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Program Effects and the Allocation of Resources Within the Household

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I. Introduction

In recent years there has been increased interest in the measurement and evaluation of program effects. In the economics literature the primary focus has been on addressing potential ecoonometric issues such as those arising if programs are not randomly placed with respect to the outcome variables of interest or if the program importantly influences the composition of the resulting population (see., e.g. Pitt and Rosenzweig 1989; Pitt, Rosenzweig and Hassan 1993; Gertler, Molyneaux and Hatmaji 1993). An alternative, and more straightforward approach is to evaluate the effects of an experimentally introduced intervention which, in principle, provides a direct estimate of the effects of the program intervention but may provide little insight into underlying mechanisms and therefore into potential long-run effects of the program.

In this paper, I argue that the approaches are complementary. In order to illustrate this point I provide three examples from rural areas of developing countries. These examples show that (1) inferences about program effects based on treatment comparison data can be misleading if fertility selection is ignored; (2) that insight into the long-run effects of a program can be gained even with a relatively short experiment; and (3) it is possible to distinguish the mechanism through which health programs affect schooling even when the program affects the health of all household members.

II. Theoretical Framework

The theoretical framework for this paper is provided by a dynamic stochastic model of household behavior. At the heart of the model is the idea that a household makes choices sequentially that influence household size (through fertility and mortality) as well as the consumption and human capital (health and schooling) of its members. In particular,

$$\sum_{t=0}^{T} u(c_{it}, h_{it}, n_{it}, i \in [0,\omega]) \tag{1}$$

where c_{it} and h_{it} are the consumption and human capital, respectively of individuals age i at time t and n_{it} is the number of individuals age i at time t. Stocks are updated in a straightforward fashion. If s_{it} is per capita human capital investment in those aged i at time t and d_{it} is the number of deaths at time t, then we may write

$$h_{i+1t+1} = h_{it} + s_{it} \tag{2}$$

and

$$n_{i+1t+1} = n_{it} - d_{it} \tag{3}$$

for all i>0. It is assumed that $d_{it}=d_i(h_{it},n_{it},\eta)$, that is the number of deaths of those aged i at time t depends on human capital, the number of individuals and a household endowment; I also assume that deaths in period t are resolved after that period's human capital investment decision is made. The number of aged 0 individuals in the household is assumed to be determined by a simple *reproduction* function that depends on fecundity μ and contraceptive use z_t in that period

$$n_{0t} = \mu - z_t \tag{4}$$

Finally, I assume that there is no saving in any period so that the single period budget constraint may be written:

$$\sum_i (p_{ct}c_{it}+p_{st}s_{it})n_{it}+p_{zt}z_t=\sum_i w_{it}h_{it}n_{it} \tag{5}$$

where w_{it} is the wage per efficiency unit, where human capital is equated with relative efficiency in the labor market. The wages, prices of consumption p_{ct}, human capital investment p_st, and contraception p_{zt} are assumed to be distributed independently over time. Under these conditions, it is well known that households decisions in each period (as well as outcomes that result from household decisions in that period such as mortality) can be written as a function of the number and human capital of household members by age at that time as well as contemporaneous wages and prices. Thus we may write:

$$K=K(\{h_{it},n_{it},w_{it},i\in[1,\omega]\},p_{zt},p_{ct},p_{st},\mu,\eta) \tag{6}$$

for K={c_{it},s_{it},z_t,d_{it+1}, n_{0t}).

There are two important features of equation (6). First, it is in principal something that can be estimated directly. Although there are important econometric problems that arise because of the fact that many of the state variables are chosen by the household and thus are likely to be correlated with μ and η, which will in general be unobservable, these problems can be dealth with through a combination of fixed-effects methods and instrumental variables. Second, these decision rules capture complex relationships between, for example, the history of past prices and current decisions in a relatively simple way: the past history of prices only influences current decisions to the extent that it affects the current stock of individuals and their human capital.Both of these features will be incorporated in the examples discussed below.

III. Illustrations

A. Fertility Selection and Health Program Effects

The first example comes from the Matlab study area in rural Bangladesh, which has been the sight of a number of important health and demographic studies since 1966.[1] Of particular interest in this regard is the maternal child health and family planning project (MCH-FP) that was set up in 1978 on a treatment-control basis. Although logistical and other considerations precluded the random allocation of programs to households, a careful selection of treatment and comparison areas provides an imperfect but reasonable approximation to a true experimental program.

The results from Matlab are widely thought to provide good evidence that a carefully designed and administered program to deliver family planning services can have an important impact on fertility in a low-income rural setting (Menken and Phillips 1990). The program involves frequent household visits by female family planning workers providing in-home access to a variety of contraceptive methods along with careful management of side effects. Starting from the 1982, maternal and child health services were integrated into the program in subregions of the intervention area. The comparison area received regular government services, which involved the use of poorly supervised male workers and a limited menu of methods that were inconsistently available. While some methods would have been available at subsidized prices through local clinics and and/or pharmacies in the comparison area, restrictions on travel for women in this conservative, largely muslim, area substantially limited access to the most popular methods such as injectables.

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[1]Menken and Phillips (1988) provide a lucid overview of the principle demographic insights gained from analyses of data from this area.

Following the introduction of the Matlab project in 1978, contraceptive prevalence rates in the treatment area increased from 7 percent to 20 percent and reached 33 percent after 18 months of project intervention. A new plateau of 45 percent was reached approximately six years following the introduction of the program (Phillips *et al.* 1988) and the level was 57.6 percent in 1990. The contraceptive prevalence rate in the comparison area was only 15.8 percent in 1984 and had risen to 27.9 percent in 1990 (Keonig *et al.* 1991). Changes in fertility rates were equally dramatic. While the total fertility rate in the treatment area before the start of the program was 6.2 percent lower than that in the comparison area, this difference is not significant at the 5 percent level. The year following the introduction of the program fertility was 25 percent lower in the treatment than in the comparison area with differences of 48.7 and 55.6 percent in the 35-39 and 40-44 year age groups respectively (Phillips *et al.* 1981, p23).

While these dramatic effects of the program on fertility are well known, it is perhaps less well known that the measured effects of the treatment program on mortality were relatively small until 1982 when an intensive maternal and child health program including measles immunization was increasingly integrated into the treatment area. It is this issue that I consider below: in particular, I ask whether the lack of a substantial emerging differential in mortality reflects the fact that the intensive family planning services and low-level maternal and child-health services (including tetanus toxoid) had little impact on mortality risk for children.

A potential competing hypothesis is that the composition of births was importantly influenced by the introduction of the treatment program.[2] The idea is simple: if those who chose to make use of the family planning program were, for some reason, on average of lower mortality than those who did not make use of the program, then one might see little differential in average mortality rates for treatment and control areas after the introduction of the program even if the effects of the program on a particular child's mortality risk were quite large.

In the context of the above theoretical model, this issue is most easily addressed by allowing for women-specific heterogeneity in mortality risk and assuming that fertility is decreasing in the stock of surviving older children.[3] The idea is simply that for a given level of fertility high risk women will be less likely to make use of family planning services if they become available because they will have fewer surviving children at each point in time. I use a simplified version of the decision rule (6) in which the cost of contraception and human capital (i.e., health care) are prohibitive (i.e., effectively infinite) in the first period and differ by area in the second period and the resulting childbearing decision rules and mortality outcomes may be written :

$$\begin{gathered} n_{01}=\mu \\ n_{12}=\mu-\eta_j \\ n_{02}=\mu-n_{12}/p_{z2} \end{gathered} \tag{7}$$

and

[2]See Pitt and Rosenzweig (1989) for a technical discussion of fertility selection as well as more sophisticated methodologies for addressing the problem than that used in this paper.

[3]Evidence on this latter assumption will be presented in the second example.

$$d_{01}=\eta_j$$
$$d_{02}=\eta_j-1/p_{s2} \tag{8}$$

respectively, where η is given a subscript j to indicate that there is permanent variation across women in mortality risk for her children. Under these conditions it is easily seen that the death rate in the second period among women with a birth ($n_{02}>0$) may be written

$$E(d_{02}|n_{02}>0)=E(\eta|(\eta>(1-p_{z2})\mu)-1/p_{s2}>=E(\eta)-1/p_{s2} \tag{9}$$

where the inequality (equality) is strict if p_{z2} is not (is) prohibitive:[4] thus even if the treatment program lowers p_{s2}, mortality in the second period in the treatment area may not be lower than that in the comparison area which will continue to have a mortality rate of $E(\eta)$. It is also easily seen that the difference $(d_{02}-d_{01}) =-1/p_{s2}$ measures the reduction in the cost of human capital directly: thus a comparison in the mortality change for women in the treatment area who have births in both periods with that for women in the comparison area will provide an accurate picture of how much the program reduced mortality of a particular child.

An illustration of this point is provided in Table 1. In this case all births over the period 1974-1982 from the Matlab vital registration records are linked to deaths in the first year of life as well as socio-economic information from the 1974 census. We first carry out a logisitic regression of the probability of death on a treatment area dummy, year of birth and an interaction of the treatment area with the dummy indicating the

[4]In this case prohibitive means so large that it does not influence the conditional distribution of η.

birth was after 1978; this latter variable picks up births that took place in the treatment area after the introduction of the MCH-FP program. The second column includes these variables as well as a number of socio-economic measures. The main point to be noticed is that the treatment area-interaction is positive and not significant--suggesting that the program had little effect on infant mortality and that controlling for a few observed characteristics does not change this basic conclusion.

The third column in Table 2 contains fixed-effects logit (Chamberlain 1988) estimates that condition on women who had a birth both before and after 1978. This estimator, given our model, provides an accurate picture of the true effect of the treatment program on infant mortality.[5] The results are striking--there is a strong and significant effect of the treatment program on mortality. The coefficent of .21, given the average infant morality of 110/1000 in the data, implies a reduction in mortality of approximately 18%. While given the simplicity of the specification, one ought to be cautions about direct interpretation of this figure, the main prediction of the model--that selectivity may be resulting in an underestimate of the effects of the treatment program on mortality--is strongly supported.

B. Inferring Long-run Effects from Short-run Data

The second example also focusses on the MCH-FP program in Matlab--in particular it explores the question of whether the treatment program has had an effect on the level of schooling provided to children and if so what the longer-run

[5]The reason the sample size is so small is that the fixed-effects logit procedure only makes use of women who (1) had a birth both before and after and (2) experienced a death in one period but not in the other. Thus despite the fact that fewer cases available, the number of deaths represented is somewhat smaller than is the case for columns (1) and (2) but not substantially so.

implications of the program for changes in levels of schooling are likely to be.[6]

Although it has long been thought that family planning programs in developing countries can have an impact on human capital formation and thus be a contributor to the process of economic growth, the evidence has thus far been extremely thin. Despite the negative cross-sectional relationship that is generally observed between family size and educational attainment, it is not obvious that the inability to control reproduction is an important barrier to increased education in low-income countries. Researchers in this area have been troubled by a number of econometric problems including the endogeneity of program placement, the absence of direct measures of the costs and returns to childbearing, and the long period that must be used to relate changes in reproductive behavior to changes in human capital formation.

Moreover, in part because of the difficulty associated with obtaining direct estimates of the cost of fertility control, little is known about the relative importance of different mechanisms underlying the schooling-fertility tradeoff. For example, if schooling costs are high and families cannot transfer resources across time then a decrease in the cost of fertility control may affect schooling by leading to a reduction in the number of children who are school-aged at any particular point in time. Alternatively, if school-aged children provide an important source of income then an increase in the number of school-aged children might increase the level of schooling provided to each. In this case, a decrease in the cost of fertility control might increase schooling through a reduction in the number of preschool children who increase consumption needs, provide little additional household income and may require care

[6] This section summarizes Foster and Roy (1993).

on the part of older siblings. A similar point can be made with respect to the implications of the schooling-fertility tradeoff for allocations of schooling by sex. Girls may receive lower schooling than boys because (1) parents perceive that the returns to schooling for girls are low (2) households with school-aged girls may be more likely to have young children in the household

To illustrate these issues I again turn to a simplified model in which there are two childbearing ages. I also assume that each period is approximately 8 years in the length so that the schooling of first "batch" of children is determined simultaneously with the number of children in the second "batch". Thus childbearing in the first period is

$$n_{0t}=\mu+\gamma_2 p_{zt} \tag{10}$$

and the schooling provided to those children is

$$s_{1t+1}=-\alpha_1 n_{1t+1}-\alpha_2 p_{zt+1} \tag{11}$$

where the negative coefficient on n_{1t} reflects the assumption that an increase in the number of school aged children may decrease the average schooling recieved by each and the negative coeficient on p_{zt+1} reflects the fact that the cost of fertility control will influence childbearing, which will also may influence the level of schooling provided to older children. The number of children born in the second period will also reflect these variables:

$$n_{0t+1}=-\gamma_1 n_{1t+1}+\gamma_2 p_{zt+1} \tag{12}$$

and they will be provided with a level of schooling:

$$s_{1t+2} = -\alpha_0 n_{2t+2} - \alpha_1 n_{1t+2} \tag{13}$$

It should be clear that given parameter estimates for each of these equations (or a generalizaiton of these equations to more than two periods) it would be possible to determine pattern of schooling and human capital for any possible history of prices, and thus to determine the long-run effects, for example, of a permanent reduction in the cost of family planning services.

One to the estimation of the parameters in equations 9-13 would of course be to solve them completely and thus to obtain equations relating schooling, say, in period t+2 to the full history of past prices (in this special case prices in periods 1 and 2). The problem with this approach is that it demands a great deal of the data--one needs access to information on the cost of contraception over the full history of a woman's reproductive career. In Foster and Roy (1993) it is suggested that a more attractive procedure is to estimates these equations directly, in which case one needs only information on subset of a woman's reproductive career (i.e., about 8 years) as well as the stocks of children she has in each period.

The estimates of these equations were obtained from a combination of the 1974 and 1982 censuses from Matlab and the 1990 KAP survey. The KAP survey is of a stratified sample of 8500 currently married, reproductive-aged women with approximately equal numbers of women in the treatment and comparison areas (Keonig *et al* 1991). Included in this latter survey is information on educational attainment of children and distance to school. The sample used in the analysis is made up of women from the 1990 survey along with all women of reproductive ages from the

1974 and 1982 censuses, who were, at the time of the respective census, resident in one of the 67 villages that were part of the sampling frame of the 1990 survey. While the 1990 survey contains information on the ages, sexes and educational attainment of children under the age of 15 at the time of the survey, the censuses may only be used to construct such measures for those who remain resident in the study area. Fortunately, the vital registration data indicate that migration for either boys and gilrs is unlikely before this age. Fertility (net of mortality) is measured by the number of children under the age of eight in the appropriate census and schooling is measured by years of completed schooling at the date of the census.

Estimates of the education and fertility decision rules are presented in Table 2. First, the fertility decision rules are much as might be expected: each additional male child over the age of 8 results in a reduction of approximately .8 in the average number of children born in a given 8-year interval. The results are quite different for girls, a result that likely reflects parental aspirations for male children: an extra girl child results in on only a .07 reduction in subsequent childbearing--thus a woman with male school-aged (and older) children is more likely to limit her fertility than a woman with only female school-aged and older children. Third, the greater the level of schooling of older children in the household, the lower the level of fertility that is chosen. And most importantly, the treatment-year interactions indicate a substantial reduction in fertility by 1982 (4-years after the introduction of the program) and an even larger effect by 1990--given the average level of fertility in this population these results correspond to approximately a 25% decline in fertility in the treatment relative to the comparision area. .

The education decision rules are perhaps more unexpected. In contrast to the usual assumption that increases in the number of school-aged children results in lower schooling, we find here that an extra male child actually increases the average schooling of his siblings (regardless of sex) by .84 years, given that the mean level of schooling in this population in 1982 was only 2.4, this difference is of some importance. These results are consistent with the notion that school-aged children in this population may contribute importantly to household resources: an extra sibling means that each child can spend at least a little time at school. The point estimates for girls suggest that the effect is somewhat less: each additional girl results in only a .5 year increase in the average schooling of her siblings.

There is, however, also evidence of the more tradeoff between the number and schooling of children: given the current stock of school-aged children the MCH-FP program (as measured by the treatment-year interaction) resulted in a .317 year increase in average schooling in 1990. This result likely reflects the reduction in fertility that is a result of the family planning services:[7] in contrast to a school-aged child who can contribute importantly to household income, a pre-school child is likely to be a net consumer of household resources[8] (see, e.g., Cain 1977), and therefore to keep older siblings at home rather than in school. This pattern is also likely reflected in the fact

[7]Given the results from the previous section, it might be argued that this effect operates through the increased health of children in the treatment area. Foster and Roy (1990) construct a test using an indirectly calculated measure of fecundity that suggests that the fertility effect is dominant. In addition, results from the next section (from the Philippines) provide no support for the idea that the schooling of older children is importantly influeced by the health of the younger children.

[8]This net consumption may be in the form of nutrition, for example, as well as through the need to provide pre-school children with childcare.

that girls appear to contribute less to the level of schooling: the issue is not necessarily that girls contribute less to household resources but the fact that a household with school-aged children that are girls are more likely to go on and have additional children as reflected in the fertility decision rules.

Estimates of the fertility and education decision rules can then be used to obtain estimates of the long-run effects of the introduction of a family planning program through recursive substitution. By carrying out this substitution Foster and Roy (1990) report that for a woman who experienced the family planning program over her lifetime there would be an increase of 20% in mean schooling and a 30% decrase in mean fertility. The reason the long-run effect of the program on schooling is not much different from its short-run effect is precisely that a reduction in the number of school aged children results, *ceteris paribus* in a reduction in schooling.

C. Whose health affects schooling decisions?

The third example I will discuss involves the question of how to determine the mechanism by which an experimentally introduced health program influences levels of schooling. This example shares with the previous example the basic insight that a given health program will influence schooling through a number of different channels: in particular it may affect schooling through its effects on the health of adults (and thus for example resource availability in the household), through changes in the health of the school-aged children (and thus their ability to attend school), or through changes in the health of preschool children (who might then be more likely to require childcare). While it might be argued that it is sufficient to know that a health program affects schooling without knowing the particular channel through which it operates, it might nonetheless help one to determine how programs could be better designed to augment schooling.

As Pitt and Rosenzweig (1988) point out assessment of the mechanism through which access to health care affects decisions is difficult in circumstances in which the price of health care varies similarly for all members of the household. The problem may be thought of as one arising from a lack of sufficient instruments. Estimation of decision rules such as equation (6) which condition on the health of various household members must condition on the health of all household members--if, for example, the health of the father is left out of the decision rule, then the estimates of the effect of the health of a son on his own schooling would also partly reflect the correlation between the health of the son and the health of the father arising from the fact that they face the same prices. However, if the health of all members is included in the decision rule it is not clear, at least in the static case, what variables are ommitted from the decision rule other than the price of health care. Thus in a household with 4 individuals (or 4 types of individuals) there would only be a single available instrument (namely the price of health care). .

In a dynamic context such as that being discussed in this paper, this problem can be dealt with because the human capital of the different individuals in a particular period is affected by the full history of decisions made by the household. Net of the current state variables (e.g., the health of each household member, the number and composition of the household, and assets) this history does not influence current decisions, thus the history may be used to construct instruments. Moreover, over a relatively short period such as a number of seasons within the year for which household composition and household assets are likely to be fixed, the primary source of variation in the state variables will be seasonal variation in health: thus a fixed-effects instrumental variables procedure looking at the effects of changes in health status on changes in schooling decisions can be easily identified.

The data used to illustrate this point come from stratified random panel of 448 farming households in Bukidnon in northern Mindanao, Philippines. These households were interviewed in four rounds at four month intervals in 1984-85 as part of an International Food Policy Research Institute study by Bouis and Haddad (1990). In addition to detailed information in each round on illness, body size, and caloric intake, there is information on whether these children attend school and if so, how much school (if any) they missed school in the previous week. In the subsequent analysis we use as a dependent variable of the decision rule the share of school time missed and as a measure illness whether they reported themselves ill in the previous two weekss.

In order to accommodate variation in household size and structure individuals were categorized into one of four age groups and the average illness within the age group was computed. Thus the primary state variables of interest are the mean levels of illness by group; also the illness of the index child (i.e., the child whose school time is used as the decision variable) is included separately to capture any potential effects of own-illness on schooling arising from, say the inability to attend school, as opposed to the more general effects of illness on resource availability. The level of missed school in this population is high 21.7% of potential hours in school are missed on average in this population. Illness was not especially high, however, with an average of .90 days in the last two weeks spent ill; 10% of all adults were were ill in the previous two week period.

The results are quite instructive: the primary effects of illness on missed school operate through the health of the parent--while none of the other measures is signficant, the estimates suggest that if the adults in a household are all ill there is a

64.8% increase in the share of hours of school being missed. The implication is that the effects of programs, to the extent that they influence the health of individuals, primarily affect schooling through their effects on adult health. It appears that periodic illness of parents may be an important factor resulting in sporadic attendance at school. Unfortunately, the absence of changes in programs over the study period makes it difficult to assess whether health programs themselves can in fact have an effect on adult health, an issue that must be resolved before proper use can be made of the above results.

IV Conclusion

In this paper I have presented three examples in which the estimation of dynamic decision rules can importantly aid in the interpretation and understanding of the effects of health and family planning programs on human capital investment. My purpose in doing so is not to argue that the kind of detailed household level data used in this analysis can substitute for a quasi-experimental trial. The latter can provide invaluable insight into the design and implemenation of programs that can only be evaluated with great difficulty if at all with data collected based on pre-existing programs. Instead, my purpose is to suggest that policy makers be ecouraged to design and implement experimental trials of various types of programs, but in the process to be sure that sufficient resources are available to collect and link detailed household-level information. These detailed data can importantly increase the nature of the lessons that can be learned from the introduction of a carefully designed program experiment.

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Table 1
Logit and Fixed-Effect Logit Estimates of the Effects
of the Treament Program on Infant Mortality

	Logistic Regression		Fixed-Effects Logit
	I	II	III
Treat*1978+	.038 (.789)[a]	.020 (.404)	-.209 (2.61)
Treat	-.118 (3.23)	-.102 (2.82)	--
Year of Birth	-.067 (9.49)	-.067 (9.20)	-.065 (2.13)
Mother's Age x 10^{-2}	--	-.817 (3.92)	--
Land x 10^{-2}	--	-.149 (1.65)	--
Not in Census	--	.143 (1.86)	--
Constant	3.20 (5.85)	3.45 (5.87)	-.277 (5.15)

[a]Absolute asymptotic t-ratios based on Huber standard errors in parentheses

Table 2
Instrumental Variables Fixed-Effects Estimates of
Education and Fertility Decision Rules
(N=5567)

	Education		Fertility Decision Rule	
	Coefficient	T-ratio[a]	Coefficient	T-ratio[a]
Children 8-15[b]	.840	11.5	-.816	12.1
Girls 8-15[b]	-.330	2.98	.750	13.1
Children 15+	.706	5.56	-.841	4.86
Girls 15+	-.400	4.51	.760	9.15
Education 15+[b]	.201	4.33	-.089	2.82
Treat*1982	-.012	.148	-.276	3.26
Treat*1990	.317	2.40	-.453	2.29
Mother's Age	.384	1.34	-.571	2.74
1982	-2.37	2.33	1.89	4.87
1990	-4.05	2.03	3.84	5.21
Characteristics of child:				
Male	.412	3.02	--	--
Age	-.137	.396	--	--
Age^2	.024	1.52	--	--

Hypotheses	F-stat (df1,df2)	P-value	F-stat (df1,df2)	P-value
Girls Not Different	15.31 (2,5554)	.000	86.76 (2,5576)	.000
Children Effects the Same by Age	1.66 (1,5554)	.197	.05 (1,5576)	.828
Girl Effects the Same by Age	.23 (1,5554)	.629	.03 (1,5576)	.874

[a]Absolute t-ratios based on Huber standard errors
[b]Endogenous variable instrumented using initial period state variables, land ownership, head's education, and village-level characteristics

Table 4
IV Fixed-Effects Estimates of Missed Schooling Decision Rules

	I		II	
	Coefficients	T-ratios[a]	Coefficients	T-ratios[a]
Illness[b]				
Index Child	-.010	.062	--	--
Age 24+	.648	4.45	.458	5.10
Age 16-23	-.372	.844	--	--
Age 8-15	-.288	.619	--	--
Age 0-8	.111	.368	--	--
Round 2	-.070	2.294	-.055	1.70
Round 3	-.153	3.549	-.103	2.85
			F-test (df1,df2)	P-value
Joint significance of illness other tha.n that of the adults			.42 (4,582)	.791

[a]Absolute t-ratios derived from Huber standard errors
[b]Endogenous variables, instrumented using program and wealth interacted with round