

**CREDIT PROGRAMS FOR THE POOR AND THE HEALTH STATUS
OF CHILDREN IN RURAL BANGLADESH***

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The impact of participation in group-based credit programs, by gender of participant, on the health status of children by gender in rural Bangladesh is investigated. These credit programs are well suited to studies of how gender-specific resources alter intra-household allocations because they induce differential participation by gender. Women's credit is found to have a large and statistically significant impact on two of three measures of the healthiness of both boy and girl children. Credit provided to men has no statistically significant impact and the null hypothesis of equal credit effects by gender of participant is rejected.

1. INTRODUCTION

This article examines the effect of additional resources supplied to and controlled by women, as compared to men, on child health outcomes, by gender of child. The source of these additional resources is group-based credit programs for the poor in rural Bangladesh. These credit programs are well suited to studying how gender-specific resources alter intra-household allocations because they induce differential participation by gender through the requirement that only one adult member per household can participate in any micro-credit program. This article thus contributes to the growing literature in which joint family decisions are derived from the possibly divergent interests of husbands and wives. The empirical literature has suggested that a mother's relative control over resources importantly alters the human capital of her children; specifically that children seem to be better off when their mothers control relatively more of their family's resources. Some of this literature has used the relative earnings of mothers and fathers to indicate control over resources. The endogeneity of earnings makes any

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inferences drawn using such measures suspect without some source of exogenous variation.²

The gender-specific additional resources in this analysis are the loans provided by the Grameen Bank, Bangladesh Rural Advancement Committee (BRAC), and Bangladesh Rural Development Board's (BRDB) Rural Development RD-12 program. These are the major small-scale credit programs in Bangladesh, which provide production credit and other services to the poor. The largest group of program participants has been women, most of whom had no direct contact with the credit market prior to participating in these programs. The self-employment activities financed by these credit programs may affect the health status of children through the standard income and substitution effects, as well as by increasing the role and power of women in the household resource allocation process. Earlier work on the effect of these credit programs on other behaviors (Pitt and Khandker, 1998; Khandker et al., 1995; McKernan, 2001) has demonstrated that the magnitude of the impact of borrowing on labor supply, expenditure, assets, schooling of children, fertility, contraceptive use, and self-employment profits depends on the gender of credit program participant. In this article we extend those results by not only asking if the size of the effect of program participation on child health depends upon the sex of the program participant, but also whether the effects differ by gender of the child.

All three of the Bangladesh programs examined below work exclusively with the rural poor. Although the sequence of delivery and the provision of inputs vary some from program to program, all three programs essentially offer production credit to the landless rural poor (defined as those who own less than half an acre of land) using peer monitoring as a substitute for collateral. For example, the Grameen Bank 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 weekly to make repayments on their loans as well as mandatory contributions to savings and insurance funds. The Grameen Bank, BRAC, and BRDB also provide non-credit services in areas such as consciousness raising, skill development training, literacy, bank rules, investment strategies, health, schooling, civil responsibilities, and altering the attitudes of and toward women.

The research results presented below are based upon a 1991/92 survey of 1,798 households in 87 villages in rural Bangladesh. The earlier studies, cited above, which used these survey data confirm that program participation by households is self-selective. The estimation method used in these earlier studies corrects for the potential bias arising from unobserved individual- and village-level heterogeneity by taking advantage of a quasi-experimental survey design to provide statistical identification of the program effects. The survey design covers one group of households which has the choice to enter a credit program and which may alter their

² Thomas (1990), among others, seeks to avoid this problem by using measures of unearned income. Unearned income is not entirely exogenous with respect to past or present household behavior, and the quality of unearned income data is suspect in poor societies. Moreover, these data are contaminated by marriage market selection.

behavior in response to the program, and a “control” group that is not given the choice of entering the program but whose behavior is still measured. Similarly, the identification of these programs’ impact by the gender of the participant is accomplished based on the comparison between groups of each gender with and without the choice to participate. These programs, whose professed goal is to better the lives of the poor, may have chosen villages in a conscious manner based on their wealth, attitudes or other attributes. To deal with the possibility of endogenous program placement, these studies couple the quasi-experimental design with village-level fixed effects to sweep out village unobservables that might otherwise bias estimates of the impacts of these credit programs.

The 1991/92 survey included a special health status module that collected anthropometric data on all children under the age of 15 years in 15 of the 87 villages surveyed. Unfortunately, only villages with a credit program and only households who were eligible to participate in the programs were included in this module, making identification via the quasi-experimental survey design impossible. Another identification strategy is required. As Pitt and Khandker (1998) have argued at length, an instrumental variable approach based upon exclusion restrictions is not applicable because there are unlikely to be valid identifying instruments; that is, exogenous variables affecting credit program participation (by sex) that do not also affect resource allocations to children conditional on participation. This is related to the well known “more goods than prices” problem that naturally arises in the study of intra-household resource allocation. Pitt (1997) reviews some of the approaches to estimating the demand for goods within the household conditional on some individual- or household-specific endogenous choice, and suggests cross-person restrictions on demands within the household as one possible direction.³ For example, Pitt and Rosenzweig (1990) use cross-person restrictions on regression parameters to estimate the effect of infant illness on the time allocation of the male and female teenage siblings and the mother of the infant. The estimated conditional demand equations in that article, the time allocation of male and female teenagers conditional on the health of an infant sibling, have the same structure as in this article—the health of male and female children conditional on the credit program participation of an adult household member. The idea is to estimate the differenced male–female equation conditional on the health of the infant sibling. If the effects of some subset of exogenous regressors (prices) on time allocation conditional on infant health are restricted to be the same for male and female teenagers, these regressors became available as identifying instrumental variables for infant health in the differenced regression.

This method has two weaknesses if applied to our problem. First, it identifies the *differential* effects of credit program participation on the health status of boys and girls, not their level effects. That is, we could learn whether credit program participation augmented the health status of girls relative to boys, but not whether

³ The problem with applying household fixed-effects estimation is that one cannot identify the level parameters of household-specific (as opposed to person-specific) regressors. Credit program participation by either the father or mother of the children in a household is a household-specific variable from the perspective of the individual children in the household, who are the units of observation in the estimation.

the total effect for either gender was positive or negative, or the magnitude of the effect. We believe it is important to know if credit programs affect the health of children, not just if they differentially affect health, and thus we require another identification strategy. Second, as noted above, we need to apply a village fixed-effects estimation method in order to sweep out village unobservables that might otherwise bias estimates of the impacts of credit programs. The placement of these credit programs (by sex of participants) in villages has been shown to be correlated with unobserved village characteristics in Pitt and Khandker (1998). Differencing across opposite-sex siblings within the family will not sweep out the effect of village unobservables unless they have exactly the same effect on each sex. This would be an unrealistic assumption to make. For example, one would have to argue that village-level attitudes on the treatment of girls relative to boys that affect their relative health status are not associated with the probability of having a woman's or man's credit group in the village. In other words, one would need to assume that village attitudes associated with the relatively equal treatment of girls are unrelated to attitudes on the participation of women in self-employment activities. We do not believe that parameter identification can rest on such a clearly unrealistic assumption. To sweep out all of these village unobserved factors associated with the availability of credit program by gender and with health status by gender requires actually sweeping out all gender-specific village fixed effects. Detailed price data, collected in each village market, are perfectly correlated with these village fixed effects and hence are unavailable as instruments as they are in Pitt and Rosenzweig (1990).

To estimate the level effects of credit programs by gender of the credit program participant on individual-specific indicators of well-being, we propose a set of restrictions on the error covariances of a set of gender-specific health behaviors and the decision to participate in a credit program. Chamberlain (1976, 1977a, 1977b) and Chamberlain and Griliches (1975) first demonstrated how to identify the parameters of a structural equation model from restrictions on error covariances.⁴ Extending their approach to our estimation problem permits identification of the level effects of credit program participation by gender of participant without requiring the imposition of difficult to justify zero restrictions that are inconsistent

⁴ There was much subsequent work using covariance restrictions to estimate simultaneous equations models. Bound et al. (1986) extend Chamberlain and Griliches' framework to opposite-sex siblings in studying the effect of ability on wages and schooling. Many applications of covariance restrictions make use of twins or impose a recursive structure arising out of the assumed sequential nature of the decision process, in which, for example, the schooling decision precedes the occupational decision which in turn precedes wage realizations. More recent applications make comparisons of children within the household along with their mother, as in Rosenzweig and Wolpin (1994, 1995), or comparisons of husbands and wives, as in Millimet (2000). In our application we do not assume a recursive decision-making process or make use of the peculiar restrictions that arise from comparing sets of twin. Our approach differs from most others in that it uses unbalanced cross- and own-sibling comparisons to formulate restrictions, makes use of a short panel to aid identification, permits interdependencies in health outcomes across siblings to arise from other sources, such as compensatory resource allocations, besides household-specific factors, and examines the effect of household-specific endogenous variables (female and male credit program participation), rather than individual-specific variables such as ability, on the outcomes of interest.

with a general household model, or relying exclusively on arbitrary assumptions about the distribution of errors. It does, however, require restrictions on the nature of the regression errors. The idea is to place a factor-analytic structure on the residuals of a set of equations for female and male credit program participation and a set of health measures, which in our study are arm circumference, body mass index, and height-for-age. In the estimation of the determinants of these health outcomes, the residual includes left-out household- and individual-specific variables such as relative bargaining power in the household, innate healthiness (health endowments), and preferences. These omitted variables may affect the health (and other) resources allocated to boy and girl children, but not necessarily in the same way.

The imposition of a factor-analytic structure can identify structural parameters with a single cross section of data providing that (i) we jointly estimate the determinants of a sufficiently large number of health outcomes that are likely to be influenced by a common latent factor, and (ii) we have a sufficient number of households in the sample with more than one child and with children of more than one sex. As we demonstrate below, both conditions are satisfied by our data. In addition, we have data on health status at two points in time for each sampled household. Adding a time-period variance-components structure to the factor-analytic structure adds additional identifying restrictions to the residual covariance matrix (as in Hsiao, 1986).⁵ Estimation is by maximum likelihood.

Our results are striking. After taking into account the endogeneity of individual participation in these credit programs and the placement of these credit programs across areas, we find that women's credit has a large, positive, and statistically significant impact on two of three measures of the nutritional well-being of both boy and girl children. Credit provided to men has no statistically significant impact. These patterns are not apparent when credit program participation is treated as exogenous in the determination of child health.

Section 2 of this article outlines an economic theory of household allocation to guide in the interpretation of the empirical results. Section 3 describes the estimation and identification of structural equations with covariance restrictions based upon regression errors with a factor-analytic structure. The precise form of the log-likelihood maximized is fairly messy and its derivation is relegated to an appendix. Section 4 describes the 1991/92 Bangladesh micro-credit survey and the data used in the estimations. Section 5 presents and interprets the parameter estimates of our structural model and compares these estimates to those obtained from models that alternatively assume that household participation in these credit programs is exogenous and that credit programs are randomly placed across villages. Section 6 summarizes our results.

⁵ The panel nature of the data suggests that *individual* fixed-effects estimation may be another approach to dealing with the heterogeneity bias. Unfortunately, the two survey rounds are quite closely spaced, only 6 months apart. It is well known that the signal-to-noise ratio can get dangerously low in this case leading to severe attenuation bias. Moreover, the endogenous regressor, participation in a group-based credit program, changed only slightly, if at all, for most households over this short period of time.

2. THEORY

Our objective is to estimate the effects of group-based credit program participation (separately for husbands and wives) on the production of child health (distinguished by gender). To motivate the evaluation of such program participation, consider a simple one-period collective model of household decision making. Since our goal is to illustrate in a simple manner the multiple avenues by which credit program participation may impact child quality, we treat the credit program as a fixed endowment of specific capital. This capital is acquired as a consequence of the endogenous choice to participate in a credit program, where participation entails some fixed cost of participation. The model allows for the preferences of men and women to differ within the household, for the possibility of gender-related productivity differences in the production of child health and other home-produced goods, and for their strength in household bargaining to depend on gender-specific program participation.

Assume that households consist of a boy and a girl child and two working-age adults—the male head and his wife. Each adult has preferences described by a person-specific utility function

$$(1) \quad u_s = u_s(h_b, h_g, Q, l_m, l_f), \quad s = m, f$$

where h_b and h_g are the quality (health) of the boy and girl child, respectively, Q is a set of jointly consumed market goods, and l_m and l_f are the leisure of the male and female adult household members, respectively. The household's social welfare is some function of the individual utility functions $U = U(u_f, u_m)$. Browning and Chiappori (1998) have shown that if behavior in the household is Pareto efficient, the household's objective function takes the form of a weighted sum of individual utilities,

$$(2) \quad U = \tau u_f + (1 - \tau)u_m, \quad \tau \in [0, 1]$$

The parameter τ can be thought of as representing the bargaining power of the female household member relative to the male household member in determining the intra-household allocation of resources. When $\tau = 0$, female preferences are given no weight and the household's social welfare function is identically that of the male. In accord with much of the literature, we presume that τ is increasing in the relative value of female time and her money income.

To the extent that participation in a group-based credit program increases the value of female time and her control of money income, it also increases her preference weight in (2). Without substantial loss of generality, τ is assumed to take one of two values given by

$$(3) \quad \tau = \begin{cases} \tau_1 & \text{if female participates} \\ \tau_0 & \text{otherwise} \end{cases}$$

where $\tau_1 \geq \tau_0$.

The household produces the child quality good h provided to each child according to

$$(4) \quad h_i = h_i(L_{fhi}, L_{mhi}, F_i), \quad i = b, g$$

where L_{fhi} and L_{mhi} is the time devoted to the production of h for children of sex i by females and males, respectively, and F_i is a purchased input (food). In rural Bangladesh, as in much of the developing world, females supply substantially greater time to the production of child quality than do males.

Very few women in rural Bangladesh work in the wage labor market or in own-farm production that takes place outside of the home. It is a conservative Islamic society that encourages the seclusion of women. Lacking other opportunities, women are engaged in the production of household goods, to the exclusion of employment in market activities. These effects are magnified if τ is small and male preferences tend to favor production of certain kinds of household goods intensive in the time of women.

There are also economic activities that produce goods for market sale that are considered culturally acceptable. These activities, which produce what we refer to as Z -goods, do not require production 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, K_{\min} , 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 where the indivisibility of physical capital is not an issue, such as paddy husking, transactions 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 costs of initiating production at minimal economic levels are 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, as shown in Gersovitz (1983).

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

$$(5) \quad Z = Z(L_{fz}, L_{mz}, A, K)$$

where L_{sz} is the time by adult s , $s = m, f$, devoted to the production of Z , K is capital used in Z production, and A is a vector of variable inputs. If a female is the program participant ($I^f = 1$), she is allocated capital $K^f \geq K_{\min}$. This endowment from the credit program comes with the requirement that the borrower operate the self-employment activity, which is taken to mean that all labor is provided by the program participant ($L_{mz} = 0$). This rules out the possibility of the male obtaining the loan, but the female supplying the labor involved in the production of Z . Similarly, if a male participates in the program, he receives an

endowment of capital $K^m \geq K_{\min}$ and must supply all of the labor (i.e., $L_{fz} = 0$). There are costs to participating in and borrowing from a group-based credit program. As our interest is not in modeling all the considerations that underlie the cost of borrowing from these programs, but rather in the implications of participation on intra-household resource allocation, we do not model these costs explicitly and simply denote them as p_{cf} and p_{cm} for females and males, respectively. An important part of these costs is the interest charged on loans, but they also include the disutility of participating in mandatory weekly group meetings, and the requirement that one monitor other group members and be monitored by them.

To complete the model, we note that the household is subject to several additional constraints. First, the household's budget constraint is

$$(6) \quad Q + p_F(F_b + F_g) = v + L_m w_m + (I^m + I^f)(p_z Z(L_{fz}, L_{mz}, A, K) - p_A A) - I^f p_{cf} - I^m p_{cm}$$

where the jointly consumed market good Q is the numeraire; p_A , p_F , and p_z are the exogenous prices of A (inputs to the Z -good), the purchased input F used to produce child quality, and the Z -good, respectively; v is nonearnings income, and w_m is the male wage. Second, the credit programs restrict participation to at most only one adult member of the household (so that $I^m + I^f$ must equal either zero or one). Finally, both adults are endowed with time L . Men's time is allocated to wage labor L_{mw} , production of the child quality L_{mh} , and the Z -good, L_{mz} , and to leisure, l_m :

$$(7) \quad L = L_{mw} + L_{mh} + L_{mz} + l_m$$

Women's time is spent in the production of the h -good, L_{fh} , production of the Z -good, L_{fz} , and in leisure, l_f :

$$(8) \quad L = L_{fh} + L_{fz} + l_f$$

The household's problem is to decide which adult, if either, participates in the program and the allocation of household resources conditional on that participation decision. Since program participation by a married woman requires her husband's assent, it is assumed that the male unilaterally makes the credit program participation decision for the household. He must choose among three different states: (i) no participation, (ii) he participates, or (iii) his wife participates. Fully informed about the effect of participation on household income and the bargaining weight, τ , the male chooses the state that maximizes his own utility, u_m . It is clear that he will allow his wife to participate only if household full income increases by more than enough to offset his (potential) loss in utility due to her increased bargaining power. He will participate if his full income from producing the Z -good exceeds that determined by his market wage.

Given the solution to the participation decision, the conditional demand equations for the inputs into the child health production function take the general form

$$(9) \quad L_{shi}^* = L_{shi}(p_A, p_F, p_z, p_{cf}, p_{mf}, v, K^f, K^m), \quad s = m, f; i = b, g$$

$$F_i^* = F_i(p_A, p_F, p_z, p_{cf}, p_{mf}, v, K^f, K^m), \quad i = b, g$$

where K^m or K^f (or both) must be zero. Combining (4) and (9), the effect of an exogenous increase in gender-specific capital on the health of boys and girls is

$$(10) \quad \frac{\partial h_i^*}{\partial K^s} = \frac{\partial h_i}{\partial L_{fhi}^*} \frac{\partial L_{fhi}^*}{\partial K^s} + \frac{\partial h_i}{\partial L_{mhi}^*} \frac{\partial L_{mhi}^*}{\partial K^s} + \frac{\partial h_i}{\partial F_i^*} \frac{\partial F_i^*}{\partial K^s}, \quad i = b, g; s = m, f$$

where h_i^* , $i = b, g$, is the optimal allocation of health to children.

There are three interesting questions that arise from examination of (10). First, does an influx of capital necessarily improve the health of children and, if so, by how much? That is, what is the sign and magnitude of (10)? Second, does the effect differ by gender of the participant? Third, does the effect differ by the gender of the child? Assuming the marginal products of the three inputs in (4) are positive, the sign of (10) depends on the effect of incremental productive capital on the demand for the three inputs, which are the partial derivatives of the conditional demand system in (9). Consequently, the sign of (10) is ambiguous since these partial derivatives reflect changes in the marital bargaining weight, τ , as well as the usual income and substitution effects, the net effect of which is not theoretically signed.

The second question asks what is the differential impact of an exogenous increase in male capital compared to female capital, which is given by

$$(11) \quad \frac{\partial h_i^*}{\partial K^m} - \frac{\partial h_i^*}{\partial K^f} = \frac{\partial h_i}{\partial L_{fhi}^*} \left(\underbrace{\frac{\partial L_{fhi}^*}{\partial K^m} - \frac{\partial L_{fhi}^*}{\partial K^f}}_A \right) + \frac{\partial h_i}{\partial L_{mhi}^*} \left(\underbrace{\frac{\partial L_{mhi}^*}{\partial K^m} - \frac{\partial L_{mhi}^*}{\partial K^f}}_B \right)$$

$$+ \frac{\partial h_i}{\partial F_i^*} \left(\underbrace{\frac{\partial F_i^*}{\partial K^m} - \frac{\partial F_i^*}{\partial K^f}}_C \right), \quad i = b, g$$

The sign of A and B are ambiguous and depend on the relative size of the own and cross effects of an exogenous increase in (gender-specific) capital on time spent in the production of child health (4).

Finally, the third question pertains to the differential effects of incremental capital on sons versus daughters (conditional on gender of the participant),

$$(12) \quad \frac{\partial h_b^*}{\partial K^s} - \frac{\partial h_g^*}{\partial K^s} = \underbrace{\left(\frac{\partial h_b}{\partial L_{fhb}^*} \frac{\partial L_{fhb}^*}{\partial K^s} - \frac{\partial h_g}{\partial L_{fhg}^*} \frac{\partial L_{fhg}^*}{\partial K^s} \right)}_A + \underbrace{\left(\frac{\partial h_b}{\partial L_{mhb}^*} \frac{\partial L_{mhb}^*}{\partial K^s} - \frac{\partial h_g}{\partial L_{mhg}^*} \frac{\partial L_{mhg}^*}{\partial K^s} \right)}_B + \underbrace{\left(\frac{\partial h_b}{\partial F_b^*} \frac{\partial F_b^*}{\partial K^s} - \frac{\partial h_g}{\partial F_g^*} \frac{\partial F_g^*}{\partial K^s} \right)}_C, \quad s = m, f$$

The terms A and B on the right-hand side of (12) may be nonzero if the productivity of adult time in the production of child health differs by the gender of the child, or if the time reallocated from the production of child health as a consequence of the incremental capital differs by the gender of the child. Similarly, the sign of C depends both on the relative productivity of F and any differential change in its demand by gender of child. Changes in time and F_i reflect changes in τ as well as income and substitution effects.

Since women do not have access to the wage labor market, the shadow price of women's time depends on program participation, both through the increase in the productivity of women's time associated with the self-employment that the capital endowment K^f enables, and through the change in the preference weight τ associated with women's participation. In contrast, if men still provide time to the wage labor market, the shadow value of their time is unaffected by program participation. Moreover, since the preference weight is unaffected by male participation, such participation does not alter the shadow price of women's time either. The only source of change in the demand for child health arises from the income effect associated with the rental value of the capital endowment provided by the credit program to the male participant. Note that although male participation identifies the income effect (conditional on τ), this information does not help disentangle the substitution effect from the bargaining effect induced by women's participation. The substitution effect of a change in the value of women's time on child health may be positive if, for example, the increase in the value of their time induces men to devote more time to the production of child health, and this reallocation produces a net increase in child health. Even if this substitution effect was assumed to be strictly negative, one could still not sign its effect on the bargaining

weight, τ , even if it were found that women's program participation increased child health, that is, if the sign of (10) is positive. The income effect associated with women's program participation differs from that of men's because the increased shadow value of women's time induces an income effect above and beyond that associated with the rental value of the capital endowment. Thus, a finding that the effect of women's program participation on child health differs from the effect of men's program participation does not necessarily imply that women have gained power in the household, even if women are assumed to prefer child quality more than their husbands. While it is not possible to separately identify the various avenues by which participation maps into changes in child quality, empirical analysis will identify the signs of the comparative static results in (10), and thus (11) and (12) as well.

3. STRUCTURAL EQUATION ESTIMATION IDENTIFIED WITH COVARIANCE RESTRICTIONS

Our empirical objective is to estimate the demand equations for child health by gender conditional on the gender-specific resources provided by group-based credit programs for the landless poor. As noted in Section 2, there are not likely to be valid identifying instruments; that is, exogenous variables affecting credit program participation (by sex) that do not also affect resource allocations to children conditional on participation. This is related to the well-known "more goods than prices" problem that naturally arises in the study of intra-household resource allocation. When there are more goods than prices, even if price data were available for each good, the person-specific parameters of the unconditional demand equations would still be unidentifiable. When prices are not person-specific, own- and cross-price effects cannot readily be disentangled (Pitt, 1997).

3.1. *Structural Estimation with Sibling Data.* We begin by specifying a simplified version of the model we actually estimate in which a set of child-specific health behaviors depends, in part, on a common latent variable that also determines the value of a household-specific behavior, credit program participation. Consider a sample of households indexed by i consisting of siblings indexed by j for which we observe three related behaviors, h_{ij} , m_{ij} , and f_{ij} , in addition to the value of a household-specific endogenous variable y_i . Unobserved heterogeneity, which affects y_i , is also believed likely to affect these child-specific health behaviors conditional on y_i . As a result, estimating the determinants of these three behaviors conditional on the household-specific variable raises issues of heterogeneity bias. However, if there is a common source or sources to this heterogeneity, it is possible to estimate the parameters of the conditional demand equations by estimating the reduced forms imposing this *factor structure* on the residual covariance matrix. We write the conditional demand equations for the three behaviors h_{ij} , m_{ij} , and f_{ij} , conditional on a household choice variable y_i (the amount borrowed from

group-based credit programs):

$$(13) \quad h_{ij} = X_{ij} \beta_h + \delta_h y_i + \lambda_h \mu_i + \varepsilon_{ij}$$

$$(14) \quad m_{ij} = X_{ij} \beta_m + \delta_m y_i + \lambda_m \mu_i + \eta_{ij}$$

$$(15) \quad f_{ij} = X_{ij} \beta_f + \delta_f y_i + \lambda_f \mu_i + v_{ij}$$

and the reduced form household demand for y_i ,

$$(16) \quad y_i = X_{ij} \beta_y + \lambda_y \mu_i + \xi_i, \quad i = 1, \dots, n$$

where X_{ij} is a set of exogenous regressors not all of which necessarily vary across siblings in the same household, μ_i is the unobserved source of household heterogeneity normalized to have unit variance, ε_{ij} , η_{ij} , v_{ij} , and ξ_{ij} are error terms uncorrelated with μ_i , and β , δ , and the *factor-loadings* λ are parameters to estimate. To keep the exposition simple, we have conditioned the three health behaviors h_{ij} , m_{ij} , and f_{ij} on only one endogenous variable y_i , although in the estimation there are two gender-specific conditioned-upon endogenous variables (female and male credit). The appendix provides a complete characterization of the econometric model and the likelihood function maximized.

As a further expository simplification, we begin by assuming that the only source of correlation among these conditional demand equations is the μ component and that the errors ε , η , v , and ξ are uncorrelated across behaviors and individuals.⁶ In the context of the theoretical model presented in Section 2, the factor μ can be thought of as preference heterogeneity. The probability that a woman will join a credit program (which precludes her husband from joining) quite likely depends on how her preferences in (1) differ from those of her husband, and the change in τ associated with her participation. In addition, the maternal altruism hypothesis argues that women prefer investments in children more than men and are less likely to favor boys, so that nutritional outcomes by gender are also likely to be correlated with unobserved components of τ .⁷

Estimation of Equations (13)–(15), which does not take into account the correlation between the right-hand-side regressor y_i and the common source of error correlation, μ_i , will result in biased estimates of the δ parameters, the effects of y_i on health outcomes. We can substitute Equation (16) into the

⁶ This restriction is made only to simplify the demonstration of identification and is relaxed in the empirical implementation.

⁷ In addition, the factor μ_i may reflect the family health endowment. Poor health may alter the ability of parents, women in particular, to engage in time-consuming self-employment activities, and also affect the nutritional status of children.

conditional demand Equations (13)–(15) to get the reduced form demand equations for h , m , and f ,

$$(17) \quad h_{ij} = X_{ij} \tilde{\beta}_h + \tilde{\lambda}_h \mu_i + \tilde{\varepsilon}_{ij}$$

$$(18) \quad m_{ij} = X_{ij} \tilde{\beta}_m + \tilde{\lambda}_m \mu_i + \tilde{\eta}_{ij}$$

$$(19) \quad f_{ij} = X_{ij} \tilde{\beta}_f + \tilde{\lambda}_f \mu_i + \tilde{\nu}_{ij}$$

where

$$(20) \quad \begin{aligned} \tilde{\beta}_k &= \beta_k + \delta_k \beta_y, & k &= h, f, m \\ \tilde{\lambda}_k &= \lambda_k + \delta_k \lambda_y, & k &= h, f, m \end{aligned}$$

and

$$(21) \quad \begin{aligned} \tilde{\varepsilon}_{ij} &= \varepsilon_{ij} + \delta_h \xi_i \\ \tilde{\eta}_{ij} &= \eta_{ij} + \delta_m \xi_i \\ \tilde{\nu}_{ij} &= \nu_{ij} + \delta_f \xi_i \end{aligned}$$

These reduced forms are correlated with each other through the common error components μ_i and ξ_i and have a variance–covariance matrix of the form,

$$(22) \quad \Sigma = \begin{pmatrix} \Sigma_h & \Sigma_{hm} & \Sigma_{hf} \\ \Sigma'_{hm} & \Sigma_m & \Sigma_{mf} \\ \Sigma'_{hf} & \Sigma'_{mf} & \Sigma_f \end{pmatrix}$$

with diagonal submatrices of the form

$$(23) \quad \Sigma_m = \begin{pmatrix} \tilde{\lambda}_m^2 + \sigma_{\tilde{\eta}}^2 & \tilde{\lambda}_m^2 \\ \tilde{\lambda}_m^2 & \tilde{\lambda}_m^2 + \sigma_{\tilde{\nu}}^2 \end{pmatrix} \equiv \begin{pmatrix} \sigma_{mv}^2 & \sigma_{mc} \\ \sigma_{mc} & \sigma_{mv}^2 \end{pmatrix}$$

and off-diagonal submatrices of the form

$$(24) \quad \Sigma_{hm} = \begin{pmatrix} \tilde{\lambda}_h \tilde{\lambda}_m & \tilde{\lambda}_h \tilde{\lambda}_m \\ \tilde{\lambda}_h \tilde{\lambda}_m & \tilde{\lambda}_h \tilde{\lambda}_m \end{pmatrix} \equiv \begin{pmatrix} \sigma_{hm} & \sigma_{hm} \\ \sigma_{hm} & \sigma_{hm} \end{pmatrix}$$

In households containing two children of the same gender (brothers), there are 13 unique variance–covariance terms that are functions of 11 free parameters ($\lambda_h, \lambda_m, \lambda_f, \lambda_y, \delta_h, \delta_m, \delta_f, \sigma_h, \sigma_m, \sigma_f, \sigma_y$). Thus, there are two overidentifying

restrictions. Consider the *unique* variance–covariance terms

$$\begin{aligned}
 \sigma_{hv}^2 &= \tilde{\lambda}_h^2 + \sigma_\varepsilon^2 + \delta_h^2 \sigma_\xi^2 \\
 \sigma_{mv}^2 &= \tilde{\lambda}_m^2 + \sigma_\eta^2 + \delta_m^2 \sigma_\xi^2 \\
 \sigma_{fv}^2 &= \tilde{\lambda}_f^2 + \sigma_v^2 + \delta_f^2 \sigma_\xi^2 \\
 \sigma_{hc} &= \tilde{\lambda}_h^2 + \delta_h^2 \sigma_\xi^2 \\
 \sigma_{mc} &= \tilde{\lambda}_m^2 + \delta_m^2 \sigma_\xi^2 \\
 \sigma_{fc} &= \tilde{\lambda}_f^2 + \delta_f^2 \sigma_\xi^2 \\
 \sigma_{hm} &= \tilde{\lambda}_h \tilde{\lambda}_m + \delta_h \delta_m \sigma_\xi^2 \\
 \sigma_{hf} &= \tilde{\lambda}_h \tilde{\lambda}_f + \delta_h \delta_f \sigma_\xi^2 \\
 \sigma_{mf} &= \tilde{\lambda}_m \tilde{\lambda}_f + \delta_m \delta_f \sigma_\xi^2 \\
 \sigma_y^2 &= \lambda_y^2 + \sigma_\xi^2 \\
 \sigma_{hy} &= \tilde{\lambda}_h \lambda_y + \delta_h \sigma_\xi^2 \\
 \sigma_{my} &= \tilde{\lambda}_m \lambda_y + \delta_m \sigma_\xi^2 \\
 \sigma_{fy} &= \tilde{\lambda}_f \lambda_y + \delta_f \sigma_\xi^2
 \end{aligned}
 \tag{25}$$

where σ_y^2 is the variance of the composite regression error for the reduced form determinants of y_i given by Equation (16). Recall that $\sigma_\mu^2 = 1$ so that the λ_k , $k = h, j, m$, represents the standard deviations of the factor μ in the conditional demand Equations (13)–(15).

A solution for δ_k , $k = h, f, m$, the effect of y_i , credit program participation, can be obtained from this set of moment conditions. To demonstrate that this is so, consider an instrumental variables interpretation of parameter identification derived from this factor-analytic structure. Instrumental variable estimation of the system of Equations (13) through (16) can proceed as follows. First, eliminate the household effect μ_i from Equation (13) by solving Equation (14) for μ_i and substituting this expression into (13), which yields

$$\tag{26} \quad h_{ij} = X_{ij} \left(\beta_h - \lambda_h \frac{\beta_m}{\lambda_m} \right) + \left(\delta_h - \lambda_h \frac{\delta_m}{\lambda_m} \right) y_i + \frac{\lambda_h}{\lambda_m} m_{ij} + \left(\varepsilon_{ij} - \lambda_h \frac{\eta_{ij}}{\lambda_m} \right)$$

In Equation (26), the variable y_i is no longer correlated with the error since the sole source of its correlation with the residual of (13), μ_i , has been purged. On the other hand, the health behavior m_{ij} now appears on the right-hand side of (26) and it is correlated with the residual through the error component η_{ij} . However, valid instruments for m_{ij} exist. The values of m_{ik} , h_{ik} and f_{ik} for all siblings of brother j ($k \neq j$) are correlated with m_{ij} through the common component μ_i but are uncorrelated with the error in Equation (26). Note that this model is identified by the purged instrumental variables even if the child-specific error components

of Equations (13) to (15) are correlated, that is, $E(\varepsilon_{ij}, \eta_{ij}) \neq 0$, $E(\varepsilon_{ij}, v_{ij}) \neq 0$, and $E(\eta_{ij}, v_{ij}) \neq 0$, correlations that are permitted in the empirical application reported below, although it is far less transparent than the more restrictive model used in this example.

3.2. *Interdependency of Health Outcomes across Siblings.* It is clear that one can estimate the complete set of child-specific health outcome equations corresponding to Equations (13) through (15) by purging the μ_i component of each equation, as above, and identify the parameters of interest, δ . However, in its current form this model retains the unappealing assumption that health outcomes across siblings are correlated only through the household-specific component μ_i . This assumption requires that child health be determined by the labor supply decision of parents only, but not that of siblings. Empirical work on this topic suggests that the labor input of siblings, particularly older girls, has a significant impact on the health of younger children, as in Pitt and Rosenzweig (1990). In addition to this possible effect, interdependencies in child health may arise through the structure of household preferences. Households may compensate individual children endowed with poorer health than their siblings by transferring additional resources to them at the expense of those siblings. If we were analyzing a single outcome of children, as in Pitt and Rosenzweig (1990), allowing for interdependencies among children would make identification impossible. However, with three health outcomes in each of two periods for each child, interdependency can be permitted without destroying identification. In particular, we allow the child-specific components (the ε_{ij} , η_{ij} , and v_{ij}) of the conditional (on credit) health demand equations (13)–(15) to be correlated among siblings. We assume a common correlation ρ_b of the ε_{ij} , η_{ij} , and v_{ij} among boys, a common correlation ρ_g of the ε_{ij} , η_{ij} , and v_{ij} among girls, and a common correlation ρ_{bg} of the ε_{ij} , η_{ij} , and v_{ij} across boys and girls.⁸ Test statistics reveal that in our data these interdependencies are statistically important.

3.3. *Maximum Likelihood Estimation.* The estimation problem described above is one of generalized least squares with an unknown error covariance matrix $\Sigma = \sigma^2 \Omega$.⁹ If Ω contains a sufficiently small number of parameters θ such that $\Omega = \Omega(\theta)$, then this is feasible generalized least squares. The reduced form model described by Equations (17) through (19) can be estimated by maximum likelihood. If the disturbances are distributed as multivariate normal, the log-likelihood for a household is

$$(27) \quad \ln L = -\frac{n}{2} \ln(2\pi) - \frac{1}{2} \ln |\sigma^2 \Omega(\theta)| - \frac{1}{2} \zeta' (\sigma^2 \Omega(\theta))^{-1} \zeta$$

⁸ Attempts to further generalize the correlations of these child-specific health errors resulted in our inability to numerically identify the model. Since the variances of the child-specific health errors are allowed to differ across health behaviors and gender, the pattern of cross-child effects (covariances) is reasonably general.

⁹ The parameter σ^2 is not a new unknown parameter since the unknown matrix Ω can be scaled arbitrarily. Its introduction is in keeping with the common nomenclature of the least squares literature.

where ζ represents the stacked vector of residuals of Equations (16) through (19). In the two-brother example, $n = 7$ because there are six child-specific behavioral equations: three health behaviors for each of two brothers plus the household-specific behavior (credit program participation). All of the estimates reported below were obtained by the method of maximum likelihood, which estimates the covariance parameters θ simultaneously with the regression parameters β .¹⁰

Whereas the likelihood given by (27) illustrates the general principle and method used, the actual likelihoods maximized have been altered to take into account six other features of the data. The first is that there are two (gender-specific) credit variables, not one as in the example above, and both are limited dependent variables. There is a substantial mass at zero for both men's and women's program credit. The likelihood (27) has been appropriately altered to treat the reduced form demands for credit as Tobits.

Second, the sample design is choice-based (see Section 4). In particular, program participants and target nonparticipating households are purposely oversampled. The weighted exogenous sampling maximum likelihood (WESML) methods of Manski and Lerman (1977) were grafted onto the maximum likelihood method described above in the estimation of both parameters and the parameter covariance matrix. The weights used in the WESML estimation, in combination with area-level fixed effects, correct for all the endogenous stratification built into the sample design.

Third, the model estimated allows for the conditional demand equations (13)–(15) to vary between boys and girls. Not only are the regression parameters allowed to vary freely, but the parameters that describe the residual variance-covariance matrix are allowed to vary. In models of this sort, distinguishing among more member-types within the household, while adding many new parameters to be estimated, can in fact aid in identification as long as the same latent factor structure underlies the behavior of these member types.

Fourth, we use data from both survey rounds in the estimation. Doing so aids in identification but requires that we add a time-varying error component to the model to be estimated. Thus, the child-specific error associated with each health outcome is allowed to be imperfectly correlated across rounds.

Fifth, the number of boy and girl children in each household varies across households, resulting in an “unbalanced” design. The likelihood is tailored to include all sampled children and not just a fixed number per household.

Finally, in line with our earlier work, we do not assume that the placement of these group-based credit programs across the villages of Bangladesh is exogenous. Program officials note that they often place programs in poorer and more flood prone areas, as well as in areas in which villagers have requested program services. Treating the timing and placement of programs as random can lead to serious mismeasurement of program effectiveness (Pitt et al., 1993). Consider the implications of a program allocation rule that is more likely to place credit programs in poorer villages than in richer ones. Comparison of the two sets of

¹⁰ Estimation could also be accomplished with the generalized method of moments (GMM) using the methods described by Hansen (1982). The moment conditions, given by Equation (25), define $\Omega(\theta)$, the relationship between elements of $\sigma^2\Omega$ and the parameters θ .

villages as in a treatment/control framework would lead to a downward bias in the estimated effect of the program on household income and wealth (and other outcomes associated with income and wealth) and could even erroneously suggest that credit programs reduce income and wealth if the positive effect of the credit program on the difference between “treatment” and “control” villages did not exceed the negative village effect that induced the nonrandom placement. Village fixed-effects estimation, which treats the village-specific component of the error as a parameter to be estimated, eliminates the endogeneity caused by unmeasured village attributes including nonrandom program placement.¹¹

4. SURVEY DESIGN AND DESCRIPTION OF THE DATA

A multipurpose quasi-experimental household survey was conducted in 87 villages of 29 thanas (subdistricts) in rural Bangladesh during the year of 1991–1992. Eight thanas per program were randomly selected from the set of thanas served by that program, and five thanas were randomly selected from the list of nonprogram thanas. There are 391 thanas in Bangladesh.

Three villages in each program thana were randomly selected from a list of villages, supplied by the program’s local office, 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 the Government of Bangladesh. A household census was conducted in each village to classify households as target (i.e., those that 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 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.¹²

Indicators of each individual’s anthropometric status—height, weight, and arm circumference—were collected from 15 of these villages. Collecting such information at the individual level is costly and time consuming; therefore, the study was limited to a subsample of the original 1,798 households. The anthropometric data were collected from three villages in five program thanas chosen at random. Two of the five thanas (Patuakhali and Shakhipur) contain a Grameen Bank, two (Rangpur Sadar and Habgjanj Sadar) contain a BRAC program, and one (Matbaria) contains a BRDB program. Households were randomly selected to be anthropometrically measured within each of the 15 villages based

¹¹ One important drawback of estimating program impacts from this approach is the possible misinterpretation of the village fixed effects. It is possible that credit programs can alter village attitudes and other village characteristics, perhaps through demonstration or spillover effects, and thus the attitudes of those who do not participate in the credit programs as well as those who do. These spillover effects are captured by the fixed effects and not appropriately credited as a result of the program. It is generally not possible to estimate the village externality from a single cross section of data. This issue is discussed in more detail in Pitt and Khandker (1998).

¹² Note that endogenous stratification by thana does not require weighting in the estimation as long as the model is estimated with thana-level fixed effects. WESML estimation is still required to correct for endogenous sampling within thanas.

on a village census and the sample selection rules detailed in Khandker et al. (1995), altered to increase the sample size to approximately 20 households per village instead of 17 in all other sampled villages. Although the original household survey design called for anthropometric measures of individuals residing in nontarget households, the final survey included only those residing in target households. This precluded the use of the quasi-experimental survey design used to identify program effects in Pitt and Khandker (1998) and Pitt et al. (2000).

The rural economy in Bangladesh revolves around agricultural activity. Both economic welfare and food consumption are highest after the major rice crop, *aman*, is harvested and are at their lowest just prior to the harvest. To avoid confounding seasonal effects with actual credit effects, anthropometric measures were collected twice; during the peak consumption period (January to April 1992) and during the lean period (September to November 1992).

The measure of credit used below is the natural logarithm of the amount borrowed separately by male and female program participants from any of these three group-based credit programs (in constant taka) plus one. The measures of nutritional status for children under the age of 15 are the natural logarithms of arm circumference (in centimeters), body mass index (weight in kilograms divided by height in centimeters squared), and height (in centimeters) for age (in years). All regression equations also include a number of exogenous characteristics of households—the age, gender, and sex of the head of the household, the amount of land owned, and the highest education level achieved by any male and any female household resident—as well the age and age squared of individual children. Table 1 presents the weighted means and standard deviations of all these variables.

As noted above, eight equations are jointly estimated—six equations for the anthropometric status of boys and girls, and two credit equations for males and females. There are 65 parameters associated with exogenous variables (see Table 1) to be estimated, in addition to 21 factor loadings, and 12 structural parameters (credit effects). Introducing thana fixed effects adds 40 additional parameters, whereas introducing village fixed effects adds an extra 120 parameters. Finally, as discussed in Section 3.2, the child-specific interdependencies that allow the idiosyncratic components of each error term to be correlated within and across children (in the same time period) adds three additional parameters. There are a total of 141 parameters jointly estimated in the thana fixed-effects model and 221 with village fixed effects.

How many covariance restrictions are required to estimate these models and how many are actually made? The answer depends crucially on the demographic characteristics of the sample. The best-identified case is for households with two or more boys and two or more girls surveyed in both rounds. Having more than two children of any gender does not add to identification since the error covariance between brother A and brother B is not different from the error covariance between brother A and brother C or between brother B and brother C. Our data represent an unbalanced random effects design since households have differing numbers of sons and daughters, and in some cases anthropometric measurements were not taken in both rounds.

TABLE 1
SUMMARY STATISTICS

| Variables | Observations | Mean | Standard Deviation |
|---|--------------|----------|--------------------|
| Value of program borrowing by females (Taka) | 105 | 9675.951 | 9712.942 |
| Value of program borrowing by males (Taka) | 76 | 9586.895 | 10182.8 |
| Girls' weight (in kg, ages 0–14) | 401 | 12.398 | 4.379 |
| Participants | 260 | 13.239 | 4.350 |
| Nonparticipants | 141 | 11.713 | 4.294 |
| Boys' weight (in kg, ages 0–14) | 375 | 12.776 | 4.163 |
| Participants | 286 | 12.960 | 4.202 |
| Nonparticipants | 89 | 12.510 | 4.113 |
| Girls' height (in cm, ages 0–14) | 401 | 94.239 | 18.145 |
| Participants | 260 | 97.899 | 17.361 |
| Nonparticipants | 141 | 91.261 | 18.275 |
| Boys' height (in cm, ages 0–14) | 375 | 95.09 | 16.587 |
| Participants | 286 | 95.701 | 16.598 |
| Nonparticipants | 89 | 94.204 | 16.612 |
| Girls' arm circumference (in cm, ages 0–14) | 401 | 14.138 | 1.511 |
| Participants | 260 | 14.417 | 1.556 |
| Nonparticipants | 141 | 13.911 | 1.439 |
| Boys' arm circumference (in cm, ages 0–14) | 375 | 14.204 | 1.265 |
| Participants | 286 | 14.222 | 1.257 |
| Nonparticipants | 89 | 14.178 | 1.283 |
| Girls' body mass index (ages 0–14) (grams/cm ²) | 401 | 1.358 | 0.146 |
| Participants | 260 | 1.358 | 0.150 |
| Nonparticipants | 141 | 1.357 | 0.143 |
| Boys' body mass index (ages 0–14) (grams/cm ²) | 375 | 1.383 | 0.128 |
| Participants | 286 | 1.383 | 0.127 |
| Nonparticipants | 89 | 1.382 | 0.132 |
| Girls' height-for-age (ages 0–14)(cm/yr) | 401 | 31.545 | 21.593 |
| Participants | 260 | 27.424 | 18.532 |
| Nonparticipants | 141 | 34.898 | 23.323 |
| Boys' height-for-age (ages 0–14)(cm/yr) | 375 | 30.768 | 19.041 |
| Participants | 286 | 30.632 | 19.588 |
| Nonparticipants | 89 | 30.967 | 18.216 |
| Girls' age (in years) | 401 | 4.517 | 2.902 |
| Boys' age (in years) | 375 | 4.385 | 2.745 |
| Education by head of household (in years) | 233 | 2.048 | 3.154 |
| Age of head of household (in years) | 233 | 38.975 | 10.391 |
| Sex of head of household (1 = male) | 233 | 0.949 | 0.22 |
| Maximum amount of education by any female in household (in years) | 233 | 1.49 | 2.712 |
| Maximum amount of education by any male in household (in years) | 233 | 2.671 | 3.408 |
| Land owned by household (in hundredths of an acre) | 233 | 53.637 | 552.184 |

Table 2 presents a tabulation of our sample by number of boy and girl children less than 15 years of age. Only two household observations had the maximum possible identification—two or more boys and two or more girls. However, many household observations had at least two children of one gender. Table 3 presents

TABLE 2
 SAMPLE SIZE BY HOUSEHOLD TYPE (TOTAL OF BOTH PERIODS)

| Number of Boys | Number of Girls | | |
|----------------|-----------------|----|-----------|
| | 0 | 1 | 2 or more |
| 0 | 34 | 73 | 63 |
| 1 | 101 | 93 | 35 |
| 2 or more | 50 | 15 | 2 |

TABLE 3
 COVARIANCE RESTRICTIONS BY HOUSEHOLD TYPE ASSUMING TWO ENDOGENOUS REGRESSORS
 AND TWO TIME PERIODS

| Number of Boys | Number of Girls | | | | | |
|----------------|-----------------------------------|--|-----------------------------------|--|-----------------------------------|--|
| | 0 | | 1 | | 2 | |
| | Unique Terms in Covariance Matrix | Parameters to be Identified by Covariance Matrix | Unique Terms in Covariance Matrix | Parameters to be Identified by Covariance Matrix | Unique Terms in Covariance Matrix | Parameters to be Identified by Covariance Matrix |
| 0 | – | – | 21 | 19 | 27 | 19 |
| 1 | 21 | 19 | 48 | 36 | 54 | 36 |
| 2 | 27 | 19 | 54 | 36 | 60 | 36 |

the number of covariance restrictions imposed as well as the number of parameters that they identify, tabulated for households by demographic composition, under the assumption that two rounds of data are available. With the covariance restrictions associated with the factor structure of the residuals as described in Section 3 and the appendix, the number of parameters we need to identify is smaller than the number of covariance matrix elements in every case. Since the difference between the two numbers is the number of overidentifying restrictions, our model is overidentified in every case.

5. RESULTS

In this section we present and interpret the parameters of conditional demand equations of the form given by Equations (13) through (15) for three child-specific outcomes: the natural logarithms of arm circumference, body mass index (BMI), and height-for-age. In addition to estimates based upon the covariance restrictions described in Section 3, we present alternative estimates that do not fully treat credit program placement and participation as endogenous. These alternative estimates are presented to illustrate the importance of heterogeneity bias. The alternative methods of estimation ignore self-selection into credit programs, treating it as exogenous, or treat village program placement as random, and thus do

TABLE 4
ALTERNATIVE ESTIMATES OF THE IMPACT OF CREDIT ON BOYS' AND GIRLS' ARM CIRCUMFERENCE

| Variable | Exogenous Credit No FE | | Exogenous Credit Thana FE | | Endogenous Credit No FE | | Endogenous Credit Thana FE | |
|------------------------|---------------------------|---------------------|------------------------------|-------------------|----------------------------|---------------------|-------------------------------|---------------------|
| | Boy | Girl | Boy | Girl | Boy | Girl | Boy | Girl |
| Male credit | 0.0024 (0.031) | 0.1897 (1.805) | 0.0760 (0.493) | 0.3861 (2.013) | -0.1753 (-1.530) | 0.1013 (0.819) | -0.1938 (-0.935) | 0.3015 (1.431) |
| Female credit | -0.1507 (-0.468) | -0.0776 (-0.289) | -0.0573 (-0.364) | 0.0299 (0.191) | 0.4940 (2.241) | 0.4963 (2.251) | 0.5634 (2.160) | 0.6250 (2.730) |
| λ_{arm} | | | | | -4.9564 (-3.098) | -5.5933 (-3.670) | -4.5209 (-2.459) | -5.4036 (-3.783) |
| ρ (m, arm) | | | | | 0.2724 (1.266) | 0.2858 (1.892) | 0.2144 (0.851) | 0.2404 (1.248) |
| ρ (f, arm) | | | | | -0.3544 (-2.420) | -0.3718 (-2.245) | -0.3613 (-2.300) | -0.4052 (-2.890) |

NOTE: Asymptotic *t*-ratios in parentheses.

not include fixed effects, or do both.¹³ In all cases, eight equations are jointly estimated by maximum likelihood—the reduced form determinants of male and female program credit borrowing and the conditional demands for each of the three health indicators for boys and girls separately. In limiting cases of exogeneity (see below), this is equivalent to maximum likelihood (unrestricted) random effects least squares estimation of the conditional demand equations with two random effect components, one reflecting the nonindependence of errors across survey rounds, and the other nonindependence across behaviors within the household.

Tables 4 through 6 present the parameter estimates of interest. The λ 's in the third row are factor loadings for the household-specific error μ_i that generates correlation between program credit and the anthropometric outcomes.¹⁴ A sufficient condition for credit to be exogenous in the determination of child health is that all the factor loadings on μ_i , the λ parameters, are zero. Formally, a test

¹³ Hausman-like tests of the consistency of the models without location fixed effects were attempted, but the covariance matrices of the differences in the parameter vectors were not positive definite in every case tried. This problem is not uncommon in estimation problems of this kind. The test statistic computed is $(\hat{\beta}_{\text{FE}} - \hat{\beta})(\hat{\Sigma}_{\text{FE}} - \hat{\Sigma})^{-1}(\hat{\beta}_{\text{FE}} - \hat{\beta})$ where β_{FE} and β (Σ_{FE} and Σ) refer to the fixed effects and no fixed effects parameter vectors (covariance matrices) respectively. Typically, the problem is that one or more of the diagonal elements of the covariance matrix ($\Sigma_{\text{FE}} - \Sigma$) are very close to zero, and sometimes negative. Pitt and Khandker (1998) provide further statistical evidence of the bias imparted by treating village effects as random with these data. They regress village fixed effects on the regressors of the credit demand equations and find that fixed effects from the credit demand equations are significantly (at the 0.05 level) correlated with these regressors.

¹⁴ These tables present only a subset of the parameters estimated. Factor loadings representing other sources of error correlation, as well as parameters on a set of included exogenous variables—age, gender, and sex of the head of the household, the amount of land owned by the household, and the highest education level achieved by any male and any female household resident, as well as the ages of individual children—are not shown but are available from the authors.

TABLE 5
ALTERNATIVE ESTIMATES OF THE IMPACT OF CREDIT ON BOYS' AND GIRLS' BODY MASS INDEX

| Variable | Exogenous Credit No FE | | Exogenous Credit Thana FE | | Endogenous Credit No FE | | Endogenous Credit Thana FE | |
|------------------------|---------------------------|---------------------|------------------------------|---------------------|----------------------------|---------------------|-------------------------------|---------------------|
| | Boy | Girl | Boy | Girl | Boy | Girl | Boy | Girl |
| Male credit | -0.0246 (-0.217) | 0.1555 (1.709) | -0.1219 (-0.449) | 0.2952 (1.809) | -0.0178 (-0.176) | 0.1596 (1.672) | -0.0932 (-0.406) | 0.2891 (1.726) |
| Female credit | -0.1063 (-0.379) | -0.2076 (-0.620) | -0.1500 (-0.896) | -0.1113 (-0.679) | -0.0494 (-0.189) | -0.1708 (-0.873) | -0.1492 (-0.443) | -0.1387 (-0.648) |
| λ_{BMI} | | | | | -0.0676 (-0.030) | 0.2408 (0.170) | 0.0785 (0.034) | -0.0524 (-0.039) |
| ρ (m, bmi) | | | | | 0.0033 (0.023) | -0.0122 (-0.122) | -0.0033 (-0.021) | 0.0024 (0.020) |
| ρ (f, bmi) | | | | | -0.0043 (-0.028) | 0.0159 (0.143) | 0.0055 (0.031) | -0.0040 (-0.029) |

NOTE: Asymptotic t -ratios in parentheses.

TABLE 6
ALTERNATIVE ESTIMATES OF THE IMPACT OF CREDIT ON BOYS' AND GIRLS' HEIGHT-FOR-AGE

| Variable | Exogenous Credit No FE | | Exogenous Credit Thana FE | | Endogenous Credit No FE | | Endogenous Credit Thana FE | |
|---------------------------|---------------------------|---------------------|------------------------------|---------------------|----------------------------|----------------------|-------------------------------|----------------------|
| | Boy | Girl | Boy | Girl | Boy | Girl | Boy | Girl |
| Male credit | 0.0056 (0.042) | -0.1766 (-1.025) | 0.1927 (0.682) | -0.3860 (-1.272) | -0.3332 (-1.821) | -0.3182 (-1.728) | -0.3228 (-1.039) | -0.4788 (-1.406) |
| Female credit | 0.1036 (0.244) | -0.2074 (-0.585) | 0.1981 (0.785) | 0.0361 (0.166) | 1.3508 (3.105) | 0.8576 (1.919) | 1.5303 (3.766) | 1.1414 (2.376) |
| λ_{height} | | | | | -10.5310 (-4.702) | -9.6490 (-4.249) | -9.8195 (-4.661) | -9.1711 (-3.625) |
| ρ (m, height) | | | | | 0.3189 (7.382) | 0.2732 (8.057) | 0.2592 (4.071) | 0.2244 (4.761) |
| ρ (f, height) | | | | | -0.4149 (-10.848) | -0.3555 (-11.161) | -0.4368 (-13.311) | -0.3782 (-12.054) |

NOTE: Asymptotic t -ratios in parentheses.

of exogeneity requires that the correlation coefficients (ρ) between the errors of the credit reduced-form equation and the health behavior equation are zero. Estimates of the ρ 's are presented in the last row of Tables 4 through 6, as well as approximate t -statistics constructed by the delta method.

The final set of estimates in Tables 4 through 6 treats both credit program participation and program placement as endogenous. While these estimates use thana fixed effects, the model was also estimated using village fixed effects (results not reported). There are three villages in each of the five thanas in the sample. Inspection of the results suggests that there is not much difference between thana and village fixed effects, so we only report and discuss the more efficient

TABLE 7
JOINT TESTS OF THE FACTOR LOADINGS ON THE COMMON FACTOR IN THE DETERMINATION
OF CHILDREN'S HEALTH

| Anthropometric Measure | Boys | Girls | Boys and Girls |
|-----------------------------|----------------------|----------------------|----------------------|
| Arm circumference | 6.05 ($p = 0.01$) | 14.31 ($p = 0.00$) | 20.70 ($p = 0.00$) |
| BMI | 0.00 ($p = 0.97$) | 0.00 ($p = 0.97$) | 0.01 ($p = 1.00$) |
| Height-for-age | 21.73 ($p = 0.00$) | 13.14 ($p = 0.00$) | 53.88 ($p = 0.00$) |
| All anthropometric measures | 23.49 ($p = 0.00$) | 28.13 ($p = 0.00$) | 77.23 ($p = 0.00$) |

thana fixed-effects estimates.¹⁵ Finally, we also estimated the model allowing for possible differential effects across the three credit programs (BRAC, BRDB, and Grameen Bank), but could not reject the null hypothesis that the three programs have the same impact on children's health.¹⁶

The preferred model with endogenous credit and thana fixed effects rejects the restriction that all the λ factor loadings are jointly zero.¹⁷ Table 7 presents joint tests on the statistical significance of these factor loadings. A joint test that the unobserved determinants of credit program participation by both sexes, such as heterogeneous gender preferences, are not also determinants of all three anthropometric measures for both sexes of children is strongly rejected ($\chi^2(6) = 77.23$, $p = 0.00$). For the individual nutritional status measures, zero factor-loadings are rejected for arm circumference ($\chi^2(2) = 20.70$, $p = 0.00$) and height-for-age ($\chi^2(2) = 53.88$, $p = 0.00$), but not for BMI ($\chi^2(2) = 0.01$, $p = 1.00$). Moreover, the factor-loadings are statistically different from zero for the three measures of boys' health ($\chi^2(3) = 23.49$, $p = 0.00$) and girls' health ($\chi^2(3) = 28.13$, $p = 0.00$).

Table 8 presents joint tests of the exogeneity of male and female program credit on the three health outcomes of boys and girls. Credit program participation is endogenous in the determination of child health. The bottom row of the table provides test statistics for the null hypothesis that the correlation coefficients ρ between the errors of all three measures of health and the determinants of women's and men's credit program participation are jointly zero. In all cases the null hypothesis is firmly rejected. The last column of the table presents test statistics

¹⁵ A Hausman test of village fixed effects versus thana fixed effects could not be performed because the covariance matrix for the parameter differences was not positive definite, as before.

¹⁶ This result is important for the interpretation of results. The proportion of borrowers by gender differs greatly across programs (Pitt and Khandker, 1998) so that differences in the effect of credit on health could be confounded by differences in the effect of programs (BRDB, BRAC, and Grameen Bank).

¹⁷ We also reject the null hypothesis that there are no child interdependencies, that is, that the ε_{ij} , η_{ij} , and v_{ij} child-specific health errors are uncorrelated, as noted in Section 3.2 The correlation coefficients among boys ($\rho_b = 0.099$, $t = 2.56$), among girls ($\rho_g = -0.044$, $t = -1.72$), and between boys and girls ($\rho_{bg} = -0.100$, $t = -1.95$) have a Wald test of joint significance of $\chi^2(3) = 20.44$ ($p = 0.00$). One cannot draw conclusions about the magnitude of compensatory behavior by households (if any) from these correlations as they are *not* the sign of the derivative of one child's health with respect to an exogenous change in a sibling's health, all else unchanged.

TABLE 8
TESTS FOR THE JOINT EXOGENEITY OF CREDIT IN THE DETERMINATION OF CHILDREN'S HEALTH

| Anthropometric Measure | Boys | | | Girls | | | Boys and Girls | | |
|-----------------------------|-----------------|------------------|------------------|-----------------|------------------|------------------|------------------|------------------|------------------|
| | Male | Female | Both | Male | Female | Both | Male | Female | Both |
| Arm circumference | 0.72 (0.39) | 5.29 (0.02) | 7.05 (0.03) | 1.56 (0.21) | 8.35 (0.00) | 8.40 (0.01) | 1.57 (0.46) | 15.70 (0.00) | 30.98 (0.00) |
| BMI | 0.00 (0.98) | 0.00 (0.98) | 0.55 (0.76) | 0.00 (0.98) | 0.00 (0.98) | 0.43 (0.81) | 0.00 (1.00) | 0.00 (1.00) | 2.02 (0.73) |
| Height-for-age | 16.58 (0.00) | 177.21 (0.00) | 200.97 (0.00) | 22.66 (0.00) | 145.27 (0.00) | 178.01 (0.00) | 23.16 (0.00) | 228.00 (0.00) | 339.87 (0.00) |
| All anthropometric measures | 66.91 (0.00) | 223.68 (0.00) | 341.57 (0.00) | 84.23 (0.00) | 238.50 (0.00) | 351.69 (0.00) | 111.65 (0.00) | 330.74 (0.00) | 593.15 (0.00) |

NOTE: Chi-squared *p*-values in parentheses.

for each individual health measure. Only in the case of BMI can we not reject the null hypothesis of exogeneity.

Table 4 presents estimates of the impact of program credit received by males and females on the arm circumference of boy and girl children separately. The preferred model with endogenous credit and thana fixed effects estimates significant and large positive effects of female credit on girls' arm circumference, and somewhat smaller positive effects of female credit on boys' arm circumference and of male credit on girls' arm circumference. Joint tests presented in Table 9 reject the null hypothesis that female credit has no effect on arm circumference at the 0.05 level ($\chi^2(2) = 10.48, p = 0.01$) but cannot reject the same null hypothesis for male credit ($\chi^2(2) = 3.96, p = 0.14$). As the anthropometric measures and credit are both measured in log form, the parameters can be interpreted as elasticities with respect to latent credit. Thus, a 10% increase in (latent) credit provided to females increases the arm circumference of their daughters by 6.3% ($t = 2.73$), twice the increase that would be expected from a similar proportionate increase in the credit provided to men. Female credit also has a positive but somewhat smaller effect on the arm circumference of sons, although this parameter is not precisely estimated. At the means, a 10% increase in female credit increases the arm circumference of girls and boys by 0.45 cm and 0.39 cm, respectively. The

TABLE 9
WALD TESTS FOR THE JOINT SIGNIFICANCE OF CREDIT ON CHILDREN'S HEALTH BY GENDER OF PARTICIPANT

| Anthropometric Measure | Men | Women | Men and Women |
|---|----------------------|----------------------|----------------------|
| Arm circumference | 3.96 ($p = 0.14$) | 10.48 ($p = 0.01$) | 12.94 ($p = 0.01$) |
| BMI | 3.01 ($p = 0.22$) | 0.42 ($p = 0.81$) | 4.05 ($p = 0.40$) |
| Height-for-age | 2.31 ($p = 0.32$) | 17.29 ($p = 0.00$) | 20.42 ($p = 0.00$) |
| All anthropometric measures | 11.07 ($p = 0.09$) | 18.67 ($p = 0.00$) | 44.12 ($p = 0.00$) |
| Equality of credit effects on all anthropometric measures | 20.96 ($p = 0.00$) | | |

same percentage increase in male credit increases arm circumference for girls by 0.21 cm and *reduces* that of boys by 0.14 cm.

The first two columns of Table 4 provide estimates for a model that imposes exogenous self-selection of households into the credit program and random (exogenous) placement of credit programs across villages. It is equivalent to a random effects regression of the arm circumference of children by sex on a set of household and individual attributes (see Table 1) and the borrowing of male and female adults in the household. Among the four credit effects, only one, the effect of credit received by men on the arm circumference of girls, seems to have a significant (and positive) effect. This model seriously underestimates the effect of female credit on girls' and boys' arm circumference, and overestimates the effect of male credit on boys' arm circumference. The next two columns of the table present estimates with exogenous credit and thana fixed effects. These again are essentially linear random-effect estimates. Here, only the effect of men's credit on girls' arm circumference is significant. Treating credit as endogenous but program placement as exogenous yields fairly large effects for female credit on boys' and girls' arm circumference, but now male credit effects are much smaller and even negative in the case of boys' arm circumference.

Table 5 presents parameter estimates of the effects of program credit on log BMI. This is the one measure of health status for which the exogeneity of credit hypothesis cannot be rejected. The various alternative estimators provide roughly similar estimates of program effects. The only parameter close to significance is the (positive) effect of male credit on girls' BMI ($t = 1.73$) with an elasticity of 0.289.

Table 6 presents parameter estimates of the effects of program credit on log height-for-age. As in the case of arm circumference, the exogeneity of credit is strongly rejected in this case (see Table 8 for a joint test). Female credit is estimated to have large, positive, and statistically significant effects on the height-for-age of both boys and girls. The relevant elasticities are 1.53 ($t = 3.77$) for boys and 1.14 ($t = 2.38$) for girls. A 10% increase in female credit increases the height of girls and boys by 0.36 and 0.50 centimeters per year, respectively, at the mean. Male credit effects have negative point estimates, although neither is significantly different from zero. A 10% increase in male credit *reduces* the height of girls and boys by 0.16 and 0.11 centimeters per year, respectively, at the mean. A joint test (Table 9) cannot reject the null hypothesis of no effect of male credit on height-for-age ($\chi^2(2) = 2.31$, $p = 0.32$), but strongly rejects the null for female credit ($\chi^2(2) = 17.29$, $p = 0.00$).

Table 10 summarizes a set of statistical tests on the effect of program credit on the nutritional status of children by sex of children, and for all children. The Wald test statistics presented in the last column of Table 10 demonstrate that program credit is a significant (at the 0.05 level) determinant of arm circumference, height-for-age, and the full set of anthropometric measures taken together, but not of BMI. Program credit is a significant (and always positive) determinant of girls' arm circumference and height-for-age, but does not have a statistically significant effect on boys' health status except height-for-age. The test statistics of Table 9

TABLE 10

WALD TESTS FOR THE JOINT SIGNIFICANCE OF CREDIT ON CHILDREN'S HEALTH BY GENDER OF CHILD

| Anthropometric Measure | Boys | Girls | Boys and Girls |
|-----------------------------|----------------------|----------------------|----------------------|
| Arm circumference | 24.86 ($p = 0.09$) | 9.25 ($p = 0.01$) | 12.94 ($p = 0.01$) |
| BMI | 0.30 ($p = 0.86$) | 3.71 ($p = 0.16$) | 4.05 ($p = 0.40$) |
| Height-for-age | 15.45 ($p = 0.00$) | 8.25 ($p = 0.02$) | 20.42 ($p = 0.00$) |
| All anthropometric measures | 16.68 ($p = 0.01$) | 18.67 ($p = 0.00$) | 44.12 ($p = 0.00$) |

demonstrate that only credit provided to women significantly affects the health of children ($\chi^2(3) = 18.67, p = 0.00$). Men's credit is not a significant determinant of any of the three anthropometric measures or of the three taken together ($\chi^2(3) = 11.07, p = 0.09$). It is thus not surprising that the null hypothesis of equal credit effects by gender of participant is decisively rejected in Table 9 ($\chi^2(3) = 20.96, p = 0.00$). The bottom line is that there is strong evidence that only women's participation in group-based credit programs, as measured by total borrowing, has a large and important positive effect on the health status of both boys and girls.

6. SUMMARY

This article examines the effect of additional resources supplied to and controlled by women, as compared to men, on child health outcomes, by gender of child. The source of these additional resources is group-based credit programs for the poor in rural Bangladesh. These credit programs are well suited to studying how gender-specific resources alter intra-household allocations because they induce differential participation by gender through the requirement that only one adult member per household can participate in any micro-credit program. The data come from a 1991/92 survey, which includes a special health status module containing anthropometric measures for children under the age of 15 years in 15 villages. In the health module, only villages with a credit program and only households that were eligible to participate in the programs were sampled, making identification via the quasi-experimental survey design used in previous work impossible.

Lacking exclusion restrictions of the usual sort required for instrumental variables estimates, we identify the effects of credit program participation by gender of participant by placing restrictions on the covariance structure of the regression errors. In addition, we have data on health status at two points in time for each sampled household, which adds additional identifying restrictions to the residual covariance matrix. The idea is to place a factor-analytic structure on the residuals of a set of equations for female and male credit program participation and a set of health outcomes, which in our study are arm circumference, body mass index, and height-for-age. In so doing, we are assuming that there is a latent unobserved factor that influences both credit program participation and three measures of child health, and that this factor is the sole source of the correlation of credit equation errors with the child health errors. In the estimation of the determinants

of these outcomes, the residual includes left-out household-specific variables such as relative bargaining power in the household, preference heterogeneity, and innate healthiness. These omitted variables may affect the health (and other) resources allocated to boy and girl children, but not necessarily in the same way. The error structure permits interdependencies in health outcomes across siblings to arise from other sources, such as compensatory resource allocations, besides household-specific factors.

We estimate eight equations simultaneously—three health outcomes separately for boys and girls plus credit program participation for men and women—by maximum likelihood. Our results are striking. After taking into account the endogeneity of individual participation in these credit programs and the placement of these credit programs across areas, we find that women’s credit has a large and statistically significant impact on two of three measures of the health of both boy and girl children. Credit provided to men has no statistically significant impact and we are able to reject the null hypothesis of equal credit effects by gender of participant. A 10% increase in (latent) credit provided to females increases the arm circumference of their daughters by 6.3%, twice the increase that would be expected from a similar proportionate increase in credit provided to men. Female credit also has a significant, positive, but somewhat smaller effect on the arm circumference of sons. Female credit is estimated to have large, positive, and statistically significant effects on the height-for-age of both boys and girls. The relevant elasticities are 1.53 for boys and 1.14 for girls. Male credit effects have negative point estimates, although neither is significantly different from zero. We find no significant effects of female or male credit on the logarithm of the body mass index (BMI) of boys or girls. Taken together, this is persuasive further evidence that these credit programs have important effects on household well-being, particularly if the program participant is a woman.

APPENDIX

The general model contains k_1 outcome equations for two types of people—boys and girls—and are functions of k_2 endogenous variables; the model can easily be extended to incorporate more than two types of people. Explicitly, the model for a single cross section is

$$\begin{aligned} H_{bih}^s &= X_{ih}\alpha_b^s + Z_h\gamma_b^s + \sum_{r=1}^{k_2} \delta_{br}^s y_h^r + \lambda_b^s \mu_h + \xi_b^s \epsilon_{bih}^s \\ H_{gih}^s &= X_{ih}\alpha_g^s + Z_h\gamma_g^s + \sum_{r=1}^{k_2} \delta_{gr}^s y_h^r + \lambda_g^s \mu_h + \xi_g^s \epsilon_{gih}^s \\ y_h^r &= Z_h\gamma^r + \lambda^r \mu_h + \theta^r \omega_h^r \end{aligned}$$

where i indexes individuals; h indexes households; b and g represent boy and girl; H^s , $s = 1, \dots, k_1$, are the outcomes; X is a matrix of individual characteristics; Z is a matrix of household characteristics; y^r , $r = 1, \dots, k_2$, are the endogenous variables; μ is a household random effect; ϵ , ω are individual shocks; and all variances are normalized to one.

Substituting for y_h^r and collecting terms yields the following reduced form outcome equations, where $k = b, g$ indexes the equations for boys and girls,

$$H_{kih}^s = X_{ih}\alpha_k^s + Z_h[\gamma_k^s + \sum_{r=1}^{k_2} \delta_{kr}^s \gamma^r] \\ + [(\lambda_k^s + \sum_{r=1}^{k_2} \delta_{kr}^s \lambda^r) \mu_h + \sum_{r=1}^{k_2} \delta_{kr}^s \theta^r \omega_h^r + \xi_k^s \epsilon_{kih}^s]$$

In more concise notation,

$$H_{kih}^s = X_{ih}\alpha_k^s + Z_h\Psi_k^s + [\Gamma_k^s \mu_h + \sum_{r=1}^{k_2} \delta_{kr}^s \theta^r \omega_h^r + \xi_k^s \epsilon_{ihk}^s] \\ = X_{ih}\alpha_k^s + Z_h\Psi_k^s + \varphi_{ihk}^s$$

where we adopt the normalization $\theta^r = \theta$.

Prior to obtaining the covariance matrix for a household, several assumptions are made:

- $E[\mu\epsilon] = E[\mu\omega] = 0$
- $E[\epsilon_{kih}^s \epsilon_{ki'h}^{s'}] = \begin{cases} 1 & \text{if } i = i', s = s' \\ \rho_b & \text{if } i = i', s \neq s', k = b \\ \rho_g & \text{if } i = i', s \neq s', k = g \\ \rho_b & \text{if } i \neq i', k = b \quad \forall s, s' \\ \rho_g & \text{if } i \neq i', k = g \quad \forall s, s' \end{cases}$
- $E[\epsilon_{bih}^s \epsilon_{gi'h}^{s'}] = \rho_{bg} \quad \forall s, s'$

In addition, the following normalizations are required:

- $E[\epsilon_{kih}^s \epsilon_{ki'h}^{s'}] = 1 \quad \text{if } i = i', s = s'$
- $E[\mu_h \mu_h] = 1$
- $E[\omega_h^r \omega_h^{r'}] = \begin{cases} 1 & \text{if } r = r' \\ 0 & \text{otherwise} \end{cases}$

as the variances of ϵ , μ , and ω are given by ξ , λ , and θ , respectively.

Each household covariance matrix consists of five submatrices: covariances between a given boy's equations, covariances between a given girl's equations, covariances between a pair of brothers, covariances between a pair of sisters, and covariances between any opposite-sex sibling pair.

Under the various assumptions and normalizations, the submatrices take the following form, where the four equations correspond to outcomes s and s' and endogenous variables r and r' :

- Covariances for a given *individual*

$$\left[\begin{array}{cc} (\Gamma_k^s)^2 + (\xi_k^s)^2 & \Gamma_k^s \Gamma_k^{s'} + \xi_k^s \xi_k^{s'} \rho_k \\ + \sum_{r=1}^{k_2} (\delta_{kr}^s \theta^r)^2 & + \sum_{r=1}^{k_2} \delta_{kr}^s \delta_{kr}^{s'} (\theta^r)^2 \\ & (\Gamma_k^{s'})^2 + (\xi_k^{s'})^2 \\ & + \sum_{r=1}^{k_2} (\delta_{kr}^{s'} \theta^r)^2 \\ & (\lambda^r)^2 + (\theta^r)^2 \\ & (\lambda^r)^2 + (\theta^r)^2 \end{array} \quad \begin{array}{cc} \lambda^r \Gamma_k^s + \delta_{kr}^s (\theta^r)^2 & \lambda^{r'} \Gamma_k^s + \delta_{kr'}^s (\theta^{r'})^2 \\ \lambda^r \Gamma_k^{s'} + \delta_{kr}^{s'} (\theta^r)^2 & \lambda^{r'} \Gamma_k^{s'} + \delta_{kr'}^{s'} (\theta^{r'})^2 \\ \lambda^r \lambda^{r'} & \\ \lambda^r \lambda^{r'} & \end{array} \right]$$

- Covariances between any pair of *same-sex siblings*

$$\left[\begin{array}{cc} (\Gamma_k^s)^2 + (\xi_k^s)^2 \rho_k & \Gamma_k^s \Gamma_k^{s'} + \xi_k^s \xi_k^{s'} \rho_k \\ + \sum_{r=1}^{k_2} (\delta_{kr}^s \theta^r)^2 & + \sum_{r=1}^{k_2} \delta_{kr}^s \delta_{kr}^{s'} (\theta^r)^2 \\ & (\Gamma_k^{s'})^2 + (\xi_k^{s'})^2 \rho_k \\ & + \sum_{r=1}^{k_2} (\delta_{kr}^{s'} \theta^r)^2 \\ & (\lambda^r)^2 + (\theta^r)^2 \\ & (\lambda^r)^2 + (\theta^r)^2 \end{array} \quad \begin{array}{cc} \lambda^r \Gamma_k^s + \delta_{kr}^s (\theta^r)^2 & \lambda^{r'} \Gamma_k^s + \delta_{kr'}^s (\theta^{r'})^2 \\ \lambda^r \Gamma_k^{s'} + \delta_{kr}^{s'} (\theta^r)^2 & \lambda^{r'} \Gamma_k^{s'} + \delta_{kr'}^{s'} (\theta^{r'})^2 \\ \lambda^r \lambda^{r'} & \\ \lambda^r \lambda^{r'} & \end{array} \right]$$

- Covariances between any pair of *opposite-sex siblings*

$$\left[\begin{array}{cc} \Gamma_b^s \Gamma_g^s + \xi_b^s \xi_g^s \rho_{bg} & \Gamma_b^s \Gamma_g^{s'} + \xi_b^s \xi_g^{s'} \rho_{bg} \\ + \sum_{r=1}^{k_2} \delta_{br}^s \delta_{gr}^s (\theta^r)^2 & + \sum_{r=1}^{k_2} \delta_{br}^s \delta_{gr}^{s'} (\theta^r)^2 \\ \Gamma_g^s \Gamma_b^{s'} + \xi_g^s \xi_b^{s'} \rho_{bg} & \Gamma_b^s \Gamma_g^{s'} + \xi_b^s \xi_g^{s'} \rho_{bg} \\ + \sum_{r=1}^{k_2} \delta_{gr}^s \delta_{br}^{s'} (\theta^r)^2 & + \sum_{r=1}^{k_2} \delta_{br}^s \delta_{gr}^{s'} (\theta^r)^2 \\ \lambda^r \Gamma_g^s + \delta_{gr}^s (\theta^r)^2 & \lambda^r \Gamma_g^{s'} + \delta_{gr}^{s'} (\theta^r)^2 \\ \lambda^r \Gamma_g^s + \delta_{gr}^s (\theta^r)^2 & \lambda^r \Gamma_g^{s'} + \delta_{gr}^{s'} (\theta^r)^2 \end{array} \quad \begin{array}{cc} \lambda^r \Gamma_b^s + \delta_{br}^s (\theta^r)^2 & \lambda^{r'} \Gamma_b^s + \delta_{br'}^s (\theta^{r'})^2 \\ \lambda^r \Gamma_b^{s'} + \delta_{br}^{s'} (\theta^r)^2 & \lambda^{r'} \Gamma_b^{s'} + \delta_{br'}^{s'} (\theta^{r'})^2 \\ (\lambda^r)^2 + (\theta^r)^2 & \lambda^r \lambda^{r'} \\ (\lambda^r)^2 + (\theta^r)^2 & (\lambda^r)^2 + (\theta^r)^2 \end{array} \right]$$

Assuming normality and that all of the equations are continuous, the likelihood is obtained using the multivariate normal density function. Explicitly,

$$\ln L = \frac{1}{2} \ln |\Omega_h^{-1}| - \frac{1}{2} \zeta_h' \Omega_h^{-1} \zeta_h$$

where $\zeta = [\varphi \ \omega^1 \ \omega^2 \ \dots \ \omega^{k_2}]'$ is the vector of residuals for each household. However, this is not always the case. In the model presented in the text, y^r , $r = 1, 2$, are truncated from below at zero. Thus, it becomes necessary to estimate these equations as Tobits. To accomplish this, factor the likelihood as the product of a conditional and a marginal density, where now the truncated endogenous regressors are estimated conditional on the remaining continuous equations. Now, the likelihood becomes

$$\ln L = \ln [\Phi_j(\Xi_y)] + \frac{1}{2} \ln |\tilde{\Omega}_h^{-1}| - \frac{1}{2} \tilde{\zeta}'_h \tilde{\Omega}_h^{-1} \tilde{\zeta}_h$$

where Φ_j is a j -dimensional normal cumulative density function and j is the number of endogenous variables truncated at zero, Ξ_y is a j -dimensional vector of conditional means standardized by their conditional standard deviations, $\tilde{\zeta}$ is the vector of residuals from the continuous equations, and $\tilde{\Omega}_h$ is the covariance matrix for the continuous equations.

The general model can be extended to allow for multiple time periods. Rewriting the model to incorporate this new dimension, yields

$$\begin{aligned} H_{bih}^s &= \eta_b^s t + X_{iht} \alpha_b^s + Z_h \gamma_b^s + \sum_{r=1}^{k_2} \delta_{br}^s y_h^r + \lambda_b^s \mu_h + \pi_b^s \nu_{bih} + \xi_b^s \epsilon_{bih}^s \\ H_{gih}^s &= \eta_g^s t + X_{iht} \alpha_g^s + Z_h \gamma_g^s + \sum_{r=1}^{k_2} \delta_{gr}^s y_h^r + \lambda_g^s \mu_h + \pi_g^s \nu_{gih} + \xi_g^s \epsilon_{gih}^s \\ y_h^r &= Z_h \gamma^r + \lambda^r \mu_h + \theta^r \omega_h^r \end{aligned}$$

and the reduced-form outcome equations become

$$\begin{aligned} H_{kih}^s &= \eta_k^s t + X_{iht} \alpha_k^s + Z_h [\gamma_k^s + \sum_{r=1}^{k_2} \delta_{kr}^s \gamma^r] \\ &+ [(\lambda_k^s + \sum_{r=1}^{k_2} \delta_{kr}^s \lambda^r) \mu_h + \sum_{r=1}^{k_2} \delta_{kr}^s \theta^r \omega_h^r + \pi_k^s \nu_{kih} + \xi_k^s \epsilon_{kih}^s] \end{aligned}$$

More concisely,

$$H_{kih}^s = \eta_k^s t + X_{iht} \alpha_k^s + Z_h \Psi_k^s + [\Gamma_k^s \mu_h + \sum_{r=1}^{k_2} \delta_{kr}^s \theta^r \omega_h^r + \pi_k^s \nu_{kih} + \xi_k^s \epsilon_{kih}^s]$$

where we again adopt the normalization $\theta^r = \theta$.

When obtaining the covariance matrix in a model with multiple time periods, one additional normalization is required,

- $E[\nu_{kih} \nu_{kih}] = 1$.

Given this, only the submatrix for a given *individual* will change since the individual random effects do not correlate across members within a household. The

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