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CAMBODIA

# Getting Girls Into School

## Evidence from a Scholarship Program

March 2006

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# Getting Girls Into School: Evidence from a Scholarship Program in Cambodia

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March 15, 2006

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**CURRENCY EQUIVALENTS**
(Exchange Rate Effective end March 2006)

| | | |
|---|---|---|
| Currency Unit | = | Riel |
| Riel 4051 | = | US$1 |
| US$1 | = | Riel 0.00024 |

FISCAL YEAR
January 1 – December 31

ABBREVIATIONS AND ACRONYMS

| | |
|---|---|
| CCT | Conditional Cash Transfer |
| CR | Cambodian Riels |
| DHS | Demographic and Health Survey |
| JFPR | Japan Fund for Poverty Reduction |
| LMC | Local Management Committee |
| OLS | Ordinary Least Squares |
| RD | Regression Discontinuity |
| RGC | Royal Government of Cambodia |
| SES | Socio-Economic Survey |

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**Abstract**

Increasing the schooling attainment of girls is a challenge in much of the developing world. In this paper we evaluate the impact of a program that gives scholarships to girls making the transition between the last year of primary school and the first year of secondary school in Cambodia. We show that the scholarship program had a large, positive effect on the school enrollment and attendance of girls. Our preferred set of estimates suggest program effects on enrollment and attendance at program schools of 30 to 43 percentage points; scholarship recipients were also more likely to be enrolled at any school (not just program schools) by a margin of 22 to 33 percentage points. The impact of the JFPR program appears to have been largest among girls with the lowest socioeconomic status at baseline. The results we present are robust to a variety of controls for observable differences between scholarship recipients and non-recipients, to unobserved heterogeneity across girls, and to selective attrition out of the sample.

JEL Codes: I20, O12

## 1. Introduction

Increasing the schooling attainment of girls is a challenge in much of the developing world. In a large number of countries, especially in the Middle East and parts of Asia and Africa, overall education for girls is low, and lags significantly behind that of boys. Nonetheless, over 180 governments have adopted universal primary education and gender parity in schooling as Millennium Development Goals. These commitments notwithstanding, there is surprisingly little evidence on policies and programs that effectively raise school attainment, including for girls (see the reviews by Glewwe 2002; Case 2004).

In this paper we evaluate the impact of a program designed to increase the enrollment of girls in secondary school in Cambodia. Cambodia is recovering from many years of internal and external strife. The Khmer Rouge regime of the 1970s and Vietnamese occupation in the 1980s had severe repercussions for all aspects of the economy and society, including the education sector (see De Walque 2004). Most Cambodian children attend some schooling, but a large share complete only very few grades. According to the 2000 Demographic and Health Survey (DHS), 85 percent of 15 to 19 year olds had completed grade 1 while only 27 percent had completed grade 7, the first year of lower secondary. These percentages are lower for rural areas, 83 and 21 respectively, and lower yet for rural girls, 78 and 17 respectively (Filmer 2005). To address these problems, the Cambodian government has initiated a series of reforms in the education sector, including scholarship programs for students from disadvantaged backgrounds.

The program we evaluate is the Japan Fund for Poverty Reduction (JFPR) scholarship program. This program selected 93 lower secondary schools and, within each of these schools, approximately 45 girls who were beginning 7$^{th}$ grade were awarded scholarships of USD45 each. The value of the scholarship is large—in 2002, mean per capita GDP in Cambodia was approximately USD300 (World Bank 2005). Once a girl is selected for a JFPR scholarship, she is automatically eligible to continue receiving a scholarship for the three years of the lower secondary cycle. The JFPR program therefore attempts to increase the fraction of girls who make the transition from primary school to lower secondary school, and to encourage girls to complete the lower secondary school cycle. In 2003/04, there were 698 lower secondary schools in Cambodia, so the JFPR scholarship program covered approximately 15 percent of lower secondary schools in the country.

Although the JFPR program is known as a "scholarship" program, it does not directly subsidize the fees paid by parents for the education of their daughters; rather, families receive cash transfers provided their daughter is enrolled in school, maintains a passing grade, and is absent without "good reason" fewer than 10 days in a year.[1] The JFPR program therefore functions much like a "conditional cash transfer" (CCT) program of the sort that has been implemented in many Latin American countries. CCT programs in Latin America have been carefully analyzed—see for example, Schultz (2004) and Behrman, Sengupta and Todd (2005) on the PROGRESA program (now re-named Oportunidades) in Mexico, Bourguignon, Ferreira and Leite (2003) on the Bolsa Escola program (now re-named Bolsa Familia) in Brazil, and the reviews by Rawlings and Rubio (2005) and Das, Do, and Ozler (2005). In this paper we assess the impact of a similar program in a context with much lower income, lower enrollment rates, and weaker institutions.

As we show below, the JFPR scholarship program had a large, positive effect on the school enrollment and attendance of girls in Cambodia. Our preferred set of estimates suggest program effects on enrollment and attendance at program schools of 30 to 43 percentage points; scholarship recipients were also more likely to be enrolled at any school (not just program schools) by a margin of 22 to 33 percentage points. The impact of the JFPR program appears to have been largest among girls with the lowest socioeconomic status at baseline. The results we present are robust to a variety of controls for observable differences between scholarship recipients and non-recipients, to unobserved heterogeneity across girls, and to selective attrition out of the sample.

## 2. Data, Variables, and Program Selection Rules

The main sources of data for the analysis in this paper are two: Application forms to the scholarship program and data on school enrollment and attendance from an unannounced school visit. The application form contains 28 questions about parental education, demographic composition of the household, ownership of various assets, housing materials, and distance to the nearest secondary school; applicants were also asked to specify the name of the secondary school they would like to attend, with the understanding that this should be one of the 93 schools that were eligible for the scholarship program. These forms were completed by a girl in class or at home; some provisions were made for teachers to verify that the data provided was accurate.

[1] Scholarship recipients agree to use funds towards education, but no attempt was made to enforce this agreement.

We use the information on the application forms to correct for observable differences across girls. Two things are worth noting here. First, application forms were handed out at the primary "feeder schools" to the 93 secondary schools included in the scholarship program. In practice, selection of the feeder schools, and selection of the girls who were encouraged to fill out application forms by the primary school teachers appears to have been somewhat ad hoc. Second, almost 30 percent of the application forms were not filled out completely. Because we use the characteristics on the application form to correct for differences across girls, the sample for our analysis is limited to girls whose applications were complete. Appendix One suggests that the differences between the full sample of girls and the sample of girls with completed applications are generally small. Nevertheless, the fact that our data are not from a random sample of 6th grade girls in the primary feeder schools to the JFPR secondary schools, or even of all girls who filled out applications, should be kept in mind when interpreting the results in this paper.

Once application forms had been filled out, they were forwarded from the primary school to the Local Management Committee (LMC) of the relevant JFPR secondary school. The LMCs were then tasked with identifying the 45 girls who were most needy and awarding them scholarships. To assist in this process, LMCs were given a set of weights that were to be given to each question, and a formula to aggregate responses into a final score. According to program administrators, these weights were developed somewhat arbitrarily. For example, applicants who had between 3 and 5 brothers received 1 extra point, those with more than 5 brothers 2 extra points; applicants with 1 to 3 sisters received 1 extra point, those with 4 or 5 sisters received 2 extra points, and those with 6 or more sisters 3 extra points. (More points were meant to increase the probability of receiving a scholarship.) A number of characteristics, including having at least one parent who had completed at least primary school were meant to disqualify applicants from scholarships. However, the LMCs were given considerable discretion over the specifics of how to apply the selection rules—the *Programme Implementation Manual* states that "LMCs will have flexibility to adapt this process as they see fit." Once LMCs had chosen the girls who would be awarded scholarships, all applicants were notified whether they had been selected.

As we show below, it is useful for the analysis in this paper to have a measure of socioeconomic status (SES) that is close to that used by the LMCs to select scholarship recipients. We compare two ways of aggregating the information in the application form. First, we use the weights provided by the program administrators, and rank girls applying to a school by their score. By this measure, 75 percent of the girls are among the 45 girls with the lowest score and are scholarship recipients; the remaining 25 percent are either girls who did not receive a scholarship despite the fact that they were among the 45 girls with the lowest score, or they were girls who received a scholarship despite the fact that they had relatively high scores or characteristics that were meant to disqualify them from scholarships. Second, we aggregate all of the responses to questions in the application form by principal components, and give each girl a score based on the value of the first principal component (see Filmer and Pritchett 2001). By this measure, 84 percent of the girls are ranked among the 45 poorest girls and are scholarship recipients. This evidence is consistent with the LMCs having successfully identified girls with the lowest SES on the basis of the responses to the application form without strictly following the weights assigned to each individual question. Because it better predicts scholarship receipt, we use the first principal component as our preferred measure of SES. In the discussion below, we refer to this as an applicant's "score".

The second source of data for this evaluation is based on an unannounced school visit to each one of the 93 program schools. These visits were carried out by an independent firm, hired specifically for this purpose. Enumerators were given a list of applicants to each JFPR school, without information on their scholarship status. Note that girls applied for the scholarship in $6^{th}$ grade, during the 2002/2003 school year, scholarships were first awarded during the 2003/04 school year, and school visits took place during the course of the 2004/05 school year. During the school visit, enumerators checked $8^{th}$ grade enrollment rosters and physically verified whether or not a girl was attending an $8^{th}$ grade class on the day of the visit.

On the basis of the school visits, we construct three measures of enrollment and attendance. The first is a dummy variable that takes on the value of one if the girl in question was enrolled in $8^{th}$ grade at the JFPR school she had applied to. Note that a girl would only figure as enrolled by this measure if she enrolled in the school she applied to in two consecutive years, and did not repeat a grade. The second measure is physically verified attendance, a dummy variable that takes on the value of one if a girl was present in an $8^{th}$ grade class at the time of the visit by the enumerators. This variable depends on enrollment; attendance, conditional on enrollment; and adequate grade progress in a JFPR school. The third variable is a dummy variable that takes on the value of one if a girl is enrolled at any school, regardless whether this is a JFPR school or not. To construct this third variable, we proceeded as follows. When an applicant to a given JFPR school did not appear on the $8^{th}$ grade enrollment rosters, enumerators asked other $8^{th}$ grade students in that school whether they knew the missing girl. If someone knew the girl in question, they were asked whether they knew she was definitely enrolled elsewhere; definitely not enrolled; or whether respondents were not certain about the enrollment status of the missing girl. On the basis of the enrollment registers and the questions asked of $8^{th}$ grade girls during the school visits, we were able to establish the enrollment status of 95.2 percent of all of the girls who had completed scholarship applications. One way to think of the girls whose enrollment status could not be established is as attritors out of the sample. The attrition rate, 4.8 percent, is low—as a point of comparison, attrition in a recent evaluation of school voucher scheme in Colombia was almost 50 percent (Angrist et al. 2002). Nevertheless, we carefully consider the extent to which selective attrition could bias our estimates of program impact.

Table 1 compares enrollment, attendance, and the characteristics of girls who were awarded scholarships and those who were not. The table shows that, on average, scholarship recipients had significantly lower socioeconomic status than non-recipients: Recipients had parents with lower education levels; they were more likely to live in a hut or a house with an earthen floor, and less likely to have houses made of high-grade materials like cement, brick, tiles, metal or fiber; recipients were also less likely to own any one of a number of means of transportation, less likely to own more than a hectare of land, less likely to regularly lend money, and more likely to have debts. These results confirm that the LMCs were successful at targeting scholarships to girls with lower socioeconomic status. Table 1 also shows that 86.7 percent of recipients, but only 64.6 percent of non-recipients were enrolled in a JFPR program school; 80.2 percent of recipients, but only 57.9 percent of non-recipients were attending a JFPR school on the day of the visit; for those whose enrollment status could be established, 89.5 percent of scholarship recipients but only 76.5 percent of non-recipients were enrolled in any school, JFPR or otherwise.[2] Table 1 is consistent with a large program effect on school enrollment and attendance.

We next focus on comparisons of outcomes for girls with different levels of socioeconomic status, as measured by their score. In our sample, girls ranked 45 or lower by the score were 4.3 percentage points more likely to be enrolled at a JFPR school than girls ranked 46 or higher (p-value: 0.04); 3.9 percentage points more likely to be present on the day of the school visit (p-value: 0.08); and 2.0 percentage points more likely to be enrolled at any school, JFPR or otherwise (p-value: 0.29). Note that we would expect that, in the absence of a program, girls with lower SES would be *less* likely to be enrolled and attending school. These comparisons, which abstract from any possible endogeneity of scholarship receipt, therefore provide further evidence of a JFPR program effect.

[2] Comparisons for the full sample of girls, not just those with completed applications; also suggest that scholarship recipients were more likely to be enrolled and attending school. In the full sample, the difference in the probability of enrollment at a JFPR school is 22.1 percentage points; the difference in the probability of attending is 21.9 percentages; and the difference in the probability of enrollment at any school, not just a JFPR school, is 12.1 percentage points. All these differences are significant at the 1 percent level or better.

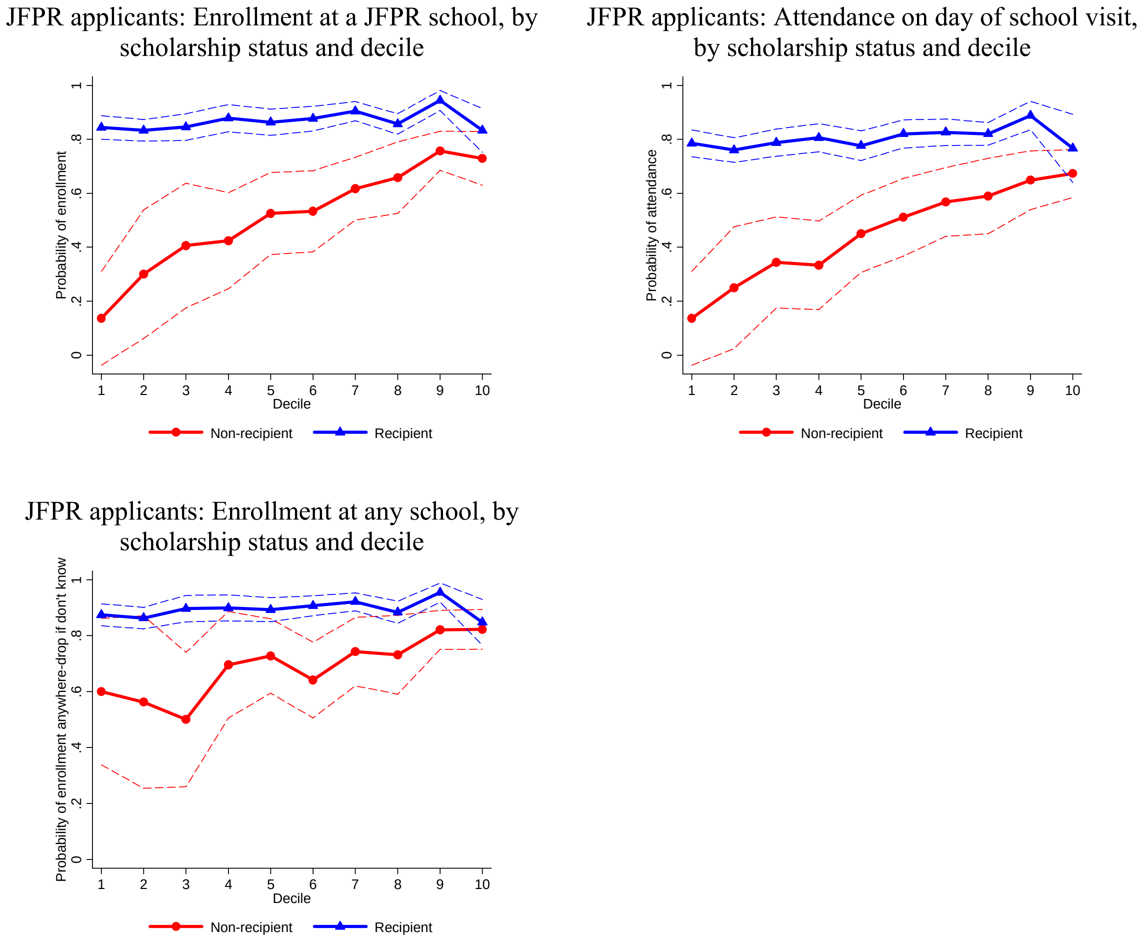
### Table 1: Characteristics of Scholarship Recipients and Non-recipients at Baseline

| | Recipients | Non-recipients | Difference | P-value |
|---|---|---|---|---|
| **Outcomes** | | | | |
| Enrolled in JFPR school | 0.87 | 0.65 | 0.22 | 0.00 |
| Attending on the day of school visit | 0.80 | 0.58 | 0.22 | 0.00 |
| Enrolled in any school | 0.90 | 0.77 | 0.13 | 0.00 |
| **Characteristics of applicants** | | | | |
| Parents own business | 0.00 | 0.08 | -0.08 | 0.02 |
| Either parent is a government employee | 0.01 | 0.14 | -0.13 | 0.00 |
| Parent lends money regularly | 0.00 | 0.06 | -0.05 | 0.05 |
| Parent completed primary school | 0.09 | 0.40 | -0.30 | 0.00 |
| Parent completed secondary school | 0.01 | 0.06 | -0.05 | 0.00 |
| Main part of house made of cement or brick | 0.00 | 0.08 | -0.08 | 0.00 |
| Roof made of tiles, metal or fibre | 0.15 | 0.56 | -0.41 | 0.00 |
| Land ownership by hh > 1 hectare | 0.07 | 0.33 | -0.26 | 0.00 |
| Own a large asset (>1M riels) | 0.01 | 0.15 | -0.14 | 0.00 |
| Own a truck | 0.00 | 0.01 | -0.01 | 0.02 |
| Own a car | 0.00 | 0.01 | -0.01 | 0.06 |
| Live with both parents | 0.70 | 0.83 | -0.13 | 0.00 |
| Live with one parent | 0.23 | 0.19 | 0.04 | 0.27 |
| Live in a hut | 0.53 | 0.30 | 0.23 | 0.00 |
| Earthen floor | 0.35 | 0.28 | 0.07 | 0.10 |
| Family has motorbike or remorque | 0.04 | 0.27 | -0.23 | 0.00 |
| Family has bicycle | 0.48 | 0.72 | -0.24 | 0.00 |
| Family has ox and cart | 0.20 | 0.44 | -0.24 | 0.00 |
| Family has pony and trap | 0.02 | 0.05 | -0.03 | 0.22 |
| Family has no means of transportation | 0.49 | 0.25 | 0.24 | 0.00 |
| Family has debts > 100,000 riels | 0.79 | 0.54 | 0.25 | 0.00 |
| Mean number of brothers | 2.26 | 2.36 | -0.10 | 0.08 |
| Mean number of sisters | 2.84 | 2.70 | 0.14 | 0.10 |
| Applicant disabled | 0.04 | 0.02 | 0.01 | 0.05 |
| Other hh member disabled | 0.15 | 0.13 | 0.02 | 0.57 |
| Applicant or other hh member has disease | 0.32 | 0.20 | 0.12 | 0.00 |
| Distance to secondary school (km.) | 4.17 | 3.61 | 0.56 | 0.01 |
| **Number of observations** | **2765** | **858** | **3623** | |

*Note***:** Outcomes are measured at the time of the school visit; applicant characteristics are based on the application form.

Finally, we calculate the mean enrollment and attendance levels, separately for scholarship recipients and non-recipients, by deciles of the score. These results are presented in Figure 1. The lines corresponding to girls who were turned down for scholarships show that, among these girls, there are clear socioeconomic gradients in enrollment and attendance. Figure 2 presents a similar figure based on the 2000 Demographic and Health Survey; here too there is a clear schooling gradient.[3] Figure 1 shows, however, that the gradient in enrollment and attendance has essentially disappeared among scholarship recipients. This is consistent with larger program effects among girls with the lowest SES, a point we examine more carefully below.

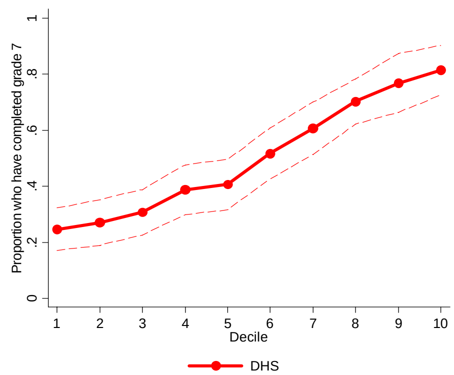
**Figure 1: Attendance and enrollment status by decile, JFPR application data**




*Note*: Dashed lines show 95 percent confidence intervals.

[3] The exact list of socioeconomic variables in the DHS is not identical to that on the JFPR application form. We derive the index of socioeconomic status in the DHS from variables describing: the ownership of a bicycle, cart, boat, motorbike, car, truck, radio, television; the conditions of the dwelling such as hard roofing and finished flooring; the availability of electric lighting; the main source of drinking water; the type of toilet facilities; and the main type of cooking fuel used.

**Figure 2: Proportion of girls who complete grade 7 among those who completed grade 5, by economic status decile, DHS data**




*Note*: Dashed lines show 95 percent confidence intervals.

## 3. Empirical strategy

The empirical strategy in this paper is based on comparisons between scholarship recipients (the "treatment" group) and non-recipients (the "control" group). The analysis begins with OLS regressions of a dummy variable that takes on the value of one if girl i is enrolled in school at the time of the follow-up survey (in the enrollment regressions) or attending school (in the attendance regressions), $Y_{it}$, on a set of school-level fixed effects, $\alpha_c$, a vector of baseline characteristics from the application form, $\mathbf{X}_{it-1}$, and a dummy variable that takes on the value of one if a girl was a scholarship recipient, $S_i$:

(1) $$Y_{it} = \alpha_c + \mathbf{X}_{it-1}\boldsymbol{\beta} + S_i\delta + \varepsilon_{it},$$

The parameter $\delta$ is a measure of the difference in the probability of enrollment or attendance between girls who received a scholarship and those who did not. Linear probability models are used to estimate (1); estimation by probit yielded very similar results.

As an alternative to OLS, we use propensity score matching to estimate program impact (Rosenbaum and Rubin 1983; Heckman, Ichimura, and Todd 1997). As a first step this involves a regression of a dummy variable that takes on a value of one if a girl received a scholarship:

(2) $S_i = \theta_c + \mathbf{X}_i\boldsymbol{\eta} + \mu_i$,

This regression is then used to predict the probability that each girl in the sample was awarded a scholarship, $\hat{S}_i$, also known as the propensity score. The second step involves nearest-neighbor matching on the basis of the propensity score. Prior to matching, observations for which there is no "common support" are discarded. These are scholarship recipients who, because of their observable characteristics, had a probability of being awarded a scholarship that was so high that there are essentially no comparable girls who did not receive a scholarship, as well as girls who did not receive a scholarship who, because of their observable characteristics, had a probability of being awarded a scholarship that was so low that there are essentially no comparable girls who received a scholarship. Once observations have been matched, the estimated program impact is given by the mean difference in enrollment or attendance between the matched pairs of recipients and non-recipients. Matching is a nonparametric alternative to OLS, and is often believed to be less subject to mis-specification biases.

We do not believe that selection on unobservables is an important concern for our analysis. Students were enrolled in *primary* school at the time they applied for scholarships, while the selection of beneficiaries was done by the LMC of a JFPR *secondary* school. In general, members of the LMC would not know the girls in question, many of whom would live in different villages or urban areas; LMC members would therefore have little information (if any) on the academic ability or socioeconomic status of a given applicant above and beyond the information provided on the application form.

Although selection on unobservables seems unlikely, we use regression discontinuity (RD) to estimate treatment effects that are arguably robust to unobserved differences between treatment and control groups. The basic logic of RD exploits a discontinuous jump in the probability of receiving an intervention for observations above and below an eligibility threshold. If the control function used to capture the relationship between the covariate that determines eligibility and outcomes is correctly specified, and if there is no discreet jump in unobservables at the threshold, RD can provide estimates of local program effects that are as good as those derived from a randomized experiment (see Hahn, Todd, and van der Klaauw 2001, and Lee 2005). In the literature on schooling outcomes, RD approaches have recently been used to estimate the impact of class size on test scores in Israel (Angrist and Lavy 1999) and Bolivia (Urquiola 2006), financial aid on college enrollment in the United States (van der Klaauw 2002), financial incentives for schools on test score outcomes in Chile (Chay, McEwan, and Urquiola 2005), and pension income on enrollment in South Africa (Edmonds 2005).

The JFPR scholarship program established a cut-off in the maximum number of scholarships awarded per school at 45. In general, the school LMCs successfully identified girls with lower SES and awarded them scholarships. Figure 3 shows that the probability of receiving a scholarship for the 40 applicants with the lowest score was very high: 0.87. Conversely, the probability of receiving a scholarship for girls ranked 50 or higher by their score was very low: 0.13. Between these ranks—that is, roughly corresponding to the threshold established by the fact that 45 scholarships could be awarded per school—there is a sharp drop in the probability of receiving a scholarship. The RD estimates of program impact exploit this discontinuity around the threshold given by the fixed number of scholarships within a school.

**Figure 3: Probability of receiving a scholarship, within-school ranking of score**


*Note*: The score is given by aggregating applicant characteristics by principal component analysis, as described in the text. These scores are ranked within schools.

Because the relationship between the rank of an application and scholarship receipt is not deterministic, this is a case of "fuzzy" (rather than "sharp") RD (Hahn, Todd, and van der Klaauw 2001). In a spirit similar to other applications of fuzzy RD, we estimate the following equation:

(3) $Y_{it} = \chi_c + f(SES_i) + S_i\pi + \nu_{it}$,

where SES is the value of the first principal component of the composite measure of socioeconomic status, and the function f(SES) is a flexible parametrization, such as a cubic. Equation (3) is then estimated by two-stage least squares, with $S_i$ instrumented with a dummy variable that takes on the value of one if a girl is ranked below 45 by the composite measure of SES. We also verify that the results we report are not sensitive to the exact parametrization used for the function f (SES), or to the choice of threshold within the 41-49 range for the SES rank within a school.

Finally, the paper presents evidence of heterogeneity in the JFPR program effects. For this purpose, we construct three dummy variables for girls who are below the median of the principal components measure of SES, girls for whom neither parent has completed primary school, and girls who lived more than 4 kilometers from the JFPR secondary school at the time they completed the application. We then run separate regressions which include one of these dummies $D_i$, the dummy variable for whether a girl received a scholarship $S_i$, and the interaction term $(D_i*S_i)$:

(4) $Y_{it} = \psi_c + \mathbf{X}_{it-1}\gamma + \tau D_i + S_i\phi_1 + (D_i*S_i)\phi_2 + e_{it}$,

Interpretation of the coefficients on these variables is straightforward: For example, in the specification that tests for heterogeneity of treatment effects by socioeconomic status, the coefficient $\tau$ is an estimate of the difference in enrollment (or attendance) between girls of "low" and "high" socio-economic status; the scholarship effect for high-SES girls is given by the coefficient $\phi_1$; the corresponding effect for low-SES girls is given by the sum of the coefficients $\phi_1$ and $\phi_2$. If $\phi_2$ is statistically significant, there is evidence of heterogeneity of treatment effects.

## 4. Results

### *A. Main results of program impact*

The main results of program impact are presented in Table 2. We present estimates of program effects with three dependent variables: Enrollment at the JFPR school that a girl applied to, school attendance at this school on the day of the unannounced school visit, and enrollment at any school. The first three columns correspond to OLS regressions of enrollment or attendance. Column 1 presents the raw difference between scholarship recipients and non-recipients; this corresponds to the differences in means in Table 1, and is included for completeness. Column 2 is based on regressions that include all of the characteristics on the application form; column 3 supplements these controls with school fixed effects. The fourth column reports program effects based on propensity score matching; the matching equation includes all of the controls and school fixed effects. The last two columns correspond to the regression-discontinuity estimates of program impact. Both sets of RD results include the cubic in socioeconomic status and school fixed effects. Column 5 corresponds to a reduced-form model; in this specification, the dummy variable that takes on the value of one if a girl had a score that placed her among the 45 girls with the lowest SES in her school enters directly in the regression. Column 6 corresponds to the instrumental variables specification; in this specification, the dummy variable for scholarship recipients is instrumented with the dummy variable for girls ranked below 45 by the composite measure of SES.

Table 2 suggests that the JFPR scholarship program had a large, positive effect on school enrollment and attendance. Controlling for observable differences between scholarship recipients and non-recipients, scholarships had an impact on enrollment at a JFPR school or attendance on the day of the school visit of between 29 and 43 percentage points; the estimates based on propensity score matching tend to be somewhat larger.[4] The regression discontinuity results in the last column suggest a program impact of 30 percentage points on enrollment, and 43.6 percentage points on attendance, although the standard errors of these estimates tend to be much larger. If the identifying assumptions hold, these estimates should be purged of possible biases introduced by any correlation between the measure of scholarship receipt $S_i$ and the regression error term.[5]

The final row of the table presents estimates of the impact of the JFPR program on enrollment in any school, regardless whether this is a JFPR school or not. These estimates of program impact drop observations from the sample whose enrollment status is unknown; below, we return to calculations that bound the effects of selective attrition. The estimated program effects on enrollment at any school are generally smaller than those for enrollment at a JFPR school. The smaller program effects when the dependent variable is enrollment at any school, not just a JFPR school, is not entirely unexpected: JFPR

[4] These estimates are not sensitive to how, exactly, we match scholarship recipients and non-recipients. For example, we experimented with various ways of trimming observations from the samples of recipients and non-recipients to ensure common support, as well as with specifications that did not include the school fixed effects; neither of these alternative specifications for the matching equation had an appreciable effect on our estimates of program impact.

[5] To test the robustness of our results, we also experimented with other parametrizations, including formulations that included a quadratic and a log term, or a quartic in SES; the estimated program effects are always positive, and are reasonably close to those reported in Table 2. The estimated program effect on enrollment in a JFPR school in the specification with a quadratic and a log term in SES is 0.255, with a standard error of 0.121 that in the specification with a quartic in SES is 0.291, with a standard error of 0.249. Similarly, the results are not sensitive to the choice of threshold between 41 and 49. For example, the estimated program effect on enrollment in a JFPR school in the cubic specification with a threshold at 42 is 0.340 (with a standard error of 0.155); when the threshold is 48, the estimated program effect is 0.361 (with a standard error of 0.255).

scholarships were not portable—students who were awarded scholarships could only receive them if they enrolled in the school they applied to. Girls who were selected for JFPR scholarships would therefore be very likely to enroll in a JFPR school if they were to enroll anywhere; similar incentives did not exist for girls who were turned down for scholarships. Nevertheless, the results in Table 2 suggest substantial and significant program effects of the scholarship program on enrollment at any school, not just at JFPR schools.

**Table 2: Basic Results on Program Impact**

| | (1) Raw difference | (2) OLS | (3) OLS + school f.e. | (4) Matching | (5) RD (reduced form) | (6) RD (instrumental variables) |
|---|---|---|---|---|---|---|
| **Enrolled at JFPR school** | 0.222*** | 0.292*** | 0.303*** | 0.413*** | 0.065* | 0.302* |
| | (0.018) | (0.021) | (0.022) | (0.056) | (0.036) | (0.166) |
| **Attending JFPR school on day of visit** | 0.223*** | 0.299*** | 0.313*** | 0.426*** | 0.094** | 0.436** |
| | (0.018) | (0.022) | (0.023) | (0.056) | (0.040) | (0.188) |
| **Enrolled at any school** | 0.130*** | 0.178*** | 0.216*** | 0.333*** | 0.043 | 0.184 |
| | (0.017) | (0.021) | (0.022) | (0.058) | (0.033) | (0.141) |

*Note*: Parameter estimates and standard errors corrected for heteroskedasticity (in parentheses). *** Significant at the 1 percent level; ** at the 5 percent level; * at the 10 percent level. Column 1 includes no controls; column (2) includes the full set of controls in table 1; column (3) includes these controls and school fixed effects; nearest-neighbor matching in column (4) is done on the basis of the full set of controls and school fixed effects; the regression discontinuity specifications in columns (5) and (6) include a cubic in socioeconomic status, and school fixed effects. The sample size for the first and second rows is 3623, except for the matching estimates, which drop observations for which there is no common support; the sample size for these matching estimates is 2601. The third row drops girls whose enrollment status could not be established; the sample size is for the regressions is 3472, that for the matching estimates is 2545.

*B. Bounding the effects of selective attrition*

We next turn to a discussion of how selective attrition could bias our estimates of program effects on the measure of enrollment at any school. It is difficult to correct for selective attrition in the absence of a credible instrument—a variable that predicts the probability of attrition, but is not correlated with the error term in the enrollment regressions (Fitzgerald, Gottschalk and Moffitt 1998). Since no such variable was apparent in our data, we calculated program effects under alternative assumptions about the enrollment behavior of girls whose enrollment status could not be established. Note that the enrollment rate among scholarship recipients is given by the following identity:

(5) $$E_t = \phi_{ot}E_{ot} + \phi_{at}E_{at}$$

where $E_t$ is the proportion of girls who are enrolled in any school. The subscript t corresponds to "treated" girls, and the subscripts o and a correspond to girls who are observed (not attrited) and not observed (attrited), respectively; $\phi_{ot}$ is the proportion of scholarship recipients who have not attrited; $E_{ot}$ is the enrollment rate in this group. $\phi_{at}$ is the proportion of scholarship recipients who have attrited; $E_{at}$ is the enrollment rate in this group. $\phi_{ot}$, $E_{ot}$, and $\phi_{at}$, but not $E_{at}$, can be calculated from the data; $E_t$ can then be estimated under alternative values for $E_{at}$. Similarly, for girls who were not awarded scholarships:

(5′) $E_c = \phi_{oc}E_{oc} + \phi_{ac}E_{ac}$

where the subscript c corresponds to girls in the "control" group. Here too $\phi_{oc}$, $E_{oc}$, and $\phi_{ac}$, and assumptions have to be made regarding $E_{ac}$. Following Smith and Welch (1989), we present values for $(E_t\text{-}E_c)$ for values for $E_{at}$ and $E_{ac}$ ranging from 0 to 1.[6] This corresponds to estimates of program impact on enrollment under alternative assumptions for the enrollment status of girls who had attrited out of the sample.

The results from these calculations are presented in Table 3. Consider first a scenario in which *none* of the missing girls are enrolled; under this assumption, total enrollment among recipients is 0.675, total enrollment among non-recipients is 0.879, and the estimated JFPR program effect on enrollment is 20.4 percentage points. The corresponding scenario in which *all* of the missing girls are assumed to be enrolled suggests program effects of 10.4 percentage points. Following Manski (1989), it is also possible to calculate upper and lower bounds on the treatment effects. The upper bound corresponds to a scenario in which all the missing girls who are scholarship recipients are enrolled, while all missing girls who did not receive scholarships are unenrolled (program effect: 0.897-0.675 = 22.2 percentage points); the lower bound corresponds to a scenario in which all the missing scholarship recipients are unenrolled, while all missing girls who did not receive scholarships are enrolled (program effect: 0.879-0.793 = 8.6 percentage points). The bounds do not include zero, which suggests that selective attrition on its own cannot account for the fact that the estimated program effects on enrollment are positive.

[6] Smith and Welch parametrize this as the enrollment ratio between attritors and non-attritors. We report this ratio in columns 2 and 5 of Table 3.

**Table 3: Bounding the Effects of Selective Attrition on Estimates of Program Impact**

| **Non-recipients**<br>Enrollment among non-attritors: 0.765<br>Proportion who attrited: 0.118 | | | **Recipients**<br>Enrollment among non-attritors: 0.895<br>Proportion who attrited: 0.018 | | |
|---|---|---|---|---|---|
| Enrollment: attritors | Ratio of enrollment: (attritors / non-attritors) | Overall enrollment | Enrollment: attritors | Ratio of enrollment: (attritors / non-attritors) | Overall enrollment |
| 0.0 | 0.000 | 0.675 | 0.0 | 0.000 | 0.879 |
| 0.1 | 0.131 | 0.687 | 0.1 | 0.112 | 0.881 |
| 0.2 | 0.261 | 0.698 | 0.2 | 0.223 | 0.882 |
| 0.3 | 0.392 | 0.710 | 0.3 | 0.335 | 0.884 |
| 0.4 | 0.523 | 0.722 | 0.4 | 0.447 | 0.886 |
| 0.5 | 0.654 | 0.734 | 0.5 | 0.559 | 0.888 |
| 0.6 | 0.784 | 0.745 | 0.6 | 0.670 | 0.890 |
| 0.7 | 0.915 | 0.757 | 0.7 | 0.782 | 0.892 |
| 0.8 | 1.046 | 0.769 | 0.8 | 0.894 | 0.893 |
| 0.9 | 1.177 | 0.781 | 0.9 | 1.006 | 0.895 |
| 1.0 | 1.307 | 0.793 | 1.0 | 1.117 | 0.897 |
| 0.123 | 0.161 | 0.689 | 0.101 | 0.113 | 0.881 |

Is it possible to make a plausible guess about where the program effects fall within these bounds? Recall that, by definition, attritors are not enrolled in a JFPR school. Arguably, the probability of enrollment for attritors who did not receive a scholarship could be approximated by the enrollment probability of other girls who were turned down for scholarships and did not enroll in a JFPR school, but whose enrollment status could be established on the basis of the answers provided during the school visits; this probability is 0.123. Similarly, the probability of enrollment for attritors who received a scholarship could be approximated by the probability of enrollment of other girls who received scholarships and did not enroll in a JFPR school, but whose enrollment status could be established; this probability is 0.101. Calculating the difference in total enrollment rates ($E_t$-$E_c$) under these values for enrollment of attrited recipient and non-recipient girls, the estimated JFPR program effect is 0.191. Note, finally, that these calculations of bounds do not adjust for observable or unobservable differences between girls who received JFPR scholarships and other girls—they correspond to the raw difference in enrollment in Table 2. Given that scholarship recipients had lower socioeconomic status, on average, than non-recipients, the results presented in Table 3 are therefore likely to be downward-biased estimates of the true JFPR program effects under different assumptions about the enrollment behavior of attrited girls.

*C. Heterogeneity of treatment effects*

We next turn to estimates of heterogeneity of program effects by the aggregate measure of socioeconomic status, by parental education, and by distance to school. These results are presented in Table 4. They correspond to OLS specifications that include all of the controls from the application form and the school fixed effects (Table 2, column 3).

The first panel in the table shows that the impact of the program is larger among girls from poorer households; for example, the first row in this panel suggests that the JFPR program increased enrollment at JFPR schools by 24.3 percentage points for girls with above-median SES, and by 43.9 percentage points for girls with below-median SES. The second panel of the table suggests that there is also heterogeneity of treatment effects by the level of parental education; the first row in this panel suggests enrollment effects of 24.2 percentage points for girls who have at least one parent who had completed at least primary school, compared to enrollment effects of 32.3 percentage points for girls where neither parent had completed primary school. The last panel suggests that enrollment effects are larger among girls who live at least four kilometers from the JFPR secondary school they applied to; the first row in this panel suggests enrollment effects of 25.4 percentage points for girls who live less than four kilometers from the school, and 37.2 percentage points for girls who live further away. Note that LMC members of the JFPR secondary school should be less likely to know girls who live far away, which minimizes the potential for selection on unobservables for this group. The fact that there are significant program effects among girls who live far from the school they applied to provide further reassurance that the estimated program effects are not driven by selection on unobservables.

The coefficients on the dummy variables for low SES, low levels of parental education, and girls living in more remote areas all suggest that, in the absence of the scholarship program, girls in these households were less likely to be enrolled in and attending school, as one would expect. However, the difference in enrollment and attendance by SES, parental education and distance to school is dampened or disappears entirely among girls who receive the JFPR scholarships. For example, the first column suggests that, in the absence of the scholarship, there is a difference of 24.3 percentage points in enrollment between girls with high and low SES; however, among girls who receive the scholarship, there is no difference in enrollment between girls with high and low SES. Put differently, because program effects are larger among girls with lower SES, the scholarship program eliminated the gradient between SES and school enrollment. The regression results in Table 4 therefore confirm the patterns observed in Figure 2. The scholarship program also reduced the gradient by parental education, although the compensatory effect of the program is not large enough to bring girls with low levels of parental education to the same enrollment and attendance levels as those with higher levels of parental education.

**Table 4: Heterogeneity of Treatment Effects**

| | (1) Enrollment at a JFPR school | (2) Attendance on the day of the school visit | (3) Enrollment at any school |
|---|---|---|---|
| By socioeconomic status | | | |
| Dummy variable $D_i$=1 | -0.195*** | -0.187*** | -0.094* |
| | (0.050) | (0.050) | (0.054) |
| Dummy variable $S_i$=1 | 0.243*** | 0.264 *** | 0.191*** |
| | (0.025) | (0.027) | (0.024) |
| Interaction term ($D_i$*$S_i$) | 0.196*** | 0.164*** | 0.096* |
| | (0.048) | (0.048) | (0.053) |
| By parental education | | | |
| Dummy variable $D_i$=1 | -0.126*** | -0.072** | -0.107*** |
| | (0.032) | (0.034) | (0.030) |
| Dummy variable $S_i$=1 | 0.242*** | 0.297*** | 0.166*** |
| | (0.033) | (0.038) | (0.030) |
| Interaction term ($D_i$*$S_i$) | 0.081** | 0.021 | 0.068** |
| | (0.037) | (0.042) | (0.034) |
| By distance to school | | | |
| Dummy variable $D_i$=1 | -0.112*** | -0.125*** | -0.044 |
| | (0.038) | (0.039) | (0.038) |
| Dummy variable $S_i$=1 | 0.254*** | 0.265*** | 0.187*** |
| | (0.024) | (0.026) | (0.024) |
| Interaction term ($D_i$*$S_i$) | 0.118*** | 0.123*** | 0.059 |
| | (0.040) | (0.042) | (0.040) |

*Note*: Parameter estimates and standard errors (in parentheses). Standard errors correct for heteroskedasticity and clustering at the school level. *** Significant at the 1 percent level; ** at the 5 percent level; * at the 10 percent level. All regressions based on OLS regressions with full set of controls and canton fixed effects.

## 5. Conclusion

Raising the schooling levels of girls is often regarded as an important priority for developing countries (World Bank 2001, Schultz 2002). In many developing countries the Mincerian rate of return to schooling for women is at least as large as that for men (for examples, see Deolalikar 1993 for Indonesia; Schultz 1993 for Thailand, Cote d'Ivoire and Ghana). There is also a large literature documenting associations between female education and a variety of social outcomes, including lower fertility, decreases in child mortality, improvements in child health and nutrition, and better education and child cognitive development (see reviews in Strauss and Thomas 1995; World Bank 2001).

In this paper we estimate the impact of a scholarship program for girls on school enrollment and attendance in Cambodia. OLS estimates of program impact indicate that the program increased enrollment and attendance at eligible schools by approximately 30 percentage points, and enrollment at any school by about 22 percentage points; estimates of program effects based on propensity score matching are larger. The program effects we estimate are robust to a variety of concerns: Regression-discontinuity specifications suggest that our estimates are not driven by unobservable differences between scholarship recipients and non-recipients, and an application of bounds shows that selective attrition cannot account for the pattern of program effects we observe. Moreover, we find that program impacts are largest among girls who come from poorer households, have parents with less education, and live farther away from a secondary school. As a result, the JFPR program appears to have dramatically reduced socioeconomic gradients in enrollment and attendance.

Much of the existing evidence on the impact of demand-side incentives on schooling outcomes is based on evaluations in middle-income countries (for example Schultz 2004 and Behrman, Sengupta, and Todd 2005 on PROGRESA Mexico; Angrist et al. 2002 on a voucher program in Colombia; an exception is Ravallion and Wodon 2000 on the Food-for-Education program Bangladesh, a low income country). Our results suggest that even in one of the world's lowest income countries, with weak public sector institutions and relatively low quality schooling, demand-side incentives can effectively increase the school enrollment and attendance of girls. Indeed, the program effects we estimate are large compared with those found in wealthier countries: As a point of comparison, Schultz (2004) estimates that PROGRESA transfers increase the transition from $6^{th}$ grade, the last grade in primary school, to $7^{th}$ grade, the first year of secondary school, by 11.1 percentage points. Because enrollment rates in low-income countries like Cambodia tend to be much lower than those in middle-income countries like Mexico, the scope for improvements, and for potential program impact, may be larger in the poorest countries.

Our estimates of program impact leave some important questions unanswered. Chief among them is the effect of the scholarship on other schooling outcomes, including repetition rates, and measures of performance like test scores. Moreover, in the absence of empirical evidence on the effectiveness of other interventions to improve schooling outcomes in Cambodia, we cannot draw up convincing cost-effectiveness comparisons, or estimate whether the scholarship amount set by the JFPR program was "too large" or "too small" to induce a given change in girl enrollment. Nevertheless, given the paucity of evidence from low income countries, these estimates of large program impacts may be cause for optimism.

**Appendix: Comparing Applicants With Full Application Forms to Those With Only Partially Completed Forms**

| | All applicants | Full application form |
|---|---|---|
| **Outcomes** | | |
| Enrolled in JFPR school | 0.81 | 0.82 |
| Attending on the day of school visit | 0.74 | 0.75 |
| Enrolled in any school | 0.87 | 0.87 |
| **Characteristics of applicants** | | |
| Parents own business | 0.02 | 0.02 |
| Either parent is a government employee | 0.05 | 0.04 |
| Parent lends money regularly | 0.02 | 0.01 |
| Parent completed primary school | 0.17 | 0.17 |
| Parent completed secondary school | 0.02 | 0.02 |
| Main part of house made of cement or brick | 0.03 | 0.02 |
| Roof made of tiles, metal or fibre | 0.27 | 0.24 |
| Land ownership by hh > 1 hectare | 0.14 | 0.13 |
| Own a large asset (>1M riels) | 0.06 | 0.05 |
| Own a truck | 0.01 | 0.00 |
| Own a car | 0.00 | 0.00 |
| Live with both parents | 0.73 | 0.73 |
| Live with one parent | 0.21 | 0.22 |
| Live in a hut | 0.44 | 0.47 |
| Earthen floor | 0.31 | 0.34 |
| Family has motorbike or remorque | 0.11 | 0.1 |
| Family has bicycle | 0.55 | 0.54 |
| Family has ox and cart | 0.28 | 0.26 |
| Family has pony and trap | 0.03 | 0.03 |
| Family has no means of transportation | 0.39 | 0.43 |
| Family has debts > 100,000 riels | 0.7.0 | 0.73 |
| Mean number of brothers | 2.28 | 2.28 |
| Mean number of sisters | 2.79 | 2.81 |
| Applicant disabled | 0.04 | 0.03 |
| Other hh member disabled | 0.15 | 0.14 |
| Applicant or other hh member has disease | 0.28 | 0.29 |
| Distance to secondary school (km.) | 4.01 | 4.04 |
| | | |
| **Number of observations** | **5134** | **3623** |

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