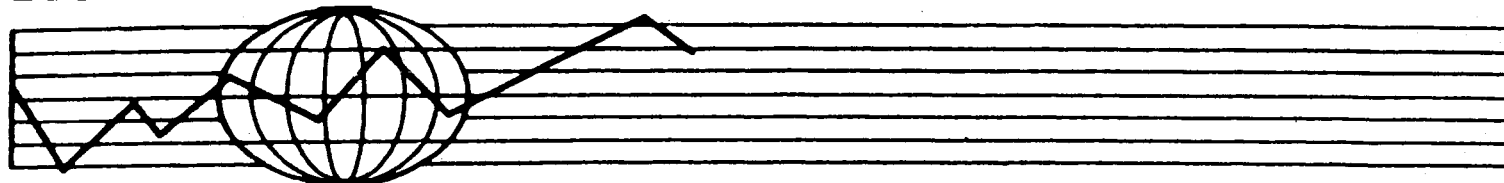


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**EXTERNALITIES, HETEROGENEITY AND  
THE OPTIMAL DISTRIBUTION OF PUBLIC  
PROGRAMS: CHILD HEALTH AND FAMILY  
PLANNING INTERVENTIONS**

Mark R. Rosenzweig

Kenneth I. Wolpin

**ECONOMIC DEVELOPMENT CENTER  
Department of Economics, Minneapolis  
Department of Agricultural and Applied Economics, St. Paul**

**UNIVERSITY OF MINNESOTA**



Externalities, Heterogeneity and the Optimal Distribution of  
Public Programs: Child Health and Family Planning Interventions

Mark R. Rosenzweig

University of Minnesota

Kenneth I. Wolpin

Ohio State University

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Considerable public resources in both high and low-income countries are devoted to the subsidization of fertility control and health investments. The effects of these programs are thus of some concern, and social scientists have devoted attention to the evaluation of these programs. Most evaluation studies (e.g., Hermalin (1972), Khan and Sirageldin (1979), Rosenzweig and Schultz (1981), Rosenzweig and Wolpin (1982)) have essentially compared the variation in measures of the intensity of program effort across localities with the corresponding inter-area variation in fertility and health. Little or no attention has been paid to the causes of the cross-area variability in the levels of such programs. Yet, if the allocation of public health and family planning services or subventions across localities is systematically related to factors determining fertility and health outcomes that are known to subsidy providers but unobserved by researchers, such cross-sectional estimates will produce misleading conclusions about program effectiveness.

Interest has grown recently in incorporating endogenous public resource allocations within models concerned with private agent behavior. Empirical applications or tests of such models, however, have been scarce and have been principally concentrated in the area of agricultural policy (Guttman (1978), Huffman and Miranowski (1981), Huffman and McNulty (forthcoming)). Moreover, existing general economic theories of public allocations do not provide much guidance for predicting how publicly-financed human capital subsidies and, in particular, "family planning" subsidies are distributed among heterogeneous recipients. Altruism theories of public transfers (Hochman and Rodgers (1969), Roberts (1984)) would appear to suggest that the least-endowed receive the highest transfers, but such models

provide no rationale for the use of subsidies to particular goods such as contraceptives. Pressure-group theory (Becker (1982)) suggests that groups that are (i) relatively small in number, (ii) have and can command resources for lobbying and (iii) derive the greatest benefits from public transfers or interventions will receive the highest transfers. This model would appear to imply that the rich--small in number and with greater resources--rather than the poor would receive the highest fertility control subsidies, since, assuming that the poor have the largest families and face the same prices, they avert less births than the rich and thus benefit least from subsidies to fertility control.

While the existence of externalities from health (infection) might provide a Pigovian (and pressure-group) rationale for the subvention of health investments among the low-health poor, the empirical and theoretical rationale for fertility control subsidization based on the existence of direct population externalities is less clear (Eckstein and Wolpin (1984)). Moreover, since a birth from any source contributes equally to population growth, the existence of population externalities (e.g., congestion) does not obviously provide a basis for selective subsidization of households by income or human capital endowments.<sup>1</sup> A model of health and family planning subsidies is needed.

In this paper we formulate and test an optimizing model determining the distribution of family planning and health subsidies across heterogenous households and assess the biases in cross-area estimates of the health effects of such subsidies due to public resource optimization. The model incorporates different features of the general theories of public allocations: the welfare of "donor" households is directly

but asymmetrically affected by the behavior of recipient households, as in altruism models; thus, recipients of subventions have an intrinsic advantage in obtaining resource transfers, as implied by competitive interest group models, but the distribution of subsidies is a function of price effects as well as recipient characteristics, as in optimal taxation models. The model is used to derive rules for the distribution of both health and fertility control subsidies and to discern the conditions under which subsidies to fertility control alone or in combination with health subsidies are optimal (Pareto-improving), without resort to arbitrary specifications of population externalities or altruistic concern for family size.

In section 1, the model is set out. It is shown that when there is a health externality, subsidization of fertility control can substitute pervasively for a health subsidy if and only if family size and health are gross substitutes. It is then shown that when only family planning subsidies are provided, such subsidies are likely to be distributed disproportionately to low-health households, resulting in underestimation of the health effects of family planning programs from cross-section data. In the next section, it is demonstrated that a combination of family planning and health subsidies are Pareto-improving in the presence of the health externality even when cross-price effects and fertility externalities are absent. It is then shown that the distribution of the two subsidies will be positively correlated across areas (even in the absence of administrative scale economics), but it is impossible to establish whether such subsidies, when jointly provided, will favor the least or most endowed of recipient households.

In section 2, longitudinal data describing child health and publicly-provided family planning and health programs in 20 barrios in Laguna Province in the Philippines are used to estimate the effects of such programs on child health and the relationships between the distribution of the programs and pre-program health levels, i.e., the governmental allocation rules. The results are consistent with the model: (i) dates of family planning and health program initiation across barrios are positively correlated, (ii) family size and health are gross substitutes among households and in some barrios family planning programs but not health programs are present; (iii) both programs were initiated earliest in the low-health barrios and (iv) as a consequence, the positive and significant child health effects of both the family planning and health programs are completely obscured when no account is taken of the systematic associations between program placement and areal health endowments.



1. Modeling the Distribution of Health and Family Planning Subsidies

a. Evaluating Subsidy Effects on Health Production Among Heterogeneous Households

Consider a set of  $T$  low-income households each residing in a different health environment. Each household  $i$  chooses a level of health for its children  $H^i$ , its family size  $N^i$  and its consumption  $Z^i$  solving the following problem:

$$(1) \quad \max U^i = U(H^i, N^i, Z^i),$$

where health production is described by the function

$$(2) \quad H^i = h(X^i, N^i) + \mu^i, \quad h_x^i > 0, h_{xx}^i, h_{NN}^i < 0,$$

subject to the full income constraint

$$(3) \quad F^i = p_N N^i + (p_c - s_c^i)(v^i - N^i) + (p_x - s_x^i) X^i N^i + p_z Z^i,$$

where  $X^i$  = per-child health input,  $\mu^i$  = exogenous, health parameter or endowment,  $F^i$  = full income net of taxes, if any;  $v^i$  = potential fertility in the absence of fertility control;  $p_K$  = price of good  $K$ ,  $K = N, X, Z$ ;  $p_c$  = cost of fertility control or averted births, and  $s_c^i$  and  $s_x^i$  are per-unit subsidies to fertility control and health inputs, respectively, provided in each health environment by a central "government" or donor.<sup>2</sup>

The solution for each household's average per-child health net of the environmental effect in terms of the exogenous variables unique to it is

$$(4) \quad H^i = H(s_x^i, s_c^i, F^i, \mu^i).$$

Estimation of (4) to obtain the average effect of the subsidy  $s_j^i$  on child health when  $\mu^i$  is unobserved yields the estimate:

$$(5) \quad \frac{dH^i}{ds_j^i} = h_x^i \frac{dX^i}{ds_j^i} + h_N^i \frac{dN^i}{ds_j^i} + \left(\frac{ds_j^i}{d\mu^i}\right)^{-1}, \quad j = x, c,$$

The true effect of a change in the subsidy  $s_j$  on the health outcome is given by the first two terms in (5): the subsidy (price) effects on the health input provided to each child and on family size weighted by their respective marginal health effects, from (2). The third term in (5) is the bias which arises when the  $\mu^i$  are unobserved by the researcher and vary with the subsidy rates. Only if the subsidies are distributed independently of the  $\mu^i$ , or, more generally, of any of the parameters unique to each area which influence health investments, will the association between the subsidies and health net of observed variables provide unbiased estimates of subsidy effects.

The sign of the bias in (5) will obviously depend on the allocation rules used by the agents who distribute the subsidies. If such agents follow a compensatory rule, for example, providing higher subsidies to less-endowed areas, then the subsidy effects obtained from (4) estimated, say, by least squares will understate the true consequences of increasing the subsidy for any randomly-chosen household; if such subsidies go to the better-endowed, their effect will be overestimated. Consideration of the possibly systematic association between subsidies provided to agents and the environmental or other characteristics of the agents in the estimation of subsidy effects clearly yields better (policy-relevant) estimates of those effects. Moreover, such estimates also permit the testing of models

of governmental resource allocations, which should provide the rules by which public resources are distributed among heterogenous agents or localities, as well as a rationale for the particular set of instruments used to effect resource transfers.

b. The Optimality of Family Planning Subsidies and their Distribution

To discern the rules by which subsidies to fertility control might be distributed among households behaving as described above in the absence of any arbitrarily-assumed direct population externality, consider a wealthy household having the same objective function as in (1) but facing a health externality. In particular, let the technology of health production for the well-off household be

$$(6) \quad H = h(X, N, H^*) \quad h_X, h_{H^*} > 0,$$

where  $H^*$  is the mean health of the children in the  $T$  low-income households, i.e.,

$$(7) \quad H^* = \sum_{i=1}^T \alpha^i H^i$$

where  $\alpha^i = N^i / (\sum_{i=1}^T N^i)$  and  $\max (H^i) < H$ . Thus, while the health of the well-off children depends on the mean health of the children of the poor, there is no direct fertility externality. As in altruism models, the externality is asymmetric--poor households do not consider or are not affected by the consumption set of the "donor" household.

Assume that the wealthy household can observe all the health endowments but, initially, cannot subsidize health investments and cannot tax fertility directly ( $s_c^i \geq 0$ ). If each household's fertility control is differentially

subsidized, the budget constraint for the wealthy household is:

$$(8) \quad G = P_N N + P_c (V - N) + P_x XN + P_z Z + \sum_i s_c^i (1 + \theta_c^i) (V^i - N^i),$$

where  $G$  = full income of the high-income household and  $\theta_c^i$  is the loss in subsidy transfers to the  $i$ th household associated with transaction costs (waste, graft). In this setup, the transfer scheme is politically feasible, since the majority of households (the poor) and possibly all households are potentially better off. The questions are: 1) under what conditions will the wealthy household subsidize fertility control and 2) how will the subsidy, if warranted, be distributed among the poor households.

Maximization of the wealthy-household utility function subject to (8), (9) and the price-taking behavior of the poor households, as described by model (1) through (3), yields the equilibrium conditions:

$$(9) \quad \frac{s_c^i}{P_c} = - \alpha^i \frac{P_x}{P_c} \frac{NX}{N^i} \frac{\epsilon_{HH^*}}{\epsilon_{HX}} \left[ \epsilon_{HX}^i \frac{\eta_{XP_c}^i}{\eta_{NP_c}^i} + \epsilon_{HN}^i + \left( \frac{H^i - H^*}{H^*} \right) \right] -$$

$$(\eta_{NP_c}^i)^{-1} \left( \frac{V^i - N^i}{N^i} \right) (1 + \theta_c^i) > 0 \quad i = 1 \dots T$$

$$\text{or } \frac{s_c^i}{P_c} = 0.$$

where  $\epsilon_{HH^*} = (\partial H / \partial H^*) (H^* / H)$  and  $\epsilon_{HX} = (\partial H / \partial X) (X / H)$ , from (7);  $\epsilon_{HX}^i = (\partial H^i / \partial X^i) (X^i / H^i)$  and  $\epsilon_{HN}^i = (\partial H^i / \partial N^i) (N^i / H^i)$  from (2); and the  $\eta^i$  are the demand price elasticities characterizing the  $i$ th household. Note  $\eta_{NP_c}^i > 0$ .

Condition (9) has two terms. The first contains the health gains to the rich household associated with increasing the fertility control subsidy. There are three sources of gains: the first term in brackets is the health return which occurs because of cross price effects. Raising the fertility control subsidy increases health

if fertility and child health are gross substitutes, as  $\eta_{xp_c}^1 < 0$ . The second term is the return due to the direct or biological effect of family size on child health, through (2), in poor households. This term corresponds to a positive gain if decreases in family size biologically augment child health. The third bracketed term is the "eradication" effect of fertility control subsidies--decreases in the size of families with below-average child health ( $H^i < H^*$ ) increase the mean health of the poor households; family planning subsidies provided to the lowest-health households thus increase the health of the wealthy households via the health externality even if fertility and child health are independent (in terms of price or biological effects) in poor households.

The second term in condition (9) is the marginal cost incurred by the wealthy from increasing the fertility control subsidy to household  $i$ . Such costs are higher the greater the number of averted births (the lower is fertility), the smaller the own price elasticity of fertility, and the higher are transaction costs.

Condition (9) indicates that fertility control subsidies can be used as substitutes for health subsidies for all poor households when there is a health but not a population externality and even when no biological relationship exists between fertility and child health, as long as the fertility control cross-price elasticity for health is sufficiently strong and negative; i.e., fertility and health are gross substitutes.<sup>3</sup> Condition (9) also suggests that, given the optimality of fertility control subsidization, the lowest-health (eradication effect) and the highest-fertility (cost effect) households would receive

the largest family planning subsidies. However, this does not imply that those households with the lowest health endowments receive the highest subsidies. Indeed, the distributional rules will depend on price effects. To see this, consider the effect on the subsidy rate to the  $i$ th household when that household experiences an increase in its health endowment  $\mu^i$  and adjusts its behavior accordingly. For simplicity, assume that the  $i$ th household's health is (initially) at the mean of the health distribution of poor households, i.e.,  $H^i - H^* = 0$ . Total differentiation of the system of first-order conditions describing the wealthy-household allocations, treating price effects as parameters, yields:

$$(10) \quad \frac{ds_c^i}{d\mu^i} = \left[ \left( \frac{p_x^N}{h_x^i} h_x^i \frac{(1 - \alpha^i)}{\Sigma N^i} \left( \frac{dX}{dp_c} \right) - 1 \right) \left( \frac{dN^i}{d\mu^i} \right) - \alpha^i \frac{p_x^N}{h_x^i} h_{xx}^i \right. \\ \left. \left( \frac{dX}{dp_c} \right) \left( \frac{dX}{d\mu^i} \right) \right] \left( \frac{ds_c^i}{d\theta^i} \right)_c + \left[ \frac{p_x^N}{h_x^i} \alpha^i - s_c^i \left( \frac{dN^i}{d\mu^i} \right) \right] \frac{ds_c^i}{dG}$$

Expression (10) has two terms, the first corresponding to the (compensated) own price or cost effect of the  $i$ th subsidy and the second associated corresponding to the/income effect on the wealthy household. The magnitudes of these terms depend in turn on the magnitudes and signs of the endowment effects on the health investments and fertility of the  $i$ th household. These are given by

$$(11) \quad \frac{dX^i}{d\mu^i} = - \frac{p_x^N}{h_x^i} \left[ h_{xx}^i \left( \frac{dX^i}{dp_x} \right)_c - \frac{dX^i}{dF^i} \right] \\ \frac{dN^i}{d\mu^i} = - \frac{p_x^N}{h_x^i} \left[ h_{xx}^i \left( \frac{dN^i}{dp_x} \right)_c - \frac{dN^i}{dF^i} \right]$$

where  $c$  denotes compensated effect.

From expressions (10) and (11), sufficient (but not necessary) conditions for larger family planning subsidies to be provided to low-endowment households (compensatory subsidization) are that (i) fertility and health are gross substitutes ( $dX/dp_c < 0$ ,  $dN/dp_x > 0$  (so that  $dN^i/d\mu^i \geq 0$ ) and (ii)  $dX^i/d\mu^i > 0$ ; i.e., more endowed households invest less in health. In that case, the returns to further health investments will be smaller in high- than in low- $\mu$  households and high- $\mu$  households will have at least as many averted births (at least as high family planning subsidy costs) as low- $\mu$  households. Thus, where fertility control subsidies but not health subsidies are prevalent, fertility and health investments will likely be gross substitutes in recipient households and such subsidies will be distributed disproportionately to the lowest endowment households.

### c. Combining Health and Family Planning Subsidies

Having shown that family planning subsidies can effectively substitute for health subsidies when there is (only) a health externality, under certain conditions, we now consider whether a combination of health and fertility control subsidies is redundant, that is, we consider whether fertility control subsidies will be used in addition to health subsidies in the presence of the health externality and in the absence of a population externality.

The budget constraint for the subsidy provider when both subsidies can be used is:

$$(12) \quad G' = G + \sum_1 s_x^i (1 + \theta_x^i) X^i N^i$$

and the equilibrium conditions for the two subsidy rates are:

$$(13) \quad \frac{s_c^i}{p_c} = -\alpha^i \frac{p_x^{NX} \epsilon_{HH^*}}{p_x^{N^i} \epsilon_{HX}} \left[ \epsilon_{HX}^i \frac{\eta_{xp}^i}{\eta_{Np_c}^i} e + \epsilon_{HN}^i + \left( \frac{H^i - H^*}{H^*} \right) \right] -$$

$$\left( \eta_{Np_c}^i \right)^{-1} \left( \frac{v^i - N^i}{N^i} \right) p_c + s_x^i \frac{X^i}{p_c} \left[ 1 + \frac{\eta_{xp}^i}{\eta_{Np_c}^i} \epsilon \right]$$

$$\text{or } \frac{s_c^i}{p_c} = 0.$$

$$(14) \quad \frac{s_x^i}{p_x} = \left[ 1 + \alpha^i \frac{NX}{N^i X^i} \frac{\epsilon_{HH^*} \epsilon_{HX}^i}{\epsilon_{HX}} \left( \eta_{xp_x}^i - \frac{\eta_{Np_x}^i \eta_{xp_c}^i}{\eta_{Np_c}^i} \right) \right. \\ \left. \frac{\eta_{Np_x}^i}{\eta_{Np_c}^i} \frac{p_c}{p_x} \frac{(v^i - N^i)}{X^i N^i} \right] \psi^{-1}$$

$$\text{or } \frac{s_x^i}{p_x} = 0,$$

$$\text{where } \psi = \left( \eta_{Np_x}^i + \eta_{xp_x}^i \right) \left[ 1 - \frac{\eta_{Np_c}^i}{\eta_{Np_c}^i} + \frac{\eta_{Np_c}^i + \eta_{xp_c}^i}{\left( \eta_{Np_x}^i + \eta_{xp_x}^i \right)} \right].$$

As before, the optimal subsidy levels depend on price effects. However, in this case both health and family planning subsidies may be used even if the objective functions for the low-income households are strongly separable, no biological relationship exists between family size and child health, and all low-income households invest equally in child health. In that case, the equilibrium conditions are:

$$(15) \quad \frac{s_x^i}{p_x} = \left[ 1 + \alpha^i \frac{NX}{N^i X^i} \frac{\epsilon_{HH^*} \epsilon_{HX}^i}{\epsilon_{HX}} \eta_{xp_x}^i \right] \left( \eta_{xp_x}^i \right)^{-1}$$

$$\text{or } \frac{s_x^i}{p_x} = 0.$$



$$(16) \quad \frac{s_c^i}{p_c} = s_x^i \frac{X^i}{p_c} - \left( \frac{v^i - N^i}{N^i} \right) (\eta_{N p_c}^i)^{-1}$$

$$\text{or } \frac{s_c^i}{p_c} = 0$$

Expression (15) indicates that the health subsidy will be used as long as there is a health externality. Expression (16) indicates that with sufficiently high health-subsidy expenditures by the subsidizing agent, positive family planning subsidies will also be optimal. Moreover, family planning and health subsidies, where both are used, will be positively correlated. The complementarity between the two subsidies, despite the single health externality, arises from the interaction between family size and per-child health expenditures in the "governmental" budget constraint (12)--an increase in the family planning subsidy to household  $i$  which lowers family size in  $i$  by one child saves the subsidizing agent the amount  $s_x^i X^i$ ; the cost of that increase depends positively on the number of births averted by the  $i$ th household and inversely on the magnitude of its (own) fertility control elasticity.

When strictly positive health and family planning subsidies are jointly optimal, the magnitudes of the subsidies will also generally depend on the differing health endowments of the recipient households. Moreover, the direction of the endowment-subsidy association is likely to be identical for both the family planning and health subsidy. However, unlike in the single-subsidy case, no simple sufficient condition regarding household demand relationships determines the sign of the associations between the two subsidies and the health endowments.

### 3. Empirical Application: Laguna Province, the Philippines

#### a. The Data and the Distribution of Government Facilities

We have shown that the effects of government interventions on per-child health within a family are incorrectly estimated if the distribution of those interventions are influenced by the health predispositions of households, associated with endowments or tastes, that are unobserved by the researcher. In order to correctly assess the impact of government programs designed to influence health outcomes and to discover the government placement rules, it is thus necessary either to estimate or to measure pre-program heterogeneity in health outcomes. We will attempt to obtain consistent estimates of both the health effects of governmental family planning and health facilities and of facility placement rules based on longitudinal data describing the distribution of such public programs and child health in 20 barrios (villages) in the lowland rice-producing areas of Laguna Province in the Philippines. Information from surveys of 240 randomly-selected households residing in these barrios on the age, height and weight of every family member was collected in 1975 and 1979. Information was also obtained in the 1979 survey round on the dates of introduction of rural health clinics, family planning clinics, and primary schools financed by the national government for each of the barrios.

The distribution of the public facilities across barrios is reported in Table 1. While all but two of the twenty barrios have a public primary school, with such schools having been in existence for at least fifteen years prior to 1979 for each of the other eighteen barrios, health and family planning facilities were more recently introduced and are less prevalent.

Table 1

Distribution of Public Facilities in Twenty Laguna Barrios by  
 Number of Years Instituted Prior to 1979

Years in Barrio	Family Planning Clinic	Rural Health Clinic	Primary School
0	8	7	2
0 - 4	4	3	0
5 - 9	5	0	0
10 - 14	2	4	0
15 - 19	0	2	4
20 +	1	4	14
Total	20	20	20

Seven barrios had no public health clinic and eight barrios had no family planning facility by 1979, with seven of the thirteen existing health facilities and eleven of the twelve family planning facilities introduced less than fifteen years prior to 1979.

The joint distribution of the family planning and health clinics appears in conformity with the health externality model, as such facilities appear to be placed in a complementary pattern--the Spearman rank correlation of establishment dates for the family planning and health clinics is .62. Moreover, of the seven barrios that had no health clinic, five also did not have a family planning clinic and of the eight barrios without a family planning clinic, five also did not have a health clinic. Five barrios had neither facility as of 1979. The existence of two barrios without a health clinic but with a family planning clinic suggests, as noted, that if direct population externalities are ruled out, child health and family size should appear to be gross substitutes among the Laguna households. This is confirmed below.

b. Estimation Framework

To exploit the longitudinal data on health and the information on the dates of program initiation, we modify the above framework to accommodate the realities that government programs are initiated at different times and that observed child health in any period is a stock variable influenced by resources allocated in the current and prior periods. The impact of a program on the current health status of a particular child will thus depend upon the length of its previous exposure to the program. We will exploit the variability in program exposure across children to estimate the effects of the health and family planning programs and to estimate the barrio-specific health endowments. Variation in program exposure across children, however,

occurs both because barrios differ in the timing of program introduction and because children within the barrio differ in their dates of birth. If child health investments differ systematically by the birthdate of the child due to health-related factors about which the researcher is unaware, a spurious relationship between child health and program exposure is generated even if the timing of government programs across barrios is unrelated to family or barrio endowments.

Let  $t_s$  represent the year of the survey,  $t_p$  the year the program was instituted, and  $t_b$  the year of birth of a child. The program will have been in effect  $t_s - t_p$  years and for children born prior to  $t_p$ , i.e.,  $t_b < t_p$ ,  $t_s - t_p$  will be the number of years each such child will have been exposed. Yet, a child born one year prior to the program will likely be more strongly impacted by the program than a child born five years prior to the program. We thus adopt as a measure of program exposure the fraction of a child's lifetime during which the child was exposed to the program. Let  $p_{i\ell}^a$  be the program exposure of child  $i$  residing in barrio  $\ell$  who is of age  $a$  at the survey date, where  $a = t_s - t_b$ . Thus,

$$\begin{aligned} p_{i\ell}^a &= 0 \text{ if the program does not exist in the barrio as of the survey date} \\ &= \frac{t_s - t_p}{a} \text{ if } t_s - t_p > t_b \text{ in barrio } \ell \\ &= 1 \text{ if } t_s - t_b > t_p \text{ in barrio } \ell. \end{aligned}$$

Consider the following child health demand equation for a child  $i$  aged  $a$  in barrio  $\ell$  observed at  $t_s$ :

$$(17) \quad H_{i\ell t_s}^a = p_{i\ell}^a \beta + u_i + \mu_\ell + \varepsilon_{i\ell t_s},$$

where  $H$  is an age-standardized measure of health,  $u_i$  is a time-invariant, child-specific health endowment, the  $\mu_\ell$  are location-specific health factors and  $\epsilon$  is a random error term. Least squares estimation of (17) when  $\mu_\ell$  is unobserved leads to a biased estimate of  $\beta$ , the program exposure effect, if  $t_p$ , the date the program was introduced, is related to the area's endowments, as would be the case with non-random program placement.

Within-family or barrio estimators of  $\beta$ , which purge out, respectively, household and locational characteristics, are also biased, however, even if program placement is uncorrelated with child or family-specific endowments  $u$  if child-specific health endowments (within-family) or household endowments (within-barrio) influence the spacing of children. In differenced form, for a family with at least one child born prior to the program's introduction, the within-family estimator is:

$$(18) \quad H_{jlt_s}^{a'} - H_{ilt_s}^a = \left[ \frac{(t_s - t_p)(t_b' - t_b)}{(t_s - t_b')(t_s - t_b)} \right] \beta + (u_j - u_i) + (\epsilon_{jt_s} - \epsilon_{it_s})$$

where  $a' = t_s - t_b' > a$ . As can be seen, even if the dates of program introduction  $t_p$  are independent of the child-specific error  $u$ , if child  $j$ 's birth date  $t_b'$  is related to his/her older siblings' health status  $u_i$  the within-family estimate of the program exposure effect is also biased. In Rosenzweig and Wolpin (1984) and Rosenzweig (forthcoming) it is shown that birth spacing and other child-specific inputs are significantly correlated with prior sibling and family-specific endowments, leading to biased estimates /of child-specific resource allocations. Thus, as long as program placement is not responsive to purely random disturbances (or perturbations with little persistence), only within-child estimators will yield consistent estimates of the effect of program exposure, given systematic program placement and endowment-conditioned birth spacing behavior.<sup>4</sup> Longitudinal information on child health outcomes is required.

c. Program Assessment: Comparisons of Cross-Sectional and Panel Estimates

To estimate the effects of the facilities on child health, we selected a sample of children (defined to be under eighteen as of 1979) for whom height and weight information exists in both years of the Laguna survey, yielding a working sample of 274 children in eighty-five households. Table 2 provides descriptive statistics for the sample children at each of the two survey dates. Height and weight are standardized by age and sex according to a national schedule. The average child in this sample in each of the two survey years is somewhat over ninety percent as tall as the average Filipino child of the same age and sex but only a little over eighty percent as heavy. However, the average child in the sample has evidently grown slightly in both dimensions relative to the standard between the two surveys.

Three separate specifications were estimated corresponding to alternative assumptions about unobservables in the determination of height and weight. In the first two columns of Table 3 ordinary least squares regressions are reported using the 1979 cross-section of 274 children. The second pair of columns repeats the cross-sectional regressions but adds barrio dummy variables. The third set of columns reports first-differenced regressions using the 1979 and 1975 (matched) samples. The first column of each set includes the child's exposure to primary schools in addition to exposure to the health and family planning clinics. In the upper half of the table the dependent variable is the log of standardized height; in the lower half the dependent variable is the log of standardized weight.

The differences in estimated program exposure effects across the specifications are striking for either health measure. In the height regressions, both the cross-section and barrio fixed-effect health and

Table 2  
Sample Statistics

Variable	Mean	S.D.
Natural logarithm of height normalized by Philippines age standard, 1975	4.525	.0715
Natural logarithm of height normalized by Philippines age standard, 1979	4.543	.0566
Natural logarithm of weight normalized by Philippines age standard, 1975	4.377	.147
Natural logarithm of weight normalized by Philippines age standard, 1979	4.407	.147
Exposure to public health unit, fraction of years, 1975	.456	.480
Exposure to public health unit, fraction of years, 1979	.512	.451
Exposure to family planning clinic, fraction of years, 1975	.162	.314
Exposure to family planning clinic, fraction of years, 1979	.285	.333
Exposure to public primary school, fraction of years, 1975	.871	.330
Exposure to public primary school, fraction of years, 1979	.872	.336
Number of years rural health in barrio, 1979	10.0	10.3
Number of years family planning clinic in barrio, 1979	6.45	13.67
Number of years public primary school in barrio, 1979	28.3	16.6
Number of barrios		20
Number of children		274



Table 3

Estimates of the Effects of Exposure to Governmental Programs  
on the Standardized Height and Weight of Children

Variable	OLS Cross-Section <sup>a</sup>		Fixed-Effect Barrio		Fixed-Effect Child	
	<u>Log of Standardized Height</u>					
Rural Health Unit Exposure	-.00976 (1.04)	-.00473 (0.53)	.00950 (0.16)	-.0205 (0.40)	.0507 (1.58)	.0511 (1.21)
Family Planning Exposure	-.00605 (0.49)	-.0131 (1.12)	-.0135 (0.40)	-.00913 (0.27)	.0709 (3.31)	.0710 (3.32)
Public Primary School Exposure	-.0193 (1.75)	-	-.172 (1.14)	-	(.0569 (0.10)	-
R <sup>2</sup>	.0448	.0339	.1738	.1695	.0741 <sup>b</sup>	.0660 <sup>b</sup>
F	2.09	1.88	2.09	2.12	7.23	9.61
d.f.	267	268	248	249	271	272
	<u>Log of Standardized Weight</u>					
Rural Health Unit Exposure	-.0443 (1.82)	-.0313 (1.35)	-.0762 (0.50)	-.162 (1.20)	.0235 (1.59)	.0992 (1.52)
Family Planning Exposure	.0446 (1.40)	.0263 (0.87)	.0677 (0.75)	.0803 (0.90)	.0990 (1.99)	.121 (2.76)
Public Primary School Exposure	-.0503 (1.76)	-	-.494 (1.24)	-	.240 (0.20)	-
R <sup>2</sup>	.0447	.0337	.1453	.1401	.0514 <sup>b</sup>	.0500 <sup>b</sup>
F	2.08	1.87	1.69	1.69	4.89	7.16

<sup>a</sup>Equation also includes the age and educational attainment of each parent.

<sup>b</sup>From first-differenced equation.

family planning clinic "effects" are generally negative with standard errors that are at least as large as the point estimates. The child fixed-effect (longitudinal) estimates, however, indicate that exposure to health and family planning clinics increases height, with the family planning effect statistically significant at the usual confidence levels and the health clinic effect marginally significant. The point estimates indicate that the height of a child for whom no health clinic existed would be five percent below that for a child always exposed to a clinic, while exposure to a family planning clinic increases height by seven percent. The same pattern emerges for public primary schools, although in that case the child fixed-effect point estimate has a very large standard error, due most likely to the small variance in exposure associated with the longevity of public schools displayed in Table 1.

The weight regressions tell a very similar story: the cross-section and within-barrio associations between health clinic exposure and age-standardized weight are negative, while the child fixed effect estimates, measured relatively precisely, indicate that exposure to either the health or family planning programs increases the weight-for-age of children. Here, however, the family planning effect is somewhat more robust to specification, although the effect of this program on child weight is underestimated by more than 100 percent when only the cross-sectional variation in program placement is utilized. The point estimates (last column) indicate that unit increases in health and family planning clinic exposure increase age-standardized child weight by nine and twelve percent respectively.

#### d. Program Placement Rules

Whether child health status is measured by age-standardized height or

weight, the estimates of the child health effects of the family planning program purged of contamination by the endogeneity of program placement or birth-spacing in Table 3 indicate that child health and family size are substitutes--subsidies to fertility control evidently augment resource allocations to child health investment among Laguna households. Thus, as we have shown, family planning clinics may substitute for health clinics in the presence of health externalities and/or may effectively complement health clinics even in the absence of other externalities, due to the interaction between family size and health investments in the "governmental" budget constraint.

In this section we seek to discern whether the dates of introduction of both the health and family planning clinics are systematically related to the average child health endowment within a barrio, i.e., we estimate the governmental program allocation rules. The child-specific effects that are estimated from (17) contain the elements  $u_i$ ,  $\mu_\ell$ , a constant, and the effects of all time-invariant determinants of height and weight, e.g., mother's schooling, but net out the effects of the programs. However, since there are only two observations on each child, the estimated fixed effect measures the true pre-program child effect with error. Averaging child-specific effects within each barrio thus yields a measure (gross of time-invariant factors and random errors) of pre-program barrio level health presumably observed by the government, though only indirectly by us, and used by it to plan the timing of public program introduction. We have two such measures, corresponding to height and to weight.

Table 4 reports the estimates of the impact of the average barrio-level health endowment as measured by (the  $\ln$  of) child height on the length of

Table 4

Estimates of the Effects of Barrio Child Health Conditions  
on the Placement of Governmental Programs<sup>a</sup>

Endowment Measure	Rural Health Unit		Public Program		
	(1)	(2)	(1)	(2)	(3)
$\ln$ Height, standardized <sup>b</sup>	-102 (0.92)	-	13.2 (0.09)	-	-
$\ln$ Height Effect	-	-145 (4.30)	-	-129 (2.30)	-
Predicted $\ln$ Height Effect	-	-199 (4.34)	-	-	-151 (1.91)
R <sup>2</sup>	.0452	.507	.0004	.228	.168
				.0114	.0316
				-81.9 (0.46)	-
				-	-58.3 (0.77)
				-	-17.3 (0.16)
				.0114	.0015

<sup>a</sup> Dependent variable = years since program was initiated.

<sup>b</sup> OLS coefficient.

time in years that each of three programs--health clinics, family planning clinics, primary schools--have been in existence in the barrio. There are thus twenty observations. Parental education levels were initially included as discussed above, but were jointly insignificant at conventional levels and so are excluded from the results actually presented. The first row uses actual mean height in the barrio and would only be correct if the programs themselves had no impact on height. The second row uses the barrio-average fixed-effect computed from the child fixed-effect height regression reported in column 6 of Table 3. The third row uses the predicted height fixed-effect obtained from a first stage regression in which the  $(\ln)$  height fixed-effect is regressed on the  $(\ln)$  weight fixed-effect, computed from the last column estimates of Table 3. The purpose of this latter procedure is to purge the estimate of the height fixed effect of measurement error under the assumption that height and weight are both measures of the same underlying health indicator.<sup>5</sup>

While the timing of program initiation for all three programs appears unrelated to average child height in the barrios (row one), when the height effects of the three programs are removed, as in rows two and three, the estimates indicate that the health and family planning clinics were distributed systematically over time and, as expected, were allocated in a similar manner. Moreover, the statistically significant, fixed effect estimates imply a compensatory government allocation rule for both of the evidently complementary health programs. Barrios with lower pre-program health "endowments" evidently received both types of health-augmenting programs earlier. The timing of primary public school placement is not significantly related to the health endowments, however, a result consistent with our finding that such schools do not appear to significantly affect child health.

The point estimates based on the predicted height measure indicate that where pre-program standardized height was one percent higher (about one-fourth of the standard deviation) the introduction of a health unit was retarded by about two years. The distribution of family planning clinics was almost as responsive to health endowment variation; their introduction was delayed by about one and one-half years for every percentage increase in standardized height. The compensatory program placement rule followed by the governmental authorities for the complementary health and family planning programs thus appears to have been responsible for the significant negative biases observed in the cross-sectional estimates of the effects of the two programs in Table 3.

### 3. Conclusion

In this paper we have specified and tested a model of the distribution by a central authority of family planning and child health investment subsidies across heterogenous localities and assessed the bias in the evaluations of such programs based on cross-sectional data implied by non-random program distribution. A basic feature of the model is the presence of a health externality, which is shown to be sufficient along with plausible features of household behavior to make selective subsidization of fertility control either alone or in combination with health investment subsidies Pareto-efficient. Thus the issue of whether or not population growth per se impedes economic development, whether there are direct population externalities, may be irrelevant to the issue of whether family planning programs are desirable instruments for promoting economic growth.

The model suggests that subsidization of fertility control is likely to be Pareto efficient in the presence of health or human capital externalities when a) human capital and family size are gross substitutes and/or b) any per-child human capital subsidies are provided to recipient households. In the first case, fertility control subsidies may substitute for direct subsidies to health investment and an equalizing distribution of the subsidies, the highest family planning subsidies to the lowest-health recipient households, is efficient. When both family planning and health subsidies are used, fertility control subsidies serve to minimize the subsidy burden for donors and will be highest where total subsidy expenditures per child are greatest, but in general the ordering of the distribution of the joint subsidies by the inherent healthiness of recipients cannot be predicted.

Longitudinal data describing the timing of program implementation in twenty randomly-sampled barrios in the Laguna Province in the Philippines revealed a systematic pattern of health and family planning program placement in accord with the model: each program was initiated earliest in the low-health barrios, most of the barrios that had any program had both programs by the date of the last survey round, and when endogenous program placement was taken into account, exposure to either program appeared to significantly improve children's health status. Family size and child health thus appeared to be gross substitutes in the Laguna households, a sufficient condition for the presence of some barrios with a family planning, but not a health, clinic.

The compensatory pattern of program placement, when not taken into account, yielded estimates of the effects of the two programs on child health that would have led to false rejection of the hypothesis that

either or both improved child health. Those results thus imply that conclusions drawn from studies exploiting the cross-sectional variation in program intensities to evaluate programs and/or to identify structural relationships characterizing household behavior should be interpreted with care. Additional empirical and theoretical work integrating central and local program determination with household optimization would appear warranted.



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## Footnotes

1. Given technological advances in contraceptive technology, a rationale for the public dissemination of general contraceptive information may be warranted. However, this does not necessarily justify subsidization of contraceptive devices or of person-specific contraceptive services. Moreover, as low-fertility households gain most from the acquisition of contraceptive information, disproportionate information provision to such households would appear to be implied by interest group theory.
2. We assume that households do not move across localities. The consequences of the selective migration of agents in response to changes in local programs are considered in Rosenzweig and Wolpin (1984b).
3. The evidence on the biological effect of family size or birth order on child health suggests that such a linkage provides little justification for subsidization of fertility control on health grounds. In Rosenzweig and Schultz (1983), birthweight is found to significantly increase with increasing birth order; in Rosenzweig and Wolpin (1984a), little or not relationship is found between birth order and birthweight, although longer (prior) birth intervals increase birthweight. Both of these studies take into account in estimation the existence of heterogeneity in health endowments.
4. Indeed, it is not necessarily true that the within-barrio regression performs better than the cross-section regression if the within-child regression is taken to be the correctly specified model.
5. The results reported in Table 4 are qualitatively identical when the standardized weight effect is used to measure the community-level health endowment.

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