77299

THE WORLD BANK ECONOMIC REVIEW. VOL. 13, NO. 3: 443-65

Can Private School Subsidies Increase Enrollment for the Poor? The Quetta Urban Fellowship Program

Jooseop Kim, Harold Alderman, and Peter F. Orazem

This study evaluates a program designed to stimulate girls' schooling through the creation of private girls' schools in poor urban neighborhoods of Quetta, Pakistan. Enrollment growth in these randomly selected neighborhoods is compared to enrollment growth in otherwise similar neighborhoods that were randomly assigned to a control group. The analysis indicates that the program increased girls' enrollment around 33 percentage points. Boys' enrollment rose as well, partly because boys were allowed to attend the new schools and partly because parents would not send their girls to school without also educating their boys. This outcome suggests that programs targeted at girls can also induce parents to invest more in their boys. The success of the program varied across neighborhoods, although success was not clearly related to the relative wealth of a neighborhood or to parents' level of education. Thus the program offers tremendous promise for increasing enrollment rates in other poor urban areas.

Private schooling, often postulated to improve school quality, may also be a means to leverage public funds in order to provide access to schooling at rates faster than are possible with public funds alone. This article measures the impact on enrollment of a program designed to encourage the creation of new private girls' schools in Quetta, the capital city of Balochistan Province in Pakistan. The analysis represents a unique opportunity to apply 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 enrollment. We avoid the bias that often arises in such assessments

Jooseop Kim is senior researcher at the Korea Institute for Vocational Education and Training, Peter F. Orazem is with the Department of Economics at Iowa State University, and Harold Alderman is with the Development Research Group at the World Bank. Their e-mail addresses are jskim@krivet.re.kr, halderman@worldbank.org, and pfo@iastate.edu. This study was supported by the research project Impact Evaluation of Education Projects Involving Decentralization and Privatization under RPO 679-18 of the World Bank, supervised by Elizabeth King. The authors are indebted to current and past members of the Balochistan Primary Education Directorate, including Ijaz Ahmed Malik, Mohammed Ishaque, Quaratul Ain, and Bill Darnell, for their kind hospitality and research support. They are also grateful to Sultan Mahmood Niazi of the Balochistan Education Foundation and to Brian Spicer and Fahim Akbar of the Balochistan Management Information System for their help in designing and implementing the data collection. Finally, the authors thank their task managers from the South Asia Division, Mae Chu Chang, Guilherme Sedlacek, and Ivar Andersen, for offering many helpful comments and providing an unusually supportive research environment amid ongoing operational support; the referees for making valuable suggestions; and Donna Otto for preparing the final manuscript.

when individuals or groups who participate in a program are those who are best suited to benefit from it.

I. The Quetta Girls Fellowship Program

Primary school enrollment rates in Pakistan are lower than in other countries at the same level of economic development, including Bangladesh, India, and Nepal. Nationally, the gross enrollment rate is 58 percent: 69 percent for boys and 42 percent for girls. This gender gap is even wider in the province of Balochistan, where 62 percent of boys and only 29 percent of girls are enrolled in school.[1] The government of Pakistan has set a target of achieving universal primary enrollment by 2006. Meeting this goal would require girls' enrollment to more than double nationally and more than triple in Balochistan.

There is evidence that supply constraints partially account for low school enrollment and achievement in Pakistan, especially in rural areas and in poor urban neighborhoods.[2] However, in Pakistan, as in many other countries, the government's ability to increase school capacity is constrained by inadequate public budgets. Expansion is also circumscribed because the government generally constructs, rather than rents, school capacity and requires recipient neighborhoods to provide land for their new government schools. Many poor urban neighborhoods have developed as squatter communities that have poorly defined property rights, limiting their ability to donate land.

In the case of educating girls, the problem is not the lack of schools per se. Cultural prohibitions in many communities mean that educational opportunities for girls are frequently restricted in the absence of gender-specific programs. If Pakistan is to achieve universal primary enrollment for girls, more segregated girls schools or coeducational schools with female teachers will be needed. Given the limitations on increased government provision, one strategy is to make private girls schools more available in poor neighborhoods. Private schools do not face the same problem of land acquisition and may be less constrained financially than government schools. Consequently, the government may devote less time and expense to increasing school capacity for girls if it partially funds the expansion of private schools rather than fully funds the expansion of government schools.

Recognizing these concerns, the Balochistan Education Foundation launched the Urban Fellowship Program in Quetta in February 1995. The purpose of this pilot project was to determine whether establishing private schools in poor neighborhoods was a cost-effective means of expanding primary education for

1. These statistics are based on 1996 data provided by the Pakistan Education Management Information System.

2. Alderman and others (1996) find that differences in school availability account for 30–40 percent of the gap in cognitive skills between boys and girls in rural Pakistan.

girls in Quetta's lower-income neighborhoods. 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 the program could get these poor girls to start school, many would persist long enough to attain literacy.

The Urban Fellowship Program encouraged private schools, which were controlled by the community, to establish new facilities by paying subsidies directly to the schools. Schools were assured of government support for three years. The initial subsidy was 100 rupees (about $3) per month per girl enrolled. There was an upper scholarship limit of 10,000 rupees (100 girls at 100 rupees per girl) per month. This subsidy was sufficient to cover typical tuition at the lowest-priced private schools. In addition, each school received 200 rupees per girl to defray start-up costs. The subsidy was reduced in the second year and again in the third year. By the fourth year schools were expected to be largely self-sufficient through fees and private support, although they would still be eligible to apply to the Balochistan Education Foundation for additional grants. It is possible that uncertainty regarding the schools' long-term sustainability discouraged some parents from enrolling their children. Nevertheless, even if the results presented here are a lower bound, they are substantial. We discuss the issue of sustainability in the concluding section.

Fellowship schools were allowed to admit boys provided that they made up less than half of total enrollment. Boys had to pay tuition at least equal to, and often greater than, that of girls. The grant was calculated only for enrolled girls; schools received no additional subsidy for enrolling boys. Schools were required to keep class sizes at or below 50 boys and girls per classroom and had to hire at least one teacher for each classroom.

To implement the program, the Balochistan Education Foundation contracted the Society for Community Support of Primary Education in Balochistan (SCSPEB), a nongovernmental organization (NGO), to conduct an initial census of each site to ensure that there were a sufficient number of girls in the target age range (four to eight years) and to inform parents of the program. The SCSPEB had several years of experience in implementing primary school projects, particularly school promotion efforts in rural communities. The goal was to create a partnership between neighborhood parents and the school operator. The SCSPEB first conducted a meeting of parents to see if they were interested in attracting a private school to their neighborhood. The parents were asked to form a committee, which would represent the neighborhood in negotiations with potential school operators. With the assistance of the SCSPEB, the parent committee developed a proposal detailing the neighborhood's need for a school, the resources it was willing to provide the school (land, buildings, equipment), and any other requirements an operator was expected to satisfy. Experienced school operators were invited to submit proposals in response. The parent committees were allowed to select their school operator from among the proposals or to choose to run the school themselves.

II. Evaluation Strategy

Because government resources are limited 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. The evaluation problem is to find an unbiased estimator of the impact of the fellowship program.

Use of Randomized Assignment

In this study the outcome variable of interest is school enrollment. However, the approach is general; the use of random assignment is not specific to the evaluation of this educational program or to educational programs per se.

Denoting school enrollment in the treatment and control neighborhoods as R_T and R_N, respectively, ideally we would like to estimate $\alpha = R_{Tit} - R_{Nit}$ for any individual i at time t.[3] However, we cannot estimate α directly, since a child cannot be simultaneously in both the treatment group and the control group.

One way to get an unbiased estimator of α is to use changes in the outcome variable over time. This approach, termed a reflexive evaluation, can be written:

$$E^R(\alpha) = E(R_{Tt}) - E(R_{T0}). \tag{1}$$

The reflexive estimator measures the expected effect of the program as the gap between the enrollment rate after the program was implemented, $E(R_{Tt})$, and the enrollment rate before the program was implemented, $E(R_{T0})$. The underlying assumption of this method is that the period t outcome in the treatment neighborhood without the program would be identical to the observed outcome before the program. In effect, the treatment group in the base period (before the intervention) serves as a control for the treatment group after implementation.[4]

However, reflexive evaluations are sensitive to trends that may be nationwide, but erroneously attributed to the intervention. Thus an alternative approach is to use a control group to derive estimates of the counterfactual state. The difference in outcomes between the treatment and control groups is then used as an estimate of α. This estimator could be either the mean difference, defined as

$$E^M(\alpha) = [E(R_{Tt}) - E(R_{Nt})], \tag{2}$$

or the difference-in-differences, defined as:

$$E^D(\alpha) = [E(R_{Tt}) - E(R_{Nt})] - [E(R_{T0}) - E(R_{N0})]. \tag{3}$$

The mean-difference estimator (equation 2) measures the expected effect of the program as the observed difference in outcomes between the treatment group

3. In this application the treatment neighborhoods are those targeted for the private school promotion and subsidy. A child is considered a member of the treatment group if he or she resides in the treatment neighborhood, whether or not the child is enrolled in a fellowship school. However, the child must be exposed to the possibility of enrolling in a fellowship school.

4. Grossman (1994) classifies a randomly assigned counterfactual group as a "control group" and a nonrandomly assigned counterfactual group as a "comparison group."

and the control group after the program was implemented. This method assumes that the control group perfectly matches the treatment group, which is often achieved by randomly assigning groups to the two populations (Newman, Rawlings, and Gertler 1994).

The difference-in-differences estimator (equation 3) measures the expected effect of the program as the difference between the outcome in the treatment group, $E(R_{Tt})$, and the control group, $E(R_{Nt})$, after program implementation adjusted by the difference between the two groups before implementation. This method assumes that the difference in outcomes between the two groups before the program was introduced would remain constant over time if it were not for the program. Thus the difference in outcomes between the two groups after the program was introduced reflects the initial difference as well as the difference brought about by the program. Differencing the differences yields an estimate of the program effect. If randomization is successful, there will be no difference between equations 2 and 3 because if the two groups are identical at the outset, the term in the second bracket of equation 3 will equal zero.

For reasons discussed below, randomization may be violated. If that happens, it becomes necessary to control for differences between the treatment and control groups that could also influence outcomes. To illustrate, consider a general model of individual enrollment choice in year t:

$$R_{it} = \mathbf{X}_{it}\beta_t + U_{it} \tag{4}$$

In equation 4, $\mathbf{X}_{it}$ is a vector of observed characteristics, U_{it} is an error term, and β_t is a vector of parameters to be estimated. We can derive a covariate post-test estimate of α from a cross-sectional regression in some period t after the program is implemented, assuming that the αs are invariant across individuals:

$$R_{it} = \mathbf{X}_{it}\beta_t + d_i\alpha + U_{it} \tag{5}$$

where d_i is a dummy variable indicating residence in a fellowship school neighborhood. With random assignment into the treatment group, we can assume that d_i is independent of the unobserved variables U_{it}, so that $E(U_{it} | d_i) = 0$. Under this assumption we obtain an unbiased estimate of α. Conversely, if d_i is correlated with the unobserved factors—as, for example, when assignment into the treatment groups is based on unobserved individual or community interest in educaton—then the estimate of α will be biased.

If the data set includes repeated observations of individuals, an alternative way to estimate the effect of the program using econometric analysis is to estimate equation 5 in terms of differences in the variables between the base period and some period, t, after the intervention has taken place.

$$R_{it} - R_{i0} = (\mathbf{X}_{it} - \mathbf{X}_{i0})\beta_0 + d_i\alpha + U_{it} - U_{i0}. \tag{6}$$

Gender Differences in Enrollment Response

Parents may make different schooling investments in girls and boys either because costs differ or because parents obtain different benefits from educating girls and boys. Differences in costs may not just be differences in fees. Gender-specific costs may reflect differences in school access, the opportunity cost of a boy's or girl's time, or any disutility parents suffer because of cultural pressures against sending their daughters to school.

International experience indicates that returns in terms of proportional increases to household and wage productivity generally do not differ appreciably by gender even when average wages or patterns of labor participation do (Behrman and Deolalikar 1995; Schultz 1995). However, differences in how parents assess returns to schooling may also reflect how they benefit from the human capital of their daughters. Their assessment may reflect marriage and residency patterns as well as gender differences in remittances across generations. Nevertheless, Alderman and King (1998) indicate that for many purposes, including that of the present study, reduced-form models of investment in schooling are identical, regardless of whether the gender difference in education reflects differences in costs or differences in how parents value the gains from schooling boys and girls.

The girls fellowship program raises the cost of schooling in terms of fees (rupees) but lowers it in terms of travel time. In addition, it may also lower the cultural disutility of educating girls, which has the same impact as lowering the rupee price. Since all of the preexisting schooling options are still available to the family, the net impact of adding the new choice should be an increase in schooling for girls.[5]

The impact of the fellowship program on boys' enrollment is, however, ambiguous. Lowering the price of girls' schooling may lead to a substitution of girls' time for boys' time, with girls spending more time outside the home and boys devoting more time to home production. But there are at least two reasons why the fellowship program may have a positive impact on boys' schooling. First, the program creates a new low-priced private school that can accept boys. Second, boys' education may increase as their sisters go to school for a very practical reason: parents may want their sons to escort their sisters to and from school. Thus boys' and girls' education may be complementary goods.

If factors that affect enrollment, such as income, the cost of schooling, and the disutility of sending girls to school, differ systematically across treatment and control neighborhoods, they may affect the apparent impact of the program. Thus the econometric specification of equations 5 and 6 includes three measures of schooling costs: the fees charged in the preexisting neighborhood schools; av-

5. Becker (1981: ch. 6) develops a model of human capital investment in children that highlights parents' incentives to equalize wealth among their children. His model suggests that neutral parents invest more in less fortunate children so that all of their children are equally well off. Of course, there is considerable evidence that parents favor boys over girls. Kim, Alderman, and Orazem (1998) analyze the impact of reducing the price of girls' schooling on boys' and girls' enrollment when parents favor boys over girls at equal schooling prices.

erage distance to schools, measured in minutes of travel time (a proxy for transport costs); and the child's age, which may determine the opportunity cost of the child's time. The specification also includes the father's and mother's education, which are assumed to influence how a household values returns to schooling. In addition, the preference for education may depend on the child's birth order (parents may have a preference for educating the eldest child, particularly the eldest boy) and on citizenship (refugees may have different expectations of returns to education).[6] These variables, along with household income and the child's gender, make up the vector of exogenous variables we use in our analysis.

III. DATA

Only a modest number of treatment groups was available because only 10 pilot sites were initially funded. For political expediency the government opted to place one neighborhood school in each of 10 urban slum areas of Quetta, ensuring that all major ethnic groups received at least one school. To accommodate this plan, the sample includes a degree of stratification, under which randomization is based on neighborhoods within each slum area.

A second problem is that no recent census of the population had been conducted from which to define treatment and control populations. The most recent census was 14 years old. The population of Quetta is estimated to have grown at about 7 percent a year since then, mainly within the neighborhoods that made up the target population. Consequently, the designers of the program chose an area frame sampling strategy to define the treatment and control neighborhoods.

They designed the area frame as follows. On a map of Quetta they outlined each of the 10 slum areas, selecting three sites in each, literally points on the map. They then randomly chose one of these areas to be the treatment neighborhood for the creation of a private school. Because all neighborhoods selected to participate accepted the invitation, the issue of self-selection was moot. The other neighborhoods became controls. The only criterion for the treatment neighborhood was that it could not already have a government girls' school. Although the control sites could have had a government girls' school, none of them did.

Given the small number of pilot sites, it is useful to see if the treatment and control populations differ in the factors that might also cause enrollment outcomes to differ. Such tests also help to determine which household characteristics contribute to program participation.

The baseline data collected in the treatment and control sites include information on households' socioeconomic characteristics, parents' education, and the educational attainment and current enrollment status of all children in the household. All of the households in treatment neighborhoods were surveyed in the summer of 1994, when the scholarship program was being promoted and before

6. Intrahousehold allocation of schooling is discussed in Parish and Willis (1993) and Butcher and Case (1994).

any fellowship schools were opened. The baseline survey of households in the control neighborhoods was conducted in July 1995. Because most of the data on household socioeconomic status do not change over a short period—no major economic transformation occurred in 1994 or 1995—the difference in the timing of the surveys should not be problematic. Information on the enrollment status of children in control neighborhoods was obtained for 1995 and retrospectively for the previous year.[7] Enrollment data were subsequently collected in 1996 in both treatment and control neighborhoods. The Balochistan Education Management Information System supervised all data collection and training of surveyors to ensure comparability of the data.

In this article we measure the program's impact using the estimators described in equations 1–6. By doing so, we can ascertain whether the results are robust. Moreover, we apply each of these estimators in two ways. First, we measure the change in enrollment for children in the target range of five to eight years. Second, we measure enrollment rates longitudinally for children ages four to seven in the initial year of the fellowship program.

IV. RESULTS

The treatment sample includes 1,310 children, 781 girls and 529 boys (table 1). The control sample includes 1,358 children, 697 girls and 661 boys. The dependent variable in table 1 is a dummy variable that equals 1 if the child was enrolled in school. The other variables are exogenous variables believed to affect parents' enrollment choices for their children. Most of the variables come directly from the questionnaire. However, distance to school and annual fees are neighborhood averages of the children in school. We estimate household income using the number of adults in the household, their educational attainment, and a set of household assets. Details on the estimate of household income are contained in the appendix.

We test for statistical significance of the differences between the treatment and the control groups in two ways.[8] First, in order to determine if the randomization yielded observationally equivalent treatment and control populations, we test for the equality of means of the endogenous and exogenous variables. Second, we estimate enrollment equations using the baseline data. These equations test the null hypothesis of the equality of behavioral coefficients in the enrollment choice models for the treatment and control neighborhoods.

7. Collecting data retrospectively raises the possibility of recall bias, although parents should be able to remember whether their children were in school a year earlier. To verify this, we use multiple methods to evaluate the change in enrollment in the treatment neighborhoods. We find that the conclusions are not sensitive to differences in evaluation method.

8. Newman, Rawlings, and Gertler (1994) point out that researchers rarely test for statistical significance of the differences, so that probabilities of receiving a program may not be equal for individuals or communities in many of the evaluation studies in developing countries, especially those with few observations in the treatment group.

Table 1. *Summary Statistics of Baseline Data and Tests of the Equality of Means between Treatment and Control Groups*

	Girls			*Boys*		
Variable	*Treatment*	*Control*	*t-value*[a]	*Treatment*	*Control*	*t-value*[a]
Endogenous						
Enrollment rate	0.366	0.300	2.67	0.486	0.398	3.03
	(0.482)	(0.459)	[1,468]	(0.500)	(0.490)	[1,180]
Exogenous						
Household income	7,108	6,808	1.03	7,005	6,592	1.41
	(7,157)	(3,011)	[1,476]	(6,815)	(2,847)	[1,188]
Age	6.026	6.001	0.19	6.040	6.003	0.44
	(1.403)	(1.429)	[1,476]	(1.426)	(1.444)	[1,188]
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)	[1,183]
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)	[1,162]
Birth order	2.832	3.004	2.21	3.074	2.965	1.27
	(1.474)	(1.510)	[1,476]	(1.447)	(1.482)	[1,188]
Citizenship	0.868	0.835	1.79	0.877	0.814	2.98
	(0.339)	(0.371)	[1,476]	(0.329)	(0.389)	[1,188]
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]	(1,036.8)	(765.1)	[505]
Joint test[b]			9.0			27.2
Number of observations	781	697		529	661	

Note: Sample includes girls and boys ages four to eight in the baseline year. The baseline data were collected in 1994 for the treatment group and in 1995 for the control group. The numbers in parentheses are the standard deviations corrected for cluster effects using Huber's method. The numbers in square brackets are the degrees of freedom. The degrees of freedom differ because of missing information in the surveys.

a. The null hypothesis is that the mean of the variable in the treatment group is equal to that in the control group. If the z-value is smaller than 1.96, the null hypothesis cannot be rejected at the 5 percent significance level.

b. Corrected F-statistics with degrees of freedom (8, 1,376) for girls and (8, 1,149) for boys. The null hypothesis is that the means of the eight exogenous variables are jointly equal across the treatment and control neighborhoods. For both boys and girls the test statistic exceeds the critical value of 1.94 at the 5 percent significance level.

Source: Authors' calculations.

Baseline enrollment rates for both sexes are significantly higher in the treatment group than in the control group (columns 4 and 7 of table 1).[9] In addition, birth order and mother's education differ significantly between girls in the treatment and control neighborhoods, although the differences in means are small numerically. For boys, citizenship and father's education are significantly higher

9. It is unclear why girls' enrollment rates are 6 percentage points higher in the treatment neighborhoods, although we do not believe that the 10 fellowship school sites were strategically selected. Of the girls in school, 39 percent attended private school and 61 percent attended government boys' schools. The large proportion in private school is not unusual, especially given the limited availability of government schools. Alderman, Orazem, and Paterno (1996) also find that poor households in Lahore, Pakistan, used private schools extensively.

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

	Girls and boys			*Girls*			*Boys*		
Variable	*Treatment*	*Control*	*Difference*[a]	*Treatment*	*Control*	*Difference*[a]	*Treatment*	*Control*	*Difference*[a]
Household income per 10,000 rupees	0.138	0.422	2.03	0.171	0.572	2.58	0.037	0.218	0.25
	(2.362)	(2.879)		(2.377)	(2.870)		(0.346)	(0.954)	
Age	1.820	2.235	0.09	1.611	2.623	0.02	2.176	1.927	0.14
	(5.226)	(6.323)		(3.612)	(4.864)		(3.674)	(3.986)	
Age squared	−0.101	−0.140	0.89	−0.089	−0.174	0.47	−0.119	−0.119	0.99
	(3.621)	(5.014)		(2.508)	(4.127)		(2.513)	(2.884)	
Mother's highest grade	0.051	0.094	1.50	0.067	0.118	0.73	0.007	0.072	1.95
	(2.443)	(3.422)		(2.500)	(2.649)		(0.197)	(1.963)	
Father's highest grade	0.023	0.065	7.68	0.027	0.084	6.88	0.025	0.050	1.09
	(2.369)	(6.634)		(2.271)	(5.997)		(1.498)	(3.500)	
Birth order	−0.029	−0.036	0.07	−0.017	−0.020	0.00	−0.036	−0.053	0.04
	(0.918)	(1.214)		(0.416)	(0.461)		(0.717)	(1.251)	
Citizenship	0.693	0.335	1.20	0.628	0.214	1.36	0.762	0.538	0.00
	(5.207)	(2.556)		(3.590)	(1.079)		(3.545)	(2.888)	
Girl	−0.419	−0.541	0.05						
	(4.878)	(5.340)							
Number of observations	1,231	1,324		725	677		506	647	
Joint test[b]			29.9			23.3			13.7
Pseudo R^2	0.277	0.295		0.230	0.331		0.358	0.293	

Note: The numbers shown in parentheses are *z*-values corrected for cluster effects using Huber's method.

a. Test of the difference in coefficients between treatment and control neighborhoods, corrected for cluster effects using Huber's method.

b. Joint chi-square test of the null hypothesis of equality of coefficients across treatment and control neighborhoods. All results reject the null hypothesis of equality.

Source: Authors' calculations.

in the treatment group, although, again, the differences in means are small numerically. The joint test that the means of the exogenous variables are equal across all variables is easily rejected for both boys and girls. Therefore, we can reach a statistical conclusion that the treatment and control samples are not identical, a problem that we address in the analysis below.

A second way in which the treatment and control neighborhoods may differ is in parents' decisionmaking processes. To check this, we estimate a probit model of school enrollment based on equations 8–9 (table 2). The estimated parameters for the control and treatment groups exhibit the same signs and are qualitatively similar to results obtained in other studies of enrollment. The coefficient on household income is positive in both samples. Mother's and father's educational attainment positively influence their children's enrollment. Enrollment increases with age, but at a diminishing rate. First-born children have a higher probability of enrolling than their younger siblings, but the coefficient is not significant. Native Pakistanis also have a higher probability of enrolling than noncitizens. After pooling the treatment and control data, we can also estimate the effects of the average distance to school and average annual fees. Both have negative coefficients, except for a positive but insignificant effect of annual fees on boys' schooling.

The coefficients for the two groups are not statistically different, except for father's educational level in the girls' enrollment equation.[10] This result suggests that parents' decisions about education are similar in the treatment and control neighborhoods. Despite significant differences in characteristics between the control and treatment groups (as reported in table 1), we can still measure the change in enrollment due to the program by measuring the difference in enrollment rates between treatment and control groups, holding constant the differences in the exogenous variables.

Comparisons of Mean Enrollment Rates

Using enrollment rates for boys and girls before and after the program intervention (table 3), we can apply the three methods based on equations 1–3 (table 4). We report both age-specific and cohort-specific effects. The age-specific analysis looks at the enrollment of children ages five to eight in a specific year, while the cohort-specific analysis follows the enrollment of a fixed group of children ages four to seven in 1994.[11]

The age-specific results are similar for the three methods. All imply that the fellowship program had a positive effect on the enrollment of girls in the target age group as well as on that of boys. Applying the same methods to two years of data yields even larger estimates of the enrollment effects.

10. See Kim, Alderman, and Orazem (1998) for details of the statistical tests.

11. The cohort-specific enrollment rates in 1994 are lower than the 1994 average for the age-specific enrollment rates. The reason is that the age-specific groups are on average one year older in 1994. By 1996 the enrollment rates in the cohort-specific groups are higher than in the age-specific groups because, by then, the cohort-specific groups are on average one year older than the age-specific groups.

Table 3. *Enrollment Rates Before and After the Program*
(percent)

	Age-specific				*Cohort-specific*			
	Treatment		*Control*		*Treatment*		*Control*	
Outcome measure	*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

Note: The age-specific analysis records the enrollment of children ages five to eight in the specified year, while the cohort-specific analysis follows the enrollment over time of a fixed group of children ages four to seven in the base year.

Source: Authors' calculations.

Table 4. *Age- and Cohort-Specific Effects of the Fellowship Program on Enrollment Rates*

Estimation method	*Mathematical expression*	*Age-specific*		*Cohort-specific*	
		Boys	*Girls*	*Boys*	*Girls*
Measure of effect using means					
Reflexive (1994–95)	$E^R(\alpha) = E(R_{Tt}) - E(R_{T0})$.	8.0	18.6	25.5	29.9
		(0.42)	(0.44)	(0.43)	(0.44)
Reflexive (1994–96)	$E^R(\alpha) = E(R_{Tt}) - E(R_{T0})$.	19.8	26.0	46.8	44.3
		(0.51)	(0.53)	(0.52)	(0.54)
Difference-in-differences (1994–95)	$E^D(\alpha) = [E(R_{Tt}) - E(R_{Nt})] - [E(R_{T0}) - E(R_{N0})]$.	9.3	15.1	12.4	20.5
		(0.53)	(0.54)	(0.54)	(0.54)
Difference-in-differences (1994–96)	$E^D(\alpha) = [E(R_{Tt}) - E(R_{Nt})] - [E(R_{T0}) - E(R_{N0})]$.	27.4	24.8	23.4	27.4
		(0.73)	(0.70)	(0.74)	(0.71)
Mean-difference (1994–95)	$E^M(\alpha) = [E(R_{Tt}) - E(R_{Nt})]$.	14.6	25.6	14.6	25.6
		(0.65)	(0.67)	(0.65)	(0.67)
Mean-difference (1994–96)	$E^M(\alpha) = [E(R_{Tt}) - E(R_{Nt})]$.	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)	$R_{it} = X_{it}\beta_t + d_t\alpha_t + U_{it}$	22.4	33.4	22.4	33.4
		(0.04)	(0.03)	(0.04)	(0.03)
Covariate post-test (1996 cross-sectional)	$R_{it} = X_{it}\beta_t + d_t\alpha_t + U_{it}$	38.4	42.7	26.8	39.9
		(0.07)	(0.05)	(0.05)	(0.04)
First-difference, time-invariant β (1994–95)	$R_{it} - R_{i0} = d_t\alpha_t + U_{it} - U_{i0}$			29.2	36.7
				(0.08)	(0.07)
First-difference, time-invariant β (1994–96)	$R_{it} - R_{i0} = d\alpha_t + U_{it} - U_{i0}$			8.8	26.4
				(0.10)	(0.08)
First-difference, time-varying β (1994–95)	$R_{it} - R_{i0} = X_i(\beta_t - \beta_{t-1}) + d_t\alpha_t + U_{it} - U_{i0}$			42.8	46.9
				(0.11)	(0.08)
First-difference, time-varying β (1994–96)	$R_{it} - R_{i0} = X_i(\beta_t - \beta_{t-2}) + d_t\alpha_t + U_{it} - U_{i0}$			24.2	28.1
				(0.14)	(0.10)

Note: Numbers in parentheses are standard errors corrected for cluster effects using Huber's method.
Source: Authors' calculations.

The cohort-specific analysis has the advantage of enabling us to control for unobservable effects that are specific to the individual and that might also be correlated with program outcomes. However, because enrollment increases with age, at least initially, some of the enrollment growth in the cohort-specific analysis will reflect a maturity effect. This effect will bias the reflexive method estimates upward. Indeed, the implied 46.8 percent increase in boys' enrollment, and 44.3 percent increase in girls' enrollment between 1994 and 1996 (the last two columns of table 4) are much higher than the corresponding estimates in the age-specific analysis.

The estimates generated from the difference-in-differences and mean-difference methods eliminate the maturity effect by assuming that it is common across neighborhoods. Consequently, the measured program effects using these methods are smaller than the reflexive estimates and are more comparable to the estimates generated with the age-specific sample. All of the results show large gains in both boys' and girls' enrollment 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 girls' enrollment rose 25–35 percent as a result of the program and that boys' enrollment rose a few percentage points less.

Comparisons Using Regression Analysis

Because the treatment and control neighborhoods have different characteristics that are believed to affect parents' educational choices, a simple comparison of unconditional means could yield biased estimates of the program effect. We use an alternative method based on equation 5 on the same samples.[12] The program effects based on the covariate post-test using cross-sectional data are reported in table 4; the full probit regression results are in table 5. The enrollment rate in fellowship neighborhoods rose 33.4 percent for girls and 22.4 percent for boys in the first year of the program (table 4). After two years enrollment in the fellowship neighborhoods had risen 42.7 percent for girls and 38.4 percent for boys using the age-specific analysis. The gain was slightly less using the cohort analysis. These results are consistent with the results based on community means.

Considering that the fellowship schools were established in February 1995 and that survey data were collected in July of that year, the response of parents in target areas was nearly instantaneous. This supports the view that there was excess demand for primary education in these poor areas. Moreover, the fellowship program grew more successful year by year. For girls the estimated program effect increased almost 10 percent in 1996 relative to the effect in 1995. Boys' enrollment rates grew 16 percent in the year after implementation.

Another possible source of bias in our estimates of the program's effect is unobserved heterogeneity in children that is correlated with the program's out-

12. We cannot use first-difference methods for the age-specific analysis because enrollment decisions for younger cohorts can be observed only after the fellowship schools are in existence.

Table 5. *Post-Test Probit Analysis of Probability of Enrollment Using Cross-Sectional Data*

	1995[a]		*1996, cohort-specific*[b]		*1996, age-specific*[c]	
Variable	*Girls*	*Boys*	*Girls*	*Boys*	*Girls*	*Boys*
Treatment dummy	0.334	0.224	0.399	0.268	0.427	0.384
	(10.148)	(5.143)	(9.679)	(5.511)	(8.488)	(5.495)
Household income per 10,000 rupees	–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 squared	–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 per 1,000 rupees	–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

Note: The coefficients reported here are dF / dX, where F is the dependent variable and X is the 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 parentheses are z-values corrected for cluster effects using Huber's method. Dummy variables for each neighborhood are included.

a. Children are ages five to eight in 1995. Dependent variable is enrollment status in 1995.

b. Children are ages five to eight in 1995. Dependent variable is enrollment status in 1996.

c. Children are ages five to eight in 1996. Dependent variable is enrollment status in 1996.

Source: Authors' calculations.

come. If cross-sectional differences in individual fixed effects are contributing to measured program effects, then we can remove the fixed effects by differencing the dependent variable.

We conduct the first-difference analysis under the assumption that the coefficients of the regressors are time-invariant, as in equation 6 (table 6). The dependent variable is the change in enrollment status before and after 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.

The results of these tests corroborate the results presented above in that the coefficient representing the program effect is significantly positive and larger for girls than for boys. However, the estimated program effect is larger after one

Table 6. *First-Difference Probits for the Change in Enrollment Decision*

	1994–95		1994–96		1994–1995		1994–96	
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)
Change in age squared	–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 in 1994 squared					–0.001	–0.022	–0.047	–0.080
					(0.040)	(0.525)	(1.055)	(1.686)
Income per 10,000 rupees					–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 per 1,000 rupees					–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

Note: The coefficients reported here are dF / dX, not actual coefficients. Children in the sample were ages four to seven in 1994.
Source: Authors' calculations.

year than after two years, in contrast to the cross-sectional results. The reason for this discrepancy is unclear, although it may be related to the fact that first-difference regressions control for fixed effects. Enrollment rates were initially higher in the fellowship neighborhoods, and children who were in school before the fellowship schools opened do not contribute to the measured enrollment effect in the first-difference analysis.

Also, the opening of the fellowship schools may have encouraged parents to send their children to school at a younger age, 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 have been related to the fellowship program if the promotion of children's education in fellowship neighborhoods spilled over to control neighborhoods. Nevertheless, the estimated two-year effect on enrollment growth is still large. Controlling for fixed effects lowers the estimated effect 12 to 30 percent, leaving the estimated enrollment impact at 24.2 percent for boys and 28.1 percent for girls.

Given the apparent success of the fellowship schools in increasing enrollment, the question remains as to how much the children are learning. Assessment efforts are in their infancy in Balochistan, but the Balochistan Education Management Information System did pilot an achievement test to several third-grade classes, including some from fellowship schools. The results are not definitive because the samples were small, but they show no significant differences in outcome between fellowship and government schools. Still, any concrete assessment of the relative quality of fellowship and government schools will require further tests.

V. Discussion

The fellowship program certainly increases enrollment. But is it cost-effective when compared with alternative policy options? One way to look at this question is in terms of the cost per student enrolled. In 1996 the recurrent cost per student in government primary schools in Balochistan was 2,500 rupees (World Bank 1997). This amount is nearly twice the subsidy per girl offered to the fellowship schools in the first year of operation and is far more than three times the subsidy per student.[13] The disparity is not due to cost recovery from students, since in the first year of operation students were asked to pay only between 10 and 25 rupees per month. The difference comes mainly from lower teacher salaries.

In addition to these recurrent costs, the Primary Education Department spent approximately 1,500 rupees per student in contracting a local NGO to help communities establish schools and train teachers. This amount also covered the monitoring of schools during their first two years. Since the fellowship program did

13. Ideally, we would want to evaluate the cost of educating a student until graduation, but the project is too new to ascertain this.

not build schools, these costs were its main start-up expenses. In the future NGO costs will be lower, since the cost of monitoring activities necessary to conduct this evaluation will not be needed in expanding the pilot program. Nevertheless, even the upper-bound estimated start-up cost of 1,500 rupees per student is far less than the estimated cost of 600,000 rupees for the construction of a government primary school, which typically has two classrooms. Assuming 50 students per classroom, this represents an initial investment of 6,000 rupees per student. Thus there is a substantial difference in the cost of establishing a government school relative to a fellowship school.

Instead of looking at the average cost per student, we can look at the marginal cost of increasing enrollment. We consider two alternative policies needed to match the enrollment increase that resulted from the fellowship program: income transfers to poor households and construction of new schools. Our estimates are based on estimated elasticities of enrollment choice with respect to income and distance to school.

Income has only a moderate impact on participation in the program. Consequently, the benefits of the program are not strongly skewed to upper-income households. The moderate income response also implies that it would take a sizable income transfer to achieve the same impact on enrollment as the program. In particular, the income response in our estimates implies that a direct subsidy of 3,471 rupees per household would be needed to raise the probability of girls' enrollment 25 percent (table 7). This is roughly 2.5 times the initial recurrent cost of a fellowship school—1,400 rupees a year per girl. As boys' enrollment is less income-sensitive, a similar increase in the probability of boys' enrollment would require an income transfer of 15,030 rupees, compared with the negligible marginal cost of increasing boys' enrollment through the girls' fellowship program.[14]

The overall impact of the fellowship program might also be influenced by the fact that it reduces the distance to schools. Unfortunately, in our sample there is insufficient variance in distance to schools to directly estimate this influence. Using the 1996 coefficient of distance for girls, –0.03 (see table 5), 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. Halving the distance to schools in a two-dimensional environment implies a fourfold increase in the number of schools.

An additional question is whether the success of the fellowship program depended on the attributes of the neighborhoods in which the new schools were instituted. There are large differences in success rates across neighborhoods, with the increase in girls' enrollment varying from 8 to 67 percentage points. Increases in boys' enrollment also differ across neighborhoods, from a drop of 2 percent-

14. The result for Quetta is similar to results for both sexes in low-income neighborhoods of Lahore, where a 10 percent increase in 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 in which overall primary school enrollment rates are more than 90 percent, an income transfer of 14,808 rupees would be required to raise enrollment 25 percent for both sexes.

Table 7. *Estimates of Alternative Ways to Raise Enrollment to the Target Level*

	Elasticities		*Change required to meet target level (25 percent)*	
Alternatives	*Girls*	*Boys*	*Girls*	*Boys*
Direct subsidy to household	0.503	0.115	3,471 rupees per household (50 percent)	15,030 rupees per household (150 percent)
Decrease distance to school	0.320	0.732	13.48 minutes (78 percent)	5.71 minutes (34 percent)

Note: Children in the sample were ages 4 to 7. The numbers in parentheses are the amount as a percentage needed to meet target effect. For example, a direct subsidy to the household that leads to a 50 percent increase in household income may raise girls' enrollment rates 25 percent.

Source: Authors' calculations.

age points to an increase of 61 percentage points. Are the fellowship schools more successful in neighborhoods where households are not as poor, better educated, or unique in other observable ways? For the most part success appears to be unrelated to neighborhood attributes.[15] The attributes of neighborhoods with above-average enrollment gains do not differ much from those with below-average gains. For example, average parental education levels are similar in the most and least successful neighborhoods, and average income levels are actually lower in the more successful neighborhoods. One intriguing result is that neighborhoods with the largest increases in girls' enrollment also have the largest increases in boys' enrollment, which is consistent with our presumption that boys' and girls' schooling are complementary goods.

To the extent that the schools benefit some types of children more or less than others, it appears that enrollment of younger children rises more than that of older children. This is a natural consequence of the fact that older children are more likely to be at an age at which parents planned to remove them from school anyway. First-born children are more positively affected, presumably because parents favor the eldest child. Nevertheless, the joint test of uniform effects across all children cannot be rejected at standard significance levels, suggesting a high probability of success from expanding the program to other poor neighborhoods, regardless of residents' socioeconomic attributes.

A final concern is whether these schools are sustainable. Ideally, we would like to know if they will continue to operate for a generation or more. Unfortunately, we cannot answer this question given the time frame of our research. There are, however, encouraging indications that the program is viable. First, there is a demand for such schools from urban neighborhoods, including other cities in the province. The program has expanded from 11 schools with slightly more than 2,000 students to 40 schools with 10,000 students. All of the schools that opened in urban areas between 1995 and 1998 remain open. Moreover, the enrollment

15. This discussion is based on Kim, Alderman, and Orazem (1998: 21–22, app. 3).

of girls in the original schools covered in this study increased 15 percent between 1996 (the last year in our household sample) and 1997, despite a nominal increase in fees of 15 percent—a small real increase of about 3 percent.[16]

Still, the schools are not financially independent. The Balochistan Education Foundation provides about 20 percent of the subsidy given to these schools in their first year of operation. This commitment—financed from an endowment that is distinct from the Primary Education Department's budget—may be intended to address the issue of equity. There is no direct indication that it is needed to ensure the viability of the schools.

VI. SUMMARY

We have used several evaluation methods based on experimental design to measure the effect of the Quetta Urban Fellowship Program on the enrollment of boys and girls in poor neighborhoods. Regardless of how the impact is measured, we find that the fellowship program raised enrollment for both boys and girls (see table 3). Most estimates show that the effect was larger for girls than for boys. We can conclude that the estimated program effects are robust to differences in assumptions about possible biases arising from measured and unmeasured differences between treatment and control neighborhoods.

Before the project was implemented, it was not clear whether girls' low enrollment rates were due to cultural barriers that cause parents to keep their daughters out of school or to an inadequate supply of girls' schools. The urban fellowship experiment provides strong evidence that subsidizing the establishment of primary schools for girls can sharply increase girls' enrollment. In addition, even though the fellowship was given only to girls, boys' enrollment in those neighborhoods also increased sharply. This suggests that boys' education and girls' education are complementary goods: by encouraging parents to send their girls to school, the program had collateral benefits of raising boys' enrollment rates.

The measured change over two years yields mixed evidence on whether the advantage for enrollment growth in fellowship neighborhoods relative to control neighborhoods continued to increase. However, even if the initial enrollment gain decreased in subsequent years, the increase in enrollment after two years was still around 25 percentage points. This is a substantial improvement over the baseline enrollment rate of 45 percent for girls who are five to eight years old. School success appears not to depend on neighborhood income or other observable socioeconomic variables, suggesting that expanding 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 sustainability of the schools and the enrollment ef-

16. The school with the largest fee increase has the largest percentage increase in enrollment, perhaps reflecting the endogeneity of fee structures.

fects after the subsidies expire must be evaluated. The short-term success of the fellowship program does not guarantee long-term success when the financial burden of supporting the schools is fully borne by the neighborhoods. School outcomes must also be assessed. The ultimate success of the fellowship program depends on whether children attain literacy.

Appendix. Details on the Estimates of Household Income

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 efforts to measure income. The budget for this project did not include resources sufficient to carry out a careful analysis of income for each household. However, the Pakistan Integrated Household Survey (PIHS) conducted such a detailed survey of household income and socioeconomic attributes in 1991. The PIHS allows us to predict household income based on a regression of income on easily observed household attributes. In this study we collected information on these attributes and then used the PIHS estimates to generate predicted income.

The PIHS income equation is reported in table A-1. The specification follows Alderman and Garcia (1996), who estimate income and expenditure equations for 217 households in a single district in Balochistan. Their estimates can serve as independent validation of the income estimates we derive from the PIHS data. They are less useful for our purpose than is the PIHS because their data are from 1986 and include only rural households. 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 men and women, the number of men and women with primary-, secondary-, and tertiary-level schooling, and the value of household assets. Alderman and Garcia find that this income specification generates predicted values that perform 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 correspond reasonably well in sign with those in Alderman and Garcia. More important, the two studies generate equivalent estimates of relative household income. The correlation in predicted income based on the PIHS compared with the Alderman-Garcia estimates is 0.82. The higher variance in income in the treatment neighborhoods is a result of three wealthy households residing in those neighborhoods. When those households are removed, the treatment and control neighborhoods have similar means and variances in estimated income.

Table A-1. *Income Equations*

Variable	*Alderman and Garcia*	*Pakistan Integrated Household Survey*
Intercept	5,999	3,303
	(2.61)	(4.64)
Number of males ages 16 or older	938	1,219
	(0.92)	(3.73)
Number of males ages 6–16	1,691	—
	(2.09)	
Number of females ages 16 or more	–709	–188
	(–0.54)	(–0.57)
Number of females ages 6–16	1,009	—
	(0.64)	
Number of children age 5 or younger	2,820	—
	(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	n.a.
	(2.34)	
Irrigated land	665	n.a.
	(4.93)	
Acres of orchards	4,065	n.a.
	(2.57)	
Value of livestock	0.335	n.a.
	(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
Number of observations	217	2,112

— Not available.
n.a. Not applicable.
Note: Numbers in parentheses are standard errors.
Source: Authors' calculations.

REFERENCES

The word "processed" describes informally reproduced works that may not be commonly available through library systems.

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):229–54.

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

Alderman, Harold, and Elizabeth King. 1998. "Gender Differences in Parental Investment in Education." *Structural Change and Economic Dynamics* 9(4):453–68.

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." Impact Evaluation of Education Reform Working Paper 2. World Bank, Development Research Group, Washington, D.C. Processed.

Becker, Gary S. 1981. *A Treatise on the Family.* Cambridge, Mass.: Harvard University Press.

Behrman, Jere, and Anil Deolalikar. 1995. "Are There Differential Returns to Schooling by Gender? The Case of Indonesian Labor Markets." *Oxford Bulletin of Economics and Statistics* 57(1):97–117.

Butcher, Kristen, and Anne Case. 1994. "The Effect of Sibling Sex Composition on Women's Education and Earnings." *Quarterly Journal of Economics* 109(3):531–62.

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

Kim, Jooseop, Harold Alderman, and Peter Orazem. 1998. "Can Private School Subsidies Increase Schooling for the Poor? The Quetta Urban Fellowship Program." Impact Evaluation of Education Reforms Working Paper 11. World Bank, Development Research Group, Washington, D.C. Processed.

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):863–98.

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

World Bank. 1997. "Pakistan: Toward a Strategy for Elementary Education." Report 16670-PAK. World Bank, South Asia Region, Washington, D.C. Processed.