Working Paper Series on

Impact Evaluation of Education Reforms

Paper No. 11

Can Private Schools Subsidies Increase Schooling for the Poor?: The Quetta Urban Fellowship Program

Jooseop Kim[a]
Harold Alderman[b]
Peter Orazem[a]

May 1998

Development Research Group
The World Bank

[a]Iowa State University
[b]World Bank
This paper is a product of the research project "Impact Evaluation of Education Projects Involving Decentralization and Privatization" which has been financially supported by the Development Research Group and the Research Support Budget (RPO No. 679-18) of the World Bank. The findings, interpretations, and conclusions are the authors' own and should not be attributed to the World Bank, its Board of Directors, or any of its member countries. Comments are welcome and should be sent directly to the author(s).
For copies, please send a request to Patricia Sader at psader@worldbank.org

Can Private School Subsidies Increase Schooling for the Poor?: the Quetta Urban Fellowship Program

Abstract

Private schooling - often postulated to improve school quality - may also prove to be a means to leverage public funds in order to provide access to schooling at rates faster than possible with public funds alone. This study measures the success of such an effort to stimulate girls' schooling through the creation of private girls' schools in poor urban neighborhoods of Quetta, Pakistan. The impact evaluation, which employs on an experimental design, indicates that the program increased girls' enrollments by an average of 33 percentage points. At the same time, boys' enrollments rose an average of 27.5 percentage points, partly because boys were also allowed to attend the new schools, and partly because parents would not send their girls to school and not also educate their boys. While the success of the program varied from one neighborhood to another, success was not clearly related to the relative wealth of a neighborhood or to parents' education levels. Thus, the program offers tremendous promise for increasing enrollment rates in other poor urban areas.

Acknowledgment

We are grateful to current and past members of the Balochistan Primary Education Directorate, particularly Ijaz Ahmed Malik, Mohammed Ishaque, Quaratul Ain and Bill Darnell, and to Sultan Mahmood Niazi of the Balochistan Education Foundation for their work in designing and implementing the Quetta Urban Fellowship program, to Brian Spicer and Fahim Akbar of the Balochistan Educational Management Information System for their help in designing and conducting surveys and preparing data sets, to the SCSPEB for carrying out the program and helping with data collection, and to Mae Chu Chang, Ivar Andersen, and Guilherme Sedlacek of the South Asia Division of the World Bank for financial support and numerous helpful discussions. Donna Otto prepared the final document.

Contents

Introduction

Primary school enrollment rates in Pakistan are lower than in other countries at the same level of economic development. The proportion of children in school is about half that in India and three quarters that in Bangladesh and Nepal. Nationally, the gross enrollment rate is 58 percent, 69 percent for boys but only 42 percent for girls. The enrollment gender gap is even wider in the province of Balochistan with 62 percent of boys but only 29 percent of girls enrolled.[1] The government of Pakistan has established a goal of universal primary enrollment by the year 2006. This would require more than doubling girls' enrollment nationally and more than tripling girls' enrollment in Balochistan. However, in Pakistan, as in many other countries, increasing government school capacity is constrained by inadequate public budgets.

Expansion of school capacity has the potential to target poor households on the basis of residency since the children least served by existing public schools reside in rural areas or poor neighborhoods of cities. There is evidence that school enrollment and achievement in Pakistan are constrained by insufficient school supply in these areas.[2] However, in addition to limitations on recurrent budget, the government is limited in that it generally constructs, rather than rents, school capacity. This poses a particular problem in poor urban neighborhoods, since the government requires that the neighborhood provide land for a new government school. Many neighborhoods have developed as squatters' communities where the necessary defined property rights are lacking.

Cultural prohibitions against exposing girls to the public have meant that the absence of girls' school in many communities has meant a lack of educational opportunities for girls. If universal primary enrollment is to be achieved for girls, more segregated girls' schools or coeducational schools with female teachers will need to be established. Given the limitations on increased government provision, one strategy is to try to increase the availability of private girls' schools in poor neighborhoods.

This study measures the success of such an effort to induce the creation of new private girls' schools in Quetta, the capital city of Balochistan. This study is one of the few attempts to use experimental design methods to evaluate an educational policy innovation. By randomizing the implementation of the pilot program, we are able to generate robust estimates of the impact of the program on enrollments. Random assignment avoids the bias in impact assessments inherent when the program is applied to individuals or groups believed to benefit atypically from the program.

This study shows that regardless of how the impact is measured, the program increased girls' enrollments by an average of 33 percentage points. At the same time, boy enrollments rose an average of 27.5 percentage points, partly because boys were also allowed to attend the new schools, and partly because parents would not send their girls to school and not also educate their boys. While neighborhoods differed in the success of the program, success was not clearly related to the relative wealth of a neighborhood or the education levels of the parents. Thus, the program offers tremendous promise for increasing enrollment rates in other poor urban areas.

I. The Urban Girls' Fellowship Program

In February 1995, the Balochistan Education Foundation launched the Urban Girls' Fellowship (UGF) Program in Quetta, the capital and largest city of Balochistan. The purpose of this pilot project was to determine whether establishing private schools in poor neighborhoods was a possible and cost effective means of expanding the delivery of primary educational services to girls in lower income neighborhoods of Quetta. Recent evidence from the Pakistan Integrated Household Survey suggests that about 77 percent of girls who start school finish the primary cycle. It was thought that if these poor girls started school, it was probable that many would persist in school long enough to attain literacy.

School establishment was encouraged through subsidies paid directly to schools. Schools were assured support from the government for a period of three years. The initial subsidy was Rs.100 (about $3) per month per girl enrolled up to an upper scholarship limit of 10,000 (100 girls x Rs.100 per girl) per month. This subsidy was sufficient to cover typical tuition at the lowest priced private schools. In addition to the 100 Rs. per month, each school received 200 Rs. per girl to defray start up costs. The subsidy was reduced in the second year, and cut again in the third year. By the fourth year, schools were expected to be largely self sufficient through fees and private support, although schools would still be eligible to apply to the Balochistan Education Foundation for additional grants.

Fellowship schools were allowed to admit boys provided that boys made up less than half of the total enrollment. Boys had to pay tuition at least equal to, and often greater than, girls. The grant calculation depended only on enrolled girls so that the school received no additional subsidy for enrolling boys. Schools were required to keep class sizes to no more than a total of 50 boys and girls per classroom and that there must be one teacher for classroom.

To implement the program, a non-governmental organization (NGO) was contracted to conduct an initial census of each site to insure that there was a sufficient number of girls in the target age range (4-8) and to inform parents of the program.[3] The emphasis was to create a partnership between parents in a neighborhood and the school operator. This was to be accomplished by first conducting a meeting of parents in a neighborhood to see if they were interested in attracting a private school to their area. The parents were asked to form a committee, which would represent the neighborhood in negotiations with potential school operators. With the assistance of the NGO, the parents' committee developed a proposal regarding the neighborhood's requirements for a school, resources the neighborhood was willing to provide the school (i.e. land, buildings, equipment) and any other requirements an operator was expected to satisfy. Experienced school operators were provided these specifications and were allowed to make proposals in response. Each parent committee was allowed to select their neighborhood school operator from among the proposals or to choose to operate the school themselves.

II. Survey Design and Data Strategies

Because government resources are in limited supply and the need to expand enrollment is so great, the government of Balochistan needed an accurate measure of the program's success and its prognosis for expansion. It was decided to use randomized assignment into treatment and control groups to accomplish this task. However, there were several factors that constrained the experimental design.[4] The government only allocated resources sufficient for 10 pilot schools. With only ten possible sites, the government opted to place one neighborhood school in each of ten urban slum areas of Quetta. This was considered politically expedient because it assured that all major ethnic groups would get at least one school. Ethnic groups tended to segregate into one or two of these slum areas, so the government could not be accused of favoritism. The only restrictions site on choice were that it had to be composed of poor households and that there be no existing government girls' school in the neighborhood.

A second problem was that there was no recent census of the population from which one could define treatment and control populations. The most recent census was fourteen years old, and the population of Quetta was estimated to have grown at about seven percent per year since then. Consequently, an area frame sampling strategy was chosen to define the treatment and control neighborhoods.

The area frame was designed as follows. A map of Quetta was produced with each of the ten slum areas outlined. In each area, three sites, literally points on a map, were selected. One of these areas was chosen randomly to be the treatment neighborhood for the creation of a private school.[5] The other neighborhoods were reserved to be controls. The only criterion for the treatment neighborhood was that it could not already have a government girls' school. While it was possible that the control sites would contain a government girls' school, it turned out that none of the control sites had girls' schools either.

By randomizing site selection, it was hoped that there would be no systematic differences in characteristics and behavioral patterns between the control and the treatment neighborhoods. However, the lack of information on population characteristics and the small number of pilot sites led to the possibility that the two groups would differ in important ways. Therefore, it was necessary to collect information on population attributes in all sites to enable us to test for statistically important differences in treatment and control populations that might also affect differences in enrollment outcomes between the two groups. We also have an interest in determining if relative success of a school depends upon observable neighborhood characteristics.

The baseline data collected in the treatment and control sites included information on socioeconomic characteristics of the households, parents' education, and educational attainment and current enrollment status of all children in the household. All households in the treatment neighborhoods were surveyed at the time of the promotion of the scholarship program in the summer of 1994 before any fellowship schools were opened. The baseline survey of households in the control group neighborhoods was conducted in July 1995. Because most of the data on socioeconomic status of the household does not change over time, the difference in the timing of the surveys should not be problematic. Information on the enrollment status of control neighborhood children was obtained for the current year (1995) and retrospectively for the previous year.[6] Subsequently, enrollment data was collected in 1996 in both treatment and control neighborhoods. All data collection and training of surveyors was supervised by the Balochistan Education Management Information System (BEMIS) to insure data comparability.

III. Theory of Enrollment Response to the Girls' Fellowship Program

Before conducting the statistical comparison of the treatment and control neighborhoods, it is important to identify the possible endogenous responses to the program. It is also important to identify the exogenous variables that might condition the magnitude of those responses. Households are assumed to have parents, a daughter and a son. Parents are assumed to derive utility from their own consumption of goods (Z_h) and from the human capital of their daughter (H_f) and their son (H_m). The utility function has the form $U=U(Z_h, H_f, H_m, T)$, where T is a vector of taste indicators that are not subject to choice. Parents maximize utility subject to a budget constraint. Sending their daughters to school requires that the household sacrifice current consumption and human capital investment for their sons.

Let Y be household income, P_z be the price of consumption goods, and P_f and P_m are the prices of schooling for their daughter and son, respectively. The schooling price includes school fees, the costs of transportation and materials, and the opportunity cost of child time. The income constraint on parental utility maximization is $P_z Z_h + P_f H_f + P_m H_m = Y$.

For cultural reasons, parents may face some disutility from sending their daughters to school. Social prohibitions against exposing their daughters to the outside world will cause them to discount the utility they get from their daughter's education by some factor $\delta_f<1$. Then the parents utility will have the form $U(Z_h, \delta_f H_f, H_m, T)$, with $U_{Hf} = \delta_f U_H(Z_h, H_f, H_m, T)$ and $U_{Hm} = U_H(Z_h, H_f, H_m, T)$.

The first order conditions yield the following relation:

$$(2) \qquad \frac{U_{Hf}}{U_{Hm}} = \frac{\delta_f U_H(Z_h, H_f, H_m, T)}{U_H(Z_h, H_f, H_m, T)} = \frac{P_f}{P_m}$$

where U_{Hf} and U_{Hm} represent the marginal utility of girls schooling and boys schooling, respectively. To get parents to equalize schooling for their boys and girls so that $H_f = H_m = H$, the cost of girls schooling must be discounted by $P_f = \delta_f P_m < P_m$. Alternatively, if the pecuniary costs of schooling are the same for boys and girls so that $P_f = P_m$, then the right-hand-side of (2) will equal one. Then, $\delta_f U_H(H_f) = U_H(H_m)$, which implies that $U_H(H_f) > U_H(H_m)$. Diminishing marginal utility would then imply that $H_m > H_f$ at the optimum.

Reduced form equations for boy's and girl's schooling have the following functional forms:

(3) $$H_f = H_f(P_f, P_m, \delta_f, Y, P_z, T)$$

(4) $$H_m = H_m(P_m, P_f, \delta_f, Y, P_z, T)$$

The reduced form equations suggest that enrollment will depend on school fees, the rate at which parents discount girls' education relative to boys, income, the price level, and tastes. Numerous studies suggest that education is a normal good so that $\partial H_m/\partial Y > 0$ and $\partial H_f/\partial Y > 0$. Those conditions are sufficient to insure that $\partial H_m/\partial P_m < 0$ and $\partial H_f/\partial P_f < 0$. The discount factor δ_f acts as an additional price on girls' schooling, so $\partial H_f/\partial \delta_f < 0$.

The girls' fellowship program will lower P_f, so girls' schooling will increase. The impact of the fellowship program on boys' enrollment is ambiguous. However, there are two reasons to believe that the girls' fellowship program will have a positive impact on boys' schooling. First, the program creates a new low-priced private school that can accept boys, lowering P_m, although it lowers P_f even more. Second, boys' education may increase as their sisters go to school for a very practical reason - parents may want their boys to escort their sisters to and from school. This implies that boys' education and girls' education may be complementary goods so that $\partial H_m/\partial P_f < 0$. In any event, it will be important to monitor both boys' and girls' enrollments in response to the program.[7]

Equations 3) and 4) suggest that income, the cost of schooling, and the disamenity of sending girls to school may condition the enrollment response to the fellowship program. Schooling costs are measured by fees charged in existing neighborhood schools, average distance to schools (a proxy for transport costs) and the opportunity cost of child time (a function of the child's age and its square). The parents' disamenity for sending girls to school is assumed to be inversely related to fathers' and mothers' education. Parents' taste for education are also assumed to depend on the child's birth order (there may be a preference for educating the eldest child, particularly the eldest boy) and on citizenship (refugees may value education less or may feel the return from education is less). These variables comprise the vector of exogenous variables we will use in the analysis below.

IV. Differences and Similarities Between Treatment and Control Neighborhoods

Statistical properties of the baseline data are described in Table 1 for both treatment and control neighborhoods. Sample statistics are reported separately for boys and girls. The treatment sample included 1,310 children, 781 girls and 529 boys. The control sample included 1,358 children, 697 girls and 661 boys. Enrollment rates for boys and girls in the treatment group were higher than those in the control group: 6.6 percent higher for girls and 8.8 percent higher for boys. The other variables in Table 1 represent the exogenous variables believed to affect parental enrollment choices for their children. Most of the variables come directly from the questionnaire. However, distance to school and annual fees were measured by neighborhood averages of the children in school. Household income was estimated using information on the number of adults in the household with various educational attainments and various household assets. Details on the estimated measure of household income are contained in Appendix 2.

The purpose of the control group is to get information on the counterfactual state.[8] A reasonable approximation of the change in outcome due to the program intervention is to measure the difference in outcomes between the treatment and control groups before and after the intervention. However, it is important to check whether there are important differences between the treatment and the control groups which might also result in different outcomes.[9]

Tests for statistical significance of the differences between the treatment and the control groups were performed in two ways. First, in order to check if the randomization yielded observationally equivalent treatment and control populations, we conducted tests of the equality of means of the endogenous and exogenous variables. A second analysis was based on estimated enrollment equations in the baseline data. These tested the null hypothesis of the equality of behavioral coefficients in the enrollment choice models for the treatment and control neighborhoods.

A. Tests of Equality of Means in the Baseline Data Sets

The third and sixth columns of Table 1 report tests of the hypotheses that the means of variables are equal across the treatment and control groups. Standard errors are corrected for the effects of cluster sampling using Huber's method. Baseline enrollment rates for both sexes were significantly higher in the treatment group.[10] In addition, there were significant differences in average mother's education and birth order between the treatment and control girls, although the differences were small numerically. For boys, father's highest grade and citizenship were significantly higher in the treatment group. Once again, the differences in means were small numerically. The joint test that the means were equal across all variables was easily rejected. Based upon the results, we can reach a statistical conclusion that the treatment and control samples are not identical, a problem which will be addressed in the analysis below.

B. Tests of Equality Coefficients in the Baseline Enrollment Model

A second way that the treatment and control neighborhoods may differ is in the decision-making process of parents. To check this, we estimated the following binary model of parental decision regarding their children's schooling:

(5) $$R^*_{it} = \beta'_t X_{it} + U_{it}$$

where $R_{it} = 1 \quad \text{if } R^*_{it} > 0$

$R_{it} = 0 \quad \text{if } R^*_{it} \leq 0$

In equation (5), an unobserved variable R^*_{it} depends on the index function, $\beta'_t X_{it}$, where X_{it} is the vector of characteristics in equations (3) and (4) which affect parental choices regarding their children's enrollment. When R^*_{it} is positive, we observe the child in school and R_{it}=1. Otherwise, the child will not enroll.

Table 2A presents the baseline probit estimates of enrollment choice for boys and girls. Separate estimates are presented for the treatment and control neighborhoods. The estimated parameters exhibit the same sign patterns in the treatment and control groups and are qualitatively similar to results obtained in other studies of enrollment. The coefficient on household income is positive in both samples. Parental education positively influences their children's enrollment, and mother's education is a more important factor influencing girl's education than father's education. Enrollment increases with age, but at a diminishing rate. First-born children have a higher probability of enrollment than their younger siblings, but the coefficient is not significant. Native Pakistanis have a higher probability of enrollment. After pooling the treatment and the control data, we can also estimate the effects of neighborhood average distance to school and average annual fees. They have negative coefficients except for a positive but insignificant effect of annual fees on boy's schooling.

Table 2B shows the result of the tests of equality of coefficients between the treatment and the control groups. The coefficients for the two groups are not statistically different, except for father's educational level in the girls' enrollment equation. This result suggests that parental decision making on education is similar in the treatment and control neighborhoods. Despite significant differences in characteristics as reported in table 1, we can still measure the change in enrollment due to the program by measuring the difference between treatment and control group enrollment rates, holding constant the differences in the exogenous variables.

V. Evaluation Strategies and Results

The evaluation problem is essentially a missing data problem. A child i cannot be simultaneously in both the treatment state (R_{Tit}) and the control state (R_{Nit}). Letting $d_i = 1$ if child i is in a treatment neighborhood, and $d_i = 0$ otherwise, the observed outcome(R_{it}) can be expressed as $R_{it} = d_i R_{Tit} + (1\text{-}d_i)R_{Nit}$. Given the impossibility of observing the true impact of the fellowship program ($\alpha_t = R_{Tit} - R_{Nit}$), the goal is to get an unbiased estimator of α_t.

One way to get an unbiased estimator of α_t is to use a control group to derive estimates of the counterfactual state. The initial group of estimators assume that the control state does not vary across individuals. Then, the difference in outcomes between the treatment and control groups is used as an estimate of α_t. These estimators depend only on comparisons of endogenous outcomes without controlling for the exogenous variables. Mathematically, these are defined as

(6) Reflexive:

$$E^R(\alpha_{it}|d_i = 1) = E(R_{Tt}) - E(R_{i0}|d_i = 1)$$

(7) Difference in Differences:

$$E^D(\alpha_{it}|d_i = 1) = [E(R_{Tt}) - E(R_{Nt})] - [E(R_{i0}|d_i = 1) - E(R_{i0}|d_i = 0)]$$

(8) Mean-Difference:

$$E^M(\alpha_{it}|d_i = 1) = [E(R_{Tt}) - E(R_{Nt})]$$

where subscripts T and N represent treatment neighborhoods and control neighborhoods, respectively. The reflexive estimator (6), measures the expected program effect as the gap between the expected enrollment rate after the program, $E(R_{Tt})$, and the expected enrollment rate before the program was implemented, $E(R_{i0}|d_i{=}1)$. The underlying assumption of this method is that the period t outcome in the treatment neighborhood without the program would have been identical to the observed pre-program outcome.

The difference in differences estimator (7), measures the expected program effect by the gap between the post-program outcome in the treatment group, $E(R_{Tt})$, and that in the control group, $E(R_{Nt})$, adjusted by the pre-program difference between the two groups. In this method, it is assumed that the difference in outcomes between the two groups before the program intervention would be constant over time if it were not for the program, so the difference in outcome between the two groups after the program intervention reflects the difference due to the program as well as to the initial difference. Differencing the differences yields an estimate of the program effect.

The mean-difference estimator (8), measures the expected program effect by the post-program observed gap in outcomes between the treatment group and the control group. This method assumes that the control group mimics perfectly the treatment group.

The methodological differences follow from different assumptions about the unobserved counterfactual state. The methods based on equations (6) through (8) assume that the counterfactual state is non-stochastic. If we relax that assumption so that the counterfactual state, R_{Nit}, follows a stochastic process, it is possible to set up the following model:

(9) $$R_{Nit} = X_{it}\beta_{it} + U_{it}$$

In equation (9), X_{it} is the vector of observed characteristics as in equations (3) and (4), U_{it} is an error term, and β_t is a vector of parameters to be estimated. Modifying equation (9) using the definition of the program effect, α_{it}, and assuming that the program effect conditional on X_{it} is invariant across individuals but not time so that $\alpha_{it}=\alpha_t$, we have

(10) Covariate post-test:

$$R_{it} = X_{it}\beta_t + d_i\alpha_t + U_{it}, \text{ for } t = 0, 1, \ldots, T$$

In equation (10), R_{it} is the observed enrollment rate, and d_i is a dummy variable indicating residence in a fellowship school neighborhood. Assuming X_{it} is independent of the unobserved variables U_{it}, so that $E(U_{it}|X_{it})=0$ for all i,t, we can estimate equation (10) using a cross-sectional data set.

If the data set includes repeated observations of individuals, an alternative way to estimate the program effect using econometric analysis is to use a first-difference model. If the effect of X_{it} varies over time, we can modify equation (10) to be

(11) First difference with time-varying covariate effects:

$$R_{it} - R_{i0} = X_{it}\beta_t - X_{i0}\beta_0 + d_i\alpha_t + U_{it} - U_0.$$

A further assumption that β_t is also time invariant simplifies equation (9) to

(12) First difference with time invariant covariate effects:

$$R_{it} - R_{i0} = \left(X_{it} - X_{i0}\right)\beta_0 + d_i\alpha_t + U_{it} - U_0.$$ [11]

There are two ways of measuring the effect of the program on enrollments. One is to measure the change in enrollment for children in the target age of 5 to 8. The other is to measure enrollment rates longitudinally for children aged 5 to 8 in the initial year of the fellowship program. The first of these will tell us if the initial impact on school enrollments carry over to children initially too young to enroll. The second will tell us if children who enroll as a result of the program are also more likely to stay in school than enrolled children in the control group.

A. Results of Age-specific Analysis

The first four columns of table 3 report age-specific enrollment rates for boys and girls before and after the program intervention. The enrollment rate for boys decreased 7.6 percent and that for girls rose 1.3 percent in the control neighborhoods. At the same time, enrollment in the fellowship school neighborhoods rose 19.8 percent for boys and 26.0 percent for girls. From this information, we can apply the three different methods based on equations (6) through (8). The results are similar across all three methods. All imply that the fellowship program had a positive effect on girls in the target age group, and that parents sent their boys to school in increasing numbers as well. Applying the same methods to two years of data yield even larger estimates of the enrollment effects of the fellowship schools.

An alternative method based on equation (10) can also be applied to the same sample.[12] The first two and the last two columns of table 4 report the results of the covariate post-test probit analysis of the probability of enrollment using cross-sectional data. The enrollment rate in fellowship neighborhoods rose 33.4 percent for girls, and 22.4 percent for boys in the first year of the fellowship program. After two years, enrollment in the fellowship neighborhoods had risen 42.7 percent for girls and 38.4 percent for boys. These results are consistent with the results in table 3. First, this result shows that parents made responses very quickly to the fellowship program. Considering the fellowship schools were established in February in 1995, and that survey data were collected in July of that year, the response of the parents in the target area was nearly instantaneous. This supports the view that there was excess demand for primary education in these poor areas. Second, the result suggests that the fellowship program was continuously successful year by year. For girls, the estimated program effect increased by almost 10 percent in 1996 over the estimated program effect in 1995. Boys' enrollment rates grew 16 percent in the year after implementation.

B. Results of Cohort-specific Analysis

The cohort-specific analysis follows the enrollments of a fixed group of children over time. This sample has two distinct advantages over the age-specific analysis. First, it allows us to see if initial gains in enrollment persist over time. Because it is assumed that five years of schooling are needed to attain permanent literacy, this program will be truly successful only if children remain in school for several years. The other advantage of the cohort-specific analysis is that we can control for individual specific unobservable effects which might also be correlated with program outcomes.

The last four columns of table 3 report the enrollment rates before and after the program intervention for fixed cohorts of boys and girls in the treatment and the control groups. We begin with the cohort aged 4-7 in 1994 to capture the children aged five to eight in 1995.[13] Because enrollments increase with age at least initially, some of the enrollment growth in the cohort-specific analysis will reflect a maturity effect. Nevertheless, the comparison between the fellowship and control neighborhoods should difference away this maturity effect, leaving an unbiased estimate of the program effect.

Estimates of the fellowship effect for the cohort-specific sample are summarized in the last three rows in table 3. The reflexive method will yield upward biased estimates because of the maturity effect, as evidenced by the 46.8 percent increase in boys' enrollment, and 44.3 percent increase in girls' enrollment. These estimated effects are much bigger than the reflexive estimates in the age-specific analysis.

The estimates from the difference in differences and mean-difference methods remove the maturity effect under the assumption of common maturity effects across neighborhoods. Consequently, the measured program effects using difference in differences and mean-difference methods are smaller than the reflexive estimate, and are more comparable to the estimates using the age-specific sample. All the results show large gains in both boy and girl enrollments following the opening of the fellowship schools. Most estimates show slightly higher enrollment gains for girls than for boys. Looking across the age-specific and cohort-specific estimates, we can conclude that girl enrollments rose by 25-35 percent as a result of the program, and that boy enrollments rose by a few percentage points less.

The first four columns of table 4 report the cohort-specific post-test probit analysis of the probability of enrollment. The inclusion of quadratic terms in age control for maturation, so the coefficients on the treatment dummy can be interpreted as an estimate of the program effect controlling for the maturity effect. The program effects for the children aged five to eight in 1995 were measured as 33.4 percent and 22.4 percent increase in enrollment for girls and boys, respectively. One year later, the measured effects grew an additional 6.5 percent for girls, and 4.4 percent for boys. Rising effects over time indicate that the large initial enrollment gains persisted over time. The persistence of the effect is a promising sign for the continued survival of these schools, particularly since fees rose in the second year in many schools.

C. Results of First-Difference Analysis

Another possible source of bias in the estimate of the program effect is unobserved heterogeneity in children that is correlated with the program effect. If cross-sectional differences in individual fixed effects are responsible for measured program effects, then we can remove the fixed effect by differencing the dependent variable.

Table 5 presents the results of the first difference analysis under the maintained assumption that the coefficients of the regressors are time invariant as in equation (12). The dependent variable is the change in enrollment status from before to after the implementation of the program. The coefficient on the treatment dummy measures the effect of the program on enrollment choice. The last two specifications of the first difference analysis allow the coefficients on the individual and neighborhood effects to vary over time as in equation (11). The results corroborate results presented above in the sense that the coefficient representing the treatment effect, is significantly positive, and the program effect was larger for girls' enrollment than boys' enrollment. However, now the estimated program effect is larger after one year than after two years, in contrast to the cross-sectional results. The cause of the discrepancy is unclear, but must be related to the control for fixed effects. Note that the enrollment rates were initially higher in the fellowship neighborhoods, and children in school before the fellowship schools opened will not contribute to the measured fellowship school effect in the first difference analysis. Note also that it is possible that the opening of the fellowship schools encouraged parents to send their children to school at younger ages, and the smaller effect over time reflects the first-time enrollment of older children in the control neighborhoods. In fact, some of the later enrollment growth in control neighborhoods may be related to the fellowship program if the promotion of children's education in fellowship neighborhoods spilled over to the control neighborhoods. Nevertheless, the estimated two years enrollment growth effects are still large. Controlling for fixed effects lowers the estimated effect by 12 to 30 percent, leaving the estimated enrollment impact of the fellowship school to be 24.2 percent for boys and 28.1 percent for girls.

D. Results of Neighborhood-Specific Analysis

Given the strong average estimated enrollment growth due to the creation of the fellowship schools, an important issue is whether there is any significant variation in program effects across the neighborhoods. If so, are there any identifiable neighborhood attributes which increase the likelihood of program success? This analysis is necessarily speculative since there are only 10 neighborhoods and therefore 10 degrees of freedom. Neighborhoods were divided into two groups, neighborhoods with over 30 percent increase in girls' enrollment, and those below 30 percent.

Table 6 reports the summary statistics for these more and less successful neighborhoods. Eight neighborhoods out of ten neighborhoods fell into the more successful group, so the less successful neighborhoods were the clear exception. Several important findings are apparent. For most variables, the sample means are similar in the two groups. One apparent difference is in household income. However, the higher average income is in the less successful neighborhoods. Clearly if the concern was that poor neighborhoods could not benefit from a subsidized private school, that fear was exaggerated.

A higher average of parental education in neighborhoods is also not a prerequisite for success. Differences in parental education were insignificant. Taking the averages at face value, the program was more successful in the neighborhoods with more educated mothers but less educated fathers.

The variables which differed significantly between the more and less successful neighborhoods were citizenship and distance, although the numerical differences were not great. Citizenship was positively related to enrollment of both boys and girls in the baseline estimates. It is reasonable to assume that the greater success in neighborhoods with higher proportions of citizens reflects a stronger taste for schooling or higher expected return to education among Pakistanis relative to Afghan refugees who make up the majority of non-citizens in the city.[14]

Shorter distance to school reflects higher density of schools in a neighborhood. It is not clear why fellowship schools in neighborhoods with more competing schools should do better. On the other hand, the difference in commuting time between more and less successful neighborhoods was only two minutes, so the difference is probably unrelated school success.

An intriguing result was that boys' enrollments rose in neighborhoods with more success raising girls' enrollments, but that boys' enrollments fell in the neighborhoods that were relatively less successful. Why this happened is unclear. However, the result is consistent with a presumption that boys' enrollment and girls' enrollment are complementary so that successfully increasing enrollment of one gender will also increase enrollments overall.

VI. Comparison with Alternative Policy Options

Given the apparent success of the fellowship program, is it cost effective when compared to alternative policy options? Table 7 reports the estimated changes in alternative policies needed to match enrollment increase that resulted from the fellowship program. Two policy options were considered: income transfers to poor households and construction of new schools. Our estimates are based on estimated enrollment choice elasticities with respect to income and distance.

Income has only a moderate impact on participation in the program. Consequently, the benefits of the project are not strongly skewed to upper income households. The moderate income response also implies that it would take a fair amount of income transfer to achieve the same impact on enrollments as the project encouraged. In particular, the income response in our estimates imply that 3471 Rs. of direct subsidy to a household would be needed to raise the probability of girls' enrollment by 25 percent. This is well above the initial subsidy - including start up cost - 1400 Rs. per year per girl in the fellowship program. As boys’ enrollment is less income sensitive, a similar increase in boys' enrollment probability would require an income transfer of 15030 Rs. Therefore, the fellowship option would be less expensive than income subsidies.

The result for Quetta is rather similar to results for both sexes in low income neighborhoods of Lahore, where 10 percent increase of household income causes a 1.2 percent increase in the enrollment rate in private schools (Alderman, Orazem, and Paterno 1996). Thus, in Lahore, a city where overall primary school enrollment rates are over 90 percent an income transfer of 14808 Rs would be required to raise enrollment 25 percent for both sexes.

The overall impact of the fellowship program might also be due to the fact that it decreased the distance to schools. Unfortunately, there was insufficient variance in the distance to schools in this sample to accurately estimate the distance response directly. Using the 1996 coefficient of distance for girls from table 4 of -0.03 in our study, we estimate that the distance to private schools would have to be cut in half to increase enrollment by the same amount as the project achieved. Halving distance to schools in a three dimensional environment implies a four-fold increase in the number of schools.

VII. Conclusions and Extensions

A summary of all measured program effects is contained in table 8. All of the results show that the fellowship program has positively affected enrollment for both boys and girls. Most show that the effect was larger for girls' enrollment. One can conclude that the estimated program effects are robust to differences in assumptions about possible biases due to measured and unmeasured differences between treatment and control neighborhoods. Before the project was implemented, it was not clear whether low girls' enrollment was due to cultural barriers which cause parents to withhold their daughters from school or to inadequate supply of girls' schools. The results of the urban fellowship experiment provide strong evidence that subsidizing the establishment of girls' primary schools can lead to sharp increases of girls' enrollment. In addition, even though the fellowship was given only to girls, boys' enrollment in those neighborhoods also sharply increased. This suggests that there also may have been excess demand for boys' primary education in these poor areas. The measured change over two years yielded mixed evidence on whether the enrollment growth advantage in fellowship neighborhoods over control neighborhoods continued to grow over time. However, even if the initial enrollment gain decreased in subsequent years, the enrollment gains after two years are around 25 percentage points. This is a substantial improvement over the baseline enrollment rate of 45 percent for 5-8 year-old girls. School success appears not to depend on neighborhood income or other observable socioeconomic variables, suggesting that expansion of the program to other poor neighborhoods is also likely to be successful.

Future work will be required to assess the long term effects of the fellowship program. In particular, the future sustainability of the schools and the enrollment effects after the subsidies expire will need to be assessed. The short term success of the fellowship program does not guarantee long term success when the financial burden of supporting the schools are fully borne by the neighborhoods. School outcomes will also need to be assessed. The ultimate success of the fellowship program depends on whether children attain literacy.

References

Alderman, Harold, Jere Behrman, David Ross, and Richard Sabot. 1996. "Decomposing the Gender Gap in Cognitive Skills in a Poor Rural Economy." *Journal of Human Resources* 32(1):229-254.

Alderman, Harold and Marito Garcia. 1996. *Poverty, Household Food Security, and Nutrition in Rural Pakistan*. Research Report 96, International Food Policy Research Institute.

Alderman, Harold, Peter Orazem, and Elizabeth M. Paterno. 1996. "School Quality, School Cost, and the Public/Private School Choices of Low-Income Households in Pakistan." *The World Bank Working Paper Series on Impact Evaluation of Education Reform #2.*

Behrman, Jere R., Robert A. Pollak, and Paul Taubman. 1995. *From Parent to Child*. The University of Chicago Press.

Boruch, Robert, John McSweeny, and John Soderstrom. 1978. "Randomized Field Experiments for Program Planning, Development, and Evaluation: An Illustrative Bibliography." *Evaluation Quarterly* 2(4):655-95.

Butcher, Kristen, and Anne Case. 1994. "The Effect of Sibling Sex Composition on Women's Education and Earnings." *Quarterly Journal of Economics* 109(3):531-562.

Cook, Thomas D., and Donald T. Campbell. 1979. *Quasi-Experimentation*. Rand McNally College Publishing Company.

Deaton, Angus, and John Muellbauer. 1980. *Economics and Consumer Behavior*. Cambridge University Press.

Grossman, Jean Baldwin. 1994. "Evaluating Social Policies: Principles and US. Experience." *The World Bank Research Observer* 9(2):159-80.

Hanushek, Eric A. 1995. "Interpreting Recent Research on Schooling in Developing Countries." *The World Bank Research Observer* 10(2):227-46.

Heckman, James J., and V. Joseph Hotz. 1989. "Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training." *Journal of the American Statistical Association* 84(408):862-74.

Hoole, Francis W. 1978. *Evaluation Research and Development Activities*. Sage

Publications.

Hsiao, Cheng. 1991. *Analysis of Panel Data*. Econometric Society Monographs No 11. Cambridge University Press.

Huber, Peter J. 1980. *Robust Statistics*. John Wiley & Sons.

LaLonde, Robert J. 1986. "Evaluating the Econometric Evaluations of Training Programs with Experimental Data." *American Economic Review* 76(4):604-18.

_______. 1995. "The Promise of Public Sector-Sponsored Training Programs." *Journal of Economic Perspectives* 9(2):149-68.

Levitan, Sar A. 1992. *Evaluation of Federal Social Programs: An Uncertain Impact, Center for Social Policy Studies*. The George Washington University Press.

Manski, Charles F., and Irwin Garfinkel. 1992. *Evaluating Welfare and Training Programs*. Harvard University Press.

Newman, John, Laura Rawlings, and Paul Gertler. 1994. "Using Randomized Control Designs in Evaluating Social Sector Programs in Developing Countries." *The World Bank Research Observer* 9(2):181-201.

Parish, William, and Robert Willis. 1993. "Daughters, Education, and Family Budgets: Taiwan Experiences." *Journal of Human Resources* 28(4):863-898.

Rossi, Peter H., and Howard Freeman. 1993. *Evaluation: a Systematic Approach*. Sage Publications.

Schultz, T. Paul. 1995. *Investment in Women's Human Capital*. The University of Chicago Press.

Silberberg, Eugene. 1990. *The Structure of Economics: A Mathematical Analysis*. McGraw-Hill Publishing Company.

Tobin, James, and H.S. Houthakker. 1951. "The Effects of Rationing on Demand Elasticities." *Review of Economic Studies* 18:1-14.

Table 1. Summary Statistics of Baseline Datasets and Tests of the Equality of Means Between the Treatment and Control Groups[1, 2]

Variable	Girls Treatment	Girls Control	Girls t-value[3]	Boys Treatment	Boys Control	Boys t-value[3]
Enrollment rate	0.366	0.300	2.67	0.486	0.398	3.03
	(0.482)	(0.459)	[1,468]	(0.500)	(0.490)	[1180]
Household income	7,108	6,808	1.03	7,005	6,592	1.41
	(7,157)	(3,011)	[1,476]	(6,815)	(2,847)	[1188]
Age	6.026	6.001	0.19	6.040	6.003	0.44
	(1.403)	(1.429)	[1,476]	(1.426)	(1.444)	[1188]
Mother's highest grade	0.619	0.395	2.08	0.623	0.414	1.74
	(2.243)	(1.844)	[1,466]	(2.208)	(1.918)	[1183]
Father's highest grade	3.405	3.079	1.27	3.635	2.723	3.38
	(4.745)	(4.882)	[1,417]	(4.579)	(4.548)	[1162]
Birth order	2.832	3.004	2.21	3.074	2.965	1.27
	(1.474)	(1.510)	[1,476]	(1.447)	(1.482)	[1188]
Citizenship	0.868	0.835	1.79	0.877	0.814	2.98
	(0.339)	(0.371)	[1,476]	(0.329)	(0.389)	[1188]
Distance to school	17.77	17.81	0.05	16.93	16.42	0.62
	(9.443)	(9.991)	[491]	(9.338)	(9.394)	[515]
Annual fees	244.3	187.0	1.19	531.3	391.7	1.73
	(536.0)	(502.5)	[480]	(1036.8)	(765.1)	[505]
Joint test[4]			121.61			82.20
Number of observations	781	697		529	661	

[1]Age 4 to 8 for both groups. The baseline data was collected in 1994 for the treatment group, and collected in 1995 for the control group.

[2]The numbers shown in parentheses are the standard deviations and those in the square brackets are the degrees of freedom. The degrees of freedom differ due to missing information in the surveys.

[3]The null hypothesis is that the mean of the variable in the treatment group is equal to that in the control group. If the t-value is smaller than 1.96, the null hypothesis cannot be rejected at the 0.05 significance level.

[4]Reported numbers are F statistics with degrees of freedom (9,1478) for girls and (9,1190) for boys. The null hypothesis was that the means of the variables between the treatment group and the control group are equal for all variables.

Table 2A. Baseline Probit Analysis of the Probability of Enrollment

	Girls and Boys				Girls		
Variable	Treatments	Controls	Pooled	Treatments	Controls	Pooled	Treatme
Household income/10,000	0.138	0.422	0.143	0.171	0.572	0.196	0.037
	(2.362)	(2.879)	(2.710)	(2.377)	(2.870)	(2.921)	(0.346
Age	1.820	2.235	2.060	1.611	2.623	2.045	2.176
	(5.226)	(6.323)	(8.396)	(3.612)	(4.864)	(6.048)	(3.674
Age square	-0.101	-0.140	-0.124	-0.089	-0.174	-0.127	-0.11
	(3.621)	(5.014)	(6.347)	(2.508)	(4.127)	(4.745)	(2.513
Mother’s highest grade	0.051	0.094	0.065	0.067	0.118	0.082	0.007
	(2.443)	(3.422)	(4.091)	(2.500)	(2.649)	(3.739)	(0.197
Father’s highest grade	0.023	0.065	0.049	0.027	0.084	0.057	0.025
	(2.369)	(6.634)	(7.337)	(2.271)	(5.997)	(6.570)	(1.498
Birth order	-0.029	-0.036	-0.030	-0.017	-0.020	-0.021	-0.03
	(0.918)	(1.214)	(1.407)	(0.416)	(0.461)	(0.732)	(0.717
Citizenship	0.693	0.335	0.569	0.628	0.214	0.482	0.762
	(5.207)	(2.556)	(6.375)	(3.590)	(1.079)	(3.839)	(3.545
Girl	-0.419	-0.541	-0.474				
	(4.878)	(5.340)	(7.106)				
Distance to school						-0.001	
						(0.036)	
Annual fees/1,000						-0.380	
						(1.185)	
Number of observations	1,231	1,324	2,555	725	677	1,402	506
Pseudo R^2	0.277	0.295	0.273	0.230	0.331	0.254	0.358

Table 2B. Chi-square Statistics of the Hypothesis of Equal Coefficients in the Treatment and Control Groups

Variable	Girls and Boys	Girls	Boys
Income	2.03	2.58	0.25
Age	0.09	0.02	0.14
Age square	0.89	0.47	0.99
Mother’s highest grade	1.50	0.73	1.95
Father’s highest grade	7.68*	6.88*	1.09
Birth order	0.07	0.00	0.04
Citizenship	1.20	1.36	0.00
Girl	0.05		
Joint Test	29.90*	23.33*	13.68*

*Null hypothesis of equality rejected at the .05 significance level.

Table 3. Enrollment Rate Before and After Program Intervention

	Age-specific				Cohort-specific			
	Treatment		Control		Treatment		Control	
	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
Enrollment Rate Before Program E_0)	56.33	45.29	51.06	34.86	38.75	34.06	36.55	29.03
Enrollment Rate in 1995 (E_{95})	64.29	63.93	49.68	38.37	64.29	63.93	49.68	38.37
Enrollment Rate in 1996 (E_{96})	76.15	71.30	43.50	36.20	85.50	78.36	59.87	45.97
E_{95} - E_0	7.96	18.64	-1.38	3.51	25.54	29.87	13.13	9.34
E_{96} - E_0	19.82	26.01	-7.56	1.34	46.75	44.30	23.32	16.94

	Age-specific		Cohort-specific	
Measure of Effect	Boys	Girls	Boys	Girls
Reflexive, 1994-1995	8.0	18.6	25.5	29.9
Reflexive, 1994-1996	19.8	26.0	46.8	44.3
Difference in Differences, 1994-1995	9.3	15.1	12.4	20.5
Difference in Differences, 1994-1996	27.4	24.8	23.4	27.4
Mean-Difference, 1994-1995	14.6	25.6	14.6	25.6
Mean-Difference, 1994-1996	32.7	35.1	25.6	32.4

Note: Since 1994 baseline data for the control group was not available, they were estimated from the 1995 baseline data in the way that children who enrolled in advanced grades in 1995 and enrolled in recall data were considered in enrolled in 1994.

Table 4. Post-test Probit Analysis of Probability of Enrollment Using Cross-sectional Data[1]

	1995[2]		1996, Cohort-specific[3]		1996, Age-specific[4]	
Variable	Girls	Boys	Girls	Boys	Girls	Boys`
Treatment dummy	0.034	0.224	0.399	0.268	0.427	0.384
	(10.148)	(5.143)	(9.679)	(5.511)	(8.488)	(5.495)
Household income/10,000	-0.001	-0.003	0.012	0.072	0.034	0.128
	(0.022)	(0.080)	(0.333)	(1.513)	(0.724)	(1.872)
Age	0.141	0.276	0.229	0.936	0.615	1.330
	(0.652)	(1.197)	(0.797)	(3.416)	(1.970)	(3.925)
Age square	-0.008	-0.016	-0.011	-0.057	-0.036	-0.083
	(0.496)	(0.890)	(0.570)	(3.113)	(1.546)	(3.268)
Mother's highest grade	0.016	0.030	0.029	0.011	0.027	0.018
	(0.040)	(2.330)	(1.505)	(0.867)	(1.822)	(1.231)
Father's highest grade	0.013	0.003	0.030	0.011	0.035	0.020
	(3.383)	(0.707)	(6.293)	(2.433)	(6.656)	(3.523)
Birth order	-0.008	-0.026	-0.016	-0.020	-0.0002	-0.031
	(0.720)	(2.042)	(1.214)	(1.516)	(0.016)	(1.904)
Citizenship	0.152	0.225	0.143	0.201	0.187	0.173
	(3.040)	(4.362)	(2.374)	(3.501)	(2.783)	(2.465)
Distance to school	-0.008	0.003	-0.029	-0.027	-0.035	-0.036
	(1.074)	(0.358)	(3.190)	(2.347)	(3.361)	(2.511)
Annual fees/1,000	-0.443	-0.030	-0.170	-0.362	-0.316	-0.618
	(3.640)	(0.241)	(1.088)	(2.535)	(1.719)	(2.723)
Number of observations	1.031	830	845	700	764	650
Pseudo R^2	0.141	0.100	0.312	0.215	0.350	0.380

[1]The coefficients reported her are dF/dX, where F is dependent variable and X is independent variable, not actual coefficients. Since the dependent variable is a discrete variable, dF/dX is not identical to actual coefficients. The numbers shown in the parentheses are z-values corrected for cluster effect. Dummy variables for each neighborhood included.

[2]Children in this data are aged 5 to 8 in 1995. Dependent variable is enrollment status in 1995.

[3]Children in this data are aged 5 to 8 in 1995. Dependent variable is enrollment status in 1996.

[4]Children in this data are aged 5 to 8 in 1996. Dependent variable is enrollment status in 1996.

Table 5. First Difference Analysis for Change of Enrollment Decision[1]

	1994-1995		1994-1996		1994-1995		1994-1996	
Variable	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys
Treatment dummy	0.367	0.292	0.264	0.088	0.469	0.428	0.281	0.242
	(5.518)	(3.591)	(3.165)	(0.909)	(5.833)	(3.755)	(2.931)	(1.723)
Δ Age square	-0.077	-0.082	-0.047	-0.046	-0.071	0.032	0.079	0.073
	(4.785)	(4.447)	(5.006)	(4.502)	(0.343)	(0.137)	(0.641)	(1.323)
Age 94 square					-0.001	-0.022	-0.047	-0.080
					(0.040)	(0.525)	(1.055)	(1.686)
Income/10,000					-0.151	-0.009	-0.009	-0.005
					(2.680)	(0.122)	(1.309)	(0.588)
Mother's highest grade					-0.007	0.016	-0.009	-0.030
					(0.374)	(0.652)	(0.380)	(1.020)
Father's highest grade					0.004	-0.028	0.043	0.014
					(0.458)	(2.751)	(4.561)	(1.226)
Birth order					-0.029	-0.050	-0.008	-0.021
					(1.152)	(1.667)	(0.250)	(0.616)
Citizenship					0.006	0.093	0.243	0.212
					(0.047)	(0.717)	(1.515)	(1.318)
Distance to school					-0.001	0.027	-0.054	0.029
					(0.051)	(1.407)	(1.936)	(1.272)
Annual fees/1,000					-0.755	-0.103	-0.588	0.765
					(2.424)	(0.299)	(1.623)	(1.813)
Number of observations					1,055	861	863	725
Pseudo R^2					0.04	0.04	0.09	0.05

[1]The coefficients reported here are dF/dX, not actual coefficients. Children in the sample are aged 4 to 7 in 1994.

Table 6. Statistical Summary of Successful and Unsuccessful Neighborhoods

	Girls		
Variable	More successful	Less successful	t-value
Income	6,819	8.060	2.05
	(6690)	(8468)	
Mother's highest grade	0.68	0.42	1.37
	(2.36)	(1.81)	
Father's highest grade	3.28	3.82	1.34
	(4.76)	(4.70)	
Citizenship	0.90	0.76	4.94
	(0.30)	(0.43)	
Distance to school	17.44	19.06	9.55
	(2.05)	(1.84)	
Annual fees	247.0	251.6	0.42
	(128.1)	(134.9)	
Number of observations	599	182	
Girls' enrollment change	41.5%	8.5%	
Boys' enrollment change	36.8%	-1.8%	

Table 7. Estimated Needs to Meet Target Effect

	Elasticities		Change required to meet target effect (25%)	
Alternatives	Girls	Boys	Girls	Boys
Direct subsidy to household	0.503	0.115	3471 Rs./household	15030Rs./household
			(50%)	(150%)
Decrease distance to school	0.320	0.732	13.48 min.	5.71 min.
			(78%)	(34%)

Note: Children in the sample were aged 4 to 7. Numbers in parentheses reports the amount as percentage needed to meet target effect. For example, direct subsidy to household which leads 50% increase in household income may increase 25% increase in girls' enrollment rate.

Table 8. Comparison of the Effect of the Fellowship Program

		Age-specific		Cohort-specific	
Methods	Mathematical Expression	Boys	Girls	Boys	Girls
Measure of effect using means					
Reflexive (1994-1995)	$E^R(\alpha_{it}\vert d_i=1)=E(R_{Tt})-E(R_{i0}\vert d_i=1)$	8.0	18.6	25.5	29.9
		(0.42)	(0.44)	(0.43)	(0.44)
Reflexive (1994-1996)	$E^R(\alpha_{it}\vert d_i=1)=E(R_{Tt})-E(R_{i0}\vert d_i=1)$	19.8	26.0	46.8	44.3
		(0.51)	(0.53)	(0.52)	(0.54)
Difference in Differences (1994-1995)	$E^D(\alpha_{it}\vert d_i=1)=[E(R_{Tt})-E(R_{Nt})]-[E(R_{i0}\vert d_i=1)-E(R_{i0}\vert d_i=0)]$	9.3	15.1	12.4	20.5
		(0.53)	(0.54)	(0.54)	(0.54)
Difference in Differences (1994-1996)	$E^D(\alpha_{it}\vert d_i=1)=[E(R_{Tt})-E(R_{Nt})]-[E(R_{i0}\vert d_i=1)-E(R_{i0}\vert d_i=0)]$	27.4	24.8	23.4	27.4
		(0.73)	(0.70)	(0.74)	(0.71)
Mean-Difference (1994-1995)	$E^M(\alpha_{it}\vert d_i=1)=E(R_{Tt})-E(R_{Nt})$	14.6	25.6	14.6	25.6
		(0.65)	(0.67)	(0.65)	(0.67)
Mean-Difference (1994-1996)	$E^M(\alpha_{it}\vert d_i=1)=E(R_{Tt})-E(R_{Nt})$	32.7	35.1	25.6	32.4
		(0.59)	(0.65)	(0.60)	(0.66)
Measure of effect using regression					
Covariate post-test (1995 cross-sectional)	$R_{it}=X_{it}\,\beta_t + d_i\alpha_t + U_{it}$	22.4	33.4	22.4	33.4
		(0.04)	(0.03)	(0.04)	(0.03)
Covariate post-test (1996 cross-sectional)	$R_{jt}=X_{jt}\,\beta_t + d_j\alpha_t + U_{jt}$	38.4	42.7	26.8	39.9
		(0.07)	(0.05)	(0.05)	(0.04)
First-difference, time-invariant β (1994-1995)	$R_{it}-R_{i0}=d_i\alpha_t+U_{it}-U_{i0}$			29.2	36.7
				(0.08)	(0.07)
First-difference, time-invariant β (1994-1996)	$R_{it}-R_{i0}=d_i\alpha_t+U_{it}-U_{i0}$			8.8	26.4
				(0.10)	(0.08)
First-difference, time-varying β (1994-1995)	$R_{it}-R_{i0}=X_I(\beta_t-\beta_{t-1})+d_i\alpha_t+U_{it}-U_{i0}$			42.8	46.9
				(0.11)	(0.08)
First-difference, time-varying β (1994-1996)	$R_{it}-R_{i0}=X_i(\beta_t-\beta_{t-2})+d_i\alpha_t+U_{it}-U_{i0}$			24.2	28.1
				(0.14)	(0.10)

Note: Numbers in parentheses report standard errors.

Appendix 1

It is difficult to derive income estimates for households in Pakistan. The relative importance of production for home consumption, informal labor market arrangements, barter trade and other economic activity occurring outside formal markets complicate income measurement. The budget for this project did not include resources sufficient to conduct a careful analysis of income for each household. However, the Pakistan Integrated Household Survey (PIHS) had conducted such a detailed survey of household incomes and socioeconomic attributes in 1991. The PIHS allows us to predict household income based on a regression of income on easily observed household attributes. The current study collected information on these household attributes and then used the PIHS estimates to generate predicted incomes based on these attributes.

The PIHS income equation is reported in Table A1. The specification follows that used by Alderman and Garcia (1996). That study estimated income and expenditure equations for 217 households in a single district in Balochistan. The Alderman-Garcia estimates can serve as independent validation of the income estimates we derive from the PIHS data. The Alderman-Garcia estimates are less useful for our purpose than is the PIHS because their data include only rural households and the data are from 1986. The PIHS has sufficient urban observations to estimate an income equation for urban households, and it is closer to our 1994 base period. The variables in the income equation include the number of adult males and females, the number of males and females with primary, secondary and tertiary level schooling, and the value of household assets. Alderman and Garcia found that this income specification generated predicted values that performed well in explaining household savings, loans, and nutrition status.

In general, the PIHS income estimates are sensible. Households with more capital assets, more human capital and more adult males have higher incomes. The results corresponded reasonably well in sign with those in Alderman and Garcia. More importantly, the two estimates generate equivalent estimates of relative household income. The correlation in predicted income based on the PIHS versus the Alderman-Garcia estimates is 0.82. It should also be noted that the higher variance in income in the treatment neighborhoods is a result of three wealthy households residing in those neighborhoods. When those households were removed, the treatment and control neighborhoods had similar means and variances in estimated incomes.

Appendix Table A1: Income Equations

Variable	Alderman and Garcia	PIHS
Intercept	5,999	3,303
	(2.61)	(4.64)
Number of males aged 16 or more	938	1,219
	(0.92)	(3.73)
Number of males aged 6-16	1,691	[a]
	(2.09)	
Number of females aged 16 or more	-709	-188
	(-0.54)	(-0.57)
Number of females aged 6-16	1,009	[a]
	(0.64)	
Number of children 5 or below	2,820	[a]
	(2.99)	
Number of males with primary schooling	6,140	-1,171
	(2.95)	(-2.55)
Number of males with secondary schooling	2,279	-364
	(1.69)	(-0.92)
Number of males with more than secondary schooling	6,435	147
	(1.41)	(0.96)
Number of females with primary schooling	6,707	-406
	(1.85)	(-0.69)
Number of females with middle schooling or more	7,758	889
	(1.35)	(3.68)
Rainfed land	110	[b]
	(2.34)	
Irrigated land	665	[b]
	(4.93)	
Acres of orchards	4,065	[b]
	(2.57)	
Value of livestock	0.335	[b]
	(1.05)	
Value of vehicles	0.171	0.012
	(8.55)	(2.48)
Value of machinery and tools	0.125	0.007
	(1.27)	(1.88)
R^2	0.747	0.03
N	217	2,112

[a]Not available in the PIHS.
[b]Not relevant for urban areas.

Appendix 2

It is possible that the imposition of the fellowship program coincided with other changes in the structure of parental decisions regarding their children's schooling. If so, then we would need to control for changes in the behavioral coefficients over time as well as measuring the program effect itself. To investigate this possibility, we tested the null hypothesis of constant behavioral coefficients against the alternative of time varying behavioral coefficients. The results of the stability tests of coefficients in the enrollment probability equation are reported in Table A2. The results are mixed. We fail to reject any individual hypothesis of equality of coefficients for any regressor. However, the joint hypothesis of no structural change in all parameters was rejected in both samples of girls and of boys. We therefore report the results using both methods. As shown in the body of the paper, the results are similar whether or not structural change is assumed.

Appendix Table A2: t-statistics of Tests of the Hypothesis of Equal Coefficients in the 1994 and 1995 Enrollment Probability Equations

Variable	Boys	Girls
Income	0.821	0.165
Age	1.907	1.305
Age square	2.213	1.911
Mother's highest grade	0.287	0.714
Father's highest grade	0.776	0.507
Birth order	0.345	0.158
Citizenship	0.209	0.507
Joint Test	12.73*	11.67*

*Null hypothesis of equality rejected at the .05 significance level.

Appendix 3

The impact of the fellowship program may be unrelated to neighborhood or individual characteristics, or it may interact with these characteristics. In the text, we show that the effect of the fellowship program did not appear to be related to any neighborhood attributes. However, the power of the test is low because of relatively few degrees of freedom. We can also investigate this issue at the household level. Table A3 reports the results of ordered probit first-difference equations in which the treatment dummy is interacted with all individual attributes. The coefficients of the interacted variables are interpretable as the impact of the fellowship program on behavioral coefficients.

The first two columns of the Table A3 show that there was no structural change in parental behavior regarding their children's enrollment choice when we allow one year time lag. In addition, none of the individual coefficients changed with the imposition of the school. The implication is that the new schools had similar effects on girls' and boys' enrollments regardless of child or household attributes. This is consistent with our earlier finding that effects were similar across all types of neighborhoods.

Some weak evidence of asymmetric effects of the program can be found after two years after the fellowship schools were opened, although the joint test still fails to reject the hypothesis of neutrality. The last two columns of the Table A3 shows that younger children had larger enrollment increases from the program than did older children. This is sensible in that children who were older when the school opened were closer to ages where the parents would remove them from school. The larger effect on girls is consistent with the cultural prohibitions on exposing girls to the public once they reach ten years of age. In addition, older children would have higher opportunity costs. Holding age fixed, first-born girls had a higher probability of enrollment gains than did later-born siblings. This may reflect parental preference for investing in their first-born children's schooling.

Appendix Table A3. First Difference Analysis With Interactions

	1994-1995		1994-1996	
Variable	Girls	Boys	Girls	Boys
Treatment dummy	1.069	0.497	1.814	2.671
	(1.41)	(0.59)	(2.79)	(3.54)
Income/10,000	0.162	0.091	-0.375	-0.003
	(0.66)	(0.03)	(1.91)	(0.11)
Age	-0.019	0.546	-0.475	1.143
	(0.03)	(0.78)	(0.85)	(1.91)
Age square	0.002	-0.054	0.029	-0.100
	(0.04)	(1.00)	(0.70)	(2.19)
Mother's education	0.032	0.002	0.018	-0.006
	(0.94)	(0.05)	(0.63)	(0.20)
Father's education	0.007	0.007	0.024	-0.009
	(0.44)	(0.43)	(1.90)	(0.75)
Citizenship	0.156	0.128	0.226	0.469
	(0.78)	(0.68)	(1.43)	(3.22)
Birth order	0.074	-0.031	0.069	0.009
	(1.88)	(0.84)	(2.15)	(0.32)
Treatment*Income/10,000	-0.103	0.034	0.336	0.124
	(0.40)	(0.10)	(1.61)	(0.46)
Treatment*Age	-0.016	0.037	-0.198	-0.026
	(0.17)	(0.36)	(2.43)	(2.91)
Treatment*Mother's education	-0.032	0.081	0.007	0.054
	(0.64)	(1.44)	(0.17)	(1.04)
Treatment*Father's education	0.002	-0.016	-0.011	-0.012
	(0.07)	(0.65)	(0.60)	(0.54)
Treatment*Citizenship	0.178	0.042	0.307	-0.256
	(0.58)	(0.12)	(1.19)	(0.84)
Treatment*Birth Order	-0.094	-0.034	-0.104	-0.044
	(1.56)	(0.48)	(1.95)	(0.70)
Number of observations	890	740	890	740
Pseudo R^2	0.07	0.07	0.08	0.09
Joint test	3.39	2.71	11.25	9.99

Note: The critical value for the joint test is 12.59 at 0.05 significance level.

Endnotes

[1] Statistics based on 1996 data provided by the Pakistan Education Management Information System.

[2] Alderman, Behrman, Ross and Sabot (1996) found that differences in school availability accounted for 30-40 percent of the gap in cognitive skills between boys and girls in rural Pakistan.

[3] The NGO, the Society for Community Support of Primary Education in Balochistan (SCSPEB), had several years of experience in implementing primary schooling projects, primarily school promotion efforts in rural communities.

[4] The areas selected were primarily areas where squatters had established residence on government land that was not served by the Quetta municipal sewer system.

[5] As all neighborhoods selected to participate accepted the invitation, the issue of self selection is moot.

[6] This does raise the possibility of recall bias, although parents should be able to remember whether their children were in school a year earlier. To provide some verification of this, the analysis that follows uses multiple methods to evaluate the change in enrollment in the treatment neighborhoods and finds that the conclusions are not sensitive to differences in evaluation method.

[7] Interhousehold allocation of schooling is discussed in Parish and Willis (1993), Butcher and Case (1994), and Kaestner (1996).

[8] Grossman (1994) classifies a randomly assigned counterfactual group as a "control group", and a nonrandomly assigned counterfactual group as a "comparison group".

[9] Newman, Rawlings, and Gertler (1994) pointed out that tests are rarely done for statistical significance of the differences, so that probabilities of receiving the program may not be equal for individuals or communities in many of the evaluation studies in developing countries.

[10] The reason girls' enrollment rates were six percentage points higher in the treatment neighborhoods is unclear, although we do not believe there was a strategic selection of the ten fellowship school sites. Of those girls in school, 39 percent were in private school, and 61 percent were in government boys' schools. The large proportion in private school is not unusual. Alderman, Orazem and Paterno (1996) also found extensive use of private schools by even the poorest households in Lahore, another city in Pakistan.

[11] Note that the estimated program effect on first difference analysis is sensitive to the stability of the coefficient over time and the length of the time lag. To validate (12), it was

necessary to perform a statistical test of stability of the coefficients over time. The results of the stability tests do not reject the hypothesis of time-invariant individual coefficients, but the joint test rejects stability. Therefore, there is some statistical support for both specifications (11) and (12). Details are are attached in Appendix 2.

[12] Note that we cannot use methods based on (11) and (12) for the age-specific analysis because enrollment decisions for younger cohorts can only be observed after the fellowship schools are in existence.

[13] The cohort-specific enrollment rates in 1994 are lower than the 1994 average for the age-specific analysis. The reason is that the age-specific groups average one year older in 1994. By 1996, the enrollment rates were higher than in the age-specific analysis because by then, the cohort was one year older on average than the age-specific sample.

[14] Lower average citizenship may also signal a neighborhood with greater ethnic diversity. Because the success of the school depended on an agreement among parents to form a committee, divisions among ethnic lines may have hindered the success of the school.