930000995/62

PN-AAP-319
ISN-33473

# FERTILITY DETERMINANTS IN A DEVELOPING COUNTRY: IS THE STANDARD APPROACH TOO SIMPLE AND POSSIBLY MISLEADING?

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1981
NPP29
(revised: March 1982)

This paper is one of a series resulting from a survey and research project to investigate the social, economic, and demographic roles of women in the developing country of Nicaragua. Funding for 1977-1978 was provided by the Population and Development Policy Research Program sponsored jointly by the Ford and Rockfeller Foundations. Further funding has been provided by the Agency for International Development Negotiated Contract AID/otr-C-1571 for 1977-1981 and the National Science Foundation Grant SES-8025356 for 1980-1983. Behrman was also a Guggenheim Foundation Fellow for the 1979-1980 academic year and a Compton Foundation Fellow for part of the 1980-1981 academic year.

The project is being jointly conducted by the Universities of Pennsylvania and Wisconsin, the Centro de Investigaciones Sociales Nicaraguenese (CISNIC), and the Banco Central de Nicaragua. Humberto Belli, Director of CISNIC, and Antonio Ybarra, former head of the Division of Social Studies and Infrastructure, Banco Central de Nicaragua, were coprincipal investigators with Behrman and Wolfe, for the early stages of the project. Belli supervised the collection of all survey data.

The authors would like to thank, but not implicate, the funding agencies, their coprincipal investigators, Bryan Boulier of Princeton University and Mark Rosenzweig of the University of Minnesota, and associates in the project, especially Kathleen Gustafson and Nancy Williamson. Behrman and Wolfe equally share responsibility for this paper.

## Abstract

Rapid population growth continues to shape economic alternatives in developing economies. A large number of empirical studies of fertility determination in developing countries exist, generally using a linear fertility determination relation and obtaining empirical support for an inverse association of fertility with female schooling which is interpreted to reflect the opportunity cost or price effect of women foregoing labor market participation in order to provide child care.

However, on a priori theoretical grounds and on the bases of casual empiricism this standard approach appears too simple and possibly quite misleading in a number of dimensions. The relevant human capital would seem to include health and nutrition status, not just schooling. Schooling, on the other hand, might represent tastes, genetic factors, and household efficiency -- and not just labor market productivity. Derivation of fertility determination relations from a maximizing model generally implies interactions and nonlinear effects, not a linear relation. Standard empirical procedures for treating incomplete observations may lead to selectivity biases. Differences in unobserved norms and prices and the extent of unperceived jointness would seem to imply different relations for different regions defined by the degree of urbanization.

We discuss these issues and explore them with a national data set of about 3500 women from different regions in Nicaragua. Somewhat to our surprise we do not find evidence of significant differences in the overall relations among regions. But we do find evidence for all of the other complications of the standard approach noted above. Therefore the conventional approach, by virtue of assuming away relevant complexities, may lead to misunderstanding of fertility determination in developing countries and to incorrect policy inferences.

## 1. INTRODUCTION

Population growth rates, though slowing down somewhat recently in some cases, remain very high in many countries in the developing world (33). At present growth rates, the aggregate population in the developing world will double in about three decades. The World Bank (33) and many other observers consider such rapid population growth to be one of the major factors which limit large numbers of developing countries from attaining economic growth and distributional goals.

In recent years demographers, economists and others have undertaken a large number of studies of fertility determinants in developing countries in hopes of helping to understand better the process, anticipate better future developments, and formulate better relevant policy.[1] The standard underlying conceptual model posits the determination of fertility by the maximization of parental preferences, including those related to the quantity and quality of children, subject to constraints imposed by market prices, household production, and given parental tastes (2, 25). This approach emphasizes the important role of human capital investments in women in the determination of fertility. Such investments have an income effect by increasing women's earnings potential, which in itself leads to an increase in the demand for children and other normal goods. But they also may have a price effect due to the increased opportunity cost of women staying at home to care for their children and thereby foregoing higher market earnings that would be commanded by their greater capital stock. Most empirical explorations of these possibilities focus on the impact of the woman's schooling on observed fertility within a linear relation with little or no direct representation of child quality, nor of the constraints imposed by prices, household technology and parental preferences since relevant variables are not observed. The most

robust result from these studies is an inverse association between fertility and women's schooling, which usually is interpreted to mean that the opportunity cost or price effect dominates so that increased education for women lowers fertility.[2]

This standard approach and interpretation has not been without criticism. To the contrary a number of questions have been raised:[3] Given the hypothesized importance of health and nutrition in developing countries in general and in the supply of births in particular, should not a broader empirical representation of woman's human capital be utilized? Might there not be important nonlinearities in the impact of each of these variables and interactions among them? Given that the unobserved prices (and possibly household technology and tastes) differ among regions defined by the degree of urbanization, might not the fertility relations differ across such regions? Might there be a selectivity problem with standard practise in most of the studies of dropping from the sample women for which there is no information on their fertility or of arbitrarily assigning such women a value of zero fertility in the El Badry (17) procedure? Should schooling be interpreted as reflecting primarily market options for women, and not tastes nor genetics?

Exploring such questions is not easy. Most (all?) data sets are deficient in not having available information on some critical variables. Nevertheless some available data sets permit some progress in exploring these questions about the standard approach.

In this paper we present the results of such an exploration which we have undertaken with a multipurpose socioeconomic data set which we collected from an area-stratified random sample of women in child-bearing ages (15-44) in Nicaragua in 1977-78.[4,5] We investigate the fertility behavior of 3,488 non-single women in the national sample, but also consider three regional samples

defined by the degree of urbanization in order to allow for the regional differences in unobserved variabes noted above: 1,436 women in the central metropolis, 1139 in other urban areas, and 913 in rural areas.

We organize our presentation as follows. Section 2 discusses our model of complete fertility. Section 3 presents estimates of fertility determinants with control for nonresponses. We emphasize the evidence regarding nonlinearities and ineractions among the extended set of human capital variables and regional differences. Section 4 gives concluding remarks. The Appendix discusses the nonresponse problem, the possibility of selectivity bias and probit estimates of response decisions.

## 2. MODEL OF COMPLETED FERTILITY AND ITS EMPIRICAL REPRESENTATION

We have in mind the basic model of complete fertility that has been extended recently by Easterlin, Pollak and Wachter (EPW, 16) to incorporate endogenous tastes, natural fertility, and unperceived jointness. In this model household utility is defined over commodities, completed family size, frequency of intercourse, infant mortality, and the intensity and use of contraceptives, all conditional on norms regarding goods consumption and completed family size. This one-period utility function is maximized subject to constraints on household commodity production, household time, the household budget, a biological births production function, a biological infant mortality function, and the definition of completed family size as equal to child births minus child deaths.

### General Implications of Derivation of Fertility Relations From Such a System

We could now derive mathematically the fertility relation implied by such

a system. Such a derivation would be rather sterile, however, because a priori most of the signs would be ambiguous and because our (and all other) data do not permit the estimation of many of the structural coefficients since some critical variables are unobserved (e.g., prices, norms). Therefore our empirical work focuses on approximations to a reduced-form relation in which complete family size is posited to depend on the woman's human capital, household income other than from her market earnings, her childhood and current background, and her marital status. Though plowing through the mathematics tying this reduced-form relation back to the underlying constrained maximization is rather arid in this context, conceptualizing the reduced-form relation as having been derived from such a process does lead to some important general insights. Five of these merit emphasis before we turn to the variable groups in the reduced-form relation.

First, such an approach emphasizes how human-capital dimensions additional to schooling might enter into the fertility relation. The woman's health and nutrition, for example, may affect the frequency of intercourse or her fecundity and thus enter in through the birth production function. But beyond such direct "supply effects," health and nutrition may be relevant because of their impact on market and household productivities.[6]

Second, consideration of the derivation of the reduced-form fertility relation from such a system does <u>not</u> lead to strong priors on many of the partial derivatives in the reduced-form relation because of counteracting income and price effects or the possibility of effects transfered through norms or ability offsetting direct effects (see discussion of schooling below). Others have posited the signs of some of these derivatives by ignoring the possible role of biological factors and unobservable norms and abilities. We do not do so because in our judgement the cost of such

simplifications is too high given the probable roles of such factors in fertility determinantion in developing countries. But the price for not doing so is having less definite theoretical implications regarding these derivatives.

Third, consideration of the underlying derivation from the EPW model points to three important reasons why estimates might differ across regions: the relevant subset of relations may differ due to what EPW term "unperceived jointness,"[7] unobserved prices may differ, and unobserved norms may differ. If observations were complete on all of the prices and the norms, the last two of these would not be relevant and we could test for the first one. Since our data are not so complete, we cannot identify why estimates differ across the regions, if indeed they do. But this discussion suggests that it is important to allow for the possibility that they do so differ.

Fourth, such a derivation generally does not lead to a linear fertility relation. The appropriate functional form depends upon all of the functions in the constraints and the utility function. Because the selection of particular underlying functional forms is somewhat arbitrary and for simplicity, the fertility determination relation usually is approximated by linear terms with an additive stochastic term to represent random elements in individual behavior. For comparability with other studies, Section 4 presents such estimates. But AID (1), the World Bank (33), Bongaarts (13) and others have emphasized the importance in the underlying functions of nonlinear effects and interaction terms, particularly for the human capital variables. For example the initial grades of schooling may affect relatively strongly opportunity costs and infant mortality, but more schooling may have a stronger relative impact on the woman's tastes. Or, as Bongaarts emphaiszes, there may be a significant effect of better nutrition on fertility thorugh reducing

subfecundity in the biological birth production function only at very low nutrition levels. For one last example, women with more schooling may more efficiently use a given level of nutrition inputs, or respond differently to illnesses than women with less schooling. Therefore the complete reduced-form model on which Section 3 focuses includes spline terms for thresholds in human capital variables and multiplicative terms for interaction among human capital variables in order to approximate such nonlinear possibilities.

Fifth, the EPW theoretical discussion, as well as most others, concentrates on completed fertility. But, in order to have observations on behavior in the recent past which have more relevance for future behavior than would more dated observations, most empirical studies include women whose fertility probably is incomplete. To control for the possibility of incomplete fertility in our estimated relations discussed in Section 3 below we follow the Boulier-Rosenzweig (14) recommendation of standardizing by the Coale and Trussel international natural fertility standards conditional on age of the woman's first cohabitation and her current age.[8]

## Variables Included in Our Fertility Relation

We now discuss our representation of each of the groups of variables included in our reduced-form fertility relation, our rationale for including them, and our priors for signs of coefficients — though consideration of an underlying structural model such as that of EPW leads to considerable ambiquity in regard to some signs as noted above. Table 1 gives the means and standard deviations of these variables for the nation and for the three regional samples.

Woman's Human Capital. Schooling is the standard variable which is used to represent woman's human capital in fertility studies. The usual

interpretation is that schooling is representing the "price effect" or the opportunity cost of having children in terms of paid labor force participation. However, we adopt a better representation of this opportunity cost in the form of the woman's predicted earnings per unit time in the relevant market conditions. Our prior is that this variable has a negative coefficient in the reduced-form fertility relation because of the opportunity cost or price of time interpretation. We construct this variable on the bases of each woman's individual characteristics and regional earnings per fortnight function estimated with controls for selectivity into the labor force and for reporting earnings for those who do participate in the labor force.[9]

Of course the woman's schooling may affect completed fertility indirectly through altering her predicted market wage rate. However, there may be a number of additional effects of the woman's schooling. For example, such schooling may alter norms or tastes for children versus commodities, for using contraceptives, and for frequency of exposure to possible conception. Schooling may also alter her efficiency in household production, inlcuding childcare and the probability of child deaths. Moreover schooling may increase the probability of knowing about contraceptive options. Furthermore schooling may alter the power of the woman in making household decisions regarding the number of children. Finally, schooling may be partially a proxy for genetic endowments and for dimensions of her childhood background which affect her abilities and attitudes. Because of such possibilities we include the woman's grades of schooling completed in addition to her predicted market wage rate.[10,11]

We are willing to speculate on the signs of some of these individual schooling effects: More schooling may be associated with more fertility due to greater efficiency in general household production or due to serving as a

proxy for better health and better family background. On the other hand more schooling may be associated with smaller completed family size due to an induced shift in norms away from the quantity of children and towards reducing the disutility costs of contraception, by increasing efficiency in preventing child deaths (and thus reducing the need for "insurance" births beyond desired levels), by increasing the probability of knowing about contraceptive options, and by increasing the power of the women in household decision making if there is merit to the stereotypic view that large families in some Latin cultures reflect the dominance of "macho" males with pro-fertility preferences in such decisions. However because of the opposing possible signs of the individual effects and the impossibility of confidently identifying the individual effects we do not have a strong prior about the sign of the combination of these effects, even with control for the standard opportunity cost interpretation by including the predicted wage rate.

In addition to the predicted wage rate and schooling human capital variables, we include proxies for nutrition and health status. We consider these variables because of the emphasis of EPW (16) and others on their role in fertility determination through biological "supply" processes and the more general emphasis of Leibenstein (20), World Bank (33) and others on their role in household productivity. To the extent that they affect market productivity and thus opportunity costs, nutrition and health work through the predicted wage rate variable. They also may have direct impact through biological supply factors, household efficiency, or be proxies for genetic and family background or economic status. As with schooling there is ambiguity about the a priori expected signs of the direct effects. Better nutrition and health may be associatd with lesser fertility, even after controlling for the market opportunity cost impact through lessening the need for insurance births due to

reduced probability of child mortality or through association with norms towards substitution away from children or with lessened disutility costs of contraception. However in this case we believe that pro-fertility effects through biological supply factors and increased household productivity (given the control for market productivity) probably dominate, so we anticipate a positive impact of improved nutrition and health status.

Our proxies for nutrition and health status are based on recall data. For nutrition status we use the average household dietary caloric input (standardized for household composition) for the week prior to the survey. This has measurement error due to recall problems and beause it reflects the average household input over a short period of time, not the long-run nutrition state of the woman. It also is an input (not an output) measure and may reflect income constraints. But the latter possibility should not be too troublesome since other studies indicate that the income elasticity of this variable is very small in our sample.[13]

For health status we use dichotomous variables for whether or not the respondent has had therapeutically-treatable (e.g., anemia, tetanus, veneral disease), parasitic, or medically-preventable (e.g., tumors, hernia, skin disease, pneumonia, bronchitis, asthma, typhoid, high-blood pressure) diseases. These proxies are subject to recall and self-diagnosis measurement error and possible simulataniety with income, though once again we have been unable to find statistical evidence of a substantial association with income in another study.[14]

Other income. This variable includes all household income other than the woman's earnings (which are not included in order to attempt to separate the income from the price effect and to control for the endogenous labor force participation decision). For households engaged primarily in own-farm

production a dummy variable also is included because of definitional differences in this case for other income which probably result in it being an underestimate of long-run income or wealth.

Under the standard interpretation of the other income variables as representing income effects with the quantity of children being a normal good, we expect a positive effect on fertility. However we note that there could be a partially offsetting negative association if norms are shifted away from quantity of children or towards lessen disutility costs of contraception by changing exposure or reference groups which depend upon income. Casual observation suggests that higher-income households in developing countries indeed are more exposed to factors which may so shape their norms through the so-called "international demonstration effect." An additional partially offsetting negative effect may be that households with higher income may derive less returns from the pension motive for having children since they are more likely to have access to formal pensions, insurance programs, or other capital-market alternatives. Despite these possibilities, our prior remains that a positive income effect dominates.

Background Variables. Family background may affect ability, motivation and norms beyond the effects represented by human capital variables like schooling.[15] Therefore we include several proxies for the woman's childhood background: age, presence of father during childhood, number of siblings, and dichotomous variables for whether or not the woman had an urban upbringing and had never migrated.

We generally do not have strong priors on the derivatives with respect to these background variables, once again, because they may work indirectly through ability in ways that may counter their direct impact through the norms. Nevertheless at least in one case we expect that the direct effect

dominates any indirect ones. This prior is that the derivative with respect to number of siblings is positive both because of the dominance of the direct effect on norms for family size and because of possible genetic effects on fecundity.

Marital Status: All of the women in our sample have been married or, to be more precise, accompanied by a male since common-law unions are frequent. We posit that the type of union may be associated with fertility through norms. Those in religious and perhaps in common-law[17] unions are likely to have more traditional and more pro-fertility norms for children than those who are in civil marriages.[18] We further posit that those who have cohabitated with a male previous to their current arrangement are likely to have fewer children due to interruption of exposure to conception and probable greater perception of the possibility of the dissolution of a current union, with negative permanent income implications.

## 3. FERTILITY ESTIMATES

Table 2 gives estimates of the linear and the more complete (ie., with nonlinear terms in the human capital variables) fertility relations in Section 2 with the Heckman control for selectivity based on the probits in the Appendix for each of the three regions and for the combined sample. Because of the multiplicative and the spline terms, the fullest specification of the complete model includes a large number of right-side variables. For compactness, this table presents a more pared-down specification in which variables are not included (1) which do not have significantly nonzero coefficient estimates at the 10% level and (2) the exclusion of which does not change substantially any of the other coefficient estimates. Notes b through f of the table indiate which variables have been excluded on this basis.

Differences by degree of urbanization. One question of basic interest is whether or not the relations differ significantly among the three regions. Section 2 argues that such differences might be expected on *a priori* grounds because different subsets of relations may apply to households in different regions and because unobservable variables related to prices and norms probably vary with the degree of urbanization.

A casual comparison of the coefficient estimates in the first three columns of Table 2 suggests that there might be some regional differences, with schooling more important in the two urban regions, predicted earnings least important in other urban areas, father present in childhood and having had parasitic diseases (in interaction with schooling) most important in the central metropolis, and nutrition, marital status, and never migrated most important in the rural areas. The estimates of the linear model in columns 5 through 7 suggest a similar, although not identical, pattern.

However a F test does not reject the hypothesis that the coefficients in the three regional estimates of the complete model are identical at the 5% level.[19] This may seem surprising. But, in the words of Maddala (21, p. 199),

> "If there is high multicollinearity in the regressors, it is not unusual that what looks to us like drastic differences in the coefficients turn out to be 'statistically insignificant'. In such cases one should try to solve the basic problem of multicollinearity and not get too excited about having found the differences 'statistically insignificant,' because from the practical point of view these differences are often 'very significant'."

But Maddala does not give insight into how "to solve the basic problem of multicollinearity."[20] In the complete model **prima facie** it might seem to be particularly severe because of the multiplicative and spline terms. However, an F-test applied to the linear specification also does not reject the

hypothesis that the coefficients for the three regions are identical. Therefore any multicollinearity is not attributable only to the multiplicative and spline terms. Finally we experiment with interactions between regions and certain of the exogenous variables. These interaction terms are also generally not significant.

Where does this discussion leave us? Despite our priors to the contrary, the estimates in Table 2 do not provide evidence of significant differences across regions. If such differences exist, they are masked by multicollinearity which we are unable to disentangle. Therefore, in what follows the coefficient estimates from the overall sample are considered in addition to those from the regional ones. Of course, all of our comments must be qualified because of our incapacity of disentangling the apparent multicollinearity.[21]

Response selectivity. The coefficient estimates of the response selectivity control terms (see the Appendix) are significantly nonzero at the 5% level for all but the other urban area in the linear model and for the overall sample and at the 10% level for the central metropolis in the complete model. This pattern suggests that the selectivity terms may be representing some of the nonlinear effects in the linear model. Nevertheless there may be a selectivity problem in the complete model with the controls for nonlinear terms. In fact if the selectivity control is not included in the complete model, some of the point estimates change significantly. For example, in the regression for the combined sample the absolute values of the coefficients of the linear terms for schooling and for having had therapeutically-treatable diseases both change by about a third if there is no control for selectivity. Thus, the standard procedures of dropping nonresponses from the sample or of setting their fertility equal to zero as El Badry (17) recommends

may be misleading for some of the coefficient estimates.

Background variables. The background variable with the strongest effect in the complete model is the presence of the woman's father in her childhood household, with significant positive coefficients at the 5% level for the central metropolis and the combined sample. There is not much variation across regions in the proportions of women who had their father present during childhood, with values close to three-fifths in each case (Table 1). This variable may be working through expectations by increasing the subjective probability regarding a stable relation with her companion for the woman herself. Those women without a father present in their childhood, in contrast, may be more pessimistic about the prospects of a stable relation for themselves, more sensitive to difficulties of bringing up children without a male companion, and thus desirous of having fewer children.

The only other background variable with a significantly nonzero coefficient even at the 10% level in the complete model is the negative one in the rural regions for never migrating. The proportion who have never migrated in the rural area is lower than in the urban sample (Table 1). The sign of the estimated effect on rural fertility may be surprising since those who do not migrate often are characterized as being more traditional and with norms for more children than those who do migrate. However, if migration of women is substantially for marriage or cohabitation purposes, as Thadani and Todaro (23) and we (8) hypothesize, those with higher norms for children may be more likely to migrate.[22]

None of the other background variables have significantly nonzero coefficient estimates even at the 20% level in the complete model. These variables include the number of siblings of the women, which often has been hypothesized to be an important determinant of norms for family size (eg.,

15). Thus, to the extent our data permit the quantification of the woman's background, our results suggest that such background does not have a very important direct role in fertility determination through norms or abilities. However, background may have an indirect role through human capital and marital status variables which are discussed below.[23]

Marital status. In the rural area a higher proportion of women are in common-law arrangements than in the urban areas, and a higher proportion are in religious marriages than in the central metropolis (but about the same as in other urban areas). The coefficient estimates for religious and common-law marital arrangements in the rural region are significantly positive at the 5% level. The regression estimates imply that ceteris paribus women in religious marriages (32% of the ever-accompanied women in rural areas) have significantly more children than those in common-law unions (48%), who in turn have significantly more than those in civil marriages or those currently unaccompanied. This pattern probably reflects a combination of taste (ie., norms) and income factors. Those in religious unions may tend to be most traditonal and have the highest norms for children and the highest disutility costs for contraception. Those in common-law unions also may tend to be relatively traditional and to have relatively low income. Those who elect civil marriages may tend to have higher income and less traditional norms regarding number of children and contraceptives.[24]

For the nonrural regressons, none of the marital status variables has significantly nonzero coefficients even at the 25% level in the complete model.[25] This includes the variable for having had a previous companion different from the current one (which 38% of the women have had). Thus, interruption of exposure to possible contraception through the dissolution of a union does not seem to alter significantly fertility outcomes (with the

possible exception of currently unaccompanied women in the rural areas).

Income. We do not find significantly nonzero coefficient estimates for other income even at the 20% level. in the complete model.[26] Thus, these estimates do not support any pure income effect, despite considerable variance in other income within regions (particularly in the rural areas) and some positive association between mean incomes and urbanization (Table 1).

The coefficients of the own-farm operation variable, however, are significantly positive at the 5% level in both the rural and the combined regressions. This may be representing an income or a wealth effect since the other income variable probably does not adequately capture longer-run income for own-farm households. If this is the case, for such households there may be a positive pure income effect. But this variable also may be representing differential prices of children (ie., the net child costs may be less than elsewhere due to their employment in own-farm operations), or differential norms for own-farmers. We can not identify the relative importance of each of these possibilities.

Human capital. Because of our interest in the human capital variables and frequent hypotheses about the importance of nonlinear effects involving such variables, in the complete model we include spline and interaction terms for these variables (Section 2). Before turning to the implications of the coefficient estimates of the complete model, therefore, it is of interest to ask whether the inclusion of such terms increases the consistency of the model with observed fertility behavior. F tests at the 1% level indicate that the answer is positive with the exception of the rural region. Since the nonlinear terms often are important and their inclusion sometimes alters the other coefficient estimates,[27] we focus on the complete model estimates in the discussion of the estimated impact of human capital variables that follows.

Predicted earnings for women tend to reduce fertility. The coefficient estimates of predicted earnings in market activities are significantly negative at the 10% level for the central metropolitan, rural and combined samples (but not for other urban areas).[28] This is consistent with there being a price effect for child care in terms of the opportunity cost of paid market participation. Ceteris paribus, the factors which increase paid market earnings would reduce fertility. These factors include more schooling, better nutrition, more on-the-job training and possibly better health and more integrated labor markets with less sexual discrimination (3, 6, 10, 11, 12).

Women's schooling tends to be much greater in the urban than in the rural areas (Table 1). The variables which directly involve schooling have a number of significantly nonzero coefficient estimates in the complete model, though many of these are lost in the linear version. Those for the linear schooling terms in the complete model are positive and larger in the urban than in the rural regions.[29] Those for the interaction with age are significantly negative at the 5% level for all but the rural area. That for the interaction with having had a therapeutically-treatable disease is significantly positive at the 10% level for the combined sample. Those for the interaction with having had parasitic diseases are significantly negative for the central metropolis and the combined sample (the former at the 5% and the latter at the 10% level). The spline for schooling of at least 6 grades is significantly negative and that for the interaction with age is significantly positive at the 10% level for other urban areas. Thus the results suggest a pervasive direct role for schooling, beyond the indirect price effect through opportunty costs in terms of market earnings, with some nonlinearities and interactions with other variables. Of course, as Section 2 discusses, schooling may be representing ability or determinants of norms, or both.

What are the implications of these multiple estimates for the direct impact of schooling on fertility outcomes? Table 3 summarizes the total implied marginal direct impact of an additional year of schooling on the standardized fertility dependent variable. In the first row are the effects at the sample means of the relevant variables (from Table 1). These estimates are positive except for other urban areas. They imply a direct impact of slightly more than 1% of the dependent variable for an additional year of schooling in the rural areas, but less than 1% elsewhere. The second row shows how these effects change if the woman has not had parasitic nor therapeutically-treatable diseases. For women with better health the effects are slightly more positive in the urban areas (and virtually zero in other urban areas) and for the combined sample. The third row reflects the added effect of having completed six or more grades of schoolng. As compared with those of less schooling (ie., the estimates in the first row), the implied marginal impact of schooling is higher in urban areas. The total estimated direct effect of an additional grade for the more schooled is about 2% of mean fertility, except n other urban areas for which it is zero.

The last two rows in Table 3 relate to the impact of the interaction between age and schoolng. The estimates basically imply that the direct impact of schooling is more negative for women who were older at the time of our survey. Probably this reflects a stronger inverse assocation between schooling and norms for children for the older cohorts than for the younger ones because schooling for females was much less common when the former were young. As schooling for females became more widespread, girls attended primary schooling more, independently of other factors that might condition subsequent adult norms for family size.

But Table 3 refers only to the direct effects of women's schooling. In

addition there may be indirect effects that operate through other variables in the model. The most important of these probably is the impact of schooling on the opportunity cost of the woman's time in terms of paid labor force partipation, as represented by the predicted earnings variable. The coefficient estimates for the earnings functions imply indirect effects of an added grade of schooling of -.008 for the central metropolis, -.001 for other urban areas, -.009 for rural areas and -.003 for the combined sample.[30]

The total effects are the sum of the direct and indirect effects. At the sample means the implied total effects of an additional year of schooling are -.007 for the central metropolis and for other urban areas, .003 for rural areas, and .004 for the combined sample. Therefore the direct effects in Tables 3 and 4 tend to offset at least partially the impact on opportunity costs which usually is emphasized, so that the implied total effect is not very large and perhaps not even negative.

Nutrition tends to be best in the other urban areas and worst in the rural region (Table 1). The direct impact of nutrition on fertility outcomes is significant only at low nutrition levels in the rural areas. In rural households with less than half of the international nutrition standards, there is a significantly positive effect. Increasing their nutrition input is associated with more fertility, perhaps through reducing subfecundity and through increasing energy levels for child care and other activities.[31] For higher levels of nutrition in the rural areas the direct effect of nutrition is not significantly nonzero since the coefficient estimtes of the spline term offsets that for the linear nutrition term. For all but the malnourished rural households the total effect of nutrition is to reduce fertility, if anything, by increasng relative productivity in market and possibly other nonchild care actvities. For each of the three regions and for the combined

sample, for example, the woman's opportunity cost as represented by predicted earnings embodies a significant positive association with nutrition (4), which indirectly tends to reduce fertility.

The health (disease) variables indicate the greatest prevalence of therapeutically-treatable and parasitic diseases in other urban areas, but with parasitic diseases more common in rural areas than in the central metropolis (Table 1). The estimates in Table 3 suggest some significant effects in part in interaction with schooling. For the combined sample they imply a significantly negative effect on fertility of having had therapeutically- treatable diseases for women with less than about 7 grades of schooling completed (as compared with the overall sample mean of about 4 grades).[32] For the overall sample and particularly for the central metropolis, the estimates imply an increasingly strong inverse association between schooling and fertility for women who have had parasitic diseases. Since 33% of the whole sample has had therapeutically-treatable diseases and 48% has had parasites, the estimated direct impact of these diseases on fertility is widespread.[33]

Better health and lessened incidence of these diseases thereby may have a direct pro-fertlity effect.[34] And in other studies we find no evidence of an offsetting indirect impact of improved health on women's productivity in market activities and thus their opportunity costs in such activities (3, 6, 10, 12).

## 4. CONCLUDING REMARKS

In the introduction we note a number of questions which have been raised about the standard approach to estimation of fertility determinantion relations for developing countries. We return to these questions to summarize

our empirical exploration.

Is a broader definition of the woman's human capital than just her schooling relevant? A priori a broader definition might seem relevant because of plausible relations between health and nutrition on one hand and fecundity, household productivity and market productivity on the other. Our estimates suggest that indeed a broader view of the women's human capital is useful in understanding fertility determinants. For example woman's nutritional state and work experience (like her schooling) work through her predicted earnings to lower fertility by increasing the opportunity cost to time spent in childcare and foregoing market activities. In addition there are direct effects of health and nutrition which may be working through reducing subfecundity, increasing productivity in nonmarket activities or changing tastes. In any case the significant direct role of health and direct and indirect (i.e., through market productivity and earnings) roles of nutrition are consistent with the EPW (16) extension of the fertility model to include biological factors and the Leibenstienian (20) emphasis on the relation between health and nutrition status and productivity. The interrelation between health and nutrition and fertility also imply that there may be high payoffs in terms of understanding and in terms of policy formulation of more integrated treatment of various dimensions of human capital formation.

Are there important nonlinearities in the fertility impact of the human capital variables? A priori considerations suggest that nonlinearities might be important since the typical linear fertility relation can be viewed as an approximation to a much more complicated reduced form, the exact functional form of which reflects the functional forms of the parental preference function and of a number of constraints. On the bases of field observations, too, AID (1) and others have argued that nonlinear "threshold effects" may be

important. Our estimates suggest that nonlinear spline terms indeed may be important. For example, schooling has a differential effect if 6 or more grades are completed than if less than 6 are completed. Also improved nutrition may directly increase fertility for very malnourished households, but not have significant impact on somewhat better nourished households. Incidentally, the latter result is consistent with the possibility that extreme malnourishment causes biological subfecundity, which contrasts with Bongaarts' (13) recent survey.

Are there important interactions among the human capital variables in fertility determination? Considerations of an underlying theoretical bases for the reduced-form fertility relation, once again, leads to the possibility of there being important interactions, depending on the nature of the underlying functional forms for preferences and for constraints. On the bases of more causal observations, moreover, the World Bank (33) and others have emphasized the possible importance of interaction effects. Our estimates provide some support for the relevance of such interactions. Those of schooling with disease experience, for example, are significant. Thus incorporating such possibilities into the analysis may enrich understanding and lead to better policy analysis.

Are there regional differences associated with the degree of urbanization in the fertility determination relation? *A priori* considerations suggest that such differences may exist because different subsets of the fertility model may be more relevant for more rural and traditional areas than for more urban areas and because certain unobserved variables relating to prices and norm formation probably differ with the degree of urbanization. Our point estimates suggest that there are some plausible differences across regions, e.g., a greater role for nutrition in the more malnourished rural areas.

However, an F-test suggests that the overall relations do not differ significantly among our three regions. Therefore our estimates do not support frequently-hypothesized and a priori plausible regional differentials.

Are there selectivity biases if nonrespondents are dropped from the sample or treated as if they had zero births in the El Badry procedure? A priori selectivity bias is a possibility if the disturbance term in the selection rule for whether or not fertility is reported is associated with the disturbance term in the fertility determination relationship. Our estimates suggest that there is some possibility of selectivity bias if the standard procedures are followed. However the apparent selectivity bias may be overstated if the fertility relation is constrained to be linear since the selectivity control may serve partially as a proxy for omitted nonlinear and interaction terms.

Is the common interpretation warranted that woman's schooling represents the opportunity cost of time spent out of the market place to care for children? Both the a priori discussion of Section 2 and the estimates in Section 3 suggest that the standard interpretation has some validity, but that it nevertheless may be misleading in some important respects. Emprically, our measure of woman's predicted earnings per fortnight does have significant negative coefficient estimates in several of the fertility relations, and schooling is a major determinant of these predicted earnings. To that point our results support the standard interpretation.

But there are two important caveats. First, in addition to schooling, other dimensions of the woman's human capital, such as her work experience and her nutrition state, also affect her predicted earnings. Therefore the existence of other important human capital means that schooling by itself is far from a perfect proxy for the labor-market alternatives.

Second, even after controlling for the labor market alternatives with the predicted earnings variable, schooling still has other significant coefficient estimates. These suggest that schooling is representing more than just market productivity — perhaps home efficiency, genetics, or tastes effects. Unfortunately it is hard to sort out the other possibilities with our (and other) data sets. But we expect that they are not dominated by household efficiency since most dimensions of household efficiency would seem to be highly associated with market efficiency, for which we include a control in the form of the predicted earnings variable. Therefore, taste effects may be an important factor. And if schooling indeed is altering fertility partially by changing tastes, the private welfare effects of reducing fertility by increasing female schooling are much more ambiguous than is ususally assumed. Care must be taken not to base analysis or policy formulation on private welfare if that welfare is conditional on changed tastes induced by schooling!

Thus our exploration suggests that some of the criticisms of the standard approach and interpretation have merit in this particular empirical context. A richer specification seems warranted, with a wider range of human capital variables, nonlinearities, interactions and possibly selectivity controls for nonresponse. The standard interpretation of female schooling as basically representing the "price effect" of foregoing labor market participation also may be misleading by ignoring both other forms of human capital and induced taste changes with their ambiguous private welfare impliations. The real world seems more complicated in important dimensions than the standard approach assumes. Better analysis and better policy formulation could result from enriched specifications which incorporate these complications.

Table 1
Means and standard deviations (in parentheses) of variables in fertility regressions[a]

| | Central metropolis | Other urban | Rural | Combined |
|---|---|---|---|---|
| 1. Fertility (with Boulier-Rosenzweig control) | .76<br>(.59) | .76<br>(.49) | .84<br>(.48) | .78<br>(.53) |
| 2. Background | | | | |
| Father present in childhood | .60<br>(.49) | .58<br>(.49) | .64<br>(.48) | .60<br>(.49) |
| Number of siblings | 4.7<br>(3.0) | 4.9<br>(3.1) | 5.3<br>(3.0) | 4.9<br>(3.0) |
| Never migrated | .45<br>(.50) | .53<br>(.50) | .38<br>(.49) | .46<br>(.50) |
| Urban upbringing | .90<br>(.30) | .90<br>(.30) | .32<br>(.47) | .75<br>(.43) |
| Age | 29.3<br>(7.3) | 29.7<br>(7.6) | 29.6<br>(7.9) | 29.5<br>(7.5) |
| 3. Marital status | | | | |
| Religious marriage | .25<br>(.43) | .32<br>(.47) | .32<br>(.47) | .29<br>(.45) |
| Common law | .36<br>(.48) | .32<br>(.47) | .48<br>(.50) | .38<br>(.49) |
| Previous cohabitation (prior to present state) | .39<br>(.49) | .36<br>(.48) | .40<br>(.48) | .38<br>(.49) |
| 4. Income | | | | |
| Other income | .73<br>(.78) | .68<br>(.80) | .57<br>(4.57) | .67<br>(2.43) |
| Own-farm | .00<br>(.00) | .00<br>(.00) | .33<br>(.47) | .087<br>(.28) |
| 5. Human capital | | | | |
| Predicted earnings | .23<br>(.18) | .18<br>(.16) | .12<br>(.40) | .19<br>(.16) |
| Schooling | 5.1<br>(3.4) | 4.8<br>(3.6) | 1.4<br>(2.1) | 4.0<br>(3.5) |
| Nutrition | .60<br>(.15) | .74<br>(.17) | .51<br>(.17) | .62<br>(.19) |
| Therapeutically-treatable disease | .31<br>(.46) | .38<br>(.49) | .30<br>(.46) | .33<br>(.47) |

| | | | | |
|---|---|---|---|---|
| Parasites | .41 | .55 | .50 | .48 |
| | (.49) | (.50) | (.50) | (.50) |

[a] Fertility is current number of living children divided by the Boulier-Rosenzweig (14) control for international fertility patterns given age of first cohabitation and years since. Number of siblings is measured in number of people. Age is in years. Other income and predicted earnings are measured in 1000's of cordobas per fortnight (at the time of the survey, 7 cordobas equaled 1 U.S. dollar). Schooling is grades completed. Nutrition is the proportion of international standards for calories. All other variables are dichotomous with values of one in the indicated state and zero otherwise. For much more detail concerning these distributions see (31).

Table 2. Fertility regressions for central metropolis, other urban, rural and combined sample for Nicaragua in 1977 with selectivity control and Boulier-Rosenzweig control for incomplete fertility.[a]

| Right-hand side variables | Complete Model | | | | Model with Linear Terms Only | | | | Complete Model Combined Sample with Region-Specific Interaction Terms[h] |
|---|---|---|---|---|---|---|---|---|---|
| | Central metropolis | Other urban | Rural | Combined | Central metropolis | Other urban | Rural | Combined | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| 1. Response selectivity | -.828 | -.658 | -.612 | -1.75 | -1.88 | -.676 | -.765 | -2.94 | -1.77 |
| | (1.6) | (1.0) | (1.5) | (4.0) | (4.3) | (1.0) | (2.0) | (7.4) | (4.0) |
| 2. Background[b] | | | | | | | | | |
| Father present in childhood | .085 | -.009 | .045 | .040 | .087 | -.013 | .046 | 0.36 | .015 |
| | (2.5) | (0.3) | (1.3) | (2.1) | (2.6) | (0.4) | (1.3) | (1.9) | (0.6) |
| Number of siblings | .001 | .005 | .007 | .002 | .003 | .009 | .007 | .003 | .002 |
| | (0.1) | (1.0) | (1.2) | (0.8) | (0.5) | (1.8) | (1.2) | (0.9) | (0.7) |
| Never migrated | .009 | .011 | -.070 | .003 | .027 | .000 | -.066 | .012 | .003 |
| | (0.3) | (0.4) | (1.7) | (0.2) | (0.8) | ([illegible] | (1.7) | (0.7) | (0.1) |
| 3. Marital status[c] | | | | | | | | | |
| Religious marriage | .028 | .038 | .141 | .011 | -.028 | .002 | .130 | -.055 | -.008 |
| | (0.6) | (0.9) | (3.0) | (0.4) | (0.6) | (0.0) | (2.8) | (2.0) | (0.2) |
| Common law | .003 | .009 | .089 | -.001 | -.004 | .027 | .098 | -.010 | -.008 |
| | (0.1) | (0.3) | (2.1) | (0.0) | (0.1) | (0.7) | (2.3) | (0.4) | (0.3) |
| 4. Income | | | | | | | | | |
| Other income | -.029 | .001 | .001 | .000 | -.059 | -.013 | .001 | -.004 | -.001 |
| | (1.2) | (0.1) | (0.3) | (0.1) | (2.6) | (0.6) | (0.1) | (1.0) | (0.3) |
| Own-farm | | | .081 | .092 | | | .073 | .080 | .057 |
| | | | (2.2) | (2.7) | | | (2.0) | (2.4) | (1.5) |

Table 2: continued

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| 5. Human capital | | | | | | | | | |
| 5a Linear terms[d] | | | | | | | | | |
| Predicted earnings | -.279 | -.039 | -1.46 | -.151 | -.246 | -.043 | -1.26 | -.125 | .024 |
| | (1.9) | (0.3) | (1.8) | (1.6) | (2.0) | (0.3) | (1.8) | (1.6) | (0.2) |
| Schooling | .083 | .084 | .055 | .068 | .013 | .006 | .015 | .001 | .074 |
| | (3.6) | (4.8) | (1.8) | (5.2) | (1.9) | (0.9) | (1.5) | (0.3) | (5.4) |
| Nutrition | -.056 | -.112 | .365 | -.044 | -.119 | -.116 | .135 | -.201 | -.052 |
| | (0.3) | (0.4) | (2.0) | (0.4) | (1.0) | (1.2) | (1.1) | (3.6) | (0.5) |
| Therapeutically-treatable disease | -.058 | -.077 | -.038 | -.067 | -.039 | -.025 | -.041 | -.052 | -.069 |
| | (1.0) | (1.5) | (0.9) | (2.3) | (1.1) | (0.8) | (1.1) | (2.6) | (2.3) |
| Parasites | .050 | -.046 | -.026 | .013 | -.028 | -.047 | -.028 | -.017 | .009 |
| | (0.8) | (0.9) | (0.7) | (0.5) | (0.8) | (1.6) | (0.9) | (0.9) | (0.3) |
| 5b Multiplicative terms[e] | | | | | | | | | |
| Schooling x age | -2.62[g] | -3.25[g] | -1.44[g] | -2.51[g] | | | | | -2.67[g] |
| | (3.6) | (6.0) | (1.4) | (6.2) | | | | | (6.0) |
| Schooling x therapeutically-treatable disease | 10.4[g] | 11.3[g] | 2.68[g] | 9.33[g] | | | | | 9.56[g] |
| | (1.1) | (1.4) | (0.2) | (1.8) | | | | | (1.8) |
| Schooling x parasites | -20.1[g] | 4.51[g] | -2.81[g] | -8.87[g] | | | | | -2.80[g] |
| | (2.2) | (0.5) | (0.2) | (1.7) | | | | | (0.4) |
| 5c Spline terms[f] | | | | | | | | | |
| Schooling (≥ 6 grades) | .026 | -.096 | -.172 | -.004 | | | | | -.010 |
| | (0.4) | (1.8) | (0.8) | (0.1) | | | | | (0.3) |

Table 2: Continued

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Schooling (≥6 grades) x age | -.475[g] (0.2) | 3.44[g] (1.9) | 6.08[g] (0.8) | .530[g] (0.4) | | | | | .869[g] (0.7) |
| Nutrition (≥ .5 international standards) | .099 (0.3) | .013 (0.0) | -.626 (1.7) | -.108 (0.7) | | | | | -.125 (.7) |
| 6. Constant | .803 (6.5) | .880 (5.6) | .704 (6.6) | .869 (11.8) | .268 (10.4) | .837 (9.5) | .783 (8.5) | 1.00 (19.1) | .868 (11.2) |
| $\bar{R}^2$ | .055 | .038 | .028 | .053 | .029 | .000 | .028 | .030 | .054 |
| SE | .574 | .478 | .478 | .518 | .582 | .487 | .478 | .524 | .518 |
| Degrees of freedom | 1396 | 1111 | 883 | 3428 | 1402 | 1117 | 889 | 3434 | 3417 |

[a]Beneath the point estimates in parentheses are the absolute values of t statistics.

[b]In a more expanded version dummy variables for the presence in childhood of the mother and for urban residence during childhood were included. However, neither of these variables had significantly nonzero coefficient estimates at the 10% level, nor did dropping them substantially alter any of the other estimated coefficients. Therefore, they are not included in this table in order to keep the results more compact.

[c]A comment parallel to that in note b applies for a dummy variable for having cohabitated previously with someone other than the woman's current companion.

[d]A comment parallel to that in note b applies for having had a medically preventable disease.

Table 3
Implications of complete model for direct impact of one grade more of women's schooling on fertility[a]

| Values of age and of human capital variables | Central metropolis | Other urban | Rural | Combined |
|---|---|---|---|---|
| All at sample means | .001 | -.006 | .012 | .007 |
| All at sample means except: | | | | |
| no disease | .006 | .000 | .012 | .008 |
| schooling $\geq 6$[b] | .013 | .000 | .020 | .019 |
| age = 20 | .022 | .025 | .026 | .017 |
| age = 40 | -.027 | -.040 | -.003 | -.037 |

[a] These are the total estimated marginal schooling coefficients implied by all the direct effects (but not including indirect effects, such as through predicted earnings) of schooling in the regressions in the first four columns of Table 3. All relevant variables are valued at the sample means from Table 1, except as noted in the left-hand column.

[b] Since the sample means for schooling are below completion of grade 6, the spline terms do not enter into the above figures, but they are included in this row.

Table 4

Probit estimates for completed responses for fertility data for regional and combined samples[a]

| Variables | Central metropolis | Other urban | Rural | Combined sample |
|---|---|---|---|---|
| 1. Woman's human capital | | | | |
| Woman's predicted earnings | -.03 (0.0) | 1.6 (0.5) | 18.0 (1.8) | -.15 (0.5) |
| Schooling | -.00 (0.0) | .03 (0.4) | -.04 (0.4) | .02 (0.5) |
| Nutrition (calories) | .27 (0.3) | .45 (0.5) | -.25 (0.2) | .74 (1.8) |
| Had therapeutically treatable diseases | .25 (1.1) | .07 (0.2) | .52 (1.1) | .21 (1.4) |
| Had parasitic diseases | -.52 (2.5) | -.13 (0.5) | .16 (0.5) | -.20 (1.5) |
| 2. Other income | .64 (2.0) | .15 (0.3) | .48 (1.1) | .36 (1.9) |
| Own-farm | | | .44 (1.0) | .38 (1.1) |
| 3. Background | | | | |
| Male raiser present | .17 (0.7) | -.17 (0.4) | -.67 (1.3) | -.06 (0.3) |
| Never migrated | -.17 (0.8) | -.20 (0.6) | .37 (0.9) | -.11 (0.8) |
| Urban upraising | .20 (0.6) | -3.0 (0.1) | .28 (0.7) | -.03 (0.2) |
| Number of siblings | -.00 (0.1) | .01 (0.2) | .01 (1.8) | .03 (1.1) |
| 4. Marital status | | | | |
| Religious marriage | .48 (1.4) | .26 (0.1) | 3.0 (0.2) | .69 (2.5) |
| Common law marriage | .26 (1.0) | .21 (0.7) | -.64 (1.5) | -.13 (0.9) |
| Previous cohabitation | 0.4 (0.2) | -.65 (1.6) | -.10 (0.3) | -.13 (0.9) |

Table 4 (continued)

| | | | | |
|---|---|---|---|---|
| 5. Age | -.19<br>(1.3) | -.01<br>(0.0) | -.30<br>(1.3) | -.09<br>(1.1) |
| Age squared | .002<br>(1.0) | .000<br>(0.1) | .004<br>(1.1) | .001<br>(0.8) |
| 6. Constant | 5.43<br>(2.2) | 5.14<br>(0.1) | 5.63<br>(1.5) | 3.44<br>(2.7) |
| -2 log likelihood ratio | 38.2 | 18.7 | 30.8 | 52.9 |
| Number of observations | 1436 | 1139 | 913 | 3488 |
| Number of non-responses | 21 | 9 | 10 | 40 |

[a] Beneath the point estimates in parentheses are the absolute values of the asymptotic t statistics.

## APPENDIX: NONRESPONSE ON FERTILITY AND SELECTIVITY BIAS

Forty of the 3488 women in the sample did not respond to the fertility question. In most previous studies such nonresponses either have been dropped from the sample or have been assigned a value of zero birth in the El Badry (17) procedure. If the nonrespondents are not randomly selected, either procedure may cause a selection bias in the estimated coefficients for the fertility relation.

We now demonstrate the formal possibility of such bias. The fertility relation that we wish to estimate for the $i^{th}$ household is:

$$N_i = Q_{1i} G_1 + V_{1i},$$

where $N_i$ is completed fertility for the $i^{th}$ household, $Q_{1i}$ is a vector of coefficients to be estimated, and $V_{1i}$ is the disturbance term for the $i^{th}$ household. However the fertility data that are necessary to estimate this relation are not observed for all households. There is a selection rule that gives the probability of providing the fertility data as a function of the respondent's characteristics and background variables $(Q_{2i})$:

$$R_i = Q_{2i} G_2 + V_{2i},$$

where $R_i$ is an indication of whether or not complete data are provided. The $i^{th}$ household responds to the fertility question if and only if $R_i$ has a positive value. The selection rule for responding, therefore is

$$R_i > 0 \text{ or } V_{2i} > -Q_{2i}G_2.$$

Fertility can be estimated only for the households which satisfy this selection rule. In other words, for which

$$E\ (N_i \mid R_i > 0) = Q_{1i}\ G_i + E\ (V_{1i} \mid R_i > 0).$$

If the expectation of the disturbance term in the fertility relation conditional on the selection rule for responding (i.e., the last right-side term in this relation) is not zero, simply dropping incomplete observations or assigning them all a zero value as in the El Badry (17) procedure may cause biases. Such practices are equivalent to excluding the last term in this relation and may cause a selectivity bias that is akin to omitted variable bias from excluding the conditional expectation.

To our knowledge, no one who has estimated fertility determinants has controlled for such selectivity bias, except for our initial exploratory study in which we do find some evidence consistent with substantial response selectivity bias for a subsample of our central metropolis respondents (4). Yet selectivity bias in regard to providing data is a real possibility. A priori we think it most likely that women who are formally married, who have more education and other forms of human capital, and who come from better economic backgrounds are more likely to provide data. Therefore simply dropping nonrespondents from the sample or assigning them a zero value is not likely to be random with respect to the fertility relation.

To deal with this aspect of incompleteness in our data, we apply the

Heckman (19) procedure for selectivity problems.[36] First a probit relation for the probability that there are complete fertility observations for the $i^{th}$ household is estimated. From this probit estimate is calculated the inverse of the Mill's ratio ($\lambda_i$) which is used to control for the selection decision about reporting fertility data since the previous relation can be rewritten as

$$E\ (N_i \mid R_i > 0) = Q_{1i}\ G_i + \frac{\sigma_{12}}{(\sigma_{22})^{1/2}} \lambda_i.$$

The estimate of the inverse of the Mill's ratio as a regressor was included in the ordinary least squares estimates of this relation in Table 2 under the assumption that there also is an additive conditional disturbance term with desirable properties.

Table 4 gives the first-step probit estimates for each of the three regions and for the combined samples. $\chi^2$ tests indicate that, except for other urban areas, the relations are significant at the 5% level and that they differ across regions. Thus nonresponse is not random, but systematically reflects some of the hypothesized characteristics. The significant (at least at the 10% level) coefficient estimates suggest that women with more human capital (ie., less parasitic diseases in the central metropolis, higher predicted earnings in the rural areas, and better nutrition in the combined sample), more siblings (in rural areas), and in religious marriages (in the combined sample) are more likely to provide fertility information. This pattern generally is in accord with our expectations. The estimated inverses of the Mill's ratios calculated from these probits are included in our fertility estimates in Section 3 to control for selectivity.

## NOTES

1. In addition to fertility, mortality and migration also are important in determining population growth in the devloping countries and also have been studied more extensively recently (e.g., 8, 27), though not as much as fertility.

2. Williams (24) surveys many of these estimates. The World Bank (33) gives a more recent, though less extensive summary.

3. Questions about a wider representation of human capital arise in (4, 16, 20), about nonlinearities in (1), about interactions in (33), about regional differentials in (18), about selectivity in (4), and about the role of tastes in (7, 15, 16).

4. Although recently population growth rates in many developing countries have declined significantly, those in pre-revolutionary Nicaragua remained very high at 3.3% per year, a rate exceeded in only 1 of the 90 developing countries included in World Bank (33).

5. For further information concerning these data and other studies undertaken with them see (3-12, 26-32).

6. This possibility has long been emphasized by Leibenstein (20), among others. For some support of such an effect in this sample, see (3, 6, 10, 12).

7. For example, a couple may not perceive the joint impact of household decisions regarding nutrition on their fertility through the biological birth function in addition to the direct satisfaction of their consumption needs for food.

8. For a subset of the central metropolitan sample we have explored some alternatives in (4).

9. For more details see (3, 6, 10, 12).

10. In our other studies we present evidence supporitng some of these effects of schooling on information (7), household efficiency (5, 7, 27, 28), and tastes (5, 7, 9, 29) and as a proxy for genetics (9, 29) and for nonobserved family backgound effects (9, 29).

11. We assume that grades of schooling are determined recursively by family characteristics. For supporting evidence see (9, 30).

12. We focus on the caloric input because of the recent emphasis on it being the best single measure of nutrition inputs (e.g., 33). We also have used as an alternative a similar measure based on protein input which is highly correlated with the caloric input (the correlation coefficient is 0.90 for this sample).

13. See (5, 28) which also provide more information about this variable.

14. See (26), which also provides more information about these health

indices.

15. Recent studies for both developing and developed economies suggest that in socioeconomic studies, schooling frequently serves as a proxy for omitted family background controls for family-related ability and motivation (9, 29).

16. For a study of the extent of and of the determinants of migration for this sample, see (8).

17. Common-law unions seem to be associated with lower income and more traditional backgrounds. See (31).

18. Prima facie it might seem that common-law unions would be less permanent than more formal ones, which might be offsetting in so far as permanent income affected fertility. However in our sample those currently in common-law unions are less likely to have had previous unions than those in religious or civil unions.

19. The F statistic is 1.05, with a 5% significance point of 1.57. The estimates in the last column of Table 2 give an alternative illustration of the insignificance of regional differences. In the regression the complete model for the combined sample is supplemented by allowing for the apparent region-specific differences noted in the previous paragraph in the text. But the estimates indicate no case in which these differences are significantly nonzero at the 5% level and only three cases (ie., larger for father present in central metropolis; smaller for schooling but larger for schooling x age in rural) in which these estimates are significantly nonzero at the 10% level. Moreover an F test indicates that this regression is not significantly different at the 5% level from that in column 4.

20. As Maddala (21, pp. 190-194) discusses, the standard "solutions" for multicollinearity (ie., dropping variables, using extraneous estimates, using ridge regressions, using ratios, using principal components, getting more data) are not very helpful for our situation.

21. This blanket qualification is not repeated over and over below.

22. We find support for the Thadani and Todaro (23) demographic marriage market inducement for female migration in (8). Also see the next note regarding possible indirect effects of migration on fertility through predicted earnings.

23. But regressions with only the background variables included do not provide much support for these indirect roles being very large. The only additional coefficient estimate which is significantly nonzero even at the 10% level in such regressions is the number of siblings in other urban areas (which also is significantly nonzero at the 10% level in the linear model in column 6). However, our estimates in (6) imply that never migrating is associated with a significant increase in predicted earnings in the central metropolis (because of having better contacts in the relatively renumerative labor amrket there and having more "modern" work attitudes?) and a significant decrease in predicted earnings in the rural region (because of limited options and more traditional work habits in many rural areas for those who do not migrate?). These estimates imply an indirect effect on fertility through the predicted

earnings variables of never migrating of -.014 for the central metropolis and .005 for the rural areas (at the point of means for the predicted earnings variables in Table 1).

24. These conjectures are difficult to test because of their dependence on unobserved variables regarding norms for children and disutility costs of contraception. In regard to the income variables, however, bivariate correlations for the rural sample are consistent with such possibilities. The correlations between other income and these marital status variables is most negative for common-law marriages, also negative for religious marriages, and positive for other marital arrangements.

25. In the linear version the coefficient estimate of the religious marriage variable is significantly negative for the combined sample. Examination of the bivariate correlations suggests that this result may reflect omitted variable bias due to the exclusion of nonlinear terms involving schooling.

26. In the linear version the coefficient estimate of the other income variable is significantly negative for the central metropolis. Examination of the bivariate correlations suggests that this result may reflect omitted variable bias due to the exclusion of nonlinear terms involving schooling.

27. See the previous two notes.

28. There are not significantly nonzero coefficient estimates for interactions and spline terms involving this variable.

29. Also the latter is significantly nonzero only at the 10% level, but the others are significantly nonzero at the 5% level.

30. The estimated earnings functions are from (6). They are in semilog form, so the partial derivative of predicted eanrings with respect to schooling is the product of the coefficient of schooling in the earnings function and the level of predicted earnings. The indirect effects in the text are calculated for mean predicted earnings For higher predicted earnings (e.g. due to more schooling) the indirect effects would be larger since the predicted earnings would be greater.

31. A priori one might think that better nutrition would increase fertility through reducing subfecundity. However, Bongaarts (13) argues that empirically such an effect is very small. In our earlier study for a smaller sample from the central metropolis we found no evidence to support such an effect (4). In the present study only the estimates for the very poorly nourished rural households are consistent with such a possibility.

32. In the linear model there also is a significantly negative coefficient estimate in the combined sample for this variable.

33. However, we find no evidence of a significant impact of medically-preventable diseases which 42% of the women in the overall sample report.

34 We report similar results in (4).

35. For other evidence suggestive that schooling may represent other than

efficiency effects in a variety of contexts, see (7, 9, 29, 30).

36. An alternative approach would be to use a nonlinear maximization procedure to estimate the parameters that maximize the likelihood of obtaining complete fertility observations for the subsample of 3488 women and incomplete observations for the other 40. We choose the Heckman procedure because it is computationally easier and still consistent, although less efficient than the maximum likelihood alternative.

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