The Dynamics of Education and Fertility:
Evidence From a Family Planning Experiment

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I. Introduction

Recent theoretical and empirical studies suggesting that there are important externalities to human capital formation in the process of economic development (e.g., Lucas 1988; Eckstein and Zilcha 1994; Foster and Rosenzweig 1994) have renewed interest in the evaluation of policies designed to increase schooling in low-income countries. While there is general agreement that direct schooling subsidies can play a role in this regard, there remains substantial disagreement about the relative merits of using less direct policies including family planning programs to influence levels of schooling.[1] Although it has frequently been argued that family planning subsidies may play an important role in increasing schooling by decreasing family sizes and thus increasing the willingness and/or ability of parents to invest in schooling (e.g., Birdsall and Griffith 1988), there is also evidence suggesting that these effects, if present, are likely to be small: the effects of family planning programs on fertility have generally been found to be limited (Schultz 1994; Gertler and Molyneaux 1994; Pitt, Rosenzweig, and Gibbons 1994) as have the effects of exogenous variation in fertility on schooling (Rosenzweig and Schultz 1987; Rosenzweig and Wolpin 1980).

There is little direct evidence on the effects of family planning programs on schooling and those few studies that are available provide an incomplete picture of the process. One reason for this is that some component of the effect of such programs on schooling are only likely to be observed after a considerable lag, a problem that takes on particular significance when, as is the case for most of the above studies,

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[1]It might be argued that since policy makers can directly subsidize schooling there is no particular need to consider alternative mechanisms for increasing schooling. The problem with this argument is that public subsidies to schooling may themselves introduce externalities to childbearing because (1) the per-capita burden associated with financing these subsidies is affected by the age distribution of a population and thus levels of fertility (Lee 1990) (2) unless they condition on family size these subsidies will in general distort the return to childbearing. Family planning programs may also be a cost effective way of increasing schooling when there are substantial fixed costs associated with setting up new programs and there is a basis for subsidizing contraceptive use unrelated to schooling attainment.

region-specific fixed effects are removed to avoid bias associated with the endogeneity of program placement (e.g., Pitt, Rosenzweig, and Gibbons 1994). When identifying program effects using changes over time, one can evaluate the short-run effects of programs such as those that would arise, for example, if parents are less willing or able to send their older children to school when younger siblings are present; however, unless the study interval in question is of sufficient duration one is likely to miss longer-run effects such as those that would arise if schooling is costly and credit markets are imperfect so that households with a large number of school-age children at any particular point in time are likely to provide these children with less schooling[2]--in this case family-planning effects might only be observed after a lag of 8-10 years at the earliest.

Even less is known about the implications of family planning programs for sex differentials in schooling. In a society with strong differences in attitudes towards male and female children such as the one studied in this paper (as evident, for example, in sex differentials in mortality (D'Souza and Chen, 1980) and use of health services (Chen, Huq and D'Souza, 1981)), it is possible that the introduction of a family planning program may either decrease or reinforce these differentials. A decrease in sex differentials associated with the introduction of family planning services might arise if child care responsibilities are differentially assigned to girls thus precluding their regular attendance at school under a high fertility regime, while the opposite effect might be observed if there is substantial son preference so that households with sons are more likely to make use of contraceptive services than are households with daughters. While studies characterizing differences in schooling levels by family size and sex composition (Parish and Willis 1993; Butcher and Case, 1994) can provide some insight into the likely effects of family planning programs on sex differentials in schooling, interpretation of the results from these studies is difficult because they do not in general examine the extent to which fertility choices are themselves influenced by schooling

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[2]Jacoby (1994) and Jacoby and Skoufias (1994) provide evidence that credit market imperfections affect educational allocations in low-income areas.

decisions.[3]

In this paper we examine the effects of an experimental family planning program that was begun in rural Bangladesh in 1978. We first develop a multi-period model in which parents repeatedly face a tradeoff between childbearing and providing education to school-age children. This model is used to construct decision rules for fertility and schooling, which are then estimated. The estimates show that: (1) the family planning program reduced fertility and increased schooling; (2) an increase in the number of school-age and older children lowers subsequent fertility and raises educational attainment; (3) the primary mechanism by which the number of school-age and older children affect schooling is through their effect on fertility; (4) there are significant sex composition effects on schooling that arise because couples with daughters are less likely than those with sons to limit subsequent fertility; and (5) short-run estimates of a family planning programs that do not account for the indirect effects of the program operating through changes in family size and composition tend to overestimate reductions in fertility and underestimate increases in schooling.

II. Model and Empirical Implementation

The theoretical framework for this analysis is provided by a multi-period model in which parents in each period choose contraceptive use, consumption, and human capital inputs for their children.[4] In order to capture the notion that children play an important role as a source of old age support, it is assumed that parents cannot transfer resources across time except through investment in their children, either by

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[3]For example, suppose that there is heterogeneity in tastes for schooling, that couples with higher preferences for schooling have smaller families, and that son preference results in differences in fertility by sex composition of older siblings. Then a regression of schooling on the number of children and the proportion female will yield biased estimates of both effects (the latter by virtue of the correlation between family size and proportion female). If the variances in taste heterogeneity for female and male schooling differ, one could even get the appearance of differential effects of sex composition on male and female schooling as in Butcher and Case (1994) when no such differential were present.

[4]This model is essentially a dynamic version of Rosenzweig and Evensen (1977) which made use of variation in child wages to study the fertility-schooling tradeoff.

changing the number of children or the level of schooling provided to each. The specification of the model is as follows. Parents maximize expected discounted utility over their lifetime

$$E_1 \sum_{x=1}^{X} \beta^x u(c_x) \tag{1}$$

where x denotes age, with x=1 and x=X being the initial and final ages at childbearing, respectively, β is the discount factor, and $u(c_x)$ is the single-period utility received by the parents when they consume c_x. It is also assumed that parents cannot borrow or lend, that income is provided by grown children and by school-age children when they are not attending school but not by pre-school children, and that each period represents approximately 8 years so that a child born when parents are age x will be school-age when the parents are age x+1 and grown when parents are age x+2.[5] Thus the budget constraint when the parents are age x may be written:

$$0 = y_x + \sum_{s=0}^{x-2} \sum_{i=1}^{n_s} r(\theta_{si}, h_{si}) + w_x T n_{x-1} - p_{hx} \sum_{i=1}^{n_{x-1}} h_{x-1i} - p_{cx} c_x - p_{nx}(n_x + n_{x-1}) - p_{zx} z_x \tag{2}$$

where y_x is single-period parental income, n_s is the number of children born when the parents are age s, θ_{si} is an indicator of the sex of the ith child born to age s parents with $\theta_{si}=0$ and 1 for boys and girls, respectively, $r(\theta, h)$ with $r_h > 0$ and $r_{hh} < 0$ is the annual return to the parents from grown children of sex θ and human capital h, w_x is the child wage, T is the time endowment per child per period, p_{hx} is the unit cost of schooling including the opportunity cost w_x of time spent in school, h_{x-1i} is the schooling provided to the ith child born when the parents are age x-1, p_{cx} is the price of consumption, p_{nx} is the fixed per-period cost associated with raising a child, and p_{zx} and z_t represent the price and amount of contraception used by the parents when age x, respectively. Childbearing in each period is determined by parental age, contraceptive

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[5]The division of childhood into pre-school (age 0-7), school-age (8-15), and adult 16+ is of course somewhat arbitrary, but proves to be very convenient because the surveys used for the analysis were conducted at 8 year intervals.

use, and a random variable μ_x, which represents the stochastic component of fertility:

$$n_x = f(x, z_x, \mu_x) \tag{3}$$

The stochastic components of the model are specified as follows. Parental income y_x is assumed to consist of the sum two components: a deterministic function of parental age (reflecting diminished earning capacity with age) and fixed productive assets such as land and an i.i.d. shock reflecting the effects of weather and other similar shocks on agricultural earnings. Consumption and other prices are similarly assumed to follow an i.i.d. process. Because individuals experiencing a program in one year can reasonably expect that program to persist, however, this i.i.d. assumption does not seem reasonable for programmatic variables such as the cost of fertility control (p_{zx}) . Thus it is assumed that p_{zx} exhibits first-order autocorrelation with i.i.d. shocks.[6]

In order to allow for the possibility that parents cannot perfectly forecast fertility outcomes given contraceptive use, it is assumed that the stochastic component of fertility and the sex of the resulting children are only realized after contraceptive choices are made. Thus, within each parental age, the following order of events is assumed: (1) income and prices, including the cost of contraception, become known (2) contraceptive use is chosen (3) fertility outcomes by sex become known (4) human capital allocations for school-age children (i.e., those born in the previous period) and parental consumption are determined.

Under these conditions the state space relevant to the contraceptive decision rule includes contemporaneous income, prices, the stock of school-age children, the contributions of grown children, the distribution with respect to the stochastic component of fertility, which will be denoted F, and the

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[6]While first-order autocorrelation does not affect the structure of the decision rules that are estimated in this paper it does affect the interpretation of the resulting coefficients. In particular, a measured program effect on fertility will under these circumstances reflect both the current cost of fertility control and the effect of the program on expectations about the future cost of fertility control.

distribution over income and price shocks, which will be denoted G. Thus, the contraceptive decision rule may be written

$$z_x = z(x, \sum_{s=0}^{x-2} \sum_{i=1}^{n_s} r(\theta_{si}, h_{si}), \bar{\theta}_{x-1} n_{x-1}, n_{x-1}, y_x, \boldsymbol{p_x}, F, G) \qquad \textbf{(4)}$$

where $\boldsymbol{p}_x$ denotes a vector of prices and wages and $\bar{\theta}_{x-1}$ is the proportion of children born to age x-1 parents that is female and thus $\bar{\theta}_{x-1} n_{x-1}$ the number of daughters in that cohort. Substitution of (4) into (3) yields fertility as a function of the contraception state variables and the fertility shock

$$n_x = n(x, \sum_{s=0}^{x-2} \sum_{i=1}^{n_s} r(\theta_{si}, h_{si}), \bar{\theta}_{x-1} n_{x-1}, n_{x-1}, y_x, \boldsymbol{p_x}, F, G, \mu_x) \qquad \textbf{(5)}$$

Because human capital allocations are assumed to be made after the stochastic component of fertility is realized the decision rule for human capital allocations may be written conditional on fertility outcomes as well as the state variables in (5):[7]

$$h_{x-1i} = h^{c}(x, \theta_{xi}, \sum_{s=0}^{x-2} \sum_{i=1}^{n_s} r(\theta_{si}, h_{si}), \bar{\theta}_{x-1} n_{x-1}, n_{x-1}, \bar{\theta}_x n_x, n_x, y_x, \boldsymbol{p_x}, F, G) \qquad \textbf{(6)}$$

Substitution of (5) into (6) yields

$$h_{x-1i} = h(x, \theta_{xi}, \sum_{s=0}^{x-2} \sum_{i=1}^{n_s} r(\theta_{si}, h_{si}), \bar{\theta}_{x-1} n_{x-1}, n_{x-1}, y_x, \boldsymbol{p_x}, F, G, \epsilon_{hx}) \qquad \textbf{(7)}$$

where ϵ_{ht} is a compound residual reflecting both the fertility shock and the sex of children born to parents at age x.

Equations (5), (6) and (7) characterize the dynamics of education and fertility over the life-cycle of

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[7]Equation (6) is obtained by solving the first order conditions for schooling conditional on fertility outcomes and contraceptive expenditures (which, given the conditioning on fertility, simply are subtracted from income) in the corresponding period and then substituting in (4).

a woman and thus permit an assessment of contemporaneous and lagged effects of family planning

programs on fertility as well as the implications of differences in the number and sex-composition of

siblings for schooling decisions. Although comparative statics are complex because of the dynamic

structure of the model, it is helpful to note that for any variable K that is an argument of h() in (7),

$$\frac{\partial h_{x-1}}{\partial K} = \frac{\partial h_{x-1}^c}{\partial K} + \left[\frac{\partial h_{x-1}^c}{\partial n_x} + \frac{\partial h_{x-1}^c}{\partial(\bar{\theta}_x n_x)} \bar{\theta}_x \right] \frac{\partial n_x}{\partial K} \tag{8}$$

This equation states, for example, that an additional school-age sibling affects educational attainment in

two ways, by influencing subsequent childbearing and by affecting the current and future availability of

resources net of subsequent childbearing.

Because childbearing and schooling only enter the model through the budget constraint, the signs of

the individual terms in (8) are determined by the relative effects of the relevant variables on current and

future consumption. It may be shown that an increase in the number and schooling of older children, given

the number of pre-school children, augments consumption both in the current and future periods; that an

increase in the number of school-age children increases future consumption but may either increase or

decrease current consumption, depending on whether, given time in school, school-age children are net

contributors of household income; and that the number of pre-school children decreases current

consumption but increases future consumption.[8] Because current schooling investment transfers resources

from the current period to the future it will increase under conditions that make such a transfer more

attractive. Thus, the signs of $\frac{\partial h_{x-1}^c}{\partial h_{x-2}}$ and $\frac{\partial h_{x-1}^c}{\partial n_{x-2}}$ are not easily determined, but $\frac{\partial h_{x-1}^c}{\partial n_{x-1}}$ if $w_x T - p_{hx} h_{xi} - p_{nx} < 0$

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[8]This assumes, as seems plausible, that the cost of contraception needed to prevent a birth is lower
than the fixed cost p_{nx}.

and $\dfrac{\partial h^c_{x-1}}{\partial n_x}$ in general may be shown to be negative.[9] It is worth noting that only the latter of these

expressions unambiguously yields the negative relationship between family size and schooling that has been

the focus of previous studies of the fertility-schooling relationship.

Some implications of changes in family composition may also be derived. In particular, if r() is

such that in equilibrium boys receive higher levels of schooling and provide greater remittances than do

girls then $\dfrac{\partial h^c_{x-1}}{\partial(\theta_x n_{x-1})}>0$: net of the total number of school-age children and the sex of a particular school-age

child, an increase in the fraction of school-age girls in the household will yield higher levels of schooling

conditional on fertility. The fact that girls receive lower schooling and provide lower returns implies that

transferring resources forward in time through schooling investment is relatively more attractive when

school-age children are primarily daughters. Also, $sign(\dfrac{\partial h^c_{x-1}}{\partial n_{x-2}})=-sign(\dfrac{\partial h^c_{x-1}}{\partial(\bar{\theta}_{x-2} n_{x-2})})$: if and only if

additional 16+ children result in higher schooling will an increase in the fraction female in this age group

yield lower schooling in the fertility-conditional schooling decision rule.

Because fertility choice is substantially complicated by the fact that the returns to fertility are

mediated by subsequent decisions about schooling investment, explicit determination of the signs of various

effects would require a series of additional assumptions. Informally, however, since the model assumes that

children are the only mechanism for saving in this population and, in this regard, are substitutes for each

other, there is reason to expect that fertility will be decreasing in the current stock of children and in

particular in the number of boys if boys provide a better savings return than do girls.

II. Data

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[9]For example, in period x=X-1 and ignoring for simplicity sex differentials in remittances and thus,
given the model, schooling,

$$\frac{\partial h^c_{x-1}}{\partial n_{x-1}}=-\frac{-u''(c_x)(w_x T-p_{hx}h_{x-1}-p_{nx})p_{hx}+\beta E u''(c_{x+1})\partial r_{x-1}/\partial h_{x-1}}{u''(c_x)p^2_{hx}n_{x-1}+\beta E(u'(c_{x+1})\partial^2 r_{x-1}/\partial h^2_{x-1}+u''(c_{x+1})n_{x-1}(\partial r_{x-1}/\partial h_{x-1})^2)}$$

where $r_{x-1i}=r(\theta_{x-1i},h_{x-1i})$. Given the concavity of u(), the sign of this expression is determined only if $w_x T-p_{hx}h_{x-1}-p_{nx}<0$, that is school-age children are net consumers of income, in which case it is negative.

The data from this study are taken from the Matlab area of Bangladesh, which is widely recognized to have provided some of the clearest evidence that a well designed family planning program can have an important impact on fertility in the absence of substantial social and economic change. (Cleland *et al.* 1994). An important feature of this program was the selection in 1978 of treatment and control areas within a homogeneous region of about 70 square miles. Although logistical and other considerations precluded the random allocation of programs to households, a careful selection of treatment and comparison areas provides a reasonable approximation to the desired experiment. The program involves frequent household visits by female family planning workers providing in-home access to a variety of contraceptive methods along with careful management of side effects. Starting from the 1982, maternal and child health services were integrated into the program in subregions of the intervention area. The comparison area received regular government services, which involved the use of poorly supervised male workers and a limited menu of methods that were inconsistently available. While some methods would have been available at subsidized prices through local clinics and/or pharmacies in the comparison area, restrictions on travel for women in this conservative, largely Muslim, area substantially limited access to the most popular methods such as injectables.

Following the introduction of the Matlab project in 1978, contraceptive prevalence rates in the treatment area increased from 7 percent to 20 percent and reached 33 percent after 18 months of project intervention. A new plateau of 45 percent was reached approximately six years following the introduction of the program (Phillips *et al*. 1988) and the level was 57.6 percent in 1990. The contraceptive prevalence rate in the comparison area was only 15.8 percent in 1984 and had risen to 27.9 percent in 1990 (Keonig *et al*. 1991). Changes in fertility rates were equally dramatic. While the total fertility rate in the treatment area before the start of the program was 6.2 percent lower than that in the comparison area, this difference is not significant at the 5 percent level. The year following the introduction of the program fertility was 25 percent lower in the treatment than in the comparison area with differences of 48.7 and 55.6 percent in the

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35-39 and 40-44 year age groups respectively (Phillips *et al.* 1981, p23).

While the fact that the location of the program in this area was exogenously determined sets it apart from most other studies examining the effects of family planning interventions, it is reasonable to question whether factors other than those related to the provision of family planning services are responsible for the observed differences in the two areas. The first issue has to do with the nature of the program itself. Starting in 1982 maternal and child health services including immunization were integrated into sub-regions of the intervention area. As such it seems reasonable to interpret the effects of the Matlab program at least in part as the result of an integrated maternal child health (MCH) and family planning program rather than simply a family planning program. Nonetheless, the general conclusion that has emerged in the literature is that the impact of the maternal child health component on fertility was limited. As is evident from the contraceptive statistics the treatment-comparison differential emerged well before the introduction of MCH services in 1982. In addition, there was little differential effect on fertility associated with the those sub-regions within the treatment area that received the MCH services in 1982 as opposed to those that received these services in 1986 (Phillips *et al.* 1984). The reductions in mortality were also quite small and may themselves be result rather than a cause of the fertility decline.[10] Finally, the fact that desired family sizes were substantially below the total fertility rate and remained comparable in the treatment and comparison areas following the introduction of the program suggests that increased availability of contraception played a primary role (Keonig *et al.*1991).

It is also possible that some variable other than that associated with the program changed in the two areas. While detailed longitudinal economic data are not available for the treatment and comparison areas, the proximity of the two areas makes it unlikely that wages or prices changed differentially; indeed, there were no significant differences in wages at the end of the study period (BRAC 1994). Moreover, as

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[10]For example the differentials in 1982 were 8/1000 for infant mortality and 7/1000 for child mortality (Phillips 1987).

will be evident below controls for observable as well as unobservable differences in the populations in the two areas have little impact on the measured effect of the intervention program on fertility. More significantly, the proposition that reliable in-home delivery of popular contraceptive methods can reduce fertility in the absence of substantial economic change has been tested in other parts of Bangladesh with similar results. Indeed, the fact that this approach has been increasingly incorporated into the government programs is thought to be at least partly responsible for the striking reductions in fertility in the country as a whole in recent years (Cleland *et al* 1994).

The specific data used in this study were compiled from a variety of data sets collected in the study area, which consists of a geographically contiguous population of approximately 200,000 in 149 villages, between 1974 and 1990. The three primary data sets that are used in this analysis are the 1974 census, the 1982 census and the KAP (Knowledge, Attitudes and Practice) survey conducted in 1990. Each individual in the populations is assigned a unique registration number that is fixed over time, permitting the three data sets to be linked at the level of the individual.

The censuses contain basic information on educational attainment, occupation, and land ownership of the population. Data are organized by household, but the mother of each child and the husband of each woman is identified, so it is possible to link co-resident children with their parents. Vital registration, which has been carried out continuously in this area since 1966 through regular household visits, provides the reproductive histories of women after 1974 and ensures that the ages of children are accurately recorded.

The KAP survey is of a stratified sample of 8500 currently married, reproductive-aged women with approximately equal numbers of women in the treatment and comparison areas (Keonig *et al* 1991). Included in this latter survey is information on educational attainment of children.[11] Two other data sets are

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[11]While the 1990 survey contains information on the ages, sexes and educational attainment of children under the age of 16 at the time of the survey, the censuses may only be used to construct such

also used. One consists of a village level survey conducted in 1978 on the availability of programs and

services, including distance to school, the proximity of roads, and so forth. The second consists of

longitudinally collected, monthly data on contraceptive use, breast-feeding, reproductive status and the

fraction of the month spent in the household by the husband for women in the treatment area. Unfortunately

comparable information is not available in the comparison area.

The data file used in the analysis was constructed from the sample of women in the 1990 survey

along with all women aged 24-48[12] from the 1974 and 1982 censuses, who were, at the time of the

respective census, resident in one of the 67 villages that were part of the sampling frame of the 1990 survey

(see Table 1). In particular, observations on women from each of the three samples were linked across time

using the unique individual registration number. Because estimation of the fertility and schooling decision

rules makes use of a first differencing procedure, as discussed below, the analysis focuses on the subsample

of the women from the three surveys observed to contribute at least two consecutive observations (i.e., they

appear in both the 1974 and 1982 censuses, the 1982 census and the 1990 sample, or all three).

Descriptive analysis of the educational change in the treatment and comparison areas suggests that

the program did indeed, have effects on education, although levels of schooling remained quite low in both

areas. In particular, in 1974 the mean completed schooling for boys and girls aged 13-15 were 2.33 and

1.76 years respectively, with 62% of boys and 41% of girls having some schooling. Over the next 16 years

means schooling rose by 40% to 3.25 for boys and by 107% to 3.19 for girls. Figures 1 and 2 report

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measures for those who remain resident in the study area. Fortunately, the vital registration data indicate
that migration is unlikely before this age.

[12]The restriction to women aged 24 and over at the time of observation is somewhat misleading.
Because fertility and education decision rules, as discussed below, are estimated on an 8-year time frame,
the fertility of a woman aged 24 at the time of observation refers to the number of childen she had while age
16-24. Because the mean age at marriage at the beginning of the study period was 15.4 (Foster and Khan
1995), women over the age of 24 at the time of the respective observation could reasonably be assumed to
have been exposed to the risk of marriage and childbirth for a full 8 years. Had younger women been used
it would have been necessary to adjust for the reduced exposure time.

deviations from the average education by age and year for treatment and comparison-area children by sex. It is evident that although girls received less schooling than boys in both regions, there was little difference according to residence in the treatment or comparison subareas in the schooling of boys and girls in 1974, before the introduction of the program. By 1990, the situation had changed markedly. While boys in the treatment area increased their education at age 14 by 39 percent, boys in the comparison area experienced a 17 percent decrease in mean schooling. Girls in the comparison area did better than boys in either area in percentage terms, with an increase of 48 percent; those in the treatment area experienced an increase of 77 percent. Evidently something changed quite markedly in this period that favored increased levels of schooling in the treatment relative to the comparison area.[13]

III. Estimation

Our empirical strategy is to estimate linear approximations to these decision rules using longitudinal data over a relatively short period and then to use these approximations to simulate the effects of making family planning services available at the beginning of a woman's reproductive years. Estimation is complicated by the fact that the distributions of income and fertility outcomes, inclusive of the means of these distributions, are not observed in the data and are likely to vary across individuals. As a result OLS estimates of these decision rules will, in general, be inconsistent. Women endowed with higher levels of fecundity, for example, are likely to have more children at each age, resulting in a correlation between at least one regressor and the residual.

To address this problem we combine an instrumental variables approach with first differencing. In particular, letting J_{ijx} denote the value at age x for the ith woman of the jth decision variable (schooling fertility) and K_{ikx} the kth state variable at age x for the ith woman, we may write equations (5)-(7) as:

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[13]It is worth noting that despite the dramatic increase in percentage terms of girls schooling, the absolute change for girls in the treatment area was actually less (.7 years) than the absolute change for boys in the treatment area (1.3 years), a result that reflects the fact that the initial level of schooling for girls was considerably lower than that for boys.

$$J_{ijx} = \sum_k \beta_{jk} K_{ikx} + \phi_{ij} + \epsilon_{ijx} \tag{9}$$

where ϕ_{ij} summarizes the effects of variables that are fixed over time for each individual (e.g., the fixed component of fecundity and the distributions F and G) and ϵ_{ijx} summarizes the shocks realized at age x including income and price shocks as well as, except in the case of equation (6), the fecundity shock. Differencing (9) across ages for the same woman yields:

$$\Delta J_{ijx} = \sum_k \beta_{jk} \Delta K_{ikx} + \Delta \epsilon_{ijx}. \tag{10}$$

where $\Delta J_{ijx} = J_{ijx+1} - J_{ijx}$ and so forth. While this approach removes the fixed effect it introduces another statistical problem: the model implies that shocks realized at age x affect the change in the state variables over time. For example, a positive fertility shock when a woman is aged 24-32 will directly increase the number of school-age children she has when aged 32-40 and thus, in general, the change in the number of school-age children between these two age groups. To address this problem we use as instruments data on land ownership in the initial period, village level variables (distance to roads and banks) as well as initial values of the state variables, which will be uncorrelated with the shock changes given the assumption that income and prices are i.i.d..

An additional problem arises with respect to the estimation of equation (6) because fertility and schooling decisions are both assumed to be made after prices and income are realized. This not only means that fertility is contemporaneously correlated with the residual in (6) but it also implies that initial-period state variables are not suitable instruments for the differenced fertility measure.[14] We address this problem

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[14]Note that the fixed instruments such as landholding and schooling used to instrument the state variables such as the stock and sex composition of older children cannot be used as instruments for fertility in the conditional schooling decision rule (6) because they are by assumption uncorrelated with changes in fertility net of the differenced state variables. Put differently, if these fixed instruments were correlated with changes in fertility net of the state variables and thus could serve as instruments in the conditional decision rule then they would not be admissible as instruments in fertility decision rule (4).

by extending an approach suggested by Rosenzweig and Schultz (1987) who use the residual from an estimated reproduction function (3) as a noisy estimate of fecundity (i.e., $E(\mu_t)$ which may be thought of as a woman's fixed propensity to conceive in the absence of contraception). While the differencing procedure used in this study implies that the fixed component of this residual cannot be used to predict changes in fertility, changes in this residual over time, which reflect the fact that there is an important stochastic component to fertility, are suitable instruments. Thus we estimate equation (3) on the sub-sample of women for whom detailed contraceptive data are available. The residuals from the estimated equations are, to a linear approximation, noisy (due to estimation error) estimates of the stochastic component of fertility μ_t that are correlated with realized fertility in that period (n_t) but not with schooling net of the observable state variables inclusive of realized fertility. These residual can thus be used to instrument fertility in the conditional decision rules (6).

A potential problem with the use of linear approximations to examine decision variables such as fertility or schooling is the fact that these outcome variables are bounded below at zero, a fact that is obviously not incorporated into these linear approximations. Thus in addition to using standard linear methods we make use of a multiplicative fixed-effects IV procedure (Chamberlain 1993). This procedure permits semi-parametric estimation of models in which a fixed effect is multiplied by a non-stochastic function of the data and makes use of the idea that if the undifferenced equations are first divided by the multiplicative term then differencing these normalized equations will remove the fixed effect. Thus if equation (9) is replaced by

$$J_{ijx} = \exp(\sum_k \beta_{jk} K_{ikx} + \phi_{ij}) + \epsilon_{ijx} \tag{11}$$

parameter estimates may be obtained using the method of moments applied to moment conditions of the

form:[15]

$$E[J_{ijx} - \exp(-\sum_k \beta_{jk}\Delta K_{ikx})J_{ijx+1}|K_{ikx}] = E[\epsilon_{ijx} - \exp(-\sum_k \beta_{jk}\Delta K_{ikx})\epsilon_{ijx+1}|K_{ikx}] = 0. \qquad \textbf{(12)}$$

A useful feature of this specification is that parameter estimates may be interpreted directly as the percentage effect of an increase in K on the expected value of the outcome.

Several additional methodological points should be made. First, as is evident from equations (6) and (7) the derived education decision rules refer to allocations to individual children rather than just sibling averages, thus permitting a distinction to be drawn between the effect of an individual child's sex on his or her education and the effects of the sex-composition of children on levels of educational attainment within the family. In the data set to be used for estimating the models, women can be observed for a up to three eight-year periods. We therefore estimate equations (6) and (7) by randomly pairing children from the same mother during consecutive eight-year intervals.[16] Interaction of the sex of the selected child with the other state variables before differencing permits assessment of whether the effects of the state variables differ by sex. Second, because the only education measure available in the data is years of school completed at the time of particular surveys, there is a need to adjust for differences in age among school-age children. We therefore implicitly standardize by including an age effect. Third, rather than directly using a measure of fertility we use the number of surviving children under 8 at the time of the census.

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[15]The latter equality follows from the assumption that the tth period shock is independent of the tth and previous period state variables for all t and the fact that,
$$E[\exp(-\sum_k \beta_{jk}\Delta K_{ikx})\epsilon_{ijx+1}|K_{ikx}] = E[\exp(-\sum_k \beta_{jk}\Delta K_{ikx})E[\epsilon_{ijx+1}|K_{ik+1},K_{ijx}]|K_{ijx}].$$

[16]There is a potential problem of selection bias because cases in which there were no school-age children in a particular interval had to be dropped: women with low income shocks and thus low levels of schooling in the first of two consecutive intervals will be more likely to be excluded from the sample because they will also be more likely to have no births in that interval. We do not attempt to correct the bias here because, given the high level of fertility in this population, the bias is small. For example, of women age 24-40 with at least one 8-15 year-old child, 93% of those in the comparison area and 91% of those in the treatment area also had at least one 0-7 year old pre-school child at that point in time.

Since most child deaths occur within the first year of life and there is a large stochastic component associated with child mortality, the effects of non-surviving children on schooling allocation decisions are likely to be quite small; it is thus reasonable to focus on surviving children rather than children ever born as the "fertility" outcome of interest.

IV. Results

Additive and multiplicative IV fixed-effects estimates of the linear fertility decision rule (equation 5) are presented in Table 2. The first column presents additive IV fixed-effects estimates for the entire study area using the 1974 and 1982 census observations as well as the 1990 KAP survey and thus, given the use of children under the age of 8 as a measure of fertility, covers the period 1967-1990. The second column presents comparable multiplicative fixed-effects estimates while the final two columns contain multiplicative fixed effects estimates for the comparison and treatment areas, respectively, using only the 1982 and 1990 observations and thus referring to the interval 1975-1990.

It is, first of all, evident that, consistent with the existing literature examining the effects of the Matlab program, the family planning program had a substantial effect on fertility. In particular, the coefficient of -.333 on the treatment x 1982 dummy indicates that the program resulted in a 20% reduction in fertility by 1982 (using the mean number of children 0-8 of 1.63 as the base). Note, however, that this measure is likely to underestimate the full effect of the program because, by 1982, the program had only been in place for half of the 8-year interval to which the childbearing data refer. Indeed, the magnitude of the coefficient on the treatment x 1990 dummy, which refers to childbearing in the 1983-1990 period, is larger, indicating that the treatment program lowered childbearing in the treatment area by 25% relative to that in the comparison area.

A central feature of the model is that fertility choices and human capital allocations play an important role as a mechanism for transferring resources across time. If this is indeed the case then one would expect parents with greater numbers of children as well as greater levels of schooling per child to be

17

more likely to reduce subsequent childbearing. Moreover, in a setting such as this one in which employment of women outside the home is limited and in which girls leave home for marriage, it is likely that girls provide a less efficient mechanism of transferring resources across time than do boys.[17] As argued above, to the extent that girls receive less schooling and provide a lower return on a given level of schooling, there will be more resources for spending on young children when daughters are present and parents with daughters will be in greater need of additional "investment vehicles" to finance old-age consumption in the form of subsequent children. Thus one should also expect to see higher fertility for households with girls compared to households with boys.

These patterns are indeed evident in the data. In column 1 of Table 2, the coefficient for children 8-15 of -.138 indicates that for each additional school-age boy there is a 8.4% reduction in fertility while the positive and significant coefficient for girls 8-15 indicates that the effects of school-age boy and girl children on subsequent fertility are significantly different. In particular, each additional school-age girl results in only a 4.8% decrease in fertility . Finally, as expected an increase in the average schooling of 16+ children decreases childbearing, with an additional year of average schooling resulting in an 8.4% reduction in childbearing.

The other coefficient estimates are of less substantive interest but appear reasonable. The positive trend effect, which captures both the linear effect of maternal age and any secular trend in fertility,[18] and the negative effect on maternal age squared indicates that the estimates conform to the standard concave age pattern of fertility. The insignificant coefficient on the 1982 dummy indicates that fertility in the

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[17]There is an extensive literature arguing that sons provide a much more important source support in old age in rural Bangladesh than do daughters. See, for example, Cain (1986). More recently Rahman (1997) has shown that sons, but not daughters, significantly reduces adult mortality in the Matlab population that is the focus of this study.

[18]As the age increase between consecutive observations for each women is always 8 years the age effect cannot be distinguished from any overall trend in fertility.

comparison area in the 1974-1982 period did not deviate significantly from that predicted by a linear trend over the 1968-1990 period.

The results for the comparable multiplicative fixed-effects model, which are presented in the second column and can be read directly in terms of percentage changes in fertility, provide a similar picture. In particular, the program reduced fertility in the treatment relative to the comparison area by 28% over the 1974-1982 period and 47% over the 1982-1990 period. An additional school-age boy results in an 18% reduction in fertility while an additional girl yields a significantly lower figure of 13%. The corresponding figures for older children are 14 and 8%, respectively.

The fact that childbearing is influenced by the sex composition of children can be illustrated using a simple graph. In particular, to the extent that households with sons are more likely to limit subsequent fertility than are those with daughters, large families should consist of a higher fraction of girl children, particularly in the treatment area following the introduction of the program.[19] This result is clearly evident in Figure 3, which plots, based on the sample of women used in the analysis, the fraction of children under 16 that are female as a function of the total number of children under 16, for each of the sample periods for the two areas.[20] Although the dotted lines, which correspond to the pre-program period (1974), are essentially flat, the dashed line (1982) and to an even greater extent the solid line (1990) for the treatment area have a pronounced upward slope. The corresponding comparison area lines show a more limited effect of sex composition on childbearing. The correlations between proportion female and children under 16 are significant at the 0.01 level only for the treatment area in 1982 and 1990 and the comparison area in 1990.

These results suggest not only that fertility is generally responsive to the sex composition of

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[19]I am grateful to an anonymous referee for suggesting this graph.

[20]Ideally this analysis would not be restricted to children <16; however, as noted, due to the absence of fertility histories for women in the 1974 and 1982 censuses childbearing must be inferred from the children living in their mother's household. This becomes problematic over the age of 16 due to marriage and migration.

children, but also that the extent of this response may be influenced by the availability of contraceptive services. Estimation of the fertility decision rules separately for the treatment and comparison area and using only observations from the 1982 and 1990 surveys are presented in the third and fourth column and provide further evidence on this point. The magnitudes of the coefficients on sex increase substantially in both areas relative to their values using all three surveys, with a somewhat larger differential occurring in the treatment area. In particular, the estimated effect of an additional school-age girl, net of the total number of school-age children, is 48% higher in the comparison area and 107% higher in the treatment area over the 1975-1990 interval than was obtained using the longer interval. There was also a substantial increase in both areas in the extent to which women with more children reduced their subsequent fertility as evidenced by the larger coefficients on Children 8-15 and 16+ obtained using only the data from the latter surveys.

Estimates of the education decision rule (equation 7) appear in Table 3 and confirm the results from Figures 1 and 2 indicating a positive effect of the family planning program on schooling. Although there is no evidence of an effect by 1982, shortly after the program had been introduced, the treatment x 1990 dummy, which captures the effect of the program on schooling over the 1983-1990 period shows a positive and significant effect. The coefficient of .313 in column 1 given mean schooling of children aged 8-15 of 1.045 corresponds to a 30% increase in schooling net of the other state variables. The results for the multiplicative specification (column 2) are comparable, showing an insignificant effect of the program in 1982 and a 20% increase in schooling in the treatment area relative to the comparison area. The fact that the contemporaneous effects of the program were to raise schooling and lower fertility (Table 2) is consistent with previous evidence testing the proposition that childbearing and schooling are gross substitutes (Rosenzweig 1988) and thus indicate that exogenous increases in the quantity of children should result in reductions in child quality as predicted by the quantity-quality model of fertility.

The notion that higher numbers of children result in lower schooling seems to fare less well when

20

dynamic considerations are examined. We first consider the effects of the number and composition of children on child schooling net of the sex of a particular child, which measure the impact of changing the number and composition of a child's siblings on his or her schooling. Focusing on the multiplicative estimates, additional male children seem to have a small positive effect on schooling. Although, neither the coefficient on Children 8-15 or Children 16+ is individually significant, they are jointly significant at the 5% level, with the point estimate for school-age males indicating that an additional male sibling increases schooling by 13%.[21] Thus there is no evidence that an additional school-age male child decreases the schooling of his siblings. Interestingly, however, there is evidence that additional girls do have a negative impact on schooling. In particular, an additional school-age girl results in a marginally significant 8% reduction in schooling of her siblings while an additional older girl sibling results in a 16% reduction.

There are also significant differences in schooling by sex, given the sex-composition of the household, as captured by the coefficients on Girl and the Girl x Trend interaction. These coefficients, for the multiplicative model, indicate that, net of family size and composition, girls received 48% less schooling than boys in the early 1970s (for which Trend=0), but this differential decreases 22 percentage points over each of the eight-year intervals between the surveys so that by 1990 (Trend=2) the differential was only 4%. This substantial secular rise in female relative to male schooling over the study period was also evident in Figures 1 and 2 and may reflect any of a number of changes over the interval such as expansions in female employment or changes in the returns to schooling in the marriage market that are common to both the treatment and comparison area.

It is important to recognize, however, that the virtual disappearance of sex-based differentials in

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[21]Although most of the coefficients and standard errors in the additive and multiplicative specifications are similar, there appears to be a substantive difference in terms of the measured effect of the stock of school-age and older male children on schooling, although it should be noted that the coefficients are positive and jointly significant in both specifications. We focus here on the multiplicative specification because, as noted, it better captures the limited-dependent variable character of the schooling measure.

schooling net of family size and composition does not imply an absence of average differentials of schooling for boys and girls, since households with different numbers of boys and girls may still have different average levels of schooling that operate through the sex-composition variables. That is the conclusion that girls and boys in any given household receive equal levels of schooling still allows for the possibility that girls receive lower levels of schooling than boys on average because households with more girls tend to receive lower levels of schooling. The overall effect is determined by the combination of the sex-composition and direct sex effects. As of 1990, these estimates predict a total of 12% lower schooling for a girl compared to a boy with the same number and composition of siblings, a figure three times as large as the 4% estimated differential for girls relative to boys within any given household.

The importance of these sex-composition effects for overall sex differentials of schooling is even more evident when the analysis is restricted to the latter two surveys (columns 3 and 4). The magnitude of the sex composition effect approximately doubles when the interval is restricted to 1975-1990, with, as for fertility, a somewhat larger and more precisely measured differential being observed in the treatment area. In particular, an additional school-age sister in the treatment area, given the total number of school-age and older children, results in a 15% reduction in schooling and an additional older school-age sister results in a 27% reduction in schooling of school-age children in the household. For each of these sets of estimates there is no significant effect, individually or jointly, of the total number of school-age and older children net of the sex of a particular child.

The finding that the sex composition of school-age and older siblings importantly affects schooling attainment is best understood with reference to the estimates of the fertility decision rule. As is evident in equation (8), the model implies that the total effect of any given argument in equation (7) can be divided into two components: a component reflecting the consequences of the relevant variable for fertility and a component reflecting the consequences of that variable for schooling net of fertility, where the latter component yields an inverse correspondence between the effect of a given variable on fertility and the effect

of that variable on schooling gross of fertility. The fact that the treatment program and sex composition variable coefficients in Table 2 and 3 are of opposite signs thus leads to the conjecture, given equation (8), that the primary effects of these variables operate through their effect on fertility.

This question can be examined directly by estimating the conditional demand equation for schooling (equation (6)). As noted above, identification of this model requires that one obtain a measure of the stochastic component of fertility, which can be done by first estimating a reproductive function (equation 4) and then constructing residuals. Estimation is, as discussed, restricted to women from the treatment area subsequent to 1978 and is based on three 4-year observations per woman (1978-1982, 1983-1986, 1987-1990).[22] The dependent variable is the number of births in the associated interval and, with the exception of maternal age, the right-hand side variables refer to the share of the corresponding period spent in a particular state (i.e., using modern contraception, breastfeeding, etc.). The first pair of columns in Table 4 are OLS estimates and their associated t-ratios while the second pair are IV estimates. Consistent with the results of Rosenzweig and Schultz (1987) the use of modern efficient methods (especially injectables and IUDs) in the OLS specification has a strong positive bias, a result that may be attributed to the fact that more fecund women will on average choose to use more efficient methods.

Column 5 of Table 3 presents the preferred estimates of the fertility-conditional schooling decision rule. Fertility outcomes as measured by the number of pre-school children in the household have a significant impact on the schooling provided to older children. In particular, each additional pre-school child yields a 27% reduction in schooling on average for his school-age siblings with no significant difference observed between male and female pre-school children. Net of the level of childbearing there is

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[22]As noted the fecundity estimate is only available for the treatment population. Thus the instrument is constructed from estimated fecundity for the treatment area over the relevant intervals and dummy variables for the comparison area and 1974 treatment area populations. This is sufficient to identify the estimated effect of childbearing on human capital investment under the assumption that the relationships are the same in the different areas but does not permit stratification of the analysis by treatment and comparison area.

also no evidence of significant differences between treatment control area in the level of schooling provided. This result is consistent with the premise that the treatment area dummies capture the effects of the family planning program; if the treatment area dummies captured, for example, changes in school availability that happened to coincide with the introduction of the program one would expect to see schooling increase differentially in the treatment area net of fertility.[23]

It is worth emphasizing that the use of estimated fecundity as an instrument is critical here. Suppose there were unobserved heterogeneity across villages in treatment and comparison areas in the accessibility of schools that changed across time. Then fertility would in general be lower in the higher accessibility areas to the extent that the presence of young children makes it more difficult to send older children to school. This would negatively bias the fertility coefficient estimate in the conditional decision rule and, since fertility and the treatment program interaction are negatively correlated also tend to downwardly bias the treatment dummy. It might thus appear that the treatment program had no effect net of fertility even if it in fact did. By instrumenting with fecundity we ensure that the coefficient on fertility reflects the consequences of randomly allocating an additional child to the household rather than the response to differential accessibility. .

With regard to the other coefficients, although the total number of children effect is not much influenced when one controls for childbearing (column 2-4 versus column 5), the significant sex-composition effects disappear. In particular, net of the number of pre-school children an extra sister results in an insignificant 3.5% decline in schooling as compared to an extra brother. The coefficients for grown daughters is similar, yielding an insignificant 6.5% reduction. The composition effects thus appear to

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[23]Due to the income effect associated with lowering the cost of fertility control one might expect to see an effect of the treatment variable net of childbearing: given couples in the treatment and comparison areas with the same resources, contraceptive use, and fertility, the treatment-area couples will have spent less on contraception and thus have more resources available for other purposes. This possibility does not, given its sign, cast doubt on the above conclusion that the treatment area effect operated primarily through its influence on the cost of fertility control.

operate primarily through the implications of composition for subsequent childbearing. Indeed, the small

magnitudes of the family size, composition, and treatment program variables conforms, given equation (8),

to the finding that these variables in general had opposite signs in the fertility and unconditional schooling

decision rules.[24]

There are two implications of these results. First, the coefficient estimates do not support the

notion that the dilution of household resources associated with having many school-age children at the same

point in time substantially diminishes school attendance as might be the case if school fees were high and

school-age children did not contribute to household resources. As noted in Section II, a negative net

contribution of school-age children to household income is necessary if one wishes to sign the conditional

effect $\partial h_{x-1}^{c}/\partial n_{x-1}$. A corollary of this result is that a moderate positive net contribution to income by

school-age children is required for a zero or small negative effect. The fact that the estimates for older and

school-age children are comparable provides additional evidence that school-age children are not

substantial draws on household income. Finally, the absence of a substantial sex-composition effect

conforms with the prediction that a non-zero effect of the number of children is necessary for a non-zero

sex differential to be observed.

Second, a substantial share of the male-female difference in schooling in this population may be

attributed to the fact that, given the total number of children, a couple with girls is on average more likely

to have additional children and these pre-school children, in turn, to significantly decrease schooling

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[24]The schooling of older children differs from the other state variables in that its coefficient is significant in the conditional specification suggesting that there is an effect on schooling operating net of its effect on fertility. This result nonetheless conforms with equation (8) in that the effect of the schooling of grown children diminishes both fertility (Table 2) and schooling in the unconditional specification (Table 3, columns 1-3). While it would seem difficult to obtain this kind of result from the model as written, it could easily be accommodated, for example, if the age pattern of remittances were allowed to differ by level of schooling. Similarly, the imprecisely measured negative point estimate on the number of female school-age children is not as predicted in section II, but could be accommodated by assuming differential wages for male and female school-age children.

investment for school-age children of both sexes by putting increased demand on their time (i.e., to provide childcare or generate income). The fact that these compositional effects operate primarily through their effect on fertility also suggests that the introduction of family-planning services may have differential effects on the schooling of boys and girls. On the one hand, if the availability of services primarily results in a reduction in fertility among households with a sufficient number of sons, a family-planning program could increase sex differentials. By contrast, if, as implicitly assumed in the multiplicative fixed-effects specification, the percentage declines in fertility resulting from these services are the same for couples with school-age sons as for those with school-age daughters then absolute declines in fertility will be larger for the latter. The evidence from Table 2 (columns (3) and (4)) which is stratified by treatment area, provides support for both positions: while the sex composition effects are somewhat larger in the treatment area, the differences are not substantial.

Another reason that fertility reduction might differentially affect female schooling is that girls may be differentially affected by the presence of pre-school children as might be expected, for example, if girls are more likely to be assigned child-care responsibilities a possibility that is captured only indirectly by the model.[25] If this were the case then one would expect to see differences in the effect of pre-school children on schooling by sex of the school-age child. The final two columns in Table 3 provide no evidence to support this proposition. The coefficients on the number of pre-school siblings by sex interacted with the sex of the selected child is insignificant.[26] Nor is there any evidence of differential effects by sex of either the

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[25]This follows from the assumption implicit in the budget constraint (2) that there are perfect markets for child labor and childcare and thus no associated price effects; indirect effects operating through the budget constraint may nonetheless be observed for certain specifications of $r(\theta,h)$. A more direct approach would allow the opportunity cost of time of sons and daughters (i.e., there wage) to differ and depend on the number of pre-school children in the household.

[26]The only coefficient that even approaches statistical significance (P=.19) suggests that pre-school girls detract more from the schooling of older sisters. Interestingly, this effect conforms broadly with the results of Butcher and Case (1994) for the US using a less structural approach.

treatment program or the number and sex composition of school-age children.[27]

V Simulations

The implications of these various estimates for the total effects of the program on fertility and schooling by sex may be assessed by recursively applying the estimated decision rules, disregarding any estimated trend effect. In particular, we consider the level of fertility and schooling that would be realized by a woman who is first given access to the treatment program at different stages in her reproductive career. In particular, we make use of the estimates presented in column (2) from Table 2 and column (5) from Table 3 augmented by calibrated values of the constant and an age effect.[28] These latter values were chosen so that the predicted values of lifetime fertility, fertility at age 24, average schooling, and schooling at age 32 correspond to their actual values in this population in 1974 (total fertility of 7.1, 1.9 children per woman at age 24, 1.95 mean years of schooling for all mothers, and 2.0 mean years of schooling for children with age 32 mothers.). It was also assumed that each cohort of children (i.e., children born over an 8-year interval) was 50 percent male and 50 percent female.

Table 5 presents the simulation results. The first panel shows the effects of the program on fertility. These estimates indicate that, if available throughout a woman's reproductive career, the treatment program services would reduce fertility by 30% relative to the 1974 value of 7.1, with a considerably smaller effect of only 11% if the services were introduced after the woman reached the age of 32. This difference is primarily due to the differences in exposure time to the program: as is evident from the third row of numbers in the panel, women over age 32 who first receive the services at that age exhibit

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[27]The absence of a full set of interactions reflects the fact that level data on 16+ children and their schooling is not available. Such data are required to estimate the differential effects of these variables on children by sex using this specification. An alternative solution would have been to estimate separate differenced equations for girls and boys, but this would exclude women without same sex children in consecutive years and thus yield substantial sample size reductions.

[28]Recall that the age and trend effect are not separately identified in a differenced specification.

60% lower fertility than those of that age not receiving the services. The fact that fertility is lower among this age group for those just receiving the program at age 32 than the group experiencing the program at younger ages reflects the lower cumulated fertility to age 32 of this latter group.

The magnitudes of the schooling effects are somewhat smaller. The program, as determined by a comparison of mean schooling for children with mothers who receive services starting at age 16 to that for children with mothers who never receive services, is predicted to result overall in a 15% increase in schooling for daughters and a 12% increase in schooling for sons. The effects on children of women who only receive services starting at age 32 are much smaller, being 6% and 5% for daughters and sons respectively. Not surprisingly there is essentially no effect for women who receive access to the treatment at age 40, when their childbearing (but not their investments in schooling) is essentially complete.

The age-specific simulation estimates indicate that one would substantially overestimate the lifetime effects of the program on fertility, but underestimate the effects on schooling if one were to simply compare age-specific fertility and schooling rates in the treatment and control areas in the immediate post-treatment period. In particular, the total fertility rate of the synthetic cohort consisting of those benefitting from the program for only 8 years is 4.45 (i.e., the sum of fertility of the 16-24 year olds among those fist receiving the program at age 16, the 24-32 year olds among those first receiving the program at age 24, etc.), which is 37% below initial fertility indicating that the initial effects of the program overestimate by 7 percentage points the eventual effects of the program. The reason is that, as noted, family sizes and thus fertility reductions are substantially greater among those first experiencing the program at relatively advanced ages compared to those benefitting from the program throughout their lives. Similarly, the mean schooling of girls in this synthetic cohort is 1.47, yielding an improvement of only 4%, while the mean schooling of boys is 2.57 yielding an improvement of only 3%. In this case, the difference reflects the fact that the largest increments in schooling on an age-specific basis are observed by those who have benefitted from the program throughout their lives and thus have small completed family sizes. Among those 40+

who benefitted from the program from age 16, for example, there is a 43% predicted increase in schooling for girls although it is worth noting that this relatively large effect reflects at least in part the insignificant (but persistently negative) effects of the size and composition variables in the column 5 estimates of Table 3.

VI. Conclusion

In this paper we have examined data on educational attainment collected before and up to 12 years following the introduction of an experimental intensive family planning program in a low-income rural area that is thought to have had a major influence on fertility. Estimation of decision rules for fertility and educational attainment, with appropriate correction for unobservables and the endogeneity of household size and composition, has provided evidence that a family planning program can influence education in a low-income rural environment. This effect accounts for a 15 percent overall increase in mean schooling for girls and a 12 percent increase for boys. About one third of this effect is observed shortly after the introduction of the program and arises from reductions in the number of preschool children in the household; the remainder of the effect occurs at a lag with the full effect only accruing to the children of women who have access to services throughout their reproductive years.

While the primary purpose of this paper has been to evaluate the long-run effects of programs subsidizing contraceptive services on schooling, in the course of the analysis the paper has also provided a number of implications for studies of the schooling-fertility relationship. Most significantly, although the explicit effect of the number of children on the shadow price of schooling plays a prominent role in the Becker and Lewis (1973) quantity-quality model, these effects appear to play a minor role in this population. Given the low financial costs of primary schooling and the importance of children as a source of income, school-age children may actually be net contributors to household income even if they spend

some time in school.[29]

There are two aspects, however, in which the results exhibit a quantity-quality tradeoff. First, as would be expected in a context in which there are diminishing returns to total schooling investment from the perspective of the parents, parents with more school-age and older children are likely to provide each of their children with somewhat less schooling. Although the effects associated with each additional child are small, they are large enough given the differences in fertility of the magnitude observed in this population (2 to 3 children per women) to importantly affect mean educational attainment. Second, the number of pre-school children in the household has a substantial negative effect on the schooling provided to school-age siblings, a result that presumably reflects the fact that these children are net consumers of household resources as well as the diminishing returns to parental investment in old-age support. While the resulting dilution of household resources is temporary, credit markets appear to be sufficiently thin that parents are unwilling or unable to borrow against the future income of these children.

Although we do not find evidence that the program or even the number and composition of siblings had significantly different effects on the schooling of boys and girls, the fact that pre-school children are an important barrier to the schooling of older siblings does turn out to importantly affect sex differentials in schooling in this population. Consistent with the notion that the return to schooling from the perspective of the parents is higher for boys than girls, boys receive greater schooling than do girls given family size and composition; moreover, parents with girls are evidently more likely to want additional children to support them in old age and/or be able to provide for the needs of additional children because the girls are less likely to be in school. As a result, girls are more likely to have younger siblings, a response that substantially increases male-female differentials in schooling.

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[29]Time-use studies document the importance of work by children in rural Bangladesh (Cain 1977, Caldwell et al. 1984). Evidence from the latter suggests that while schooling is associated with lower levels of work, in rural areas even those children attending school make appreciable contributions.

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Table 1
Descriptive Statistics[a]

	Mean	St. Dev.
Schooling (age 8-15)	1.045	1.582
Boys	0.52	0.499
Age	10.741	1.926
Children <8	1.63	0.979
Children 8-15	1.076	1.033
Girls 8-15	0.515	0.719
Treatment Area	0.548	0.497
Father's educ.	2.974	3.687
Mother's educ.	1.302	2.292
Father's age	43.068	11.933
Mother's age	31.771	0.896

[a]Based on the sample 4206 women. Parental variables, sex, age and schooling refer to the children of these women.

Table 2
IV Fixed-Effects Estimates of Fertility Decision Rules[a]

	Additive		Multiplicative	
	Both Areas 1967-1990	Both Areas 1967-1990	Comparison Area 1975-1990	Treatment Area 1975-1990
Children 8-15	-.138 (5.16)	-.183 (1.55)	-.652 (2.58)	-.672 (2.67)
Girls 8-15	.059 (2.40)	.054 (2.60)	.080 (1.86)	.112 (2.22)
Children 16+	-.290 (2.00)	-.136 (1.09)	-.615 (1.94)	-.745 (2.69)
Girls 16+	.035 (1.05)	.064 (2.20)	.138 (1.97)	.156 (2.39)
Schooling 16+	-.138 (5.16)	-.044 (3.01)	-.017 (0.41)	-.023 (0.71)
Treat x 1982	-.333 (8.67)	-.280 (8.11)		
Treat x 1990	-.414 (6.91)	-.468 (8.60)		
Trend	1.95 (3.97)	2.53 (7.61)	2.88 (4.20)	1.92 (2.84)
1982	-.024 (0.86)	-.015 (0.80)		
Mother's Age^2 x10^-2	-.375 (7.91)	-.485 (17.06)	-.385 (7.33)	-.227 (4.20)
Joint Significance of Children 8-15 and 15+	$F_{(2,5175)}$=2.13 P=0.12	χ_2^2=1.04 P=0.59	χ_2^2=14.52 P=0.00	χ_2^2=7.31 P=0.03
Joint Significance of Girls 8-15 and 15+	$F_{(2,5175)}$=3.32 P=0.041	χ_2^2=7.88 P=0.02	χ_2^2=4.30 P=0.12	χ_2^2=6.48 P=0.04

[a]Based on 5566 women. For 1982-1990 analyses, 2215 women in treatment area and 1952 women in comparison area.
[b]Asymptotic absolute t-ratios based on heteroskedasticity consistent standard errors.

Table 3
IV Fixed-Effects Estimates of Schooling Decision Rules[a]

	Additive				Multiplicative		
	Both Areas 1967-1990	Both Areas 1967-1990	Comparison Area 1975-1990	Treatment Area 1975-1990	Both Areas 1967-1990	Both Areas 1967-1990	
						Level	Inter-actions with Girl
Children 0-7					-.265	-.144	.081
					(2.36)	(0.87)	(0.89)
Girls 0-7					-.020	-.107	-.185
					(0.35)	(1.40)	(1.33)
Children 8-15	.764	.129	.056	-.247	-.064	-.087	-.054
	(2.86)	(0.60)	(0.13)	(1.09)	(0.34)	(0.31)	(0.16)
Girls 8-15	-.018	-.079	-.149	-.152	-.035	-.082	-.035
	(0.30)	(1.41)	(1.29)	(1.68)	(0.36)	(0.31)	(0.07)
Children 16+	.725	.012	.094	-.014	-.034	-.017	
	(2.89)	(0.06)	(0.24)	(0.05)	(0.18)	(0.08)	
Girls 16+	-.138	-.158	-.235	-.265	-.065	-.101	
	(1.96)	(2.43)	(1.68)	(2.37)	(0.59)	(0.77)	
Schooling 16+	-.062	-.029	-.066	-.113	-.106	-.103	
	(3.02)	(1.51)	(1.93)	(4.50)	(5.49)	(3.57)	
Treat x 1982	.009	.080			-.013	.035	.104
	(0.14)	(1.32)			(0.14)	(0.29)	(0.64)
Treat x 1990	.313	.194			.037	-.047	.043
	(3.06)	(2.08)			(0.30)	(0.27)	(0.21)
Trend	-3.88	-.230	.366	.858	-.849	-.286	
	(3.82)	(0.26)	(0.28)	(1.06)	(1.32)	(0.53)	
Girl	-.451	-.475	-.445	-.395	-.45	.021	
	(7.21)	(8.35)	(3.86)	(4.17)	(5.60)	(0.05)	
Girl x Trend[c]	.147	.216	.162	.250	.167	.049	
	(2.50)	(4.18)	(1.84)	(3.55)	(2.49)	(0.34)	
Age	.710	.151	.348	.293	.396	.388	
	(3.94)	(1.91)	(17.14)	(16.88)	(24.57)	(21.95)	
1982	-.335	-.439			-.407	-.537	
	(6.58)	(11.66)			(6.31)	(5.41)	
Mother's Age^2 x10^-2	.461	.065	.060	.042	.159	.109	
	(3.92)	(0.70)	(0.51)	(0.46)	(2.16)	(1.35)	
Joint Significance of Children 8-15 and 15+	$F_{2,6425}$ =4.21	χ_2^2 =6.50	χ_2^2 =0.17	χ_2^2 =8.59	χ_2^2 =2.46	χ_2^2 = 0.16	
	P=0.01	P-0.04	P=0.92	P=0.14	P=0.29	P=0.92	
Joint Significance of Girls 8-15 and 15+	$F_{2,6425}$ =3.25	χ_2^2 =6.55	χ_2^2 =2.90	χ_2^2=5.67	χ_2^2 =0.44	χ_2^2 = 0.60	
	P=0.04	P=0.04	P=0.24	P=0.06	P=0.80	P=0.74	

[a]Based on 4206 women. For 1982-1990 analyses, 814 women in treatment area and 983 women in comparison area.

[b]Asymptotic absolute t-ratios based on heteroskedasticity consistent standard errors.

[c]Girl x Trend included in separate row rather than in interactions column to aid in comparison with subsequent specifications

Table 4
OLS and IV Estimates of Reproduction Function[a]

	Coef.	Abs. T-ratio[c]	Coef.	Abs. T-ratio[c]
Mod. Meth. (Eff.)[b]	0.028	10.39	-0.015	2.74
Mod. Meth. (Ineff)[b]	-0.007	1.81	-0.012	2.41
Traditional Meth[b]	-0.003	1.01	0.001	0.54
Full Breastfeeding[b]	-0.026	7.32	-0.032	4.65
Husband's Absence[b]	-0.01	2.01	-0.012	1.98
Mother's Age $(x10^{-1})$	0.02	4.13	0.004	2.91
Mother's Age$^2(x10^{-1})$	-0.002	3.82	-0.001	2.31
Constant	-0.012	6.71	0.023	4.19

[a]Based on 2716 women from the treatment area.
[b]Endogenous variable; instrumented using education of woman and husband, age of husband, land holdings and community characteristics.
[c]Asymptotic absolute t-ratios based on heteroskedasticity consistent standard errors

Table 5
Simulated Effects of Family Planning Program on Fertility
and Schooling for Different Ages at Start of Program[a]

	Fertility Rate				
	No Program	Program from Age 40	Program from Age 32	Program from Age 24	Program from Age 16
Age 16-24	1.90	1.90	1.90	1.90	1.19
Age 24-32	3.12	3.12	3.12	1.96	2.11
Age 32+	2.08	2.08	1.30	1.47	1.65
All Ages	7.10	7.10	6.32	5.32	4.94
	Mean Schooling Provided to Daughters				
	No Program	Program from Age 40	Program from Age 32	Program from Age 24	Program from Age 16
Age 24-32	1.56	1.56	1.56	1.62	1.62
Age 32-40	1.56	1.56	1.62	1.62	1.71
Age 40+	1.06	1.10	1.10	1.20	1.52
All Ages	1.41	1.43	1.49	1.50	1.62
	Mean Schooling Provided to Sons				
	No Program	Program from Age 40	Program from Age 32	Program from Age 24	Program from Age 16
Age 24-32	2.44	2.44	2.44	2.54	2.54
Age 32-40	2.79	2.79	2.89	2.89	2.93
Age 40+	2.05	2.13	2.13	2.16	2.76
All Ages	2.49	2.51	2.61	2.57	2.79

[a]Simulations based on column 2 of Table 2 and column 5 of Table 3 with fixed effects and age effected selected to give to match aggregate statistics in 1974 (7.1 children per women, 1.9 children per women age 24, 1.95 mean years of schooling, 2.0 mean years of schooling children per women age 32).

Figure 1: Deviations in Mean Schooling by Sex for Treatment and Comparison Areas Before Introduction of Family Planning Program (1974)
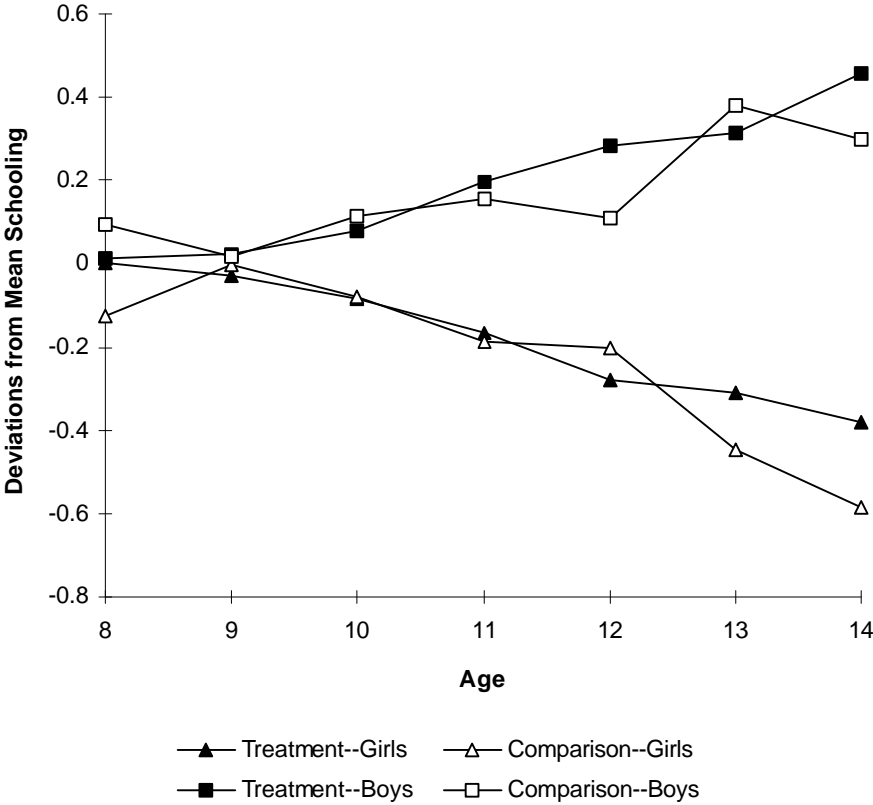
Figure 2: Deviations in Mean Schooling by Sex for Treatment and Comparison
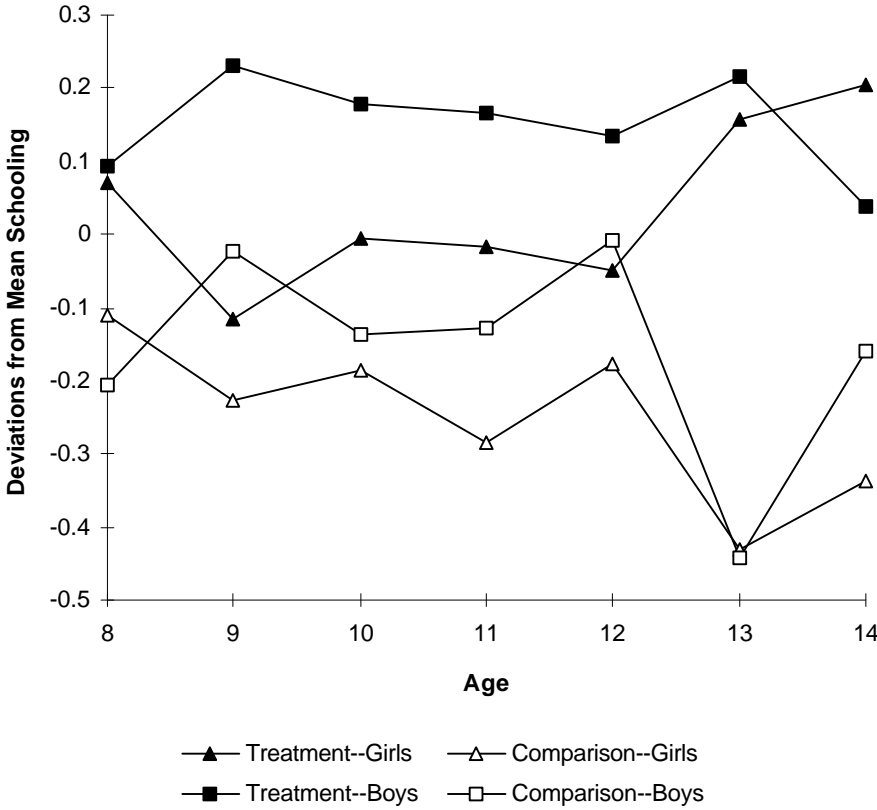Areas 12 Years After Introduction of Family Planning Program (1990)

Figure 3: Relationship between number of children 0-15
and proportion female in treatment and comparison areas by year