# CREDIT PROGRAMS FOR THE POOR AND REPRODUCTIVE BEHAVIOR IN LOW-INCOME COUNTRIES: ARE THE REPORTED CAUSAL RELATIONSHIPS THE RESULT OF HETEROGENEITY BIAS?*

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*Group-based lending programs for the poor have drawn much attention recently. As many of these programs target women, an important research question is whether program participation significantly changes reproductive behavior and whether the gender of the participant matters. Using survey data from 87 Bangladeshi villages, we estimate the impact of female and male participation in group-based credit programs on reproductive behavior while attending to issues of self-selection and endogeneity. We find no evidence that women's participation in group-based credit programs increases contraceptive use or reduces fertility. Men's participation reduces fertility and may slightly increase contraceptive use.*

In recent years, governmental and nongovernmental agencies in many low-income countries have introduced credit programs targeted to the poor to promote self-employment and income growth. The Grameen Bank of Bangladesh is perhaps the best-known example of these small-scale credit programs for the poor. The Grameen Bank was founded in 1976 with the idea that lack of capital was the primary obstacle to productive self-employment among the poor. By the end of 1997, it had served over 2 million borrowers, of whom 95% were women. With loan recovery rates of over 90%, the Grameen Bank has been touted as one of the most successful credit programs for the poor, and its model for group lending has been used for delivering credit in over 40 countries (see Rahman 1993 for a list of similar programs in other countries).

Group lending programs such as the Grameen Bank affect the behavior of the poor both by altering economic incentives through the provision of credit and by providing social development inputs intended to influence a variety of behaviors, including fertility and human-capital investments in children. Because the largest proportion of borrowers from such programs are women, an important research question is whether program participation significantly changes the reproductive behavior of women. We estimate the impact of three group-based credit programs—Grameen Bank (GB), Bangladesh Rural Advancement Committee (BRAC), and Bangladesh Rural Development Board's (BRDB) Rural Development RD-12 program—on reproductive behavior while illustrating the importance of heterogeneity bias and gender differences.

## BACKGROUND

Group-based lending programs provide an innovative and promising mechanism for the delivery of credit to the poor and thus for poverty alleviation. By replacing collateral with peer monitoring and by working with groups rather than individuals to lower transactions costs, group-based credit programs have made formal credit available to the poorest poor. GB, BRAC, and BRDB's RD-12 program are the major group-based credit programs in Bangladesh that provide credit and other services to the poor (for more information on GB see Fuglesang and Chandler 1986, Hossain 1988, and Khandker, Khalily, and Khan 1994; for information on BRAC and BRDB see Khandker and Khalily 1994 and Khandker, Khan, and Khalily 1994).

Although the sequence of delivery and the provision of inputs vary across programs, all three programs offer credit to the poor (defined as those who own less than half an acre of land) using peer monitoring as a substitute for collateral. For example, the GB provides credit to members who form self-selected groups of five. Loans are given to individual group members, but the whole group becomes ineligible for further loans if any member defaults. The groups meet to make weekly repayments on their loans as well as mandatory contributions to savings and insurance funds. Programs such as GB, BRAC, and BRDB also provide noncredit services in areas such as consciousness raising, skill development training, literacy, bank rules, investment strategies, health, schooling, civil responsibilities, and the empowerment of women. Although none of the three programs provided family planning services at the time of our survey (1991–1992), all have family planning awareness programs that encourage their members to have small families and educate their children. For instance, as part of GB's social development program, all members are required to memorize, chant, and follow the "Sixteen Decisions." These decisions include "We shall keep our families small," "We shall not take any dowry in our sons' wedding, neither shall we give

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any dowry in our daughters' wedding," "We shall not practice child marriage," and "We shall educate our children." There are no striking differences in the organization of these programs that would suggest that they would differentially affect fertility behavior. Anecdotal and personal observation suggests that the greater group cohesion of GB members, in part a reflection of the bank's philosophy and the quality and training of its staff, might induce larger effects on social development than the other programs.

## GROUP-BASED CREDIT PROGRAMS AND REPRODUCTIVE BEHAVIOR

Group-based credit programs may affect reproductive behavior by providing credit and through their social development programs. The provision of credit may affect reproductive behavior in several ways. First, credit increases the value of women's market time through the financing of complementary inputs required for self-employment. The increased value of women's market time may have both positive and negative effects on the demand for children. The "income effect" arising from the additional income earned from the increased value of women's time will have a positive effect on fertility; given an increase in income (and no change in the cost of having children), the demand for children is likely to increase because children are considered a normal good. On the other hand, the increase in the value of women's market time increases the opportunity cost of time spent childrearing and creates a "substitution effect." Women substitute market time for time previously spent childrearing, thereby reducing fertility. Second, the provision of credit may increase the demand for children as an investment good. If children can devote time to the self-employment activity or substitute for the mother's time in the production of household goods, credit may increase the value of children's time, resulting in an increase in their value as an investment good. Finally, credit may affect reproductive behavior by increasing the power of women relative to their husbands and other male adults in household decision-making by increasing bargaining power associated with cash income. The social development programs could affect behavior by altering attitudes of male and female members, providing contraceptive information, and providing social power arising from the formation of coalitions (groups) of women members in a village.

The impact of group lending programs on reproductive behavior may differ depending on the sex of the program participant. GB targets women in their credit program, but other programs such as BRAC and BRDB also emphasize men's credit groups. Benefo and Raghupathy (1995), Coombs and Fernandez (1978), Olson-Prather (1976), and others document differences in men's and women's reproductive goals and the importance of targeting men as well as women in family planning programs. Yet no known study of the impact of credit programs on reproductive behavior examines the effect of men's program participation. Altering women's attitudes may not be enough to change contraceptive practices if the husband is not in agreement. How does a credit program for men affect women's reproductive behavior? By examining credit effects by gender, we differentiate between the impact of men's and women's participation on reproductive behavior.

## SOURCES OF BIAS IN ESTIMATING THE EFFECT OF GROUP-BASED CREDIT PROGRAMS ON REPRODUCTIVE BEHAVIOR

Choice-based sampling, self-selection into programs, and nonrandom program placement can contribute to biased estimates of the effects of credit programs on reproductive (and other) behaviors. The data available for analyzing the impact of a credit program (or a training program) on an outcome are often nonrandom samples and are frequently pooled data from two sources: a sample of program participants from program records or a sample of nonparticipants from some larger national sample. Typically such pooled samples overrepresent program participants, thereby creating the problem of choice-based sampling. That is, if data from the two sources are combined and the sample proportion of program participants does not equal the population proportion of program participants, the combined sample is a choice-based sample (Heckman and Robb 1985). If the probability that a program participant appears in the sample and the probability that a program participant appears in the population are known, one can reweight the data to achieve the proportions of program participants that would be produced by a random sample in order to obtain consistent estimates of the program's effect. Choice-based sampling, if not corrected for, can lead to inconsistent estimates of a program's effect.

Self-selection into programs may also lead to biased estimates of a program's impact. Participation in a targeted credit program, such as GB, is self-selective. A household member, given that the household is eligible to participate, may choose whether to participate in a program. The decision of any member to participate is based on her/his expected costs and benefits from program participation. Although membership is free, program participation is costly, as group formation, training, and other group activities are time-consuming and thus involve opportunity costs. Program membership may incur additional social costs to women and their families. Requiring female members to interact with nonfamily individuals, such as program organizers and other group members, challenges the Muslim practice of *purdah*, a system that secludes and protects women in order to uphold standards of modesty and morality. There are also strong incentives to join. Program participation (joining the group) provides access to institutional credit and other organizational inputs that are often inaccessible to many rural households. Unobserved household attributes (preferences, health, fecundity, and socioeconomic status) are likely to affect both the probability of program participation and the propensity to use contraceptives. For example, in rural Bangladesh, households that are more likely to use contraceptives may also be more likely to allow female members to participate in credit programs. Ignoring this self-selection would wrongly ascribe to the credit program the higher inherent propensity of credit program participants to use contraception.

Similarly, nonrandom placement of credit programs could also incorrectly attribute higher contraceptive use among women in program villages to credit programs. Credit programs may not be randomly allocated across the villages of Bangladesh. Indeed, programs may be placed in poorer villages or in villages where families are less traditional in their views toward women's roles in society, including their reproductive roles. Thus, failure to control for the unobserved village attributes in statistical analysis would lead to upward bias in the estimated effect of the program participation on women's contraceptive behavior.

## PREVIOUS LITERATURE

In a study of GB participants in the village of Baruthan, Bangladesh, Shehabuddin (1992) highlights some of the reasons that, even though they feel it would be beneficial to have a small family, women do not use contraception. First, their husbands object to the use of contraception. Some husbands believe that the use of contraception is an act of defiance against God and that it is a woman's duty to bear children without complaint. Second, women believe that if their other children die, they will be left with nothing. Despite these objections to contraceptive use, Shehabuddin found that of the 32 sexually active program participants, 21 admitted having attempted to protect themselves from further pregnancies, but most had encountered problems. Those interested in family planning found that the government-sponsored family planning clinics are too far away and to visit one would entail a gross violation of *purdah*; that the ligation operation was too drastic a step; and that there was no one to turn to for advice if they had difficulties with the more temporary forms of contraception.

Several studies estimate the impact of group-based credit programs on reproductive behavior, but few control for self-selection into programs or choice-based sampling. No known study controls for nonrandom placement of programs or differentiates between male and female program participants.

In their study of the effect of GB and BRAC on reproductive behavior, Schuler and Hashemi (1994) use pooled data of 1,305 married women from four separately selected samples. The first two are random samples of GB and BRAC members selected from membership lists; the third sample is a comparison group of women living in villages not served by either program; and the fourth is a comparison group of nonmembers from GB villages. Unless the number of program participants were drawn in proportion to the population program participants (the authors do not address this issue), their sample is a choice-based sample. The authors, however, discuss issues of selection bias. They use an indicator of the respondent's contraceptive behavior before joining the program to control for possible self-selection into the program. Although they do not control for possible village differences and nonrandom program placement, they choose comparison villages in the same vicinity as the program village in order to minimize systematic village differences. Schuler and Hashemi find that participation in GB increases contraceptive use, but that participation in BRAC has no significant effect. They also find that nonprogram members in GB villages are more likely than women in nonprogram villages to use contraceptives. They conclude that "the effects on contraceptive use apparently spread from participants to nonmembers in the same communities" (p. 74). The result for nonmembers of the GB, however, may also be evidence that program placement is not random; perhaps GB programs are placed in villages where relative attitudes toward contraceptive use are more favorable.

Schuler, Hashemi, and Riley (1997), using the same pooled sample of 1,305 married women (but excluding those who were pregnant at the time of the survey), estimate the impact of participation in BRAC and GB on current contraceptive use while including a variable indicating duration of membership to control for selection bias. Schuler et al. acknowledge that the membership duration variable may not fully eliminate selection factors, because women with a higher propensity to use contraception may have joined the credit programs earlier than other women and hence have longer membership. They argue, however, that the differences in length of membership among women in their survey samples more likely reflect differential access to the credit programs. Schuler et al. find that GB members are more likely than BRAC members to have used contraception before joining, and, when both prior contraceptive use and membership duration are controlled for, GB members are no longer more likely than BRAC members to use contraception. Their results also indicate that the probability of contraceptive use increases with the length of time that a woman participates in either BRAC or GB, suggesting that there is an additional boost from belonging to a credit program, as opposed merely to living in a GB village. Finally, Schuler et al. find that measures of women's empowerment account for surprisingly little of the effect of credit on contraceptive use.

In their study of participation in the BRAC, BRDB, and GB credit programs and reproductive behavior in Bangladesh, Amin, Kabir et al. (1994) use what is likely a choice-based sample and do not use weights. They explain that the number of women in their sample is not based on their proportion in the population because the proportions of participants and nonparticipants in the population were not known. For the same reason, the sample cannot be weighted in the analysis of the data. They control for some aspects of village-level heterogeneity by selecting the comparison group from neighboring geographic areas with similar communication facilities and socioeconomic characteristics. The authors find that credit program beneficiaries are more likely to use contraceptives, are of higher socioeconomic status, and are better educated than nonbeneficiaries. They suggest that this difference may be due to the self-selection of women who were initially better off educationally and financially. But given their data, they cannot disentangle program effects from selectivity and therefore leave the question of causality unanswered.

Using the same sample of 3,453 women, Amin, Ahmed et al. (1994) find a strong positive effect of participation in the credit programs on contraceptive use. This positive effect increases with the duration of membership and with the number of times loans are received. Participation in the education component of the programs increase contraceptive use over and above the effect of participation.

Amin, Hill, and Li (1995) also use this sample of 3,453 participants and nonparticipants in BRAC, BRDB, and GB, but add a variable, the respondents' contraceptive status before participation, to control for self-selection into programs. They find that participation in a credit organization has a significant positive effect on current contraceptive use and a negative effect on fertility, even after they control for the incidence of contraceptive use before joining.

In their study of five credit programs for women and family planning, Amin, Li, and Ahmed (1996) do not use a choice-based sample. The 1995 household survey they use is based on a sample for which the number of program members and nonmembers were randomly chosen in numbers proportionate to the size of the group of enrollees in each village. They control for some aspects of village-level heterogeneity by choosing comparison areas that neighbor program areas and which have similar characteristics. They do not control for individual-level heterogeneity and explain that the higher socioeconomic status and empowerment scores of credit-program members may reflect partly the effect of participation and partly its attraction to those who are already empowered or who are of higher socioeconomic status. Amin et al. (1996) find that women participating in a credit program are significantly more likely than nonprogram women to be current contraceptive users.

Using the same choice-based data set we use, Khandker and Latif (1994) provide informative descriptive statistics on the reproductive behavior of BRAC, BRDB, and GB participants and nonparticipants. Assuming exogenous program placement and using weights to correct for choice-based sampling in their regression analysis, they find that the presence of a credit program in a village has a significant and positive impact on contraceptive use.

All the studies we reviewed find positive and significant effects of group-based credit on contraceptive use, though none of them control for choice-based sampling, self-selection into credit programs (individual heterogeneity bias), *and* nonrandom program placement (village heterogeneity bias).[1] We contribute to this literature by estimating the effect of women's and men's credit program participation on reproductive behavior while controlling for all three possible sources of bias: choice-based sampling, individual-level heterogeneity, and village-level heterogeneity. To test for the importance of these sources of bias and to decompose the possible heterogeneity bias as between individual-level and village-level sources, we also present estimates that do not control for one or more of the delineated sources of bias.

1. Rahman and DaVanzo (1997) attempt to address self-selection and program-placement biases by using retrospective data that permit a before-and-after and treatment group/comparison group framework of analysis.

## THEORY

To illustrate the impact of credit program participation on fertility most clearly, we assume a one-period model and treat the credit program as an exogenous endowment of specific capital. That is, we abstract from other motivations for borrowing, such as smoothing consumption over seasons and years, and treat the programs as providing only the physical capital required to undertake a self-employment enterprise. This one-period model generates an efficiency argument for targeted credit for the rural poor and characterizes the allocation of women's time and reproductive behavior while allowing for the preferences of men and women to differ within the household.

Assume that households consisting of two working-age adults, the male head and his wife, maximize a utility function:

$$U = U(n, Q, X, \mathbb{1}_m, \mathbb{1}_f), \tag{1}$$

where $n$ is the quantity of children, $Q$ is a home-produced good provided to each child (child quality), $X$ is a set of jointly consumed market goods, and $\mathbb{1}_m$ and $\mathbb{1}_f$ are the leisure of the male and female adult household members, respectively. Adding leisure to the utility function permits time to have utility value beyond that obtained by its use in producing goods (including children). As a generalization of Eq. (1), each of the two adult household members, denoted by $f$ and $m$, wishes to maximizes his (if $m$) or her (if $f$) own utility, $u_s$:

$$u_s = u_s(n, Q, X, \mathbb{1}_m, \mathbb{1}_f), \; s = f, m, \tag{2}$$

where the household's social welfare is some function of the individual utility functions $U = U(u_f, u_m)$, a simple form of which is

$$U = \alpha u_f + (1 - \alpha)u_m, \; 0 \leq \alpha \leq 1, \tag{3}$$

in which $\alpha$ is the weight given to women's preferences in the household's social welfare function. The parameter $\alpha$ can be thought of as representing the bargaining power of female household members relative to males in determining the intrahousehold allocation of resources. When $\alpha = 0$, female preferences are given no weight, and the household's social welfare function equals that of the males.[2]

The household-produced good $Q$ provided to each of the $n$ children encompasses child care and child quality more broadly:

$$Q = Q(L_{fq}; \boldsymbol{F}), \tag{4}$$

where $L_{fq}$ is time devoted to the production of $Q$ by females, and $\boldsymbol{F}$ is a vector of technology parameters that affect effi-

2. Browning and Chiappori (1996) show that if behavior in the household is Pareto efficient, the household's objective function takes the form of a weighted sum of individual utilities as in Eq. (3). See McElroy (1990), McElroy and Horney (1981), and Manser and Brown (1980) for a formal exposition of game-theoretic approaches to household decision-making.

ciency in $Q$ good production. Males are assumed not to supply time to the production of the $Q$ child good.[3]

Few rural women work in the wage labor market in Bangladesh. It is a conservative Islamic society that encourages the seclusion of women. Lacking other opportunities, women are engaged in the production of household goods $Q$ to the exclusion of employment in market activities. These effects are magnified if $\alpha$ is small and men's preferences tend to favor certain kinds of $Q$ goods that are production-intensive for women household members.

There are also economic activities that produce goods for market sale that are culturally acceptable. These activities, which produce what we refer to as $Z$ goods, do not require that production occur away from the home and permit part-day labor for those who reside at the workplace. Although some of these production activities can be operated at low levels of capital intensity, for many $Z$ goods, a minimum level of capital is needed. This minimum is often the result of the indivisibility of capital items. For example, dairy farming requires no less than one cow, whereas hand-powered looms have a minimum size. For other activities for which the indivisibility of physical capital is not an issue, such as paddy husking, transaction costs and the high costs of information place a floor on the minimal level of operations. In many societies these indivisibilities may be inconsequential, but household income and wealth among the rural poor of many developing countries, including Bangladesh, is so low that the cost of initiating production at minimal economic levels is quite high. At very low levels of income and consumption, reducing current consumption to accumulate assets for this purpose may not be optimal because it may seriously threaten health (and production efficiency) and life expectancy (Gersovitz 1983). In addition, either both adult household members are credit constrained or only the female is and credit is not fungible within the household.

Formally, we represent the production function for the $Z$ goods as

$$Z = Z(K, L_{mz}, L^*_{fz}, \boldsymbol{A}; \boldsymbol{J}), \tag{5}$$

where $L_{mz}$ is the time of the male devoted to the production of $Z$, $L^*_{fz}$ is efficiency units of labor time of the female (defined below) devoted to the production of $Z$, $K$ is capital in $Z$ production, $\boldsymbol{A}$ is a vector of variable inputs, and $\boldsymbol{J}$ is a vector of technology parameters that affect efficiency in $Z$ good production (information). Positive production requires a minimal level of capital $K$, $K \geq K_{\min}$. The production function (5) can be operated at a nonzero level when $L_{mz}$ or $L^*_{fz}$ are zero, but not when both are zero. For example, although at least one cow is required for milk production, any person's labor can be used to obtain the milk.

There is jointness between the production of the good $Z$ and the production of the per-child good $Q$. This jointness is represented here by an equation defining efficiency units of women's time devoted to the production of $Z$:

$$L^*_{fz} = L_{fz} + \omega n L_{fq}, \; 0 \leq \omega \leq 1, \tag{6}$$

where $L_{fz}$ is time spent exclusively on the production of $Z$. (Because males devote no time to producing $Q$, their efficiency time input into the production of $Z$ is trivially $L^*_{mz} = L_{mz}$.) This formulation allows for time devoted to producing a good for the market in the home ($Z$) to jointly produce the child good ($Q$), albeit at possibly reduced efficiency. If $\omega = 0$, the production of $Q$ and $Z$ are nonjoint: The reallocation of a unit of women's (clock) time from the production of $Q$ to the production of $Z$ reduces the production of $Q$ by the marginal product of labor. In contrast, if $\omega = 1$, $Q$ and $Z$ are maximally joint: Reallocating a unit of (clock) time from producing $Q$ to producing $Z$ (all else equal) has no effect on the quantity of efficiency time devoted to producing $Q$, and hence the output of $Q$ remains unchanged. For example, a woman can tend her cow and care for her child at the same time. In this case, the time reallocated to the production of $Z$ by women has zero opportunity cost in terms of $Q$.

Households maximize utility subject to the budget constraint, which says that expenditure on goods including leisure equals income:

$$X + p_A A + p_n n + (L_{mz} + l_m) w_m = v + L w_m + p_z z, \tag{7}$$

where the jointly consumed market good $X$ is the numeraire; $p_A$, $p_n$, and $p_z$ are the prices of $A$ (inputs to the $Z$ good), fixed per-child cost of child quantity ($n$), and the $Z$ good, respectively; $v$ is nonearnings income; $w_m$ is the male wage; and both adults are endowed with time $L$. The time endowment $L$ can be thought of as the number of hours in a week, month, or year. Men's time is allocated to wage labor $L_{mw}$, the production of the $Z$ good, $L_{mz}$, and to leisure, $l_m$:

$$L = L_{mw} + L_{mz} + l_m. \tag{8}$$

Women's time is spent in the production of the child-quality good $Q$ (which may be joint with the production of the $Z$ good), $L_{fq}$, the exclusive production of the $Z$ good, $L_{fz}$, and in leisure, $l_f$:

$$L = nL_{fq} + L_{fz} + l_f. \tag{9}$$

To evaluate the effect of credit on reproductive behavior, we compare the shadow cost of a child with and without the endowment $K_{\min}$. In the absence of sufficient capital ($K_{\min}$) to operate the $Z$ activity, the utility maximizing necessary first-order condition for the quantity of children $n$ is

$$\partial U/\partial n - L_{fq}(\partial U/\partial l_f) = \lambda p_n, \tag{10}$$

where $\lambda$ is the marginal utility of income. The right-hand side of Eq. (10) is the marginal utility of the consumption of other goods forgone in order to purchase an additional unit of child quantity. The left-hand side is the direct utility of an additional child ($\partial U/\partial n$), less the loss in utility from the diversion of $L_{fq}$ units of mother's time to produce $Q$ units of child quality for this additional child ($L_{fq}(\partial U/\partial l_f)$). Eq. (10) states that, at the optimum, the marginal utility of a child, less the utility forgone by the loss of leisure of the mother (the shadow cost of a child) equals the cost of child quantity con-

3. We do not distinguish between various types of household good production because it would not clarify the basic point to be made.

verted to utility by the marginal utility of income $\lambda$. In contrast, if capital $K_{\min}$ specific to the $Z$ activity is available, the utility-maximizing, necessary first-order condition is

$$\begin{aligned}\partial U/\partial n &= L_{fq}(\partial U/\partial 1_f)\\ &= \lambda[p_n + p_z(\partial Z/\partial L^*_{fz})(1-\omega)L_{fq}].\end{aligned} \tag{11}$$

This first-order condition differs from Eq. (10) by a term on its right-hand side that adds to the shadow cost of a child the costs of forgone production of $Z$ resulting from the need to divert the mother's time to the production of $Q$ for the additional child. If time devoted to the production of $Z$ is maximally joint with the production of $Q$ (i.e., $\omega = 1$), however, the additional term in Eq. (11) is zero, and the shadow value of a child is the same whether or not the $Z$ activity operates. In this limiting case, all time devoted to producing $Q$ also produces the $Z$ good with the same efficiency as time devoted exclusively to the production of $Z$ ($L_{fz}$); thus there is no opportunity cost in terms of $Z$ from the reallocation of time. For example, mothers can weave at their loom or care for their hens without reducing the level of child quality provided to their children or reducing their leisure. Their time in self-employment is, in that sense, costless. In contrast, the smaller the w the larger the impact of operating the $Z$ activity on the cost of a child. If $\omega = 0$ we have the usual case of increased labor market opportunities for women, including wage labor market opportunities, increasing the shadow cost of a child. In the general (intermediate) case, $0 < \omega < 1$ and one unit of women's time allocated to the production of child quality is not costless but does not reduce the time available for other activities by one unit.

The model demonstrates that the effect of credit on reproductive behavior depends on the relationship between the self-employment activity and childrearing. If the self-employment activity and childrearing cannot be combined, there will be large increases in the shadow (opportunity) cost of childrearing, as women can earn money by spending time in the self-employment activity. The increased opportunity cost of a child creates a substitution effect: The woman substitutes time in the self-employment activity for time previously spent in childrearing, thereby reducing fertility. On the other hand, if the self-employment activity and childrearing are compatible (e.g., if the woman uses her loan to buy a dairy cow, and she can milk the cow and care for the child at the same time), then there will be little increase in the shadow price of a child and a small substitution effect. A large substitution effect (with its negative effect on fertility) is more likely to outweigh the income effect (with its positive effect on fertility), thereby causing an overall reduction in fertility. A small substitution effect is less likely to outweigh the income effect, resulting in an overall increase in fertility.

In summary, unlike the well-known impact of increased market wages of women on fertility, the impact of providing self-employment at home to women on the shadow cost of a child, and hence on fertility, may be lessened if time in self-employment can jointly produce child quality. In the presence of credit constraints and imperfect fungibility, the production inefficiency associated with the lack of a labor market outlet for women's time provides a rationale for targeting them in group-based loan programs.

## SOURCES OF ENDOGENEITY[4]

In this paper we estimate the *conditional* demands for fertility and contraceptive use, conditioned on the household's program participation as measured by the quantity of credit borrowed. The quantity of credit is, of course, only one measure of the flow of services associated with participation in any one of the group-based lending programs, but it is the most obvious and well measured of the services provided.

Consider the reduced-form Eq. (12) for the level of participation in one of the credit programs ($C_{ij}$), where level of participation will be taken to be the value of program credit,

$$C_{ij} = \boldsymbol{X}_{ij}\beta_c + \mu^c_j + Z_{ij}\pi + \varepsilon^c_{ij}, \tag{12}$$

that household $i$ in village $j$ borrows. $\boldsymbol{X}_{ij}$ is a vector of household characteristics (e.g., age and education of the household head); $\mu^c_j$ are measured and unmeasured determinants of $C_{ij}$ that are fixed within a village (e.g., prices and community infrastructure); $Z_{ij}$ is a set of household or village characteristics distinct from the $X$'s in that they effect $C_{ij}$ but not other household behaviors conditional on $C_{ij}$ (see below); $\beta_c$, $\gamma_c$, and $\pi$ are unknown parameters; and $\varepsilon^c_{ij}$ is a random error.

The demand for outcome $Y_{ij}$ (contraceptive use or fertility) conditional on the level of program participation $C_{ij}$ is

$$Y_{ij} = \boldsymbol{X}_{ij}\beta_y + \mu^y_j + C_{ij}\delta + \varepsilon^y_{ij}, \tag{13}$$

where $\beta_y$, $\gamma_y$, and $\delta$ are unknown parameters, $\mu^y_j$ are measured and unmeasured determinants of $y_{ij}$ that are fixed within a village, and $\varepsilon^y_{ij}$ is an error possibly correlated with the error $\varepsilon^c_{ij}$. Econometric estimation that does not consider this correlation will yield biased estimates of the parameters of Eq. (13) due to the endogeneity of credit program participation $C_{ij}$.

The endogeneity of credit program participation (represented here by the amount of credit borrowed from the targeted credit program) in the demographic outcome ($Y_{ij}$) equations may arise from common household-specific unobservable variables and common village-specific unobservable variables. Endogeneity may arise from three sources. First, endogeneity may arise from unmeasured household attributes that affect both credit demand and the household outcomes $Y_{ij}$. These attributes include endowments of innate health, ability, and fecundity, as well as preference heterogeneity. Households may be heterogeneous in their preferences with respect to the relative treatment of males and females within the household. Households that are egalitarian in their treatment of the sexes may be more likely to provide additional resources to women, to accede to their fertility and contraceptive desires, and to have female household members participate in credit programs than otherwise identical but less egalitarian households. Ignoring this heterogeneity would wrongly ascribe to the credit program that part of the more

4. The next two sections borrow extensively from Pitt and Khandker (1998).

egalitarian intrahousehold distribution of resources due to the more egalitarian preferences of households that self-select into the program.

Second, credit programs are not likely to be allocated randomly across the villages of Bangladesh. Indeed, program officials note that they often place programs in poorer and more flood-prone areas and in areas in which villagers have requested program services. Pitt, Rosenzweig, and Gibbons (1993) show that treating the timing and placement of programs as random can lead to serious mismeasurement of program effectiveness in Indonesia. Consider the implications of a program allocation rule that was more likely to place credit programs in villages having more positive attitudes about the role of women and the use of contraception. Comparison of the two sets of villages, as in a treatment group/control group framework, would lead to a upward bias in the estimated effect of the program on contraception.

Third, unmeasured village attributes affect both program credit demand and household outcomes $Y_{ij}$. Even if credit programs are randomly placed by the agencies involved, attributes of villages that are not well measured in the data may affect both the demand for program credit and the household outcomes of interest. These attributes include prices, infrastructure, village attitudes, and the nature of the environment (e.g., climate and propensity to natural disaster). For example, the proximity of villages to urban areas may influence the demand for credit to undertake small-scale activities but may also affect household behavior through altering attitudes.

## ECONOMETRIC APPROACH

The standard approach to the problem of estimating equations with endogenous regressors, such as Eq. (13), is to use instrumental variables. In the model we set out, the exogenous regressors $Z_{ij}$ in Eq. (12) are the identifying instruments. Unfortunately, it is difficult to find any regressors $Z_{ij}$ that can be used as identifying instrumental variables. The exogenous regressors $Z_{ij}$ must satisfy two conditions: They must affect the decision to participate in a credit program (that is, $\pi \neq 0$), and they must not affect the household outcomes of interest, $Y_{ij}$, conditional on program participation. Lacking any $Z_{ij}$ (or panel data on individuals before and after treatment availability), one method of identifying the effect of the treatment is based on (presumed) knowledge of the distribution of the errors (i.e., the standard sample-selection framework of Heckman 1976 and Lee 1976). If the errors are assumed to be normally distributed, as is common, the treatment effect is implicitly identified from the deviations from normality within the sample of treatment participants (Moffitt 1991). The nonlinearity of the presumed distribution is crucial. If both the treatment and the outcome are measured as binary indicators, identification of the treatment effect is generally not possible even with the specification of an error distribution. Other methods must be used to control for the endogenous regressor(s).

We use two estimation methods to control for the endogeneity of credit program participation in the outcome (contraceptive use or fertility) equation: fixed effects and limited information, maximum likelihood. Village fixed-effects (FE) estimation, which treats the village-specific component of the errors as a parameter to be estimated, eliminates the endogeneity caused by unmeasured village attributes, including nonrandom program placement. Even with village fixed effects, the endogeneity problem remains if there are common household-specific unobservable variables affecting credit demand and household outcomes. To control for common household-specific unobservable variables, we use limited information, maximum likelihood (LIML) and the quasi-experimental design of our survey. LIML enables us to estimate the equations for program participation (12) and outcome (13) jointly, thereby allowing the error terms, $\varepsilon^c_{ij}$ and $\varepsilon^y_{ij}$, to be correlated. The quasi-experimental design of our survey provides the exogenous variation necessary to identify the credit program participation variable in the joint estimation.

### Identification From a Quasi-Experimental Survey Design

Lacking identifying instruments $Z_{ij}$ (exclusion restrictions) to identify the credit program participation variable, we construct the sample survey to provide identification through a quasi-experimental design. To understand the nature of this quasi-experimental design, consider the classic problem of program evaluation with nonexperimental data. Individuals can elect to receive a treatment offered in their village (or neighborhood). The difference between the outcome ($Y_{ij}$) of individuals who choose to receive the treatment and the outcome of those who choose not to is not a valid estimate of the treatment's effect if individuals self-select into the treatment group. For example, if individuals who join microcredit programs have a greater propensity to use contraceptives before joining the program than those who do not join, then a comparison of contraceptive use between participating and nonparticipating individuals would wrongly ascribe to the credit program that part of contraceptive use due to the greater propensity to use contraceptives among those who self-select into the program.

The parameters of interest, $\delta$, the effect of participation in a credit program on the demographic outcome $Y_{ij}$, can be identified if the sample also includes households in villages with treatment choice (*program villages*) that are excluded from making a treatment choice by random assignment or some exogenous rule. That exogenous rule in our data is the restriction that households owning more than 0.5 acres of land are precluded from joining any of the three credit programs. Data on the behavior of households exogenously denied program choice in this way are sufficient to identify the credit program effect. The quasi-experimental identification strategy used here is an example of the *regression discontinuity design* method of program evaluation: It takes advantage of a discontinuity in the program eligibility rule to identify the program treatment effect (van der Klaauw 1997).[5]

5. The eligibility rule need not be followed perfectly. Program administrators may be sensitive to pressure from individuals or may not be fully informed and may change the eligibility status of those close to the cutoff.

Two-stage instrumental variable estimation of a model of this type can be accomplished by treating dummy variables for village, and a dummy variable for program eligibility interacted with all the exogenous variables, as identifying instruments. The effect of these exogenous variables on credit demand depends on eligibility and availability, but fertility outcomes are not discontinuously affected by the exogenous regressors *conditional* on credit program participation. (See the Appendix for a formal statement of the parameter identification problem; see Pitt and Khandker 1998 for a more detailed discussion of the conceptual and econometric issues associated with the empirical method.)

Underlying identification in this model is the assumption that land ownership is exogenous in this population. Although it is clearly nonstandard to use program eligibility criteria for identification in most instances of program evaluation, we think its use is justified here. Unlike the evaluation of job training programs, health/nutrition interventions, and many other types of programs for which lack of job skills, lack of health, or insufficiency in some other behavior are criteria for eligibility and the behaviors upon which the programs directly act, land ownership is used as the primary eligibility criteria for these credit programs only to proxy for unverifiable and difficult-to-measure indicators of income, consumption, or total asset wealth. Land ownership is simple to quantify, well known within the community, and unlikely to change in the short term. Market turnover of land is known to be low in South Asia. The absence of an active land market is the rationale provided for the treatment of land ownership as an exogenous regressor in almost all the empirical work on household behavior in South Asia. For example, in a classic paper in the field, Rosenzweig (1980) tests the implications of neoclassical theory for the labor market and other behaviors of farm households in India by splitting the sample according to land ownership, treating the sample separation criterion as nonselective. He states, "It is assumed, as in almost all studies of India, that the land market is imperfect: that is, land is not readily bought or sold, and access to leased land is restricted. Bell and Zusman [1976]...provide data that suggest that landholding status is exogenous" (p. 35).

Several researchers have attempted to explain the infrequency of land sales. Binswanger and Rosenzweig (1986) analyze material and behavioral factors that are important determinants of production relations in land-scarce settings and conclude that land sales would be few and limited mainly to distress sales, particularly where national credit markets are underdeveloped. Rosenzweig and Wolpin (1985) set out an overlapping generations model incorporating returns to specific experience, which has low land turnover as an implication; using data from the Additional Rural Incomes Survey of the National Council of Applied Economic Research of India, they find a very low incidence of land sales.

This is the *fuzzy assignment* case discussed by van der Klaauw (1997), who demonstrates the conditions under which the estimation will produce an unbiased estimate of the treatment effect. There must be some discontinuity in eligibility at 0.5 acres, and the data reveal that there is.

Even if land ownership is exogenous for the purposes of this analysis, we must be able to pool the "landless" and the "landed" in the estimation. To enhance the validity of this assumption, we restrict the set of nontarget households used in the estimation to those owning less than five acres of land. In addition, we include the quantity of land owned as one of the regressors in the vector $X_{ij}$ and include a dummy variable indicating the target/nontarget status of the household.

## Identification of the Impact of Gender-Specific Credit

An important question of this research is to determine whether contraceptive and fertility behavior are affected differently if the credit program participant is a woman or a man. Thus, the reduced-form credit equation is disaggregated by gender:

$$C_{ijf} = X_{ij}\beta_{cf} + \mu^{c}_{jf} + \varepsilon^{c}_{ijf} \tag{14}$$

$$C_{ijm} = X_{ij}\beta_{cm} + \mu^{c}_{jm} + \varepsilon^{c}_{ijm}, \tag{15}$$

where the additional subscripts $f$ and $m$ refer to females and males, respectively. The conditional household outcome equation allows not only for separate credit effects for females' and males' credit but also for different effects for each of the three credit programs:

$$Y_{ij} = X_{ij}\beta_y + V_j\gamma_y + \Sigma_k C_{ijf} D_{ijk}\,\delta_{fk} + \Sigma_k C_{ijm} D_{ijk}\,\delta_{mk},$$

where $D_k$ is a dummy value such that $D_k = 1$ if the individual participates in credit program $k$, and $D_k = 0$ otherwise ($k$ = BRDB, BRAC, and GB); $C_{ijf}$ is the credit participation of females in household $i$ of village $j$; $C_{ijm}$ is similarly defined for males; and the $\delta$'s are program parameters specific to each sex.

Introducing gender-specific credit is not a trivial generalization of the econometric model. First, the errors $\varepsilon^{c}_{ijf}$ are likely to be correlated with the errors $\varepsilon^{c}_{ijm}$; that is, there are common unobservable variables that influence the credit program behavior of both women and men in the household. Second, additional identification restrictions are required when there are credit programs for both males and females that may have different effects on behavior. The first issue requires us to model and estimate the demand for credit program participation separately for each sex, allowing for correlated unobservable variables in these demands.

We address the second issue, that of identification, through an extension of the quasi-experimental design described previously. All of these group-based credit programs require single-sex groups. We can achieve identification even if program placement were nonrandom by including in the estimation sample observations for households in villages with credit programs that are unable to join because they possess more than the threshold quantity of land, considered an exogenous rule. Similarly, we identify gender-specific credit through a quasi-experimental survey design that includes some households from villages with credit groups for females only, so that even males in landless households are denied the choice of joining a credit program, and some households from villages with credit groups for males only, so that even landless females are denied program choice. Of the 87 villages in the sample, 15 had no credit program, 40 had credit groups for both females and males, 22 had female-only groups and 10 had male-only groups. Table 1 provides the distribution of villages by type of credit program. As each village had only one type of credit program available, there is no need to model which of the programs members of a household join—BRDB, BRAC, or GB.[6]

Although the likelihood, presented in the Appendix, illustrates the general principal and method used in estimating the effect of credit programs on behavior in Bangladesh, the actual likelihoods maximized are substantially more complex. The likelihoods contain trivariate normal distribution functions because two credit equations (14 and 15) are estimated simultaneously with a binary outcome equation. In addition, the sample design is choice based. Program participants are oversampled. The use of choice-based sampling somewhat complicates the econometrics but is a more efficient method of data collection. Not correcting for the choice-based nature of the sample would lead to biased parameter estimates. We graft Coslett's (1981) weighted exogenous sampling maximum likelihood (WESML) methods onto the limited information, maximum likelihood (LIML) methods described previously in the estimation of both parameters and the parameter covariance matrix. (Our method is a substantial generalization of the LIML likelihoods presented by Smith and Blundell 1986 and Rivers and Vuong 1988 for limited dependent variables.) We refer to the method as WESML–LIML–FE, that is, weighted exogenous sampling maximum likelihood–limited information maximum likelihood–fixed effects.

## SURVEY DESIGN AND DESCRIPTION OF THE DATA

A multipurpose quasi-experimental household survey was conducted in 87 villages of 29 *thanas* in rural Bangladesh during the year of 1991–1992. The survey was conducted three times over the crop cycle year, 1991–1992, to match the three cropping seasons. The first round of the survey is used for this analysis. The sample comprises 29 thanas (subdistricts) randomly drawn from 391 thanas in Bangladesh. Of the 29 thanas, 24 had at least one of the three credit programs under study in operation, whereas five thanas had none of them.

Three villages in each program thana were then randomly selected from a list, supplied by the program's local office, of villages in which the program had been in operation at least three years. Three villages in each nonprogram thana were randomly drawn from the village census of Government of Bangladesh. A village census was conducted in each village to classify households as target (i.e., those who qualify to join a program) or nontarget households, as well as to identify program participating and nonparticipating households among the target households. A stratified random sampling technique was used to oversample households participating in one of the credit programs and target nonparticipating households. Of the 1,798 households sampled, 1,538 were target households and 260 nontarget households. Among the target households 905 households (59%) were credit program participants.

**TABLE 1. DISTRIBUTION OF VILLAGES BY CREDIT PROGRAM AND TYPE OF CREDIT**

| | Credit Program | | | | |
|---|---|---|---|---|---|
| Type of Credit | BRAC | BRDB | GB | None | Total |
| Female Only | 7 | 3 | 12 | 0 | 22 |
| Male Only | 0 | 9 | 1 | 0 | 10 |
| Female and Male | 17 | 12 | 11 | 0 | 40 |
| No Program | 0 | 0 | 0 | 15 | 15 |
| Total | 24 | 24 | 24 | 15 | 87 |

The general household survey includes individual-level information on age, sex, education, borrowing, micro-credit program participation, and family planning and maternity history, and household-level information on land holdings. The family planning portion of the survey was asked for all currently married women aged 14 to 50. If the woman was not able to answer for herself, the interviewer was instructed to ask the best-informed person to respond. Questions were asked about whether any method to prevent pregnancy or to space births is currently used and which method is currently used. The methods used by the 695 women currently using a method of contraception are the pill (60%), tubal ligation (21%), injection (10%), condom, rhythm, and IUD (2% each), withdrawal, vasectomy, diaphragm, and other (1% each; numbers do not sum to 100% due to rounding). Questions about maternity history, including the date of the child's birth, were asked of all ever-married women 14–50 years old who had given birth to at least one child. For consistency between the family planning and maternity history samples used in this analysis, we limited the maternity history sample to currently married women. We also collected village-level data on wages, prices, and infrastructure to supplement the household survey.

The weighted means and standard deviations of the independent variables used in the analysis are presented in Table 2. The weights are used to adjust the choice-based sample to random sample proportions. The average age of the currently married women aged 14–50 included in the analysis is 30 years. The household head is, on average, 41 years old, with 95% of the households headed by men. Education levels are low; the highest levels completed by a female and male in the household are 1.6 years and 3 years, respectively. Average household landholdings are 76 decimals (100 decimals of land equals one acre) for the entire

6. A small number of individuals belonged to credit programs that met in other villages. For example, there were some women who belonged to GB groups even though there was not a GB group in their village. These participation decisions were treated as exogenous in the analysis.

**TABLE 2. WEIGHTED MEANS AND STANDARD DEVIATIONS OF INDEPENDENT VARIABLES**

| Independent Variable | Number of Observations | Mean | Standard Deviation |
|---|---|---|---|
| Age of the Woman | 1,733 | 30.00 | 9.00 |
| Age of Household Head (years) | 1,757 | 40.82 | 12.80 |
| Highest Grade Completed by: | | | |
| Household head | 1,757 | 2.49 | 3.50 |
| Adult female in household (in years of education) | 1,757 | 1.61 | 2.85 |
| Adult male in household (in years of education) | 1,757 | 3.08 | 3.80 |
| Sex of Household Head (1 = male) | 1,757 | 0.95 | 0.22 |
| Household Land (in decimals) | 1,757 | 76.14 | 108.54 |
| Parents of Household Head Own Land | 1,725 | 0.26 | 0.56 |
| Brothers of Household Head Own Land | 1,725 | 0.82 | 1.31 |
| Sisters of Household Head Own Land | 1,725 | 0.76 | 1.21 |
| Parents of Household Head's Spouse Own Land | 1,735 | 0.53 | 0.78 |
| Brothers of Household Head's Spouse Own Land | 1,735 | 0.92 | 1.43 |
| Sisters of Household Head's Spouse Own Land | 1,735 | 0.75 | 1.20 |
| No Spouse in Household | 1,757 | 0.13 | 0.33 |
| Nontarget Household | 1,757 | 0.30 | 0.46 |
| Has Any Primary School | 1,757 | 0.69 | 0.46 |
| Has Rural Health Center | 1,757 | 0.30 | 0.46 |
| Has Family Planning Center | 1,757 | 0.10 | 0.30 |
| Is *Dai*/Midwife Available | 1,757 | 0.67 | 0.47 |
| Price of Rice | 1,757 | 11.15 | 0.85 |
| Price of Wheat Flour | 1,757 | 9.59 | 1.00 |
| Price of Mustard Oil | 1,757 | 52.65 | 5.96 |
| Price of Hen Egg | 1,757 | 2.46 | 1.81 |
| Price of Milk | 1,757 | 12.54 | 3.04 |
| Price of Potato | 1,757 | 3.74 | 1.60 |
| Average Female Wage | 1,757 | 16.15 | 9.61 |
| Dummy Variable for No Female Wage | 1,757 | 0.19 | 0.40 |
| Average Male Wage | 1,757 | 37.89 | 9.40 |
| Distance to Bank (km) | 1,757 | 3.49 | 2.85 |
| Amount Borrowed by Female From BRAC (Tk.) | 183 | 4,678.41 | 3,561.60 |
| Amount Borrowed by Male From BRAC (Tk.) | 70 | 5,685.99 | 7,091.58 |
| Amount Borrowed by Female From BRDB (Tk.) | 108 | 4,094.27 | 1,931.91 |
| Amount Borrowed by Male From BRDB (Tk.) | 180 | 5,996.86 | 6,202.16 |
| Amount Borrowed by Female From GB (Tk.) | 233 | 14,123.59 | 9,302.40 |
| Amount Borrowed by Male From GB (Tk.) | 85 | 16,468.14 | 10,580.00 |

*Notes:* Amount borrowed, an endogenous variable, is the cumulative amount of credit (≥ Tk. 1,000) borrowed since December 1986 from any of these three credit programs, adjusted to 1992 prices. The average 1992 Tk./U.S. dollar exchange rate is 38.95 (*The Europa World Yearbook* 1995).

sample and 30 decimals for households participating in the credit programs.

The six variables that indicate the number of nonresident relatives who own more than half an acre (50 decimals) of land provide information on potential sources of transfers may substitute for credit from micro-credit programs.[7] Approximately 30% of the households are nontarget households: They do not qualify for joining one of the three credit programs.[8] In specifications that do not include village fixed effects, 14 village-level variables are included: three wage variables, six goods prices, and five measures of village infrastructure.

7. We include the dummy variable *no spouse in household* to control for the spouse's nonresident-relative variables, which are undefined but coded as 0 if no spouse resides in the household.

8. Because we restrict the set of nontarget households used in estimation to those with less than five acres of owned land, we exclude 41 households from the original sample of 1,798.

Household program participation is measured by the quantity of credit borrowed from a program. The quantity borrowed is defined as the cumulative amount of credit (greater than or equal to Tk. 1,000)[9] borrowed in the five years before the survey, that is, since December 1986. Approximately 95% of the 905 credit program participants borrow. The average amount of cumulative credit borrowed, by the program and by the sex of the participant (treated as endogenous variables in estimation), is shown in Table 2 for these participants. There are more female than male borrowers, but males, on average, borrow more. The higher loan amount for borrowers of GB likely represents longer program participation and shorter waiting period before receiving first loan compared with borrowers of the other two programs.

Table 3 presents the weighted means and standard deviations of the dependent variables disaggregated by groups: participants versus nonparticipants in program villages, individuals in program villages versus nonprogram villages, and all individuals in all villages. The first two variables, the amount borrowed from the programs by sex of the participant, are the dependent variables used in estimating the two credit equations (14 and 15). These are not the main equations of interest, but we estimate them simultaneously with the equations for reproductive behavior to control for the endogeneity of credit program participation.

Contraceptive use is defined as a dummy variable that takes on a value of 1 if the woman is currently using a method of contraception and 0 otherwise; and fertility, also a dummy variable, takes on the value 1 if the woman had a child in the in the last four years (i.e., since 1988) and 0 otherwise.[10] The means of contraceptive use show slight differences by groups. For example, 42% of married women aged 14–50 in participating households use a method of contraception versus 38% in nonparticipating households. And 39% of women aged 14–50 use contraceptives in program villages versus 32% in nonprogram villages. The means of recent fertility for married women aged 14–50 show similar differences by groups. Fifty-three percent of women in participating households have had a child in the last four years compared with 56% in nonparticipating households; and 55% of women in program villages have had a child in the past four years compared with 58% in nonprogram villages. None of these mean differences by group, however, are statistically different from zero at the .10 level.

## RESULTS

In this section we present and interpret the results of estimating conditional demand equations of the form given by Eq. (17) for recent fertility and current contraceptive use. In addition to WESML–LIML–FE (weighted exogenous sampling maximum likelihood–limited information maximum likelihood–fixed effects) estimates using the quasi-experimental identification restrictions we set out, we present four alternative sets of estimates that do not fully treat credit program placement and participation as endogenous. We present these alternative estimates to illustrate the importance of heterogeneity bias.

Table 4 summarizes the alternative methods used in estimation and the biases they control for. Three of the four alternative estimates (columns 1, 2, and 4) ignore self-selection into credit programs; two of these three treat the choice-based sampling nature of the survey appropriately and uses WESML methods (columns 2 and 4), while the other does not. The latter is more consistent with the maintained hypothesis of the naive model that choice (credit program participation) is exogenous; thus, fully consistent estimates are obtained by ignoring varying sampling proportions.[11] One of the three WESML estimators that ignore self-selection (column 4), labeled WESML–FE, treats the possibly nonrandom allocation of credit programs across villages by including village effects.[12]

The third set of alternative estimates, labeled WESML–LIML, treats credit programs participation as endogenous but

9. The average 1992 Tk./U.S. dollar exchange rate was 38.95 (*The Europa World Yearbook* 1995).

10. Defining contraceptive behavior as current use means that those with tubal ligations are counted as current contraceptors even if the sterilization occurred before any contact with the credit programs. If anything, this will tend to lead to a slight underestimate of the effect of these programs on contraceptive use because contact with the programs cannot lead to an increase or decrease in contraceptive effort by those who are sterile. Excluding women with tubal ligations from the sample would produce a more serious bias because it would select the sample on the basis of contraceptive choice. The date of sterilization is not available in our data.

When one measures the impact of credit program participation on fertility, the timing of fertility relative to program participation is important. We define recent fertility as whether a child was born in the past four years (since January 1988) and participation as the cumulative amount of credit borrowed from a program in the past five years (since December 1986). Average length of membership for participating households who have borrowed is four years. Length and intensity of participation are captured in our definition of participation, the cumulative amount of credit borrowed from a program.

11. Furthermore, neither naive model deals with the possible nonindependence of the errors. This is not atypical of much of the applied literature in this area. If the exogeneity assumption is valid, ignoring nonindependence provides consistent parameter estimates but inconsistent estimates of the parameter covariance matrix (the *t* statistics). In the case of WESML–LIML, WESML–LIML–FE, and WESML–FE estimations, we compute the parameter covariance matrices using an asymptotic bootstrap method, a variant of White's (1980) heteroskedasticity-consistent covariance estimator, to correct for the effects of nonindependent errors.

12. One important drawback of estimating program impacts from data on two groups of households—those from villages with and without programs available—is the possible misinterpretation of the village fixed effects. The discussion so far has treated the village effects as time-invariant attributes. But credit programs may alter village attitudes and other village characteristics, perhaps through *demonstration effects*, and thus the attitudes of both participants and nonparticipants. The full effect of the program on behavior must then include any such village spillovers (externalities) and not just the direct effect on credit participants. Whether or not there are nonzero credit program externalities does not effect the consistency of any estimate of credit program effect ($\delta$), only its interpretation. The program effect parameter $\delta$ estimated by WESML–LIML–FE captures all program effects only if none of the village-specific heterogeneity in behavior is caused by programs. If village externalities exist, the WESML–LIML–FE estimate of $\delta$ represents only the effect of credit on program participants above and beyond its effect on nonparticipants in the village. Unfortunately, one can measure these external effects only with data on villages before and after program introduction.

**TABLE 3. WEIGHTED MEANS AND STANDARD DEVIATIONS OF DEPENDENT VARIABLES**

| Dependent Variable | Participants in Program Villages | Observations | Nonparticipants in Program Villages | Observations | All Individuals in Program Villages | Observations | Individuals in Nonprogram Villages | Observations | All Individuals in All Villages | Observations |
|---|---|---|---|---|---|---|---|---|---|---|
| Amount Borrowed by Female From Program (Tk.) | 5,498.854 | 779 | | 326 | 2,604.454 | 1,105 | | | 2,604.454 | 1,105 |
| | (7,229.351) | | | | (5,682.398) | | | | (5,682.398) | |
| Amount Borrowed by Male From Program (Tk.) | 3,691.993 | 631 | | 263 | 1,729.631 | 894 | | | 1,729.631 | 895 |
| | (7,081.581) | | | | (5,184.668) | | | | (5,184.668) | |
| Contraceptive Use by Currently Married Women Aged 14–30 | 0.407 | 537 | 0.403 | 346 | 0.404 | 883 | 0.365 | 175 | 0.398 | 1,058 |
| | (0.492) | | (0.491) | | (0.491) | | (0.483) | | (0.490) | |
| Contraceptive Use by Currently Married Women Aged 14–50 | 0.418 | 902 | 0.375 | 546 | 0.389 | 1,448 | 0.322 | 283 | 0.378 | 1,731 |
| | (0.493) | | (0.485) | | (0.488) | | (0.468) | | (0.485) | |
| Any Child Born in Last 4 Yrs. to Currently Married Women Aged 14–30 (Yes = 1; No = 0) | 0.682 | 537 | 0.719 | 346 | 0.707 | 883 | 0.641 | 173 | 0.697 | 1,056 |
| | (0.466) | | (0.450) | | (0.455) | | (0.481) | | (0.460) | |
| Any Child Born in Last 4 Yrs. to Currently Married Women Aged 14–50 (Yes = 1; No = 0) | 0.528 | 902 | 0.558 | 546 | 0.548 | 1,448 | 0.582 | 281 | 0.553 | 1,729 |
| | (0.499) | | (0.497) | | (0.498) | | (0.494) | | (0.497) | |

*Notes:* Standard deviations are shown in parentheses. Amount borrowed is the cumulative amount of credit borrowed since December 1986 from any of the three credit programs, adjusted to 1992 prices. The average 1992 Tk./U.S. dollar exchange rate is 38.95 (*The Europa World Yearbook* 1995). Differences in mean contraceptive use and fertility between participants and nonparticipants or between program and nonprogram areas are not statistically significant at the .10 level.

treats program placement as random, and thus does not include village fixed effects. If the latter assumption is true, the WESML–LIML estimates are consistent and efficient, and the WESML–LIML–FE estimates are consistent but inefficient. If program placement is nonrandom, the WESML–LIML estimates are inconsistent. Hausman-like tests of the consistency of the WESML–LIML models were attempted, but the covariance matrices of the differences in the parameter vectors were not positive definite in every case. This problem is not uncommon in estimation problems of this kind.[13] Typically, the problem is that one or more of the diagonal elements of the covariance matrix are very close to zero and are sometimes negative. This is consistent with an infinitely large test statistic, thus leading us to reject the null hypothesis that the fixed effects and nonfixed effects parameter vectors are the same. Rejection of the null hypothesis is consistent with the nonrandom placement of credit programs.[14]

13. The test statistic computed is $(\hat{\beta}_{FE} - \hat{\beta})'(\hat{\Sigma}_{FE} - \hat{\Sigma})^{-1}(\hat{\beta}_{FE} - \hat{\beta})$, where $\beta_{FE}$ and $\beta$ ($\Sigma_{FE}$ and $\Sigma$) refer to the WESML–LIML–FE and WESML– LIML parameter vectors (covariance matrices), respectively.

14. The source of potential bias in the WESML–LIML estimates is a correlation between village fixed effects and the regressors. Pitt and Khandker (1998) check for the presence of such correlation by regressing the estimated village fixed effects on the full set of regressors in each of the WESML–LIML–FE models. The estimated village fixed effects associated with credit program participation of females, the $\mu^c_{jf}$ from Eq. (14), and the estimated village fixed effects associated with credit program participation of males, the $\mu^c_{jm}$ from Eq. (15), are significantly correlated (at the 0.05 level) with the regressors $X_{ij}$. These fixed-effects parameters are estimated in both the contraception and fertility models of behavior presented below, because the determinants of credit program participation, as measured by borrowing, are estimated jointly with each behavior in the maximum likelihood procedure. Thus, this correlation between $\mu^c_{ij}$ and the observed determinants of credit characterizes all of the behavioral models.

## The Determinants of Contraceptive Use

Table 5 presents alternative estimates of the effects of credit programs on current contraceptive use of currently married women aged 14–50. For comparative purposes, columns 1 and 2 present probit results that ignore both individual-level and village-level heterogeneity. The probit equation presented in column 1 also ignores bias from the choice-based sample. In the probit estimation, women's program participation in BRAC and GB has a positive and, in the case of GB, significant effect on contraceptive use, consistent with

**TABLE 4. ALTERNATIVE METHODS USED IN ESTIMATION AND THE BIASES THEY CONTROL FOR**

| Biases Controlled for | Probit (1) | WESML Probit (2) | WESML–LIML (3) | WESML–FE (4) | WESML–LIML–FE (5) |
|---|---|---|---|---|---|
| Choice-Based Sampling | No | Yes | Yes | Yes | Yes |
| Self-Selection into the Credit Programs (Individual-Level Heterogeneity) | No | No | Yes | No | Yes |
| Endogenous Program Placement (Village-Level Heterogeneity) | No | No | No | Yes | Yes |

*Notes:* Weighted exogenous sampling maximum likelihood (WESML) uses weights to control for choice-based sampling. Limited information maximum likelihood (LIML) jointly estimates the participation and outcome equations to control for self-selection into the programs. Village-level fixed effects are used to control for endogenous program placement.

previous research. The male participation parameter for GB is negative and significant (0.036, $t = -2.084$). The WESML probit contraceptive use equation (column 2), which controls only for choice-based sampling, does not find strong positive effects of credit program participation by women. Removing the bias from the choice-based sample by using the weights results in marginally weaker effects of the impact of credit programs on contraceptive use than using the nonweighted (column 1) estimates.

The WESML–LIML estimates (column 3) control for both choice-based sampling and self-selection into programs, but not for village fixed effects. All the credit parameters for females are positive, and the BRAC and GB parameters have $t$ statistics greater than 2.0. The credit effects for BRAC and BRDB for men change sign from positive to negative, but remain statistically insignificant.

The WESML–FE estimates presented in column 4 paint the opposite picture of the effects of women's program credit on contraceptive use and illustrate the importance of controlling for village-level heterogeneity in the analysis. The WESML–FE estimates reveal negative (though statistically insignificant) credit effects for BRDB and GB among women and a positive credit effect for BRDB among men.

The WESML–LIML–FE estimates (which control for all bias from all three sources: choice-based sampling, self-selection into programs, and village-level heterogeneity) shown in column 5 provide mixed statistical evidence of the influence of program credit on contraception. Credit for women from all three programs apparently reduces the use of contraceptives among program participants ($\chi^2(3) = 6.15$, $p = 0.10$; see Table 6, column 2 for relevant Wald test statistics), with $t$ statistics greater than 2.0 (in absolute value) for BRDB and GB. Calculated elasticities[15] for the BRDB and GB coefficients indicate that the magnitude of these effects is small. On average, a 1% increase in credit results in a 0.12% and 0.09% decrease in the probability of contraceptive use for households with female participants in BRDB and GB, respectively.[16] In contrast, a joint test of the credit coefficients for males indicates that male credit from BRAC and BRDB tends to increase the use of contraceptives ($\chi^2(3) = 8.58$, $p = 0.04$), though these coefficients are not individually statistically different from zero.

To focus on the credit program results, in Table 5 we do not present coefficients of the additional explanatory variables included in the probit contraception equation. We briefly discuss the effects of these variables in the WESML–LIML–FE model specification here. Table 2 provides summary statistics for all of the explanatory variables. The effects of these variables are generally consistent with expectations. The age coefficients for females (age and age-squared) are highly statistically significant and indicate that the probability of contraceptive use increases with age until age 32, then declines. The effect of education on the probability of contraceptive use is positive and marginally significant for males (coefficient of 0.06, $t = 1.903$), positive but insignificant for females (coefficient of 0.03, $t = 1.435$), and negative but insignificant for the head of the household (coefficient of –0.02, $t = -0.697$). Having a male (as opposed to female) household head has a positive but insignificant effect on contraceptive use (coefficient of 0.60, $t = 1.237$), whereas age of the household head has no effect. The effects of access to potential transfers are small and statistically insignificant. Land holdings, as captured by the log of household land and whether the household is a nontarget household, have negative and insignificant effects on the probability of contraceptive use (coefficient for log(land) is –0.043, $t = 1.471$; coefficient for nontarget is –0.21, $t = 1.212$).

The WESML–LIML–FE correlation coefficient ($\rho$) is positive and fairly large ($\rho = 0.425$), implying that women who join these credit programs are more likely to already use contraceptives than observationally equivalent women, when we control for village effects. Unobserved attributes (such as preferences, health, fecundity, or socioeconomic status) affect both the probability that a female participates in a credit program and her propensity to use contraceptives. For

15. Elasticities for the effect of ln(credit) on the probability of contraceptive use are given by $\beta^* f(z)$/mean(contraceptive use), where $\beta$ denotes the probit coefficient on log(credit), $z = X_{ij}\beta/\sigma$, and $f(z)$ is the standard normal density function evaluated at the means of the regressors ($X_{ij}$). We approximate $z$ by the inverse cumulative normal, where the cumulative normal is taken to be the fraction of the sample using a method of contraception.

16. Hashemi, Schuler, and Riley (1996) argue that the consistent focus of the GB on credit explains its stronger effect (relative to other programs) on women's empowerment. Although they find that BRAC also significantly affects a variety of measures of women's empowerment, GB "alone has a significant effect on women's involvement in major decisions within the family" (p. 641).

**TABLE 5. ALTERNATIVE ESTIMATES OF THE IMPACT OF CREDIT PROGRAMS ON CURRENT CONTRACEPTIVE USE OF CURRENTLY MARRIED WOMEN AGED 14–50 AND 14–30**

| Variable | Probit, Aged 14–50 (1) | WESML Probit, Aged 14–50 (2) | WESML–LIML, Aged 14–50 (3) | WESML–FE, Aged 14–50 (4) | WESML–LIML–FE, Aged 14–50 (5) | WESML–LIML–FE, Aged 14–30 (6) |
|---|---|---|---|---|---|---|
| Amount Borrowed by Female From BRAC | 0.017 | 0.006 | 0.075 | 0.008 | –0.073 | –0.143 |
| | (1.143) | (0.374) | (2.095) | (0.433) | (–1.693) | (–4.142) |
| Amount Borrowed by Male From BRAC | 0.012 | 0.004 | –0.021 | 0.008 | 0.040 | 0.028 |
| | (0.570) | (0.170) | (–0.406) | (0.289) | (0.745) | (0.566) |
| Amount Borrowed by Female From BRDB | –0.023 | –0.032 | 0.044 | –0.029 | –0.116 | –0.223 |
| | (–1.348) | (–1.285) | (1.214) | (–1.134) | (–2.421) | (–5.922) |
| Amount Borrowed by Male From BRDB | 0.026 | 0.034 | –0.067 | 0.052 | 0.084 | 0.072 |
| | (1.906) | (1.610) | (–0.132) | (2.663) | (1.475) | (1.354) |
| Amount Borrowed by Female From GB | 0.033 | 0.021 | 0.095 | –0.003 | –0.090 | –0.133 |
| | (2.842) | (1.469) | (2.580) | (–0.199) | (–2.011) | (–3.281) |
| Amount Borrowed by Male From GB | –0.036 | –0.494 | –0.088 | –0.041 | –0.001 | –0.006 |
| | (–2.084) | (–2.059) | (–1.625) | (–1.631) | (0.000) | (–0.115) |
| Contraceptive Constant | –3.363 | –2.736 | –5.203 | | | |
| | (–3.926) | (–3.175) | (–4.925) | | | |
| ρ (Women) | | | –0.325 | | 0.425 | 0.684 |
| | | | (–1.777) | | (2.075) | (4.851) |
| ρ (Men) | | | 0.199 | | –0.203 | –0.165 |
| | | | (0.643) | | (–0.700) | (–0.643) |
| Log-Likelihood | –1,028 | –1,004.101 | –2,709.301 | –3,169.27 | –2,458.954 | –2,102.486 |
| Number of Observations | 1,731 | 1,731 | 1,731 | 1,731 | 1,731 | 1,030 |

*Notes:* Numbers in parentheses are asymptotic *t* ratios. Weighted exogenous sampling maximum likelihood (WESML) uses weights to control for choice-based sampling. Limited information, maximum likelihood (LIML) jointly estimates the participation and outcome equations to control for self-selection into the programs. Village-level fixed effects (FE) are used to control for endogenous program placement.

example, in rural Bangladesh, households that are more likely to be open-minded regarding contraceptive use may also be more likely to allow female members' participation in credit programs. Ignoring self-selection based on these unobserved attributes would wrongly ascribe to the credit program the higher inherent propensity of credit program participants to use contraception.

The null hypothesis that credit program participation is exogenous in the determination of contraceptive use is tested jointly for men and women and is only marginally rejected ($\chi^2(2) = 4.90$, $p = 0.09$). The same null hypothesis for women's credit program participation is more firmly rejected ($t = 2.075$). Because of the marginal significance of the joint test, we refer the reader back to the WESML–FE estimates (which treat participation as exogenous) presented in column 4. The WESML–FE estimates reveal a higher *t* ratio of BRDB credit for men, and still find negative BRDB and GB credit effects for women, although they are no longer statistically significant. There remains a lack of evidence that women's credit program participation increases their use of contraceptives.

Contraceptive use is one behavior for which village externalities (as defined previously) might be important. Consequently, the total effect of the credit program on a program participant ($C_{ij}\delta$ + village externality) may, in fact, be positive. We are left with the implication, however, that the effect of the credit program on women participants is *less* than its effect on nonparticipants in the same village because the estimated δ's are negative. Furthermore, the WESML–LIML correlation coefficient is negative and large in absolute value ($\rho = -0.325$), whereas the WESML–LIML–FE estimate is large and positive ($\rho = 0.425$). This pattern suggests that, conditional on village characteristics, women who are more likely to use contraception (than observationally equivalent women) are more likely to join a credit program, but that villages with low rate of contraception are more likely to have access to a credit program.

The results presented in columns 1–5 of Table 5 highlight the importance of heterogeneity bias in the evaluation of the impact of credit programs on contraceptive use. The "naive" results presented in columns 1 and 2 differ substantially from the WESML–LIML–FE results, which, controlling for all three sources of bias, reveal negative program effects on contraceptive use for women and positive program effects on contraceptive use for men. A comparison of the WESML–LIML and WESML–FE results (which control for two of the three biases) with the WESML–LIML–FE results provides evidence that village-level heterogeneity plays a larger role in affecting the results than individual-level heterogeneity.

**TABLE 6. WALD TEST STATISTICS: EFFECTS OF CREDIT PROGRAMS ON CURRENT CONTRACEPTIVE USE AND FERTILITY AMONG CURRENTLY MARRIED WOMEN AGED 14–50**

| | Contraceptive Use | | Fertility | |
|---|---|---|---|---|
| | 6 Program ρ's (1) | 2 Program ρ's (2) | 6 Program ρ's (3) | 2 Program ρ's (4) |
| Equality of BRAC, BRDB, and GB Credit Variables | 6.22 ($p$ = 0.18) | 9.33 ($p$ = 0.05) | 21.73 ($p$ = 0.00) | 10.52 ($p$ = 0.03) |
| Equality of Gender Credit Variables | 13.60 ($p$ = 0.00) | 12.42 ($p$ = 0.01) | 10.47 ($p$ = 0.01) | 9.20 ($p$ = 0.03) |
| Equality of Gender ρ' s | 31.70 ($p$ = 0.00) | 3.23 ($p$ = 0.07) | 24.86 ($p$ = 0.00) | 17.85 ($p$ = 0.00) |
| Joint Significance of Credit Variables | 18.69 ($p$ = 0.00) | 16.90 ($p$ = 0.01) | 25.27 ($p$ = 0.00) | 13.87 ($p$ = 0.03) |
| Joint Significance of Credit Variables for Females | 10.44 ($p$ = 0.02) | 6.15 ($p$ = 0.10) | 3.52 ($p$ = 0.32) | 8.36 ($p$ = 0.04) |
| Joint Significance of Credit Variables for Males | 5.24 ($p$ = 0.15) | 8.58 ($p$ = 0.04) | 21.58 ($p$ = 0.00) | 8.17 ($p$ = 0.04) |
| Joint Significance of Female ρ's | 12.60 ($p$ = 0.01) | — | 31.46 ($p$ = 0.00) | — |
| Joint Significance of Male ρ's | 10.55 ($p$ = 0.01) | — | 15.69 ($p$ = 0.00) | — |
| Joint Significance of All ρ's | 35.66 ($p$ = 0.00) | 4.90 ($p$ = 0.09) | 48.20 ($p$ = 0.00) | 18.11 ($p$ = 0.00) |
| Joint Significance of Transfer Variables | 4.48 ($p$ = 0.61) | 4.53 ($p$ = 0.60) | 14.23 ($p$ = 0.03) | 14.20 ($p$ = 0.03) |

## The Determinants of Recent Fertility

The WESML–LIML–FE fertility estimates (column 5 of Table 7) are mostly consistent with the contraceptive use estimates for women's credit. Fertility increases with women's participation in BRAC and BRDB, although this is statistically significant only for BRAC. The calculated elasticity[17] of 0.04 for the BRAC coefficient indicates that a 1% increase in credit to a female participant in BRAC results in a 0.04% increase in the probability that a child was born in the last four years. The set of three women's credit parameters are jointly different from zero ($\chi^2(3) = 8.36$, $p = 0.04$), as are the men's credit parameters ($\chi^2(3) = 8.17$, $p = 0.04$). The BRDB and GB credit effects for males, however, are negative and have $t$ statistics near or above 2.0 in absolute value. The coefficients suggest that men's participation in BRDB or GB, as measured by a 1% increase in credit, reduces the probability that a child was born in the past four years by 0.04. That is, men's participation in BRDB and GB seemingly reduces fertility, whereas women's participation in BRAC and BRDB increases it. The null hypothesis that women's and men's credit effects on fertility are the same is rejected ($\chi^2(3) = 17.85$, $p = 0.00$; see Table 6, column 4 for relevant Wald $\chi^2$ test statistics). As with the results for contraceptive use, age is the most important determinant of recent fertility. The probability of having had a child in the past four years increases until age 27, then declines.

The null hypothesis that credit program participation is exogenous in the determination of fertility is rejected when tested jointly for men and women ($\chi^2(2) = 18.11$, $p = 0.00$) and when tested separately for women ($t = -2.718$) and men ($t = 2.701$). Therefore, the WESML–LIML–FE model, which controls for self-selection into credit programs, is the preferred model. In column 4 of Table 7 we present the WESML–FE results, which treat participation as exogenous. These results differ substantially from the preferred WESML–LIML–FE specification: In the WESML–FE model, women's participation in BRAC and BRDB reduces fertility (though only the BRDB coefficient is statistically significant), and the negative BRDB and GB coefficients for men are no longer statistically significant.

Controlling for heterogeneity bias appears to be important in the fertility results, as it was for contraception. The correlation coefficient (ρ) for both the WESML–LIML–FE and WESML– LIML results is negative for women's participation and positive for men's participation. This implies that households with a propensity to less recent fertility, conditional on other observed attributes, are more likely to have a female participate in a credit program and less likely to have

17. The elasticity for the effect of ln(credit) on fertility (the probability that a child was born in the past four years) is given by $\beta * f(z)$/(fraction of the sample with a child born in the past four years), where $\beta$ denotes the probit coefficient on log(credit), $z = X_{ij}\beta/\sigma$, and $f(z)$ is the standard normal density function evaluated at the means of the regressors ($X_{ij}$). We approximate $z$ by the inverse cumulative normal, where the cumulative normal is taken to be the fraction of the sample with a child born in the past four years.

**TABLE 7. ALTERNATIVE ESTIMATES OF THE IMPACT OF CREDIT PROGRAMS ON FERTILITY OF CURRENTLY MARRIED WOMEN AGED 14–50 AND 14–30**

| Variable | Probit, Aged 14–50 (1) | WESML Probit, Aged 14–50 (2) | WESML–LIML, Aged 14–50 (3) | WESML–FE, Aged 14–50 (4) | WESML–LIML–FE, Aged 14–50 (5) | WESML–LIML–FE, Aged 14–30 (6) |
|---|---|---|---|---|---|---|
| Amount Borrowed by Female From BRAC | 0.006 | –0.008 | 0.037 | –0.002 | 0.079 | 0.035 |
| | (0.414) | (–0.493) | (0.933) | (–0.125) | (2.372) | (0.538) |
| Amount Borrowed by Male From BRAC | –0.012 | –0.005 | 0.016 | –0.031 | 0.054 | 0.006 |
| | (–0.605) | (–0.237) | (0.399) | (–1.233) | (1.353) | (0.085) |
| Amount Borrowed by Female From BRDB | –0.015 | –0.106 | 0.022 | –0.038 | 0.050 | 0.009 |
| | (–0.849) | (–0.447) | (0.495) | (–2.495) | (1.312) | (0.124) |
| Amount Borrowed by Male From BRDB | 0.019 | 0.026 | –0.055 | –0.006 | –0.074 | –0.111 |
| | (1.318) | (1.215) | (–1.191) | (–0.223) | (–1.976) | (–2.590) |
| Amount Borrowed by Female From GB | –0.024 | –0.035 | –0.016 | 0.036 | –0.035 | –0.085 |
| | (–1.991) | (–2.534) | (–0.362) | (1.649) | (–0.951) | (–1.915) |
| Amount Borrowed by Male From GB | 0.013 | 0.008 | –0.042 | –0.005 | –0.074 | –0.113 |
| | (0.735) | (0.365) | (–0.851) | (–0.237) | (–2.193) | (–2.609) |
| Fertility Constant | –4.055 | –5.114 | –2.755 | | | |
| | (–4.386) | (–5.614) | (–3.887) | | | |
| ρ (Women) | | | –0.264 | | –0.432 | –0.299 |
| | | | (–1.201) | | (–2.718) | (–0.933) |
| ρ (Men) | | | 0.244 | | 0.351 | 0.508 |
| | | | (1.097) | | (2.701) | (3.295) |
| Log-Likelihood | –863.958 | –839.707 | –2,657.021 | –3,149.59 | –2,444.341 | –2,096.141 |
| Number of Observations | 1,557 | 1,557 | 1,557 | 1,557 | 1,557 | 1,056 |

*Notes:* Numbers in parentheses are asymptotic *t* ratios. Weighted exogenous sampling maximum likelihood (WESML) uses weights to control for choice-based sampling. Limited information, maximum likelihood (LIML) jointly estimates the participation and outcome equations to control for self-selection into the programs. Village-level fixed effects (FE) are used to control for endogenous program placement.

a male participate. A comparison of the "naive" probit results (Table 7, column 1) with the WESML–LIML–FE results (Table 7) illustrates that this heterogeneity may lead to biased estimates when it is not controlled for. For example, in the probit results, fertility does not increase with women's participation in BRAC or BRDB or decreasing with men's participation in BRDB and GB.

## Age Disaggregations

To investigate the robustness of our findings, we reestimate both models using a younger group of currently married women—aged 14–30—who are less likely to have completed lifetime fertility and are thus more likely to alter fertility behavior in response to a credit program.

The last column of Table 5 presents WESML–LIML–FE estimates of the determinants of contraceptive use for this younger cohort of women. The negative effects of women's credit program participation on contraceptive use are both larger and more statistically significant in the younger cohort. The three credit variables for women are jointly significant ($\chi^2(3) = 38.21$, $p = 0.00$) and none of them have a $t$ ratio less than 3.28 in absolute value. The credit variables for men remain insignificant ($\chi^2(3) = 5.61$, $p = 0.13$). Apparently, there is greater positive selection into the credit programs by (unobserved) propensity to use contraception among younger women ($\rho = 0.684$) than among all women ($\rho = 0.425$).

In the case of recent fertility (last column of Table 7), the strength of the error correlations also depends on age. The positive r for men is larger for households with younger married women ($\rho = 0.508$), whereas the negative r for women is smaller in absolute value ($\rho = -0.299$). Consequently, all the women's and men's credit parameters are algebraically smaller for the younger sample: The statistically significant positive effect of women's BRAC participation becomes insignificant, and the magnitude and significance of the men's BRDB and GB affects are enlarged. The men's credit variables are jointly significant ($\chi^2(3) = 8.23$, $p = 0.04$), whereas the women's credit variables are not ($\chi^2(3) = 4.63$, $p = 0.20$). With the possible exception of the GB ($t = -1.915$), women's credit program participation does not significantly reduce fertility, even among younger married women.

## Differential Magnitude of Self-Selection by Credit Program

To investigate whether the sign and magnitude of the self-selection effect on the determinants of contraceptive practice and recent fertility differ by credit program, we allow

**TABLE 8. ESTIMATES OF THE IMPACT OF CREDIT PROGRAMS ON CURRENT CONTRACEPTIVE USE AND FERTILITY OF CURRENTLY MARRIED WOMEN AGED 14–50: ERROR CORRELATIONS VARYING WITH CREDIT PROGRAM**

| | Contraceptive Use, WESML–LIML–FE (1) | Contraceptive Use, WESML–LIML–FE (2) | Fertility, WESML–LIML–FE (3) | Fertility, WESML–LIML–FE (4) |
|---|---|---|---|---|
| Amount Borrowed by Female From BRAC | –0.073 | –0.107 | 0.079 | 0.067 |
| | (–1.693) | (–3.057) | (2.372) | (1.788) |
| Amount Borrowed by Male From BRAC | 0.040 | 0.094 | 0.054 | 0.104 |
| | (0.745) | (1.963) | (1.353) | (3.409) |
| Amount Borrowed by Female From BRDB | –0.116 | 0.003 | 0.050 | 0.037 |
| | (–2.421) | (0.044) | (1.312) | (0.652) |
| Amount Borrowed by Male From BRDB | 0.084 | 0.048 | –0.074 | –0.081 |
| | (1.475) | (0.570) | (–1.976) | (–1.649) |
| Amount Borrowed by Female From GB | –0.090 | –0.114 | –0.035 | 0.286 |
| | (–2.011) | (–2.415) | (–0.951) | (0.616) |
| Amount Borrowed by Male From GB | –0.001 | –0.058 | –0.074 | –0.118 |
| | (0.000) | (–0.735) | (–2.193) | (–2.932) |
| ρ BRAC, Female | | 0.571 | | –0.356 |
| | | (3.350) | | (–1.881) |
| ρ BRAC, Male | | –0.561 | | 0.395 |
| | | (–2.392) | | (2.027) |
| ρ BRDB, Female | | –0.184 | | –0.702 |
| | | (–0.596) | | (–5.346) |
| ρ BRDB, Male | | 0.040 | | –0.005 |
| | | (0.086) | | (–0.025) |
| ρ GB, Female | | 0.584 | | –0.345 |
| | | (2.476) | | (–1.299) |
| ρ GB, Male | | 0.189 | | 0.593 |
| | | (0.404) | | (3.633) |
| ρ (Women) | 0.425 | | –0.432 | |
| | (2.075) | | (–2.718) | |
| ρ (Men) | –0.203 | | 0.351 | |
| | (–0.700) | | (2.701) | |
| Log-Likelihood | –2,458.954 | –2,453.503 | –2,444.341 | –2,441.470 |
| Number of Observations | 1,731 | 1,731 | 1,557 | 1,557 |
| Test of Equality of ρ's Across Credit Programs for Each Gender, $\chi^2(4)$ | | 11.32 ($p = 0.02$) | | 7.45 ($p = 0.11$) |
| Test of Equality of ρ's and Equality of Credit Effects Across Program for Each Gender, $\chi^2(8)$ | | 25.24 ($p = 0.00$) | | 25.65 ($p = 0.00$) |

for the error correlations (ρ) to vary both by program and by sex. Table 8 presents these estimates side-by-side with the earlier estimates for currently married women aged 14–50 years. The bottom row of the table presents Wald $\chi^2$ test statistics for the null hypothesis that the error correlations are equal across credit programs for each sex and for the equality of both error correlation and credit parameters across credit programs for each sex.

Both null hypotheses are rejected for the case of contraceptive behavior. Selectivity into the credit programs on the basis of unobserved contraceptive variables indeed varies by program ($\chi^2(4) = 11.32$, $p = 0.02$). The positive association between the contraceptive use errors and the credit demand errors are equally large (nearly 0.6) and statistically significant for GB and BRAC ($t = 2.48$ and $t = 3.35$, respectively), but are negative and not statistically different from zero for BRDB ($\rho = -0.184$, $t = -0.60$). (The full set of female ρ's are jointly significant; $\chi^2(3) = 12.60$, $p = 0.01$.) Thus, women's GB and BRAC credit effects become even more negative (and are statistically significant), whereas the negative and significant women's BRDB effect disappears. Nonetheless, the bottom line remains the same: Women's credit program participation does not appear to increase the use of contraceptives by program participants relative to program nonparticipants and may even reduce contraceptive use.

The GB and BRDB program-specific correlation coefficients for men are not statistically different from zero in Table 8. The BRAC correlation coefficient for men, however, is negative and quite large in absolute value ($\rho = -0.561$, $t = -2.39$). Moreover, the three $\rho$'s are jointly different from zero ($\chi^2(3) = 10.55$, $p = 0.01$). Only men's BRAC credit participation seems to have any significance in increasing contraceptive use ($t = 0.094$, $t = 1.96$). (The full set of credit effects for men are not jointly significant; $\chi^2(3) = 5.24$, $p = 1.15$.) There is no evidence that men's participation in the other credit programs had a positive influence on contraceptive use.

The fertility results on the magnitude of self-selection by credit program are mixed. As seen in the bottom of Table 8, the null hypothesis that the error correlations are equal across credit programs by sex cannot be clearly rejected ($\chi^2(4) = 7.45$, $p = 0.11$). The correlation between the fertility errors and the credit demand errors is negative for women's participation in all three credit programs, though largest for BRDB, and positive (BRAC and GB) or not different from zero (BRDB) for men's participation. The joint test for the equality of error correlations and credit effects across programs by sex, however, is rejected ($\chi^2(8) = 25.65$, $p = 0.00$). This is due to the positive and significant effect of men's BRAC participation on fertility, but negative effects of men's participation in BRDB and GB. Women's participation in three credit programs continues to have a positive, though not always significant, effect on recent fertility.

## CONCLUSION

In recent years, group-based lending programs have drawn much attention in the development community as a means of poverty alleviation. Group-based credit programs may alter reproductive behavior through the provision of credit and through their social development programs. Because many of these programs target women, an important research question is whether program participation importantly changes reproductive behavior and whether the gender of the program participant matters. There have been several studies estimating the impact of women's participation in group-based credit programs on reproductive behavior, but none that are sufficiently attentive to issues of endogeneity and self-selection. We know of no study that has examined the effect of men's credit program participation on reproductive behavior.

We presented a model of household behavior in which women do not have access to the wage labor market and are unable to borrow or save to finance a homestead-based self-employment activity. Women's time is devoted exclusively to producing a per-child quality good and to leisure. The model demonstrates that if the household is endowed with capital specific to the production of a good produced at home but for sale in the market, any increase in the shadow cost of a child depends on the extent to which the child good and the self-employment good can be jointly produced.

Using data from a special survey carried out in 87 rural Bangladeshi villages during the period 1991–1992, we estimated the impact of women's and men's participation in group-based credit programs on reproductive behavior while attending to issues of endogeneity. We used the quasi-experimental design of the survey and the credit programs to surmount the problem of identification in the presence of unobserved heterogeneity. To demonstrate the importance of unobserved heterogeneity, we presented alternative estimates of the programs' impact on reproductive behavior using simpler approaches that do not control for varying levels of endogeneity. A comparison of our econometric method with the simpler alternative approaches clearly indicates the importance of our attentiveness to endogeneity in evaluating these credit programs and the mistaken conclusions that could be drawn from the simple, naive estimates.

The empirical evidence presented provides no support for the hypothesis that women's participation in group-based credit programs increases their contraceptive use relative to nonparticipants. The fertility results, which indicate that women's participation in credit programs has a positive (though not always significant) effect on fertility relative to nonparticipants, are consistent with the contraceptive results.[18] The theoretical model offers an explanation for this result. Self-employment activities of the type fostered by these credit programs are essentially different from wage labor market opportunities in that they do not necessarily raise the shadow cost of a child. It is the substitution effect arising out of the increase in the cost of a child that is widely held responsible for the decline in fertility arising from women's increased labor force participation. If this substitution effect is small because of jointness in the production of the child good and the self-employment activity, it may be swamped by the income effect arising from the increased value of women's time endowment.

Unlike women's participation, men's participation in credit programs reduces fertility and may slightly increase contraceptive use of participants relative to nonparticipants. This finding is remarkable in that income effects for men would tend to increase fertility, and substitution effects should be small or zero given their access to the labor market and the small amount of time men devote to child rearing. Pitt and Khandker (1998) find that borrowing by men had a significantly smaller effect on household consumption than an equal

18. These results should not be generalized or misinterpreted to suggest that these three group lending programs do not influence behavior and are not successful as poverty alleviation programs. In fact, Pitt and Khandker (1998) show that these three programs have significant impacts on a range of behaviors, increasing levels of household consumption and assets, female labor supply, and children's schooling. This paper's results underscore the importance of controlling for heterogeneity bias when evaluating any program in which participation is self-selected, and the fact that increased self-employment activities for women may not have the same effect on reproductive behavior as increased wage labor market opportunities.

amount of borrowing by women, suggesting that the income effect for men resulting from any level of borrowing among men is likely to be small. Thus, in households in which men participate in credit programs, the social development programs may alter reproductive behavior by changing attitudes and providing information. That the GB had the largest negative effect of male credit on fertility (Table 8, column 4) reinforces this notion. Keeping one's family small is one of the GB's "Sixteen Decisions" that members must promise to obey and that are chanted at weekly group meetings. Women are traditionally the targets of family planning field workers, and hearing once again about the benefits of small families may not have a sizable incremental impact on them. This is a novelty to men, however, and hearing this message within the disciplined and ritualized context of an all-male weekly meeting may have a larger effect on those who have not heard this message presented forcefully before. This effect underscores the importance of targeting men as well as women in social development programs.

A comparison of the "naive" results with WESML–LIML–FE results highlights the importance of controlling for heterogeneity bias when estimating the impact of group-based credit programs on reproductive behavior. The naive results show positive contraceptive effects and negative fertility effects for women's participation in two of the three credit programs, whereas the WESML–LIML–FE results indicate clear negative effects for female participants relative to nonparticipants.

## APPENDIX: A FORMAL STATEMENT OF THE PARAMETER IDENTIFICATION PROBLEM

To illustrate the identification strategy, we consider a sample drawn from two villages—village 1 does not have the program, and village 2 does—and two types of households—landed ($X_{ij}=1$) and landless ($X_{ij}=0$). Innocuously, we assume that landed status is the only observed household-specific determinant of some behavior $y_{ij}$ in addition to any treatment effect from the program. The conditional demand equation is:

$$y_{ij} = C_{ij}\delta + X_{ij}\beta_y + \mu_j^y + \varepsilon_{ij}^y.$$

The exogeneity of land ownership is the assumption that $E(X_{ij}, \varepsilon_{ij}^y) = 0$, that is, that land ownership is uncorrelated with the unobserved household-specific effect. The expected value of $y_{ij}$ for each household type in each village is

$$E(y_{ij} \mid j = 1, X_{ij}= 0) = \mu_1^y \tag{A2a}$$

$$E(y_{ij} \mid j = 1, X_{ij}= 1) = \beta_y + \mu_1^y \tag{A2b}$$

$$E(y_{ij} \mid j = 2, X_{ij}= 1) = \beta_y + \mu_2^y \tag{A2c}$$

$$E(y_{ij} \mid j = 2, X_{ij}= 0) = p\delta + \mu_2^y, \tag{A2d}$$

where $p$ is the proportion of landless households in village 2 who choose to participate in the program. It is clear that all the parameters, including the effect of the credit program $\delta$, is identified from this design.

To illustrate the log-likelihood maximized, we consider the case of a binary treatment ($I_c = 1$ if treatment is chosen, and = 0 otherwise) and a binary outcome ($I_y = 1$ if the outcome is true, and = 0 otherwise). This is the most difficult model to identify in that nonlinearity arising from the choice of an error distribution is insufficient to identify the credit effect parameter $\delta$. In the estimation results, the treatment is measured as cumulative borrowing of program credit. Distinguishing households not having choice because they reside in a nonprogram village from households residing in a program village that do not have choice because of the application of an exogenous rule (landowning status), and suppressing the household and village subscripts $i$ and $j$, the likelihood can be written as

$$\begin{aligned}\log L(\beta, \delta, \mu, \rho) = &\\ &\Sigma_{\text{choice}}\log\Phi_2((\mu_p^c + X\beta_c)d_c, (\mu_p^y + X\beta_y + \delta I_c)d_y, \rho d_c d_y)\\ &+ \Sigma_{\text{no choice program village}}\log\Phi((\mu_p^y + X\beta_y)d_y)\\ &+ \Sigma_{\text{nonprogram village}}\log\Phi((\mu_n^y + X\beta_y)d_y),\end{aligned} \tag{A3}$$

where $\Phi_2$ is the bivariate standard normal distribution, $\Phi$ is the univariate standard normal distribution, $\mu_p^c$ are the village-specific effects influencing participation in the credit program in program villages, $\mu_p^y$ are village-specific effects influencing the binary outcome $I_y$ in program villages, $\mu_n^y$ are the corresponding village-specific effects in nonprogram villages, and $d_c = 2*I_c - 1$ and $d_y = 2*I_y - 1$.[19] The errors $\varepsilon_{ij}^c$ and $\varepsilon_{ij}^y$ are normalized to have unit variance and correlation coefficient $\rho$. Village-specific effects ($\mu_n^c$) influencing the demand for program credit are not identifiable for villages with no programs.

The first part of the likelihood is the joint probability of program participation and the binary outcome $I_y$ conditional on participation for those households that are *both* eligible to join the program (*choice*) and reside in a village with the program (*program village*). This part of the likelihood corresponds to the expectation (A2d). Without regressors ($Z$) that influence the probability of program participation but not the outcome $I_y$ conditional on participation, the parameter $\delta$, the effect of credit on the outcome $y$, is not separately identified from the parameters $\mu_p^y$ and $\beta_y$ from this part of the likelihood. The second part of the likelihood is the (univariate) probability of binary outcome $I_y$ for landed households in program villages and corresponds to expectation (A2c). These households are precluded from joining the program by their landed status. The last part of the likelihood is the probability of the outcome $I_y$ for all households, landed and landless, in villages without a program and corresponds to expectations (A2a and A2b). If one of the regressors in $X$ is a binary indicator of landed status, this part of the likelihood

19. We implicitly assume that the effect of the treatment ($\delta$) is the same for all individuals, an assumption that is common in the literature on program evaluation (Moffitt 1991). Furthermore, the model is not nonparametrically identified. That is, if the linear indices $X_c\gamma$ and $(X_y\beta + \delta I_c)$ were replaced by nonparametric functions of the $X$'s, and $I_c$, the model is not identified. To ensure that the program effect estimated is not driven by the linear relationship between land holdings and the outcome variable, we estimated the model while allowing for land to enter as a quadratic and successively higher-level polynomial. The program effect results we report were not qualitatively altered by these changes.

is required for identification. If the binary variable for landed status is replaced by a continuous measure of landholding, then all the parameters of the model are identified without the last part of the likelihood. In this case, the parameter $\beta_y$ in (A1) is identified from variation in landholding within the program villages ($j$ = 2), and a sample of nonprogram villages is not required.

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