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The use of cross-sectional data to estimate models of consumer or household behavior or to evaluate public programs has a long and continuing tradition in economics (e.g., Feldstein (1978), Meyer and Wise (1983), Johnson (1983), Pencavel (1984)). The treatment of cross-sectional or area-specific variation in prices or program characteristics as exogenous would appear, however, inconsistent with the Tiebout hypothesis (1956) that agents, heterogenous in preferences or endowments, locate in response to and/or select local program levels according to those preferences. If so, cross-sectional correlations between the observed behavior of agents and relative prices or program levels will not correspond to true price or program effects for any individual agent.

While some studies have shown that local laws reflect the preferences of local populations (Landes (1980), Farber (1984)), such studies appear to assume that interregional differences in population preferences are exogenous. Heterogeneity and selective-migration imply, however, that site-specific changes in prices or programs, whatever their source, will alter endogenously the characteristics and size of the population at the site, possibly inducing consequences unanticipated by the law-makers. Todaro's classic article (1969) presents a theoretical example in which non-selective migration thwarts the intended effects of an urban job creation _cum_ minimum wage program. A local program altering relative prices, however, may also induce countervailing changes in the population via migration selectivity; e.g., a locality initiating a program subsidizing health care might attract low-health households. Lack of attention to selective migration thus makes inferences about the effectiveness of a program to be implemented nationally based on local program initiatives potentially misleading.

Despite the importance of location-choice selectivity in local public goods theory and in the evaluation of public programs, there have been few attempts to test directly for the existence of selective migration (an exception is Schultz (1983))or to test predictions for how migration responds selectively to changes in relative prices.[1] Yet, how the characteristics of agents change across activities or locations in response to relative prices, i.e., the selectivity rules, are clearly dual to the price-theoretic implications for the observable behavior of a given agent and thus are themselves subject to verification.[2] In this paper we consider how a price change or program subsidy that is location or site-specific affects the composition of residents via selective migration and biases evaluations of the effectiveness of the local program. In particular, we assess the consequences of a site-specific program subsidizing human capital investment in terms of shifts in both population composition and a representative household's resource allocations,when optimizing households that are heterogenous in preferences and in their endowments of human capital are free to choose locations in response to changes in location-specific prices.

In Section 1 the theoretical framework is described and implications are derived for how population preferences and endowments shift within a locality in response to the human capital subsidy. We show that under plausible assumptions and under all forms of heterogeneity a program subsidizing investments in children attracts high-income households with small families; children in such households, *ceteris paribus*, may exhibit low or high levels of human capital, however, depending on whether the principal source of heterogeneity is in tastesor endowments. The relationships between the biases in estimates of the program effects and sources of heterogeneity that arise from selective migration are also derived. In Section 2, unique longitudinal data from

Colombia describing the consequences of a local health subsidy program are used to test the implications of migration selectivity. The findings confirm the existence of selective migration. The child health care program evidently induced in-migration by households characterized by high income but, within income groups, households with low-fertility and low child health were attracted to the program site. These migration patterns are shown to be consistent with the hypothesis that heterogeneity in health endowments dominates that in tastes within the population. We also show that as a consequence of this form of heterogeneity, the effectiveness of the program based on cross-child differences in health and program exposure is considerably overestimated when selective migration is not taken into account.

1. Modeling Migrant Selectivity

a. Heterogeneity and Migration Selectivity: Who Migrates?

Consider an economy consisting of heterogenous households in spatial equilibrium: all potential profits from migration are zero; i.e., no household, net of migration costs, can increase its income by changing location. Decisions by households are characterized by the static, lifetime optimization problem in which the ith household maximizes

$$(1) \quad U(H^i, N^i, Z^i; \alpha^i),$$

where N^i = number of children in household i, H^i = average human capital of children in i, Z^i = composite consumption good and α is a vector of household-specific taste parameters, subject to the human capital production function

$$(2) \quad H^i = H(X^i; \beta^i, \mu^i),$$

where X^i is the per-child human capital input, β^i is the household's technology parameter, and μ^i is the household's human capital endowment, and the lifetime budget constraint

$$(3)\quad Y^i = P_X X^i + P_N N^i + Z^i,$$

where Y^i is income, P_X is the input price and P_N the price of a child.[3]

Assume that all prices are identical across all locations but that at a particular site a program is initiated which pays a subsidy s per unit of the human capital purchased input. Each household not at the program site now faces a potential migration decision. If the household migrates to the site, the price of the human capital input is P_X^S and lifetime income is Y^{S^i}, where Y^{S^i} is income at the program site net of the cost of migration and the program tax, which is assumed to be lump-sum and levied on all residents. Let $V(P_X, Y^i; \alpha^i, \beta^i, \mu^i)$ be the indirect utility function derived from maximizing (1) subject to (2) and (3); the difference in utility dV between migrating and not migrating is then:

$$(4)\quad dV^i = \frac{\partial V^i}{\partial P_X} dP_X + \frac{\partial V^i}{\partial Y^i} dY,$$

Roy's identity, i.e., $-X = \frac{\partial V/\partial P_X}{\partial V/\partial Y}$, yields the migration decision rule

$$(5)\quad \text{migrate iff} = X^i(P_X^S - P_X) + Y^{S^i} - Y^i > 0,$$

where $P_X^S < P_X$, $Y^S < Y$. If the subsidy is proportional to P_X $(P_X^S = (1-s)\ P_X)$ and migration cum program costs are proportional to income $(Y^{S^i} = (1-C^i)\ Y^i)$ this reduces to

$$(6)\quad \text{migrate iff } \frac{P_X X^i}{Y^i} > \frac{C^i}{s},$$

namely that the household migrates if and only if the income share of the human capital input is at least as large as the ratio of the proportional migration cost to the proportional input subsidy. Notice that a secondary condition for migration to occur is that $C < s$. If the subsidy is absolute ($P_X^S = P_X - s$) and the migration cost is also absolute ($Y^{S^i} = Y^i - C^i$), then the decision rule is

$$(7) \quad \text{migrate iff } X^i > \frac{C^i}{s} .$$

Migration rule (7) can be used to derive the rules for migrant selectivity by allowing the fundamental taste, technology and endowment parameters to vary in response to changes in relative prices while holding constant the level of the utility differential between the origin and program sites. The characteristics of the "marginal" migrant household, the household that is just indifferent between moving to the site or not migrating, must change with migration costs C or the site subsidy s according to:

$$(8) \quad d\Gamma = \left(\frac{dX}{d\Gamma}\right)^{-1} \left(\frac{1}{s}\, dC - \frac{C}{s^2}\, ds\right), \qquad \Gamma = \alpha,\ \beta,\ \mu,\ Y$$

where superscripts are dropped to indicate that (8) describes a change in the type of household rather than the response of a given household.

Clearly, from (8), any characteristics of the household that increase the demand for the human capital input X must increase as C increases or must decrease as s increases in order to maintain the indifference. To discover how the observable characteristics of migrants vary with the program subsidy and/or migration costs it is thus necessary to specify how the unobservables α, μ and β affect household decision rules. Rosenzweig and Wolpin (1982) treated the special case where the human capital endowment is additive, i.e., $H = H(X;\ \beta) + \mu$, and derived general expressions for the influence of the endowment on the human capital input and on fertility, namely

$$(9) \quad \frac{dK^i}{d\mu^i} = \frac{P_X}{H^i_X}\left[-H^i_{XX}\left(\frac{dK^i}{dP_X}\right) + \frac{dK^i}{dY^i}\right] \qquad H^i_X > 0,\ H^i_{XX} < 0$$
$$K^i = X^i,\ N^i.$$

The effects of μ on the demand for X and N thus depend on the usual Hicks-Slutsky compensated price and income effects. If the fertility and the human capital of children are Hicks-Slutsky substitutes, as has often been found (e.g., Rosenzweig and Wolpin (1980)), and income effects are small relative to price effects, then a reduction in migration costs and/or an increase in the site subsidy would attract migrants with both higher human capital endowments and larger family size. If income effects dominate price effects, and are positive, then lower migration costs or a higher site subsidy induce, within income groups, less-endowed and lower-fertility in-migrants.

To generate predictions regarding the consequences of tastes and endowment heterogeneity for migration selection due to the initiation of a site-specific program subsidy requires that additional structure be imposed on the household problem. Consider a model in which the utility function is quadratic and the technology is linear for each household i:

$$(10) \quad U^i = \alpha^i_1 N^i - \alpha^i_2 N^{i^2} + \alpha^i_3 X^i - \alpha^i_4 X^{i^2} + \alpha^i_5 Z^i - \alpha^i_6 Z^{i^2}$$

and

$$(11) \quad H = \beta^i X^i + \mu^i,$$

which when solved in terms of the exogenous parameters yields the demand equations:

$$(12) \quad X^i = \frac{\psi^i(\alpha^i_3 - 2\mu^i\alpha^i_4) + (\alpha^i_2(2\alpha^i_6 Y^i - \alpha^i_5) - \alpha^i_1\alpha^i_6 P_N)}{\beta^i 2(\alpha_4\psi^i + \phi^i)}$$

$$(13)\quad N^i = \frac{\alpha^i_1 + P_N\,(2\alpha^i_6 Y^i - \alpha^i_5)}{2\psi^i} - \alpha^i_6\,\frac{P_N P_X}{\psi^i}\,X^i,$$

where $\psi^i = \beta^{i^2}\,(\alpha^i_2 + \alpha^i_6\,P_N^{\;2})$, $\phi^i = \alpha^i_2\alpha^i_6 P_X^{\;2} > 0$. Note that, in accord with most empirical findings, X and N vary inversely.

Assume that there is potential (unobserved) population heterogeneity in α_1 and α_3, reflecting preferences for family size and human capital, and in the endowments μ and β. Prior to the introduction of the program, the program-site and potential migrant populations are on average identical with respect to these fundamental parameters. As the subsidy is raised the ceteris paribus changes in these taste and endowment parameters and in income (Y) that will characterize the marginal migrant, from (8) and (12), are given by (14):

$$(14)\quad \begin{aligned}
\frac{d\alpha_1}{ds} &= \frac{C}{s^2}\left[\frac{2(\alpha_4\psi + \phi)}{\alpha_6 P_X P_N}\right] > 0\\
\frac{d\alpha_3}{ds} &= -\frac{C}{s^2\beta}\,[2\alpha_4 + \phi/\psi] < 0\\
\frac{d\mu}{ds} &= \frac{C}{s^2\beta}\,[1 + \phi/\psi\alpha_4] > 0\\
\frac{dY}{ds} &= -\frac{CP_X}{2}\,[1 + \psi\alpha_4/\phi] < 0
\end{aligned}$$

$$\frac{d\beta}{ds} = -\beta C\psi\,[(\alpha_3 - 2\mu\alpha_4)\,(\psi + \phi) + 2\beta\alpha_4 P_X\,(\alpha_1\alpha_6 P_N + \alpha_2(\alpha_5 - 2\alpha_6 Y))]\,[2s^2\alpha_4(\alpha_4\psi + \phi)^2]^{-1}.$$

Four polar cases with respect to heterogeneity are of interest: Suppose first that the populations are heterogeneous only with respect to tastes for family size and human capital investments in children. Specifically, let only α^i_1 or α^i_3 differ across households. From (14), as the subsidy increases, the migrant population as it becomes less selective will be composed of increasingly higher α_1 and increasingly lower α_3 households. If tastes are the only source of heterogeneity, observationally identical migrant households will thus have fewer children who on average will be of higher "quality", since $dN/d\alpha_1$, $dX/d\alpha_3 > 0$ by construction, and

$$(15)\quad \begin{aligned} \frac{dN^i}{d\alpha_3^i} &= -\frac{\alpha_6 P_N P_X}{2\beta(\alpha_4\psi + \phi)} < 0 \\ \frac{dX^i}{d\alpha_1^i} &= -\frac{\alpha_6 P_X P_N}{2(\alpha_4\psi + \phi)} < 0 \end{aligned}$$

With heterogeneity in the additive health endowment μ only, increasing the subsidy and thus reducing selectivity will draw households with higher endowments, since more endowed households, for given money income, will have a lower demand for X:

$$(16)\quad \frac{dX^i}{d\mu^i} = -[\beta(1 + \phi/\psi\alpha_4)]^{-1} < 0.$$

In contrast to the tastes heterogeneity scenario, lower-μ households, despite their higher demand for human capital inputs, will always have children characterized by lower levels of human capital, as

$$(17)\quad \frac{dH^i}{d\mu^i} = 1 + \beta\frac{dX^i}{d\mu^i} = [1 + \psi\alpha_4/\phi]^{-1} > 0.$$

When there is heterogeneity in the additive endowment, migrants attracted by the program subsidy will thus be observed to have lower levels of human capital. They will also have, as in the first case, fewer children since, as indicated in (7), high-X households always migrate and, from (13), X and N vary inversely.

When heterogeneity exists solely in β, the return to the input X, in-migrant households may have either lower or higher levels of human capital (expression (14)). However, whether or not high-β (and thus high-H) or low-β (and thus low-H) households demand higher levels of the X input and thus migrate to the program, such migrant households will have smaller families.[4] A human capital subsidy program will thus tend to attract and to serve disproportionally households within income groups characterized by low fertility whether heterogeneity exists in tastes or in human capital endowments. If the principal source of heterogeneity is in the latter, however, the program may attract, within income groups, households with lower levels of human capital, while tastes heterogeneity implies that the program will principally serve children already characterized by higher levels of human capital.

Independent of any heterogeneity in unobserved, fundamental parameters, however, if income effects are positive (as they are in the model), migrants will have relatively high income. In the third polar case of no heterogeneity, migrants will thus tend to have larger families and children with higher levels of human capital, as long as the program contains no means test provisions, but within income groups migrant and resident households will appear identical.

b. Program Effectiveness and Program Effects

Consider now the problem of evaluating a human capital subsidy program when the program is located at a specific site and migration is potentially self-selective. The average human capital h in a population of observationally identical migrants and residents at the program site is given by:

$$(18) \quad h = f_M h_M + f_R h_R,$$

where f_M and f_R are the relative proportions of (post-program) migrants and (pre-program) residents in the population respectively and h_M and h_R

are their average human capital levels. Note that h_M is the truncated mean of the non-site and site populations when such populations have the same distribution prior to the program. A change in the subsidy, s, will induce a change in the average human capital stock according to:

$$(19)\quad \frac{dh}{ds} = \gamma_R + \frac{df_M}{ds}(h_M - h_R) + f_M(\gamma_M - \gamma_R) + f_M \frac{dh_M}{ds},$$

where γ_R and γ_M are the respective average program subsidy effects on the levels of human capital in the resident and migrant populations; i.e., $-\beta dX/dP_X$, df_M/ds is the shift in the proportion of migrants in the population due to a change in program attractiveness, and dh_M/ds is the change in the mean human capital of the migrant population due to migrant selectivity; i.e., $(d\Gamma/ds)(dh/d\Gamma)$ from (8), where $\Gamma = \alpha, \mu, \beta$.

The total effect of a change in the locally-implemented program subsidy on the average human capital in the site population, given by (19), thus depends on (i) the direct effect of the subsidy on human capital investments by the original, resident population, (ii) the magnitude of the compositional change in the population via migration that is induced, weighted by the differential in mean human capital levels between the migrant and resident populations, (iii) the magnitude and sign of the difference in mean program effects in the two sub-populations, and (iv) the changes in the mean human capital of the migrants caused by the arrival of new, selectively drawn migrants, who will differ from those migrants already present. The average "effectiveness" of a program subsidy, the effect of the program if it were not site-selective (provided in all sites ("globally")) is given only by the first term in (19), if all pre-program site populations have the same mean characteristics or if the pre-program site population is representative. It is thus clear that the bias in the estimate of program effectiveness based on the program's site-specific effects, given by the sum of the last three terms in (19), will depend on the source and magnitude of heterogeneity in the overall population and on household decision rules.

In the case in which taste heterogeneity dominates (variation in α_3), the expression for the bias, derived from the model described by (10) and (11), is:

$$(20)\ \frac{dh}{ds} - \gamma_R = (\alpha_{3M} - \alpha_{3R})\ (\alpha_4 + \phi/\psi)^{-1}\ [\frac{df_M}{ds} + \frac{f_M}{P_X}\ (1 + \psi\alpha_4/\phi)^{-1}]$$

$$- f_M \frac{\beta C}{s^2},$$

where α_{3M} and α_{3R} are the mean human capital taste parameters for migrants and residents. The selectivity equation (14) implies, as noted, that migrants will have higher average tastes for human capital ($\alpha_{3M} > \alpha_{3R}$). Thus the first term in (20) is positive, since a higher subsidy attracts more high-α_3 and thus high-H migrants ($df_M/ds > 0$). Moreover, the (high-α_3) migrants react more positively to a subsidy than do residents.[5] The last term in (20), i.e., $d\alpha_{3M}/dS$, from (14), multiplied by $dh_M/d\alpha_{3M}$, from (11) and (12), is negative, however, reflecting the marginal decrease in the selectivity of the migrant population associated with the higher generosity of the subsidy.[6] Since this last term is a second-order effect, (20) implies that the estimated effect of a site-specific subsidy on human capital will represent an upper bound estimate of the effectiveness of the same program applied globally when variation in preferences for human capital is the principal source of population heterogeneity. Due to tastes heterogeneity, locally-implemented human capital subsidy programs will thus appear more efficacious than they really are for the average or representative household.

When heterogeneity is confined exclusively to endowments, however, the bias derived from the model cannot be signed even when the source of heterogeneity is the additive endowment. The bias in that case is:

$$(21)\quad \frac{dh}{ds} - \gamma_R = (\mu_M - \mu_R)\ (1 + \psi\alpha_r/\phi)^{-1}\ [\frac{df_M}{ds} - \frac{f_M}{P_X}\ (\alpha_4 + \phi/\psi)^{-1}]$$

$$+ f_M \frac{\beta C\phi}{s^2\psi},$$

where μ_M and μ_R are the mean human capital endowments in the migrant and resident populations. In this case, as was shown, the subsidy attracts low-endowment (and low-health) migrants ($\mu_M < \mu_R$); however, the human capital investments by (less-endowed) migrants respond more strongly to the subsidy than do those by residents. The net contribution of the negative compositional change and the positive differential in subsidy effects to the program effect bias cannot be predicted. When endowment variation is predominant, then, no inferences about the globally applied program effect on human capital can be made from the estimates of the site-specific program effects, unless migration selectivity is taken into account. Therefore unlike in the previous case, when there is endowment heterogeneity, a human capital subsidy program could lower the average human capital in the population at the site in which it is implemented as a result of migration selectivity even if it augments the human capital of any randomly-selected household.

Similar expressions can be derived for the selectivity biases in the estimated effects of a human capital subsidy on family size. In both the tastes and additive endowment heterogeneity cases, selective migration will lead to a negative bias--the human capital subsidy attracts low-fertility households in both cases, as was shown, and the negative response of fertility to the subsidy is stronger in both the low-μ and high-α_3 households, who make up the migrant population. Selective migration is likely to make a site-specific human capital subsidy program appear more anti-natalist than a similar but globally-applied program.

2. Empirical Application

a. Migrant Selectivity

To test the migration selection hypotheses requires a data set that at a minimum identifies migrants and residents at a specific site or sites and provides the characteristics of both groups before and after the implementation of and/or changes in a public program. In 1968, a program providing home-based preventive and maternal child health services was initiated in a small village in Colombia, Candelaria, and detailed longitudinal information was collected from 1968 to 1974 on the characteristics of parents and on the health of children aged less than six. All households present in Candelaria at any time during the seven-year period with a child under six years of age were included in the program, in which nurse-volunteers ("promotoras") visited each household approximately every two months.[7] Since Candelaria is a small village which serves in part as an "intermediate" stopover for many migrants from outlying rural areas to Cali, information on opportunities in Candelaria is disseminated relatively rapidly in outlying areas and in-migrants make up a significant proportion of the population. While the Candelaria data are thus unique in permitting identification of in-migrants and residents and in providing pre-program, baseline data on both migrants (at time of entry) and residents, there is no information on the characteristics of migrants at origin or of the general origin populations. Thus, the health of the children of in-migrants to Candelaria can only be compared to Candelaria residents, although comparisons of the fertility of migrants with non-Candelaria, origin populations are feasible, given the availability of the 1973 Census of Colombia.[8] Pre-migration characteristics must also be estimated.

In order to test for the existence of as well as to characterize the source of migrant selectivity, if any, arising from the incentives created by the Candelaria program, we need to compare, within observationally-identical groups, the pre-program family size and some measure of the pre-program human capital of the children of migrants (households who came to Candelaria after 1969) and residents (households residing in Candelaria when the program was initiated in 1968). We use the age-standardized weight of children as a measure of human capital, since weight is the only health outcome collected in all years of the program. Because the standardization required is one that is independent of the program and relevant to the population studied, the average weight in 1968 of (resident) Candelaria children for each age-sex group is used as the standard; that is, the age-weight distribution in effect at the initiation date of the program. Since some ages (in months) were not represented in this group and others had relatively small sample sizes, a fourth-order polynomial regression of these mean age-specific weights for each sex group was used to smooth the base. A child's weight-for-age was thus defined as the ratio of the weight at his/her own age to the standard weight at that age.

Table 1 provides the sample characteristics of resident and migrant families. As hypothesized in the previous section, migrant households have higher (age-standardized) incomes on average, although the slope of the migrant age-income profile is less steep for migrants. Migrant families also

Table 1

Sample Characteristic: Resident and Migrant Families

Variable/Statistic	Resident Families Mean	Resident Families S.D.	Migrant Families Mean	Migrant Families S.D.
Pre-program children ever born	4.49	2.75	3.22	2.46
Pre-program mean child weight-for-age, 0-6	.994	.129	.997	.151
Income	959	371	1130	597
Income when entered program	720	304	1007	594
Log of income, father aged 20 years	6.53	.134	6.88	.319
Slope of log-income profile, father aged 20 years	.0198	.00857	.00705	.0154
Curvature of log-income profile ($x10^{-3}$)	-.791	.383	-.617	.430
Years of schooling-mother	2.54	1.52	2.66	1.63
Years of schooling-father	2.76	1.58	2.98	1.54
Age of mother	28.3	6.31	26.2	6.88
Age of father	34.6	8.97	32.1	8.38
Mean program exposure of children 0-6 (months)	18.4	6.32	10.8	7.36
Mean proportion of lifetime children exposed to program	.619	.213	.475	.295
Number of families	208		280	

had fewer children than resident families prior to entering the program and lower fertility than that for all rural households in Colombia (1973 Census of Colombia). While this differential conforms to the prediction of the theoretical analysis that low-fertility households would be most attracted to a program subsidizing human capital investments, whatever the principal source of heterogeneity, migrant parents are also on average younger, more educated and wealthier than resident parents. Differences in family size may thus be due to these differences in observed characteristics and tests for selectivity must be performed within observationally identical groups. Estimation of the household demand equations for fertility and health is thus required to investigate the sources of heterogeneity and selection.

As was demonstrated, the existence of heterogeneity in tastes, technology and/or endowments implies that all of the coefficients of household demand equations will be family-specific. This suggests the following estimating relationship:

$$(22) \quad K^i = X^i g^i + \varepsilon^i$$

where K^i is either the pre-program children ever born or (log) weight-for-age variable for family i, X^i is the set of exogenous characteristics of the household conditioning these choices, g^i is the family-specific parameter vector, and ε^i is a random term.

It is assumed that:

$$E\begin{bmatrix} g^i \\ \varepsilon^i \end{bmatrix} = \begin{bmatrix} \gamma \\ 0 \end{bmatrix}$$

$$(23) \quad V\begin{bmatrix} g^i \\ \varepsilon^i \end{bmatrix} = \begin{bmatrix} \Lambda & 0 \\ 0 & \sigma^2 \end{bmatrix}$$

where $$\Lambda = \begin{bmatrix} \sigma^2_1 & & & 0 \\ & \sigma^2_2 & & \\ & & \ddots & \\ 0 & & & \sigma^2_k \end{bmatrix}$$

This is the standard random coefficients model; the estimating equation (22) may be written as

$$(24) \quad K^i = Y^i\gamma + \varepsilon^{i*}_j$$

where $\varepsilon^{i*} = (g^i - \gamma)\, X^i + \varepsilon^i$. Since the ε^{i*}'s are heteroscedastic, a generalized least squares (GLS) estimator will yield consistent and efficient estimates of the ρ's and σ's. Selectivity implies that the means of the family-specific parameters γ will differ across the migrant and resident populations.

The demand equations (24) must be modified to take into account the life-cycle nature of fertility and child health investment decisions, not incorporated, for simplicity, in the models of the previous section. First, health is a stock that is presumably a function not only of current inputs but of all inputs applied in the past, and current family size also reflects past fertility decisions. Thus, the reduced form health and fertility demand functionswill contain the determinants of all current and past inputs. Second, in a life-cycle context with perfect foresight, input demand, health demand and fertility decisions at any point in the life-cycle will depend on future, current, and past income and prices. Log income age profiles were thus estimated for residents and migrants separately using all reported income data points over the seven-year sampling period and information on occupation, age and schooling attainment.

For migrants, as noted, income prior to migration is unavailable. To the extent that there is an important structural shift in the income profile associated with migration, the profiles of migrants may be misrepresented. To ascertain if this absent information could account for any differences in parameter estimates obtained across the migrant and resident populations, we also estimate a child health equation based on the subsample of children born after the program was begun and/or after migration to Candelaria. For this subsample, children of the same age do not differ with respect to their exposure to the program and information on actual family income is available for every year of the child's life whether the child is from a migrant or resident household.

The household log income profile is measured by three statistics: the constant in the log income equation evaluated at husband's age = 20, the first derivative evaluated at husband's age = 20 and the second derivative. These terms differ in the sample by the level of husband's education and husband's occupation. Mother's schooling is included in (24) as an observable characteristic that may shift tastes, technology or endowment parameters. Mother's age is also included in the children ever born equation as a life-cycle standardization; it is not included in the weight-for-age equation, since that is already appropriately age-standardized.

Table 2 presents the relevant random-coefficient GLS demand equation estimates, the first column for fertility (children ever born) prior to entry into the program and the second column for the pre-program (log of) mean child weight-for-age. The third column reports OLS estimates for the (log of) standardized child weight for children born after program entry. Only OLS estimates are reported for that subsample because the GLS procedure produced a large number of negative variance estimates of household-specific parameters. The reported t-values in column 3 may therefore be biased, although the coefficient estimates are consistent.

The three specifications reported include only an intercept dummy variable taking on the value of one for resident households in order to distinguish

Table 2

Migrant Selectivity, Family Size, and Child Health

Dependent Variable: Variable/ Estimation Procedure	Children Ever Born at Program Entry Random Coefficient-GLS	Log of Mean Child Weight for-Age at Program Entry [b] Random Coefficient-GLS	Post-program Children: Log of Mean Weight-for-Age [c] OLS
Resident	.565 (2.90) [a]	.0209 (1.10) [a]	.0462 (2.46) [d]
Mother's schooling	-.416 (3.60)	-.00505 (0.41)	-.00764 (0.64)
Income level (log)	.269 (0.30)	.155 (1.81)	.150 (1.82)
Income slope	-9.25 (0.42)	1.62 (0.80)	.986 (0.51)
Income slope derivative	-.639 (1.42)	-.814 (0.02)	5.97 (0.18)
Age of mother	.409 (11.6)	- -	- -
Age of mother squared	-.00185 (3.70)	- -	- -
Intercept	-7.09 (1.16)	-1.07 (1.85)	-1.03 (1.86)
R^2	.489	.037	.035
n	456	458	303

a Asymptotic t-ratios beneath regression coefficients.
b Children born before the family entered program.
c Children with full program exposure since birth; i.e., born after the family entered the program.
d t-ratios beneath regression coefficients.

migrants from residents. Regressions which are fully interactive with respect to the residence dummy, as are indicated by the theory, were also estimated, but are not reported since the overall story is unchanged with the more parsimonious specification. Most of the estimates of the individual interaction coefficients were not measured with much precision; however, F-tests reject at the five percent significance level the hypothesis that the migrant and resident pre-program demand equations are identical. Migrant selectivity is indicated.

The set of resident dummy coefficients reported in Table 2 conform to the scenario in which endowment heterogeneity dominates tastes heterogeneity. Within income/schooling groups, migrants to Candelaria had both lower pre-program family size and children with lower age-standardized weight upon entry and after compared to residents. These findings thus suggest that the immigrants drawn to Candelaria were selected not only from the upper tail of the income distribution (Table 1), but, within income groups, were self-selected from the lower tail of the endowment (μ) distribution.[9] Evidently, the slightly higher child weight observed for migrants at entry in Table 1 is due to the higher household income of migrants; the estimated income level coefficients in columns 2 and 3 confirm that higher income households value health human capital more highly (one-tail test, five percent level).

b. Evaluation of the Effectiveness of the Candelaria Program

The single-site sample design of the Candelaria data set would appear to preclude any evaluation of the promotora program, since all households face the same subsidy. However, in a life-cycle context the total subsidy varies across children to the extent that children of the same age were exposed to the subsidy for different lengths of time, a greater number of health inputs being subsidized for children within the same age group but exposed earlier to the program. There are thus two sources of variations in the total program subsidy: First, since the dissemination of information about the Candelaria program to outlying areas and migration itself takes time, children of migrants, while facing the same subsidy as the children of residents when they arrive, will not be exposed to the program for the same length of time, given their ages, as resident children. This differential is evident in Table 1; mean months of program exposure for migrant children is less than 60 percent that of resident children. Since our results indicated that migrant children have lower health (due to selection), use of the cross-child variation in program exposure to assess the impact of the program without attention to migrant selectivity would appear to result in an upward bias in the estimate of program effectiveness. However, we also showed that low-μ households may respond more positively to a human capital subsidy; the direction of the selectivity bias in the program exposure estimate is thus ambiguous.

The second source of variation in program exposure arises from variation in the birth dates of resident children who were born prior to the program (1968). For such children, the sample would appear to approximate an experimental design as long as the program was unanticipated. However, since children born at later dates on average are born later in their parents' life-cycle, the cross-sectional variation in program exposure among resident children may also be correlated with their health endowments or with parental preferences for health, if these characteristics also influence the timing and spacing of births.[10] A relationship between program exposure and health might thus exist in the absence of any true program effect even among children of residents.[11]

To assess the consequences of migration selectivity for estimating the effectiveness of the Candelaria program thus requires attention to both sources of potential bias arising from the use of exposure information to measure subsidy differentials. The longitudinal data on the health of individual children permits this separation. Consider the reduced form estimating equation for child-specific health:

$$(25)\quad H^{ij} = \gamma^{ij} E^{ij} + \mu^{j} + \varepsilon^{ij},$$

where H^{ij} is the health of child j in family i, E^{ij} is the length of program exposure, γ^{ij} is a random coefficient on exposure, μ^{j} is a child-specific health endowment, and ε^{ij} is a random time-varying health component. All other family characteristics are suppressed for simplicity. Population heterogeneity implies that γ^{ij} differs across children if health endowments differ, since, as we have shown, the effect of s on the demand for the human capital input X depends upon fundamental parameters. Program exposure, which depends on the child's date of birth and/or on the timing of migration, may be correlated with the unobserved health endowment as a result of timing and spacing decisions and migration selectivity. With multiple observations for each child, however, a random coefficients fixed effect estimator can be used to purge out the family and child-specific health endowment.[12] Rewriting (25) in differential form yields

$$(26)\quad \Delta H^{ij} = \gamma^{ij}\, \Delta E^{ij} + \Delta\varepsilon^{ij}.$$

GLS estimation of equation (24) provides a consistent estimate of the program exposure effect for the sample of <u>resident</u>-household children. However, if migrant-household children are included in the sample, the distribution of the γ_{ij}'s will be truncated when migrant households are not randomly drawn. This leads to the standard sample selection problem, since $E[(\gamma^{ij} - \gamma)\ \Delta E^{ij} + \Delta\varepsilon^{ij}|\text{migrant}] \neq 0$.

To obtain a consistent estimate of the program exposure effect, and thus the effectiveness of the program, it is thus necessary to restrict attention to resident children born prior to the program or to attempt to correct for the sample (migration) selection. We refrain from employing one of the standard sample selection correction procedures (Heckman, 1979; Olsen, 1980) since that would entail imposing further, and arbitrary, structure on the problem. The resident subsample should be large enough to permit precise estimation of the program exposure effect and thus an assessment of bias due to selective migration.

Table 3 reports estimates of the program exposure effects for both the full sample of children (inclusive of migrants) and the sample of resident children. Estimates from two specifications are reported, a linear specification in which the exposure effect is assumed to be identical across education/income groups and an interactive specification, which allows exposure effects to differ by parental characteristics, as is consistent with our linear-quadratic example.[13] Both specifications are estimated using ordinary least squares and the GLS random-coefficient fixed effect estimator FE-RC). For both samples and both estimation procedures, however, F-tests reject the linear specifications; Table 4 reports the <u>per-month</u> exposure effects on standardized weight by income levels implied by the interactive estimates.

Comparisons across samples and across estimates permit an assessment of the separate roles of migrant selectivity and within-group heterogeneity. Whatever the specification or estimation procedure, however, estimates

Table 3

Migrant Selectivity and Program Exposure Effects on Log of Child Weight-for-Age

Sample:	Migrants + Residents				Residents			
Estimation Procedure	OLS	OLS	F.E.	F.E.	OLS	OLS	F.E.-R.C.	F.E.-R.C.
Exposure (months x 10^{-2})	.116 (10.58)[a]	-1.59 (1.95)[a]	.408 (37.63)[a]	-1.67 (2.02)[a]	.0736 (5.56)[a]	-4.99 (0.43)[a]	.000786 (0.11)[b]	3.57 (5.36)[b]
Log of income, father at age 20	.136 (7.77)	.0977 (3.72)	-	-	.118 (4.38)	.104 (2.45)	-	-
Income slope	1.69 (3.95)	1.30 (1.98)	-	-	2.21 (3.90)	1.58 (1.77)	-	-
Income curvature	33.7 (4.54)	41.9 (3.67)	-	-	47.9 (4.86)	68.0 (4.30)	-	-
Schooling mother: more than one standard deviation below the mean (1)	.00227 (0.34)	.00509 (0.51)	-	-	-.00184 (0.20)	-.00593 (0.40)	-	-
Schooling: within one standard deviation below the mean (2)	.00365 (0.38)	-.00711 (0.50)	-	-	-.0107 (0.80)	-.0110 (0.52)	-	-
Schooling: (3) within one standard deviation above the mean (2)	.0285 (0.21)	-.0172 (0.87)	-	-	.00376 (0.20)	-.0290 (0.99)	-	-
Income × exposure	-	.00241 (2.13)	-	.00296 (2.60)	-	.000572 (0.35)	-	-.00487 (5.24)
Slope × exposure	-	.0216 (0.76)	-	.0235 (0.83)	-	.0296 (0.87)	-	.0208 (1.07)
Curvature × exposure	-	-.549 (1.15)	-	-.915 (1.96)	-	-.991 (1.61)	-	3.23 (13.0)
Schooling (1) × exposure ($\times 10^{-3}$)	-	-.103 (0.25)	-	-.377 (0.90)	-	.150 (0.27)	-	-1.55 (4.27)
Schooling (2) × exposure ($\times 10^{-3}$)	-	.682 (1.14)	-	.485 (0.80)	-	1.02 (1.29)	-	-2.42 (4.86)
Schooling (3) × exposure ($\times 10^{-3}$)	-	.124 (1.48)	-	.230 (0.27)	-	1.55 (1.42)	-	-2.57 (4.10)
Intercept	-1.02 (8.00)	-.657 (3.48)	-	-	-.804 (4.16)	-.669 (2.20)	-	-
R^2	.036	.033	.188	.193	.021	.023	.0001	.031
n	7583	7583	6126	6126	4540	4540	1877	1877

a Absolute values of t-ratios beneath regression coefficients.
b Absolute values of asymptotic t-ratios beneath regression coefficients.

Table 4

Exposure Effects by Income Level: Percent Change in Standardized Weight per Month[a]

Exposures Estimation Procedures	Migrants + Residents OLS	 Fixed Effect	Residents OLS	 Fixed Effect
Two σ above the mean	.0081	.376	.015	.015
One σ above the mean	.0056	.337	.0071	.080
Mean	-.057	.298	.0005	.145
One σ below the mean	-.089	.258	-.008	.211
Two σ below the mean	-.121	.218	-.016	.276

a Evaluated at mean mother's education.

from the sample including both migrants and residents greatly overstate on average the health consequences of program exposure net of migration selection effects, with the greatest differential displayed by the fixed effect estimates.[14] Moreover, the patterns of exposure effects by income revealed by the fixed effect estimates taken from the resident and full samples are quite different (Table 4) -- the per-month exposure effects decline by income group in the full sample but increase with income in the resident sample.[15]

The fixed effect random coefficients model estimated in the residents-only sample, which is presumably free of selection and heterogeneity biases, indicates that a child exposed for 1.5 years to the Candelaria program (the sample mean for residents) and who lives in a household with an income level two standard deviations below the Candelaria mean would experience a five percent gain in weight-for-age; a similarly-exposed child from a household with income at the mean would experience a 2.6 percent increase in weight-for-age, while children from households with incomes more than two standard deviations above the mean would benefit little from the program.[16] The comparable full sample estimates imply that migrant selectivity leads to an overestimate of the program exposure effect by 2400 percent among households with incomes at least two standard deviations above the mean, and a 106 percent overestimate at the mean, while the exposure effect is understated by 21 percent among households with incomes less than two standard deviations below the mean. The program thus appears to have benefited most the children of poor residents and wealthy migrants, and to have attracted, among wealthy potential migrants, those who benefit most from the program and, among poor potential migrants, those who benefit least.

3. Conclusion

When agents are heterogeneous, a change in relative prices within a discrete geographical area or activity has two distinct effects. It alters the allocation of resources by each agent facing the price change and changes the composition of agents within the location or activity. While most empirical studies have been concerned with testing the allocative responses of a representative agent to changes in incentives, the change in the spatial distribution of differentiated agents in response to area-specific conditions, the central implication of the Tiebout hypothesis, has received little theoretical development or empirical verification. In this paper we have explored the consequences of a site-specific program subsidizing human capital investments in children for both the spatial distribution of heterogenous households and for the level of human capital investment by a representative household. We show that with plausible restrictions on the optimizing behavior of each household, such a program precipitates in-migration by high-income and low-fertility households, whether the principal form of heterogeneity is in tastes or in human capital endowments. With endowment heterogeneity dominant, however, households also characterized by low levels of human capital and/or with smaller returns to investments in human capital are attracted to a program subsidizing human capital investment.

Data from a village in Colombia that implemented a subsidized health program confirm these implications of selective migration--in-migrants were evidently drawn from the low-tail of the family size distribution, were of relatively high income and, within income groups, had children whose nutritional status was lower than that of observationally identical members of the resident population. As a consequence, evaluations of the program inattentive to migration selectivity based on differences in program exposure across children born prior to the program were shown to significantly overestimate the impact of the program for any randomly-drawn household. Program evaluations based on comparisons of the mean nutritional status of children born after the program with that of children born prior to the program and never exposed to it, however, would seriously understate the effectiveness of the program due to the selective migration of low-endowment households. Indeed, the empirical results suggest that it is possible that the equilibrium mean health of children in the village, due to migration, will be lower after than before the health program, with mean health levels increased in areas external to the program site.

Our empirical results suggest that in a country such as Colombia or the United States where the population is highly mobile, tests of theories of the behavior of individual agents based on cross-sectional data or studies of the determinants or consequences of laws based on the exogenous spatial distribution of population characteristics may be seriously flawed. The existence of migration selectivity has implications beyond those relevant to the estimation of behavioral models from cross-sectional data or to the evaluation of location-specific programs, however. Consider a national immigration policy, for example, that does not discriminate by an immigrant's country of origin. Due to differences in migration costs (distance) and in relative prices across potential sending countries, immigration will be differentially self-selective across country-of-origin groups. Such differential selectivity will result in the observed behavior of immigrants being correlated with their country-of-origin (Chiswick (1978)) even if the distributions of population characteristics in sending countries are identical. Moreover, our framework implies that an overall increase in barriers to immigration makes immigration more selective. How immigrants differ by country-of-origin or whether the laissez-faire selectivity arising from the decision-rules of optimizing potential immigrants is superior or inferior to selection imposed by law depend on the sources of heterogeneity and the nature of the relative price differentials across the sending and receiving countries. If immigrants are principally attracted by a country's superior opportunities for human capital investment, for example, our results imply that immigrants may be drawn from the lower tails of the human capital endowment distribution; however, the less so the smaller the direct costs of immigrating.

Finally, selective migration is a component of a broader class of problems. For example, as for migration, relative price changes as well as income growth may selectively alter fertility decisions, resulting in a change in the distribution of children across households of differing endowments and preferences for human capital investment, and thus in the endowments of the representative child. The long-term consequences of national programs may thus differ significantly from their immediate effects due to selectivity in fertility decisions. The further study of selection rules would appear warranted.

Footnotes

1. Gramlich and Rubinfeld (1982) test and confirm an implication of selective migration, that the variance in voter preferences for local public goods expenditures are smaller within than among urban localities, but do not test whether and/or how the local electoral outcomes affect residential mobility. Schultz (1983) tests for and confirms differences in behavior between migrants and residents in Colombia by origin and destination but does not incorporate migration decisions within his behavioral model or derive predictions for how interarea price differences generate selectivity rules.

2. While heterogeneity and selection, combined with information asymmetries, form the basis for many models of behavioral phenomena (e.g., Spence (1973); Guasch and Weiss (1981)), that literature has seen little empirical application. Heterogeneity and selection are also explicitly recognized in most econometric studies of labor supply behavior; however, selectivity is essentially treated as a nuisance rather than as a testable implication of the theory (an exception is Heckman (1974)).

3. The budget constraint ignores the interaction between the human capital of children and the number of children, as in Becker and Lewis (1973). Use of the non-linear budget constraint does not alter any of the testable implications of the model (Rosenzweig and Wolpin (1980)).

4. It is easy to demonstrate that households with higher returns to the human capital input X will always have higher levels of human capital. However, the higher return induces both an income effect, lowering the demand for X so as to allocate the higher wealth to the increased consumption of other goods, and a price effect, which raises the demand for

X. Note that the additive endowment only carries with it an income effect; higher μ households are wealthier and have healther children but do not obtain more human capital per unit of the input.

5. The subsidy effect on health, from (12), is

$$dh^i/ds = \beta[2\psi(\alpha^i{}_3 - 2\mu^i\alpha_4)\ \alpha_2\alpha_6 P_X\beta^{-1}\alpha_4{}^{-1} - (\phi - \beta\alpha_4\psi)\ (\alpha_2\alpha_5 - 2\alpha_2\alpha_6 Y + \alpha_1\alpha_6 P_N)]\ [2(\beta\alpha_4\psi + \phi)]^{-1},$$

from which subsidy-effect differentials can be computed when α and μ differ.

6. $dh_M/ds = (d\alpha_{3M}/ds)\ (dh_M/d\alpha_{3M}) = \beta C/s^2$.

7. The program was funded by the U.S. Agency for International Development; Candelaria residents and in-migrants thus did not incur any direct program costs.

8. Various health programs had been in operation in Candelaria before, but not after, the implementation of the "promotora" program. As a consequence, rates of malnourishment and fertility in Candelaria were lower than in the overall population in Colombia prior to 1968 (Heller and Drake (1979). Since in our sample only post-1968 migrants can be identified, recent but pre-1968 migrants attracted to the prior health programs will be counted as residents; differences between residents and post-1968 migrants will thus underestimate the selectivity induced by a health subsidy program.

9. As noted above, the existence of pre-1968 programs minimizes the estimated health differential between the post-1968 migrants and the pre-1968 residents in Candelaria since some proportion of the latter were attracted by the prior health programs. However, while the estimates in Table 2 are thus lower bound estimates of the migrant-resident health differential, it is possible that migrants are not drawn from the lower tail of the

health endowment distribution characterizing the non-Candelaria origin populations, since these populations exhibit lower mean health than do the residents in Candelaria. The Colombian Census data indicate that post-1968 migrants to Candelaria do exhibit lower fertility than in the rural population as a whole as well as in the Candelaria resident population.

10. That spacing patterns are related to household health endowments in Candelaria households is shown in Rosenzweig and Wolpin (1984).

11. Heller and Drake (1979) exploit differences in program exposure among children to evaluate the effectiveness of the Candelaria program. They ignore the selectivity associated with both migration and parental spacing decisions. Their specifications estimate program exposure effects conditional on such endogenous variables as parental breast-feeding, use of medical services, and food expenditures; their findings thus cannot be compared with our reduced-form estimates.

12. A within-family (cross-child) fixed effect estimator would not provide consistent estimates if endowments differ among children and the spacing of children responds to realizations of child-specific endowments. In that case, a child's health outcome will affect the interval to the next child so that the difference in program exposure between children within the same family will be related to their relative health as part of the family's optimization process and regardless of the program. Evidence on these dynamic spacing patterns is presented in Rosenzweig and Wolpin (1984).

13. The subsidy effect, given in footnote 5, depends on fundamental parameters as well as on income.

14. Similar results are obtained when the fraction of the child's life during which he or she is exposed to the program is used to measure exposure. With either measure, identification of the program effect arises from the nonlinear relationships among age, exposure and weight induced by the age/sex standardization.

15. These differential program effects by income level are not due to program design but reflect the nonlinearity in income of the input demand equation.

16. Part of the impact of the program appears to work via encouraging greater and/or more rapid investments in children. Fixed effect (logit) estimates indicate that children of the same age but exposed longer to the program were more likely to be receiving breastmilk and to have received innoculations against diptheria, polio or tetanus.

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