the responses to questions on this second scale, we first reversed the numerical codes for questions in which the response “always” would represent a worse outcome than the response “never”. In this way, higher numerical responses to any question always represent better outcomes.<sup>31</sup>

We estimate the associations between mental health and education, and the effects of the CSP program on mental health both for responses to individual questions, and for averages across all 12 outcomes within a scale. These averages are estimated by running seemingly unrelated regressions (SUR) for all 12 outcomes, and using the estimated variance-covariance matrix of the estimates to calculate the standard error of the average (as in Kling et al. 2007; Duflo et al. 2007; Paxson and Schady 2009). They provide useful summary measures and have the advantage that they may be more precisely estimated than the individual effects.<sup>32</sup>

Table 6 shows that adolescents with higher education levels generally have better mental health as measured by the Mental Health Scale 1. On average, an additional year of schooling is associated with 0.079 more points on this scale (with a standard error of 0.021); however, there is no evidence that CSP recipients have better mental health than non-recipients, as measured by this scale. Table 6 also shows a positive association between schooling and mental health as measured by the Mental Health Scale 2. On average, one more year of schooling is associated with 0.053 more points on this scale (with a standard

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<sup>29</sup> For example, Schnritz, Kruse, and Tress (2009); Werneke, Goldberg, Yalcin, and Ustun (2000).

<sup>30</sup> Both scales were carefully piloted in the field, and changes to the questions were made, as necessary, to ensure an adequate understanding by respondents.

<sup>31</sup> In practice, very few respondents answered “never” to any question in which higher values indicated better outcomes (for example, less than 1% of respondents stated that they have “*never* been able to concentrate” in the last month), or “always” to any question in which higher values indicated better outcomes (for example, less than 4% of respondents stated that they have “*always* lost much sleep over worry” in the last month). We therefore turned responses to each question into a indicator variable, in which “sometimes” and “always” take on a value of one for questions in which higher values indicated better outcomes (for example, the question on ability to concentrate), and “sometimes” and “never” take on a value of one for questions in which higher values indicated worse outcomes (for example, the question on sleep loss).

<sup>32</sup> Like other estimates in the paper, these averages also correct for clustering at the level of the primary feeder school.

error of 0.009); in this case, CSP recipients also score somewhat higher than non-recipients—the average coefficient is 0.028 (with a standard error of 0.01).

In sum, Table 6 suggests that CSP recipients have better knowledge of some health practices than non-recipients (in terms of their knowledge of HIV/AIDS); and they have somewhat better mental health—at least as measured by one of two scales which we implemented. However, we find no evidence that CSP recipients have better expectations about their future prospects, or that the program had an impact on the desired age of marriage or childbearing.

## **5. Conclusion**

Many programs in developing countries seek to raise school enrollment and attainment levels. Drawing on randomized evaluations of conditional cash transfers, school feeding programs, programs that provide school uniforms free of charge, and merit-based scholarships, a recent review concludes that interventions that reduce the price of schooling can have substantial effects on access to education (Kremer and Holla 2008).

However, children who are brought into school may not learn much as a result of the additional schooling. This could happen for a variety of reasons—for example, if these children are more disadvantaged in terms of their observable characteristics, including socioeconomic status, or their unobservable characteristics, including latent ability.

The evidence we present in this paper confirms that school enrollment can be very responsive to quite modest incentives provided to households: In Cambodia, the CSP program, which provided scholarships that accounted for only 2 to 3 percent of the total consumption of the average recipient household, increased school enrollment and attendance by approximately 25 percentage points. These results suggest that conditional cash transfers can be powerful tools for increasing school enrollment and attendance rates even in low-income countries, with relatively weak institutions—a very different setting from where they have been implemented and evaluated to date.

In the absence of scholarships, lower-ability children are more likely to drop out of school

between 7<sup>th</sup> and 8<sup>th</sup> grade in Cambodia than higher-ability children. Many of the children who are kept in school by the CSP program are drawn primarily from the left-hand side of the distribution of test scores. Higher-ability children who received CSP scholarships would have stayed in school anyway—they did not need the scholarships to do so. This is consistent with selection into schooling on the basis of expected gains. Lower-ability children who received scholarships do not appear to have learned much, if anything, from the additional schooling. As a result, we find no evidence that CSP recipients do better than comparable non-recipients on tests of mathematics or vocabulary, despite their higher school attainment.

The large magnitude of the program effects on school enrollment and attendance, coupled with the absence of impacts on learning, raises difficult policy questions. Two strike us as especially important. The first of these is whether there are complementary interventions on the supply side that could result in better learning outcomes among program beneficiaries. Our results suggest that interventions that bring up overall quality to the levels found among relatively higher-quality schools in Cambodia are unlikely to improve learning outcomes of CSP recipients and other low-ability students: CSP recipients did no better on the mathematics and vocabulary tests even when they attended schools that were less crowded, had teachers with more education, or teachers with more experience. On the other hand, there may be potential for interventions on the supply side that focus on poorly-performing students. For example, Banerjee et al. (2007) provide evidence that a program that made tutors available to low-performing students in a sample of Indian schools improved their test scores by about 0.3 standard deviations; Duflo, Dupas, and Kremer (2008) provide evidence that low-performing students in Kenya do better when they are placed in classrooms with other low-performing students, because tracking of this sort enables teachers to focus their teaching to a level appropriate for most students in the class; interventions that reward teachers for improvements in test scores among the lowest-performing students may also hold promise.

A second important policy question is whether a program that raises enrollment rates but does not improve learning outcomes is a sensible investment of public resources. That will depend in large part on

whether the additional educational attainment that resulted from the program will eventually result in higher wages among program beneficiaries. Of course, children may benefit from programs that increase school enrollment levels even if their cognitive skills, as measured by tests, are no higher than they would have been without the additional schooling. They may acquire important non-cognitive skills, including discipline, perseverance, motivation and a work ethic (see Heckman and Rubinstein 2001; Heckman, Stixrud, and Urzua 2006). Schooling may also have a causal effect on health-seeking behaviors, on health status, and on fertility—even when learning is limited. In practice, we find modest or no effects of the CSP scholarship program on knowledge of health practices, on adolescent mental health, and on expectations of the future. However, an important caveat is that we evaluate the impact of the program after only 18 months, and when recipients were still young. It may be too early for these effects to have materialized. Measuring the medium- and long-term impacts of programs that provide demand-side incentives for schooling is an important area for future research.

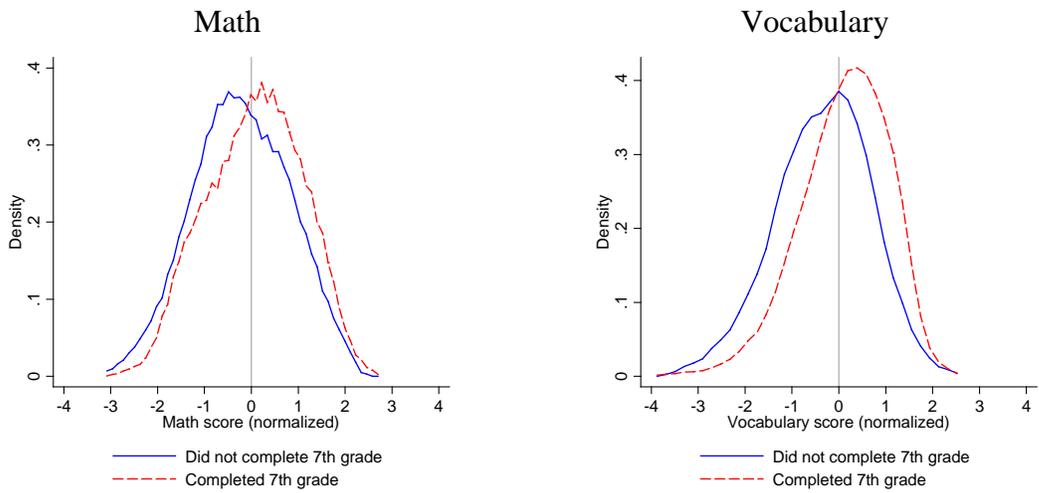
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Figure 1: Association between test scores and schooling attainment among non-recipients



Note: Scores are normalized by the mean and standard deviation of the non-recipient applicants.

Figure 2: Program effects on school enrollment and attendance

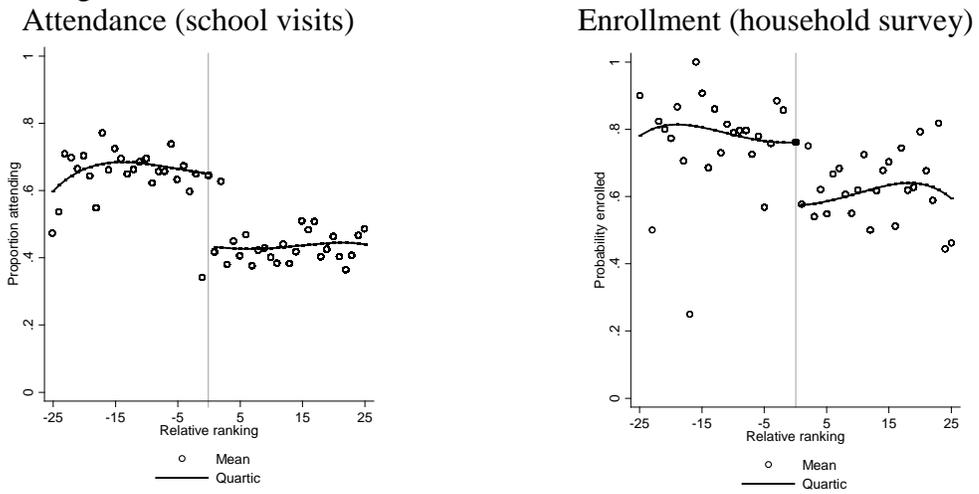
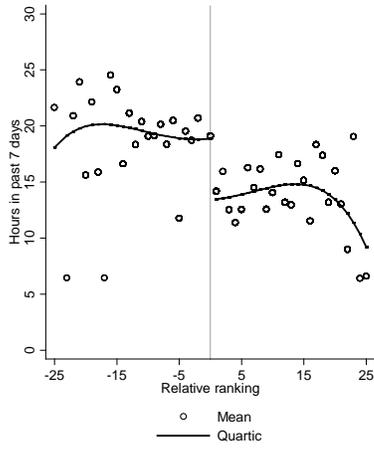
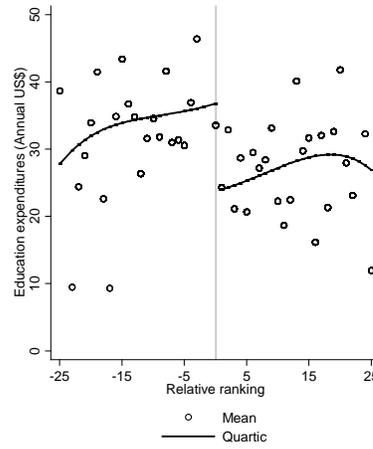


Figure 3: Program effects on time in school, education expenditures, and school attainment

Hours attended in past 7 days



Education expenditures



Schooling attained

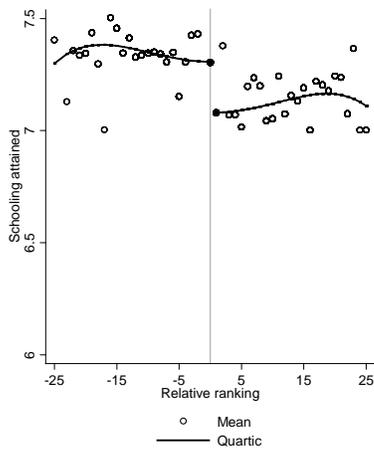
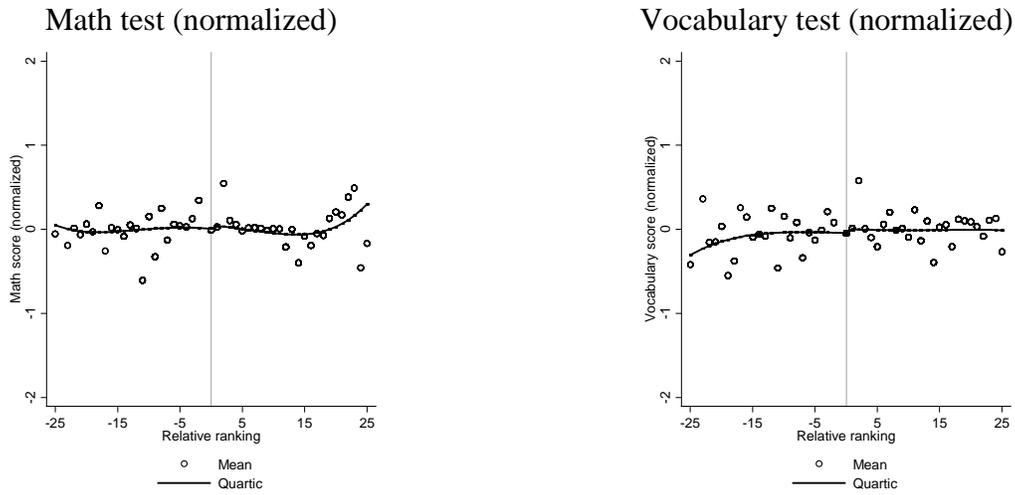


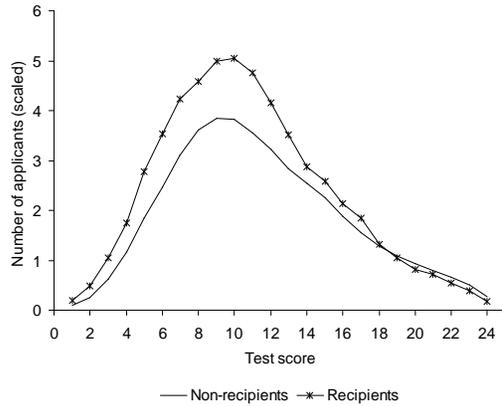
Figure 4: Test scores above and below cutoffs (mean by relative ranking, and prediction from quartic in relative ranking)



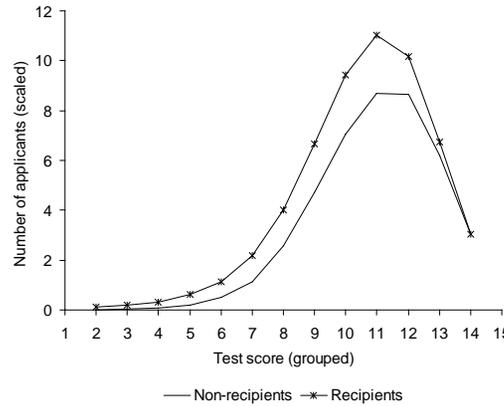
Note: Test scores normalized by mean and standard deviation among non-recipients.

**Figure 5: Number of test-takers scoring at each level of test**

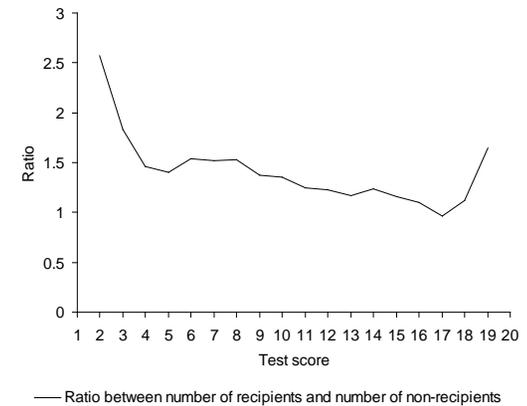
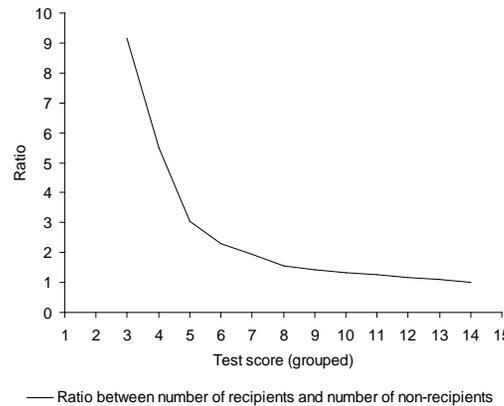
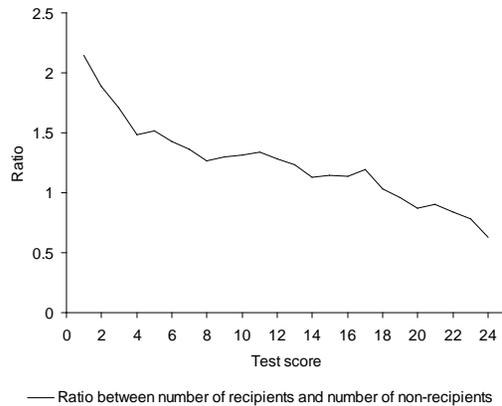
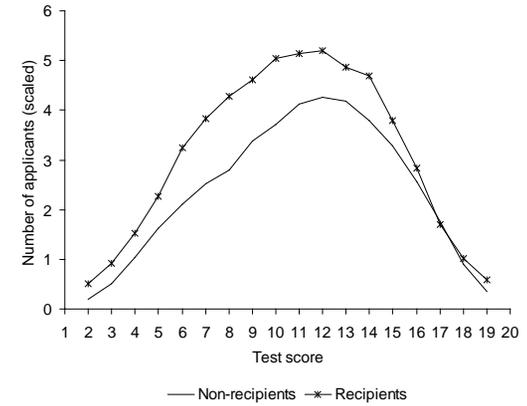
Math test administered in class—Grade 7



Vocabulary test administered in home among those enrolled in grade 8

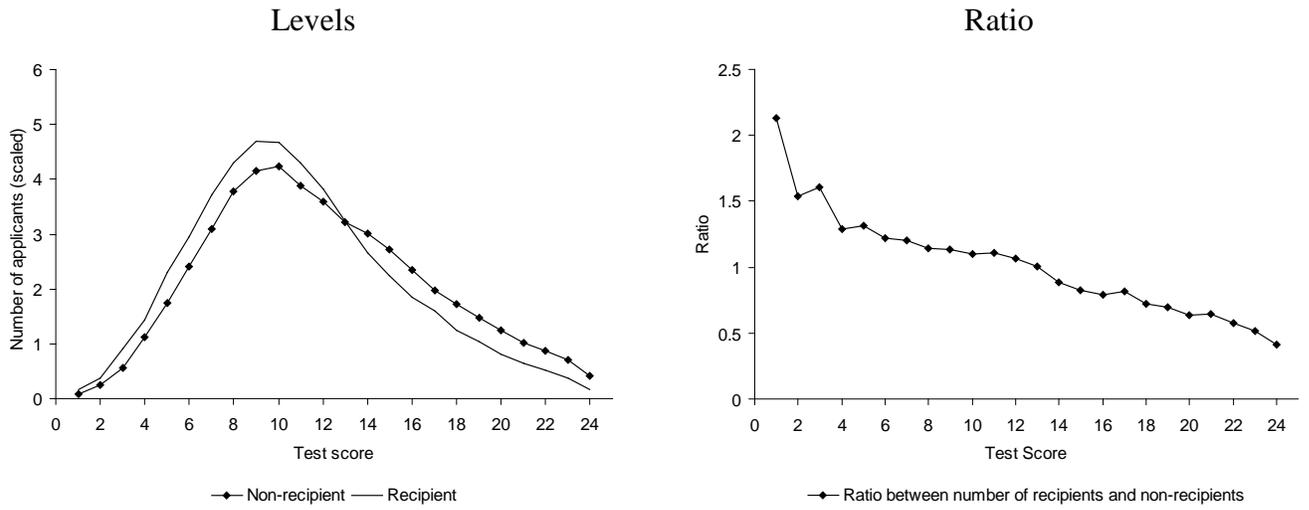


Math test administered in home among those enrolled in grade 8



Note: Vocabulary test administered in home has been remapped from values ranging from 40 to 142 into 15 categories. Data are smoothed with a moving average of window-width 3.

**Figure 6: Distribution of Grade 7 in-class mathematics test for students who will *not* drop out between Grades 7 and 8**



Note: Data are smoothed with a moving average of window-width 3.

**Table 1: Characteristics of CSP recipients and non-recipient applicants**

	Overall				Within 10 ranks of cutoff			
	Non recip- ients	Recipients	Diff.	Dummy RD	Non recip- ients	Recipients	Diff.	Dummy RD
Male	0.399	0.245	-0.154**	-0.033	0.393	0.322	-0.071*	-0.039
Live with mother	0.857	0.776	-0.081**	0.029	0.844	0.847	0.002	0.034
Mother attended school	0.490	0.357	-0.133**	0.060*	0.465	0.462	-0.003	0.064
Live with father	0.684	0.528	-0.156**	0.009	0.629	0.625	-0.005	0.049
Father attended school	0.569	0.407	-0.162**	0.018	0.558	0.487	-0.071*	0.061
Parent is civil servant	0.050	0.023	-0.027**	-0.004	0.044	0.028	-0.016	0.002
Number of other children in hh	1.299	1.353	0.054	0.114	1.369	1.409	0.040	0.051
Number of adults in household	2.964	2.740	-0.224**	0.113	2.879	2.908	0.029	0.206
Disabled household member	0.165	0.199	0.034*	-0.026	0.196	0.162	-0.034	-0.052
Own bicycle	0.759	0.543	-0.216**	-0.002	0.723	0.669	-0.055*	0.000
Own ox/horses cart	0.375	0.259	-0.116**	-0.015	0.355	0.312	-0.043	0.057
Own motorbike	0.112	0.032	-0.080**	0.003	0.079	0.060	-0.019	0.014
Own car or truck	0.018	0.001	-0.017**	0.004	0.015	0.003	-0.013	0.000
Own radio	0.390	0.254	-0.136**	-0.050	0.377	0.302	-0.075**	-0.007
Own TV	0.381	0.144	-0.238**	-0.006	0.347	0.245	-0.102**	-0.069
Roof made of solid materials	0.650	0.440	-0.210**	-0.025	0.618	0.570	-0.048	-0.043
Floors: Polished wood/Tiles	0.014	0.010	-0.005	0.006	0.007	0.015	0.009	0.011
Floors: wood planks or bamboo	0.944	0.899	-0.044**	-0.045**	0.950	0.920	-0.029*	-0.031
Drinking water: piped into house	0.003	0.001	-0.002	-0.002	0.000	0.000	0.000	0.000
Drinking water: well/pump	0.791	0.762	-0.029	-0.035	0.781	0.748	-0.033	-0.027
Drinking water: vendor purchased	0.015	0.014	-0.002	0.007	0.013	0.022	0.009	0.012
Toilet: Flush	0.031	0.005	-0.027*	0.008	0.026	0.008	-0.018	0.002
Toilet: Pit latrine	0.076	0.049	-0.027**	0.013	0.057	0.067	0.010	0.027
Lighting: Electricity from a generator	0.011	0.002	-0.009	0.002	0.004	0.001	-0.003	-0.002
Lighting: Electricity from a battery	0.515	0.312	-0.202**	-0.007	0.485	0.411	-0.073*	-0.046
Cooking fuel electricity gas kerosene	0.003	0.001	-0.002	0.001	0.000	0.003	0.003	0.008

*Note:* Based on application forms for sample of applicants covered by household survey. \*\* significant at the 1 percent level, \* at the 5 percent level.

**Table 2: Regression discontinuity estimates of impact of program on enrollment, attendance and attainment**

<i>Control function:</i>	None (raw difference)	Quartic	School-specific quartic	Quartic, Within 10 ranks of cutoff
Dependent variable: Attending on day of school visit				
Recipient	0.170 (0.011)***	0.244 (0.016)***	0.251 (0.013)***	0.250 (0.018)***
Dependent variable: Enrolled				
Recipient	0.162 (0.018)**	0.194 (0.028)**	0.200 (0.042)**	0.204 (0.046)**
Dependent variable: Hours attended in past 7 days				
Recipient	5.380 (0.521)**	5.555 (0.778)**	5.149 (1.236)**	5.689 (1.275)**
Dependent variable: Expenditures on education (Annual US\$)				
Recipient	8.950 (1.484)**	10.353 (2.108)**	11.191 (3.729)**	14.829 (4.313)**
Dependent variable: Schooling attained				
Recipient	0.206 (0.017)**	0.243 (0.026)**	0.244 (0.041)**	0.251 (0.045)**

Note: RD models include school fixed effects. Full sample size is 3225; within 10 ranks is 1098. \*\* indicates that the coefficient is significantly different from zero at the 1% level, and \* at the 5% level.

**Table 3: Regression discontinuity estimates of impact of program on learning achievement**

<i>Control function:</i>	None (raw difference)	Quartic	School-specific quartic	Quartic, Within 10 ranks of cutoff
Dependent variable: Normalized mathematics test				
Recipient	-0.055 (0.037)	0.018 (0.060)	0.009 (0.087)	-0.052 (0.092)
Dependent variable: Normalized vocabulary test				
Recipient	-0.111 (0.038)**	-0.003 (0.056)	0.015 (0.088)	-0.029 (0.098)

Note: RD models include school fixed effects. Full sample size is 3225; within 10 ranks is 1098. \*\* indicates that the coefficient is significantly different from zero at the 1% level, \* at the 5% level.

**Table 4: Heterogeneity of learning impacts**

<i>Interaction variable:</i>	Normalized Math Test		Normalized Vocabulary Test	
	Main effect	Interaction	Main effect	Interaction
Prop. teachers with upper secondary education	0.091 (0.149)	-0.128 (0.229)	0.037 (0.128)	-0.071 (0.210)
Prop. teachers with less than 5 years experience	0.168 (0.140)	-0.305 (0.239)	0.157 (0.117)	-0.325 (0.214)
Large school <sup>^</sup>	-0.019 (0.094)	0.080 (0.121)	0.057 (0.091)	-0.101 (0.116)
Prop. of students on scholarship <sup>+</sup>	0.015 (0.159)	-0.086 (0.713)	-0.123 (0.140)	0.493 (0.606)
Prop. of students on scholarship>median <sup>+</sup>	0.010 (0.080)	0.015 (0.123)	-0.078 (0.073)	0.150 (0.114)
Prop. of students on scholarship>top quartile <sup>+</sup>	0.006 (0.070)	0.014 (0.138)	-0.041 (0.063)	0.130 (0.142)
Prop. of students on scholarship<bottom quartile <sup>+</sup>	0.020 (0.071)	-0.022 (0.134)	-0.003 (0.066)	0.000 (0.123)

Note: Sample size is 3225. Regressions include school fixed effects and the quartic in the school dropout score. \*\* indicates significance at the 1% level, \* at the 5% level. <sup>+</sup> Numbers of students and teachers are based on year prior to program. <sup>^</sup>Model with “large school” includes a quartic function of total school size (the continuous underlying variable used to categorize schools).

**Table 5: Test scores, selection from 7th to 8th grade and scholarship recipient status**

<i>Parameterization of test score variable:</i>	$I(\mathbf{T}_{is}=1)$	$\text{Test}_{is}$	$I(\mathbf{T}_{is}=1)^*$ $\text{Test}_{is}$	F-test: $\beta_2+\beta_3=0$ (p-value)	F-test: $\beta_1+\beta_3=0$ (p-value)
Test score	-0.068 (0.018)**	-0.067 (0.005)**	0.041 (0.011)**	0.009	0.205
Test score>median	-0.098 (0.020)**	-0.093 (0.010)**	0.065 (0.021)**	0.126	0.132
Test score>top quartile	-0.094 (0.019)**	-0.124 (0.010)**	0.100 (0.026)**	0.341	0.823

Note: The dependent variable takes on a value of one if children dropped out of school between the June 2006 and June 2007 visits, zero otherwise. Regressions include school fixed effects and the quartic in the school dropout score. Sample size is 16,057. \*\* indicates significance at the 1% level, \* at the 5% level.

**Table 6: Association of schooling with, and impact of program on, knowledge, expectations and mental health**

	Association with schooling attained			RD estimate of program impact		
	Coef	(SE)	Obs.	Coef	(SE)	Obs.
<b>Knowledge</b>						
Number of correct HIV/AIDS preventative measures	0.336	(0.056) **	1191	0.120	(0.057) *	3185
Number of correct cigarette questions	0.066	(0.060)	1208	-0.087	(0.054)	3225
<b>Expectations</b>						
Likelihood of providing a good home	0.176	(0.046) **	1208	0.046	(0.046)	3224
Desired age for marriage	0.882	(0.161) **	1166	0.261	(0.170)	3120
Desired age for first child	1.402	(0.194) **	1163	0.255	(0.204)	3102
Child's self-assessment of rung on economic ladder	0.107	(0.087)	1208	0.003	(0.080)	3225
Child's prediction of future rung of economic ladder	0.264	(0.084) **	1200	0.004	(0.084)	3211
<b>Mental Health Scale 1<sup>+</sup></b>						
You consider your self to be a generally happy person	0.044	(0.047)	1196	-0.034	(0.048)	3191
You consider yourself to be a generally intelligent person	0.124	(0.047) **		-0.031	(0.043)	
If something goes wrong, there are people who will help you	0.102	(0.057)		0.011	(0.053)	
There is someone you feel comfortable talking about problems with	0.179	(0.062) **		0.048	(0.065)	
There are people who value and respect you	-0.020	(0.047)		-0.003	(0.049)	
There is someone who likes to do the same activities that you do	0.059	(0.056)		-0.047	(0.058)	
There is a person you view as your mentor	0.051	(0.039)		0.022	(0.041)	
It is easy for you to make close friends	-0.021	(0.052)		-0.001	(0.052)	
You feel your life has value.	0.252	(0.051) **		0.011	(0.053)	
You know the kind of person you want to become	0.071	(0.065)		-0.042	(0.061)	
You know and believe clearly what is right and wrong for you.	0.114	(0.048) *		0.043	(0.048)	
You have the freedom to make decisions affecting your life	0.010	(0.050)		-0.031	(0.043)	
<u>Average</u>	0.080	(0.021) **		-0.004	(0.021)	
<b>Mental Health Scale 2<sup>++</sup></b>						
Have you been able to concentrate	0.173	(0.027) **	1207	0.020	(0.030)	3219
Have you lost much sleep over worry <sup>+++</sup>	0.053	(0.026) *		0.019	(0.027)	
Do you feel that the activities you have done have been useful	-0.083	(0.028) **		0.023	(0.025)	
Have you felt capable of making decisions about your life	0.030	(0.022)		0.006	(0.024)	
Over the past month have you felt under strain <sup>+++</sup>	0.109	(0.028) **		0.044	(0.027)	
Have you been able to face up to your problems	0.011	(0.019)		0.004	(0.020)	
Have you felt you could overcome your difficulties	0.004	(0.016)		0.005	(0.016)	
Have you been able to enjoy your normal daily activities	0.093	(0.021) **		0.043	(0.024)	
Have you been feeling unhappy <sup>+++</sup>	0.074	(0.020) **		0.028	(0.023)	
Have you been losing confidence <sup>+++</sup>	0.040	(0.024)		0.013	(0.030)	
Have you been thinking of yourself as a worthless person <sup>+++</sup>	0.075	(0.028) **		0.069	(0.032) *	
Have you been feeling happy	0.057	(0.021) **		0.067	(0.024) **	
<u>Average</u>	0.053	(0.009) **		0.028	(0.010) **	

Note: <sup>+</sup>Responses are on a scale of 1 to 5. <sup>++</sup>Responses are 0/1 where 1 corresponds to a response of "always", except questions indicated by <sup>+++</sup> where 1 corresponds to a response of "never". \*\* indicates that the coefficient is significantly different from zero at the 1% level, \* at the 5% level.