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Life-Cycle Labor Supply and Fertility: Causal Inferences from Household Models

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I. Introduction

The purpose of this paper is to explore the theoretical and empirical relationship between female labor supply and fertility in the context of a life-cycle decision making process. Our starting point is the well documented empirical finding that younger women with lower measured fertility engage in market activity more frequently and more intensively as estimated from participation or hours regression equations that treat fertility as exogenous (Heckman 1974, Gronau 1973, Heckman and Willis 1977). This research stands in sharp contrast to an equally vast literature that treats fertility as a choice variable (Willis 1973, Becker and Lewis 1973). Recognition of this inherent conflict has spurred several attempts at specifying and estimating simultaneous equations models (Cain and Dooley 1977, Schultz 1977, Stafford and Hill 1978). Although estimates of the fertility-labor supply relationship abound, a full appreciation of the interpretation of such estimates has been lacking regardless of the empirical strategy.[1] This paper attempts to elucidate, therefore, what information is contained in the estimated association between fertility and labor supply as calculated from "single" and "simultaneous equations" estimation techniques. In addition, an empirical methodology is developed to estimate the impact of exogenously distributed children on life-cycle labor supply decisions.

In section II, several utility maximization models are presented which are intended to illuminate the essential links between empirical estimates of the fertility-labor supply association and economic theory. These models incorporate within a static framework notions of child-specific investments and market-specific human capital atrophy. In section III, we present a statistical methodology based upon the occurrence of twins on the first pregnancy and provide estimates, using that methodology, of the extent to which women respond to exogenous (and, in this case, unanticipated) extra children. Section V summarizes.

II. Models of Fertility and Labor Supply

a. Heterogeneity, Fertility Effects and "Structural" Equation Estimation

We first consider the information contained in the observed, *ceteris paribus* relationship between fertility and labor supply based on a one-period (lifetime) model incorporating a production function for child services. We assume that individuals maximize a utility function, given by (1), where ℓ is a

$$(1) \qquad U = U(\ell,n,X,q;\varepsilon)$$

measure of lifetime home time, n is the number of children, q is the services (quality) from each child, and X represents other goods consumed. Function (1) has the usual neoclassical properties, but the population is heterogenous with respect to preference orderings over the four goods, as represented by a randomly distributed shift parameter ε, such that $U_{\ell\varepsilon}$, $U_{n\varepsilon} \neq 0$, $U_{x\varepsilon}$, $U_{q\varepsilon} = 0$.[2] The production of child services q is described by a production function (2). While for simplicity it is assumed that q is produced jointly with ℓ and n, function (2) captures the

$$(2) \qquad q = \theta(\ell,n)$$

critical assumptions found in the household production fertility literature (Willis, 1975; Becker and Lewis, 1975) -- non-jointness of quality production across children and time-intensity -- since we assume that $\theta_\ell > 0$ and $\theta_n < 0$ and no purchased goods inputs are used to produce q.

Each individual owns a stock of human capital from which he earns a return r and faces with certainty prices for X, p_x, and n, p_n upon which information fertility and labor supply plans are made. It is important to note that p_n is the cost of increasing the number of children independent of q; e.g., birth and contraceptive costs, and should not be confused with the shadow price of quality.[3] The full-income budget constraint is thus:

(3) $$F = rHT + A = p_x X + p_n n + rH\ell$$

where A is exogenously-given asset income. Individuals maximize (1) subject to (3) with respect to ℓ, n, and X, given (2).

The model yields four first-order conditions as well as three demand equations for the endogenous or choice variables ℓ, n, x in terms of the exogenous variables, p_x, p_n, r, H, F and ε. The first partials of these functions are approximated by totally differentiating the four first-order conditions. Assuming interior solutions, the relationships between infinitesimal changes in n and ℓ with respect to changes in prices and ε along the indifference curve associated with the optimal solution are:

(4) $$dn = \emptyset^{-1}\lambda\left[\emptyset_{11}dp_n+(U_{n\varepsilon}\emptyset_{11}+U_{\ell\varepsilon}\emptyset_{21})d\varepsilon+r\emptyset_{21}dH+\emptyset_{31}dp_x\right]$$

(5) $$d\ell = \emptyset^{-1}\lambda\left[\emptyset_{12}dp_n+(U_{n\varepsilon}\emptyset_{12}+U_{\ell\varepsilon}\emptyset_{22})d\varepsilon+r\emptyset_{22}dH+\emptyset_{23}dp_x\right]$$

where $\emptyset$ is the determinant of the bordered Hessian, the $\emptyset_{ij}$ are the cofactors from the i^{th} row and j^{th} column of the Hessian determinant and λ is the Lagrange multiplier.[4] As is well-known, all of the predictions of the model, the tastes-constant price-effect information, are contained in the cofactor terms; i.e., $\emptyset < 0$, $\emptyset_{ii} > 0$, etc. Thus only empirical estimates which provide information on the $\emptyset_{ij}$ can be used to test the model or to ascertain the potential effects of policy on behavior. Expressions (4) and (5) also indicate that the parameterization of population heterogeneity in preferences embodied in (1) implies that the ε's affect the demand for home time and children in the same manner as do prices.

To ascertain what information the population association between n and ℓ provides, assume that F, r, H and p_x are "controlled", but we have no information on p_n or the unobservable tastes parameter ε, both of which may vary across individuals. An approximation to the slope of the relationship between ℓ and n in the population, from (4) and (5) is given by :

$$(6) \qquad \frac{d\ell}{dn} = \left[\frac{\phi_{21}}{\phi_{11}} + \frac{(U_{n\varepsilon}\phi_{12} + U_{\ell\varepsilon}\phi_{22})d\varepsilon}{\phi_{11}dp_n}\right]\left[1 + \frac{(U_{n\varepsilon}\phi_{11} + U_{\ell\varepsilon}\phi_{21})d\varepsilon}{\phi_{11}dp_n}\right]^{-1}$$

As can be seen, (6) reflects both the structure of the model as given by the ϕ_{ij} terms as well as the unobserved population difference in ε and p_n. If it could be assumed that preferences did not vary ($d\varepsilon=0$), however, the slope of the fertility labor supply relationship would provide information on price effects, being equal to ϕ_{12}/ϕ_{11}, the ratio of the compensated cross price effect of p_n on ℓ (or rH on n) to the compensated own price effect on n. Since $\phi_{11} > 0$, the sign of (6) would thus provide the sign of the effect, say, of a compensated change in contraceptive costs on labor supply.

While the actual relationship between fertility and labor supply thus cannot provide information on tastes-constant cross-price effects when both p_n and ε vary and are unobserved, if p_n did not vary, population heterogeneity in "tastes" could provide the same information as variation in p_n. If it were known that ε only affected either U_ℓ or U_n, but not both, then with $dp_n=0$, we see from (4) and (5) that the slope $d\ell/dn$ would provide the sign of $\phi_{12}(= \phi_{21})$.

It can easily be demonstrated, based on the rationing or conditional demand literature (Tobin and Houthakker, 1952; Pollak, 1969) that treating n as a parameter $\bar{n}$, and varying it around the unconditional equilibrium that would have obtained, i.e., around planned or optimal n, yields the compensated price effect information not obtainable from the actual n-ℓ association in a non-heterogenous population; i.e.,

$$(7) \qquad \frac{d\ell}{d\bar{n}} = \frac{\phi_{21}}{\phi_{11}} = \frac{d\ell/dp_n}{dn/dp_n} .$$

Thus if we had an experiment in which heterogenous (in ε) individuals were constrained randomly with respect to fertility -- some bearing more children or some less than the planned or optimal number -- the observed differences in labor supply associated with $\bar{n}$ would provide the tastes-constant compensated cross and own price effect ratio.

These considerations both illustrate the rationale for the use of simultaneous equation techniques for estimating the effect of n on ℓ as well as their redundancy. Use of two stage least squares, say, is an attempt to obtain an estimate of $d\ell/dn$ where n does not reflect variation in unobserved preferences, which it otherwise will even if tastes differ in the population only with respect to preferences for ℓ (i.e., $U_{\ell\varepsilon} \neq 0$ only). However, the only source of variation in n across individuals not due to differences in preference orderings must be differences in the own price of children, p_n (p_x, F, r, H but not ε are assumed to be fixed, or "controlled"). The identifying instrument for the second-stage labor supply equation conditional on "predicted" n is thus, in this case, p_n or its proxy. Of course, this normalization is perfectly arbitrary as it is possible to use any one of the set of labor supply determinants, omitted from that relationship, as the instrument for fertility. However, since the coefficient on predicted fertility in the conditional labor supply equation yield only the <u>ratio</u> of compensated p_n effects, as in (7), it is obvious that the same information could be obtained by estimating the usual demand equations for n and ℓ, from which Hicks-Slutsky price and income effects are obtained directly. Thus if p_n is known and ε is not, the 'exogenous' impact of n on labor supply $d\ell/d\bar{n}$ can be estimated (is identified), but the information it conveys is less informative than that from the "reduced forms." Note, however, that if ε is known but not p_n <u>and</u> it is assumed that $U_{n\varepsilon} \neq 0$ only, that is if we impose a certain structure on "tastes" for n when we have no information on prices, the same compensated <u>price</u> information can be obtained, but again through <u>either</u> the usual demand or the conditional demand equations using simultaneous equation techniques.

Thus if preference orderings for home time vary parametrically and fertility is a choice variable whose price also varies, labor supply equations which include some measure of actual fertility among the regressors will provide inconsistent estimates of conditional price effects. While simultaneous equations techniques can yield consistent estimates of (7), the effects of other prices and income on labor supply in an equation conditioned on fertility will, of course, differ from the Hicks-Slutsky effect obtained from the (unconditional) demand equations, which are functions only of prices and income. The signs of the Hicks-Slutsky and conditional compensated price effects are the same, however. For example, the compensated "wage" (rH) effect on home time, from the usual demand equation (which holds other prices fixed) is

$$(8) \qquad \frac{d\ell}{d(rH)} = \frac{\lambda \emptyset_{22}}{\emptyset}$$

while the own price effect on home time conditioned on $n=\bar{n}$ is simply a scalar times the inverse of the compensated own price effect on n, i.e.,

$$(9) \qquad \left.\frac{d\ell}{d(rH)}\right|_{n=\bar{n}} = \frac{-\lambda p_x^{\,2}}{\emptyset_{11}} = \frac{-\lambda p_x^{\,2}}{\emptyset} \left(\frac{dn}{dp_n}\right)^{-1}$$

Thus, actual fertility may be substituted as a determinant of labor supply for p_n if the latter is not known, and consistent estimates of the price effect ratio (7) would be obtained only if preference orderings do not vary for home time. In that case, however, only consistent estimates of conditional price effects such as (9) are obtained. Estimation of labor supply equations without fertility or p_n included among the regressors, however, would obviously yield biased estimates of the Hicks-Slutsky price and income effects.

In section III we demonstrate that a consistent estimate of (7) can be obtained by using twin births without the need for information on either

p_n or ε. As we have seen, such an estimate yields the sign of the compensated price effects $d\ell/dp_n$ and $dn/d(rH)$, which is not predicted by the model as structured. If additional restrictions are placed on the model described by equations (1), (2), and (3), however, more inferences can be derived about the structure of the model from the estimate of the effect of an exogenous fertility change on labor supply. To see this, assume first that q is separable in (1). The numerator in (7) can then be written as

$$(10) \qquad \emptyset_{21} = \emptyset_{21}{}^{c} + p_x{}^2\left[U_{qq}\theta_n\theta_\ell + U_q\theta_{\ell n}\right]$$

where $\emptyset_{12}{}^{c}$ is the cofactor from the three-good model in which the home produced q is not a decision variable. $\emptyset^{c}{}_{21}$ thus corresponds in sign to the standard Hicks-Slutsky compensated cross price effect in that model, defining whether ℓ and n are substitutes or complements. The second term in (10) arises from the existence of the q-function, the assumption that a parent's utility is affected by the level of "quality" per child which requires home time to produce. If we were to further assume strong separability in (1), which implies that all compensated cross price effects are positive ($\emptyset_{12}{}^{c} < 0$), then a (weak) test of the existence of a q-function like (2) is established, since given strong separability and that $\theta_n < 0$, (10) could only be positive if the second term existed.[5] Under these assumptions, a negative relationship between labor supply and fertility, estimated consistently, would lead to a rejection of the labor supply model which ignores child quality.

b. <u>Life-Cycle Labor Supply, Serial Dependence and Fertility Effects</u>

The one-period model, by construction, ignores timing considerations in both labor supply and fertility. If home time or fertility in different portions of the life-cycle are not perfect substitutes, in contrast to the assumption in (1), if the production of children and thus q is constrained biologically to occur only in a certain period, and if future prices differ as a consequence of past behavior ("serial dependence"), then an intertemporal model may be required. Such considerations may be particularly important as inferences are to be drawn about behavior in section III from cross-sectional data without retrospective work histories and containing individuals at different points in the life cycle based on exogenous fertility events at one point in the life cycle. We now construct a model which incorporates these additional generalities to see if additional insights into life-cycle labor supply behavior can be obtained from the impact of an exogenous change in fertility on labor supply.

A model which would capture many of the important aspects of the relationship between fertility and labor supply would distinguish at least three periods -- two child-bearing periods, early and late, in which q is produced with home time -- and a third period corresponding to a time when children are mature and q is not produced. Such a model could thus capture optimal spacing decisions as well as the costs of changing contraceptive strategies associated with such behavior. To reduce complexity, however, we initially ignore this latter complication, which is discussed below, and instead construct a two-period model, corresponding to the fertile and non-fertile (no q produced) periods of the life cycle. The utility function is given by (11) and is assumed to be strongly separable in its arguments.

$$(11) \qquad U = (\ell_1, n, \ell_2, X, q; \varepsilon)$$

As demonstrated above for the one-period model, no testable hypotheses can be derived from knowledge of the sign of the compensated cross price effects without additional restrictions on the utility function.

The production of q is assumed to occur in the first period and is described by (12).

$$(12) \qquad q = \theta(\ell_1, n) \qquad \theta_{\ell_1} > 0, \qquad \theta_n < 0$$

The individual begins the planning period with an exogenously given stock of human capital H_1 which then depreciates as a function of home time in period 1. The dependence of H_2 on ℓ_1, and thus the price of ℓ_2 on ℓ_1, is given by (13), and will be referred to as serial dependence.[6]

$$(13) \qquad H_2 = \psi(\ell_1) \qquad \psi' < 0$$

The lifetime full income constraint, assuming a zero rate of interest, is thus

$$(14) \qquad r\left[H_1T_1+H_2T_2\right] + A = p_n n + p_x X + r\left[H_1\ell+H_2\ell_2\right]$$

where, again, all prices are fixed and known in advance with certainty. The first-order conditions of the model are given by

$$(15) \qquad U_n + U_q\theta_n - \lambda p_n = 0$$

$$(16) \qquad U_{\ell_1} + U_q\theta_{\ell_1} - \lambda\left[r(H_1-(T_2-\ell_2)\psi')\right] = 0$$

$$(17) \qquad U_{\ell_2} - \lambda r H_2 = 0$$

$$(18) \qquad U_x - \lambda p_x = 0$$

in addition to (14).

The intertemporal model differs from the one-period model in two important dimensions -- the existence of the depreciation function (13) and the addition of another home time variable which is not associated with the production of child services. Thus, for example, in this model the shadow price of home time in period one, from (16), depends on both labor supply in period two and the rate of human capital depreciation.[7] We now demonstrate that, given strong separability, consistent estimates of $d\ell_1/dn$ and $d\ell_2/dn$, which can be easily demonstrated to provide the signs of the cofactors $\emptyset_{12}$ and $\emptyset_{13}$ respectively from the 5x5 Hessian determinant of the two-period model, can be used to test for the existence of the q function, as was true in the first model. However, the existence of serial dependence cannot be disproved with such information.

While in the one-period model separability only in q in the utility function would have allowed a decomposition of the relevant cofactor into terms embodying separately the properties of the utility and child services production functions, in the intertemporal case the q and depreciation functions cannot be disentangled even if strong separability in (11) is imposed. However, under the latter assumption, the cofactors $\emptyset_{13}$ and $\emptyset_{12}$ can be split into two such additive terms if either the q or ψ functions are dropped. Both the decomposability of the cofactors as well as the sign imputation to the conventional (non-q, non-ψ) compensated cross price effects, given by the sign of $\emptyset_{13}$ and $\emptyset_{12}$, resulting from strong separability, allow us to sign $d\ell_1/d\bar{n}$ and $d\ell_2/d\bar{n}$ with serial dependence only.

Consider first the (exogenous) effects of n on ℓ_1 and ℓ_2 with the q-relationship suppressed, given by (19) and (20), separability assumed.

$$\text{(19)} \qquad \frac{d\ell_1}{d\bar{n}} = \left[\emptyset_{12}^{c} + \lambda r^2 \psi' H_2 U_{xx}\right] \emptyset_{11}^{-1} < 0$$

$$\text{(20)} \qquad \frac{d\ell_2}{d\bar{n}} = \left[\emptyset_{13}^{c} + \lambda r p_n U_{xx}(r(T_2-\ell_2)\psi'' H_2 + \psi'(H_1-(T_2-\ell_2)\psi'))\right] \emptyset_{11}^{-1} < 0$$

where
$$\emptyset_{12}^{c} = p_n U_{xx} U_{\ell_2\ell_2} r \left[\Pi_{\ell_1}\right] > 0$$

$$\emptyset_{13}^{c} = p_n U_{xx} U_{\ell_1\ell_1} r H_2 > 0 .$$

$$\Pi_{\ell_1} = \begin{cases} H_1 \text{ if } \psi' = 0 \\ H_1 - (T_2-\ell_2)\psi' \text{ if } \psi' \neq 0 . \end{cases}$$

The second terms in the numerators of (19) and (20) result solely from the serial dependence hypothesis and are both positive, if the second derivative of the human capital ψ function is small or negative. Because the first conventional cross price effect terms are the same sign as the ψ-related terms and because it can be easily shown that the q production equation (12) would add terms to (19) and (20) which are of ambiguous sign, as in (10), the finding that an increase in the number of children decreased labor supply at any point in the life cycle would lead to a rejection of the intertemporal labor supply model without time-intensive child services production. Given the sign ambiguity associated with the q-function hypothesis, it is obvious that even under the strong separability assumption the existence of serial dependence in human capital cannot be verified.

It is important to note that the difficulty of distinguishing the importance of child services production from serial dependence in the absence of direct information on (estimates of) equations (12) and (13) does not result solely from the fact that we may be limited only to information on the signs of the fertility effects on labor supply, which we provide in the next section. If we could estimate, for example, the effect of an exogenous change in past labor supply on current labor supply, i.e., $d\ell_2/d\bar{\ell}_1$, as in Heckman (1978), we could only discern (in a weak test) the existence of serial dependence if both separability is imposed and investment in children, at least as characterized by equation (12), is ignored. The latter relationship, given by (21) with q suppressed, is:

$$(21) \qquad \frac{d\ell_2}{d\bar{\ell}_1} = \left[\phi_{23}^{c} - \lambda r\psi'(U_{nn}p_x^{\,2} + p_n^{\,2}\,U_{xx})\right]\ \phi_{22}^{\,-1}$$

where $$\phi_{23}^{c} = r^2 H_2 U_{nn} U_{xx} \left[\Pi_{\ell_1}\right] > 0 .$$

The first term in the numerator embodies the conventional compensated cross price effect and is positive, given separability, while the second term, resulting from serial dependence, is negative. If early labor supply is observed to have a positive 'effect' on labor supply later in the life-cycle, assuming that the heterogeneity problem can be solved,[8] then the absence of serial dependence could be refuted. However, such a result is also consistent with a model in which serial dependence is ignored but the production of child services is a time-intensive activity since, under those assumptions,

$$(22) \qquad \frac{d\ell_2}{d\bar{\ell}_1} = \left[\phi_{23}^{\,c} + rH_2 p_n U_{xx}(U_{qq}\theta_{\ell_1}\theta_n + U_q\theta_{\ell_1 n})\right] \phi_{22}^{-1}$$

which can take on any sign. Thus the directional impact of labor supply behavior in one period on labor supply in another (with strong separability in the utility function assumed) can only provide evidence on the existence of <u>either</u> serial dependence or time-intensive home production of child services. The sign of $d\ell/d\bar{n}$, however, can provide evidence which is only consistent with a labor supply model incorporating the production of q.

<u>c. Child Spacing, Unanticipated and 'Planned' Births</u>

The previous models assumed that individuals are indifferent as to the timing of births. We can generalize the model of fertility and labor supply by also making children in each period imperfect substitutes in the utility and q functions and thereby capture optimal birth spacing considerations. In the general case the individual maximizes utility function (23)

$$(23) \qquad U = U(n_i, \ell_j, X_j, q; \varepsilon) \qquad i=1\ldots\beta,\ j=1\ldots\omega,\quad \beta<\omega$$

subject to constraint (24) and the q and depreciation functions (25) and (26), where β defines the (biological) end of the child bearing period, and

ω the planning horizon.

$$\text{(24)} \qquad r\sum^{\omega} TH_j + A = \sum^{\omega}(p_{xj}X_j + rH_j\ell_j) + \sum^{\beta} p_{nj}n_j$$

$$\text{(25)} \qquad q = \theta\ (n_i, \ell_i) \qquad i=1\ .\ .\ .\beta$$

$$\text{(26)} \qquad H_k = \psi(\ell_{k-1}, \ .\ .\ .\ell_1) \quad k=1\ .\ .\ .\beta$$

In this formulation an increase in an n_j for given total n is equivalent to closer spacing of births. The model thus captures the interrelationships between total family size n, spacing, labor supply, wage rates (r_jH_j) and q as well as the effects on optimal trajectories $(\ell_1, \ell_2\ .\ .\ .\ell_\omega)$ resulting from changes in exogenous variables, as in Razin's model (1977).[9]

The general model also illustrates more precisely than has been evident in the literature the distinctions between planned fertility, exogenous fertility, and unanticipated fertility.[10] In the context of the model, outcomes which are planned are those which correspond to the optimal values derived from maximizing (23) subject to constraints (24), (25), and (26). Optimal or planned changes in the choice variables are those which are made based on alterations in the exogenous variables which are forseen at the beginning of the planning period. Thus if fertility is forseen to be constrained exogenously in period j $(j \neq 1)$ at a level which deviates from the unconstrained or planned value, the planned levels of the choice variables may change in every period; thus $d\ell_i/d\bar{n}_j \lesseqgtr 0$ for $j > i$, for example.

An unanticipated event, such as a contraceptive "failure" or a multiple birth, occurs during fulfillment of the optimal plan. The response to this exogenous change, or placement of a binding constraint on a planned event, can only occur in future periods, i.e., $d\ell_i/d\bar{n}_j = 0$ for $j < i$ if

$d\bar{n}_j$ is unanticipated. In a sense, an unanticipated event in period j marks the beginning of a new optimization problem in period j. The responses to an exogenous fertility change occurring in the first period as discussed in the first two models will thus be identical whether that event was forseen or unanticipated. Another justification for treating fertility as an exogenous variable in a labor supply equation therefore is that births are themselves purely unforseen stochastic events rather than events which vary solely due to anticipated price changes (not to heterogeneity in preferences).

Another aspect of the difference between responses to foreseen and unanticipated changes in fertility at any stage of the life cycle, not captured in the model, is that the costs of averting births (a component of p_n) may be a function of the quantity of births to be avoided. Thus it may be more costly to achieve the same planned total number of children by compensating later in the life-cycle for an unanticipated rise in births at a young age compared to the original optimal trajectory of births n_i. Even if births in any period are perfect substitutes, as depicted in the first two models, therefore, an unanticipated "transitory" change in an n_j could alter total family size if contraception cannot be adjusted costlessly. If such costs are also positive functions of the rapidity of adjustment, the degree of lateness of an "extra" child will be negatively correlated with subsequent contraceptive intensity. Of course, unless the relationships in the utility function between period-specific births are known, no predictions about the impact of an exogenous change in any n_j on fertility patterns can be predicted.

III. Exogenous Fertility Effects: The Twins First Methodology

In order to estimate the impact of an exogenous change in fertility on labor supply based on non-experimental data from a heterogeneous population, it is necessary to simulate the appropriate experiment in which extra children are distributed to families in a manner unconnected to preference orderings. In this section, we show how a natural event, the occurrence of a multiple birth or "twins", can be used as an instrument for exogenous fertility movements. The variable we propose, a twins outcome on the first birth, approximates the social experiment we would wish to perform not only in that some families receive an unanticipated child while others do not, but also in that the treatment and control groups are randomly selected with respect to characteristics that may be related to market participation. It is, therefore, unnecessary to utilize any information on the determinants of labor supply behavior in order to determine the "true" exogenous fertility effect by this method.

To see why the occurrence of twins on the first pregnancy, "twins first", leads to the appropriate experiment, consider a comparison instead of women who have had twins on <u>any</u> birth and women who have not. Maintain for the moment the assumption that the probability of a twins birth is independent of parity. It is obvious that women with more births, and thus women with, on average, greater desired fertility, will be overrepresented in the sample of twins families. The labor supply of women with twins will therefore reflect in part any relationship between unobserved tastes for children and/or tastes for home time and labor supply as well as the impact of the additional unanticipated child which contains the price-effect information. Moreover, the per pregnancy probability of twins appears to rise with parity (Mittler 1971), reinforcing the positive covariance between desired family size,which may reflect differences in preference orderings, and twins occurrence.[11]

The first birth has the desirable feature that the population of women who experience twins on that birth would prefer the same completed family size as women who do not experience twins on the first birth. However, the straightforward comparison between these two groups of women is complicated somewhat because the probability of twins occurrence also rises with the age of the mother at conception (Mittler 1971), which is itself subject to choice and which therefore may reflect heterogeneity in preferences.[12] However, because i) twins first is orthogonal to all other determinants of labor supply with the exception of age at first birth, i.e., holding age at first birth constant, and because ii) age at first birth is orthogonal to all other determinants of twins first occurrence, sufficient covariance restrictions are provided to enable "consistent" estimation of the twins first effect.

To demonstrate this, define P to be a continuous measure of labor supply or any other choice variable of the household, u the purely random or exogenous component of twins first occurrence, that is, net of the age at first birth effect, and ε the composite of all other (observed and unobserved) labor supply determinants, including those related to preference orderings. Consider the following estimating equation:

(27) $$P = \alpha u + \varepsilon$$

Since u and ε are uncorrelated, if u were known, α could be estimated by a simple linear regression and would be an unbiased estimate of the exogenous impact of an "extra child" on labor supply. Although u is not directly observable, it can be written as

$$(28) \qquad u = TF - \beta AFB,$$

where TF is a dichotomous variable representing the occurrence of twins on the first birth, AFB is age at first birth, and β is the systematic biological association of AFB with TF. Note that u and AFB are independent.

If β is not known, two strategies can be employed. Either β can be estimated by some "consistent" method and α determined in a two-stage procedure using estimated values of u, or one can estimate α directly by the following equation obtained by substituting (28) into (27), to obtain equation (29)

$$(29) \qquad P = \alpha TF - \alpha\beta AFB + \varepsilon$$

Estimation of (29) by ordinary least squares will yield a consistent estimate of α, the twins first coefficient, even though AFB is presumed to be correlated with ε. The reason is that all TF variation is through u once AFB is held constant and u is itself orthogonal to AFB.[13] Of course, the estimate of the AFB coefficient, $\alpha\beta$, will not reflect solely the impact of "exogenous" movements in age at first birth on P, given that ε and AFB may not be independent. However, our sole interest is in obtaining an estimate of the TF effect which is purged of the influence of differential preferences, and that is accomplished regardless of the intervening biological relationship.

The twins first methodology thus enables the identification of an exogenous fertility event without any assumptions about the population distribution of preferences and can be used to draw inferences about life cycle labor supply behavior, discussed in Section II.[14] The occurrence of a twin on the first birth is an extreme example of a "timing failure" early in the childbearing span, since two children appear simultaneously. If there is no reason to suppose that subsequent adaption should differ in direction to less extreme failures, this natural event simulates having an "extra" child in a period in which the birth was not desired, i.e., it represents an exogenous increase in n_1 in the model described by (23) through (26).[15]

The major obstacle to implementation of the twins first methodology is the availability of data. Since twins occur infrequently, and least frequently on first births, a data set with a substantial number of women for whom there is accurate information on pregnancy outcomes and measures of labor supply is required. Ideally, we would like longitudinal or retrospective data in order to get at issues concerning intertemporal substitution and serial dependence. However the first set of criteria is impossible to meet in existing data sets that include the necessary life-cycle information. In the next section we combine two cross-sectional data sets, the 1965 National Fertility Survey and the 1973 National Survey of Family Growth, to examine the impact of exogenous changes in fertility on various dimensions of female labor supply using the methodology proposed. Most of the empirical analysis is conducted on the pooled samples in order to augment the sample of twins mothers. Some limited use is made, however, of the separate samples in order to provide evidence on the life-cycle that is not contaminated by vintage differences inherent in the cross-section.

IV. The Data and Applications

In this section we briefly describe the data and implement the twins first methodology to estimate the causal impact of an unanticipated increase in fertility at one stage of the life cycle on both subsequent fertility behavior and on the life-cycle participation probabilities of women. We also obtain quantitative estimates of the extent to which the use of actual fertility in labor supply equations provides biased representations of the impact of an exogenous change in family size on female labor supply.

The National Fertility Survey of 1965 and that of 1973 are national random samples of women containing detailed information on life-cycle pregnancy outcomes. In addition, each survey ascertains current labor force status, while the 1973 survey also has information on prior employment. In combination there are over 15,000 women of whom we used 12,605. The major exclusion was necessitated by the twins first methodology itself which requires the existence of a first birth. Thus, only women having at least one child at the time of the survey were selected for the analysis. Since ages range from 15 to 44, both women who desire zero total children and those who merely wish to postpone births to ages greater than their current age are thus excluded. Women in any age group with no children are never captured in the analysis and they may react differently to an "extra" child.

Descriptive statistics for selected variables are displayed in Table 1 for the three ten-year age intervals, 15-24, 25-34 and 35-44. In all, for the 12,605 women in the sample there were 87 twins births on the first pregnancy. By the nature of the zero-children restriction, age at first birth will increase from the youngest to the oldest age group. A second column is, therefore, added for the two older age groups which restricts age at first birth to under 25 in order to facilitate the life-cycle simulation. Women who at the time of the survey were 15 to 24 years old must have had a first birth between those ages while women currently aged 25 to 34 could have had their first birth at any age under 35. For the oldest age group a third column for age at first birth under 35 could also have been added to enable comparisons with the middle age group, but its similarity to the unrestricted group makes this addition unnecessary.

Of course, even with these corrections, the cross-section can only accurately depict the life-cycle if vintage is not a relevant characteristic. Moreover, since we have pooled two cross-sections, women of different vintages are combined within a single age group which will further distort the life-cycle picture if vintage effects are non-linear. We could disentangle the vintage from the life-cycle pattern given the two separate cross-sections, but it is not our aim to directly explain movements of the variables depicted in Table 1 in either dimension.[16] Recall that the reason for pooling the two samples was to augment the number of twins first observations to gain some precision in estimating twins first effects. Therefore, although it is easily verified that the figures in Table 1 reflect differences in vintage as well as life-cycle patterns, we cannot, given the data, very precisely discover the extent to which responsiveness to the occurrence of twins first is also governed by both phenomena. We present evidence below, however, that the pooled cross-sections provide useful qualitative insights into the life-cycle response. More conclusive results can only be obtained with richer data.

1. Twins First and Subsequent Fertility

We first examine how the occurrence of a twin on the first birth exogenously alters fertility patterns with particular attention to the role of contraceptive costs in fertility adjustment. Table 2 provides

TABLE 1

DESCRIPTIVE STATISTICS FOR SELECTED VARIABLES, POOLED SAMPLE

(Means - Standard Deviations)

Variable	15-24	25-34	25-34 AFB<25	35-44	35-44 AFB<25
CEB: children ever born	1.728	2.789	2.971	3.573	3.907
	(.966)	(1.58)	(1.59)	(2.14)	(2.21)
AFB: age at first birth	18.91	21.12	20.18	22.27	20.31
	(2.19)	(3.44)	(2.61)	(4.50)	(2.58)
NOW: current employment status (1=now working)	.2903	.3620	.3780	.4476	.4689
	(.454)	(.480)	(.485)	(.497)	(.499)
CHILD: number of children under six	1.567	1.110	1.069	.3357	.2894
	(.822)	(.980)	(1.01)	(.655)	(.619)
TF: proportion of first pregnancies that are twins	.0064	.0057	.0055	.0084	.0067
	(.080)	(.075)	(.074)	(.091)	(.082)
# observations	2487	5241	4531	4871	3722
# twins on first births	16	30	25	41	25
fraction of observations in 1973 sample	.678	.685	.687	.639	.673

TABLE 2

TWINS FIRST EFFECTS ON MEASURES OF FERTILITY, POOLED SAMPLE, BY AGE GROUP
(standard errors in parentheses)

15-24	25-34		35-44		
All AFB	All AFB	AFB<25	All AFB	AFB<35	AFB<25
		CEB			
1.055	.631		.288		
(.224)	(.259)		(.310)		
		.654		.312	
		(.299)		(.312)	
					.150
					(.428)
		CHILD			
1.174	-.242		-.141		
(.200)	(.176)		(.102)		
		-.331		-.125	
		(.299)		(.101)	
					-.296
					(.124)

estimates of the impact of twins first on the number of children ever born and on the number of children currently less than six years of age for each ten-year age interval with and without the appropriate age at first birth restrictions for life-cycle comparisons. These estimates (and their associated standard errors) are from regression equations which, as noted, include age at first birth (as in equation (3)). Given our previous discussion these latter effects are not reported, although it is noteworthy that the inclusion of age at first birth in many cases substantially alters the twins first impact.

As Table 2 reveals, the incidence of twins on the first birth has a substantial causative impact on the pattern of fertility. Since women within the youngest age group, on average, desire fewer than two children within that age interval, the fact that the TF coefficient is almost exactly unity implies that women with twins on the first birth do not postpone a subsequent birth more (or less) than do women with a single child on the first birth. If the identical fertility pattern were maintained in subsequent ages by women with twins and those without, however, women aged 25-34 (AFB<25) would also have one additional child since completed family size is approximately one greater than family size at age 25-34. The fact that the difference in cumulative fertility between TF and non-TF women is only .65 implies some lengthening of the interval between children on average by women with twins. Completed family size, however, is only slightly altered by the twins first occurrence (age 35-44 AFB<25) indicating that for the pooled sample, the occurrence of twins on the first birth represents mainly a timing failure which has little impact on completed family size.

The relative inelasticity of completed fertility with respect to the timing failure is somewhat surprising even in the case in which contraceptive flexibility is costless, given the numerous avenues discussed in the theoretical section for such an effect to exist. However, the larger permanent impact identified when later age at first births are admitted implies possibly greater adjustment costs as the length of the period shortens over which adjustment must occur. Evidence concerning this interpretation as well as the importance of changes in contraceptive costs on fertility control associated with the contraceptive revolution can be obtained by estimating completed family size effects of TF for the 1965 and 1973 cohorts separately. These results, presented in Table 3, indicate a permanent effect only for the 1965 cohort of older women, who were likely to have experienced a twin on the first birth prior to the availibility of the pill and other efficient contraceptive methods.[17] Moreover, the difference in the TF effect on completed family size by AFB is also only evident for the 1965 cohort, i.e., for the women for whom contraceptive flexibility was likely to be more costly and thus was more likely to vary by the length of the adjustment process. These results thus provide some indirect confirmatory evidence of the impact of the contraceptive revolution on the ability of families to control both timing and numbers of children.

The impact of twins first on the number of children under six shown also in Table 2 cannot be as easily interpreted as children ever born effects. It is obvious that for the youngest women almost all children born within that age interval would be currently under six so that the twins first effect on CHILD is almost identical in magnitude to its effect on CEB. For the two older age groups, women with twins first births have fewer younger children presumably as a result of two phenomena. First, women with twins first have fewer extra children after the age of 25 and second, they complete their family at an earlier age. Each of these is, of course, the outcome of an optimization process and are together merely descriptive rather than true explanations of the CHILD pattern. The point is that a twins first birth gives rise to a different than average fertility pattern which may be connected to a different than average labor force participation pattern.

2. Twins First and Female Participation

As was demonstrated in Section II, by examining the impact of twins first (an increase in fertility independent of ε) on labor supply at various points in the life-cycle, we can examine how female labor supply is affected by variations in the costs of bearing (preventing) children. We can as well test some of the predictions of the multi-period labor supply models, possibly ruling out some combinations of assumptions.

Table 4 reports the impact of a twins first birth on the dichotomous current work status variable of the mother derived from a maximum likelihood logit (ML LOGIT) estimation procedure based on the pooled sample.[18] The estimates reported are transformed logit coefficients based on the population mean participation rate and should be interpreted in the same way as a regression coefficient, namely as changes in the probability of participation due to the occurrence of twins on the first birth. The results indicate that, for the youngest

TABLE 3

EFFECTS OF TWINS FIRST ON COMPLETED FAMILY SIZE, WOMEN AGED 35-44 BY VINTAGE
(standard errors in parentheses)

	All AFB	AFB<25
1965	.724 (.507)	.325 (.797)
1973	-.012 (.392)	.048 (.505)

TABLE 4

MAXIMUM LIKELIHOOD TRANSFORMED LOGIT COEFFICIENTS:
TWINS FIRST AND CURRENT PARTICIPATION BY AGE GROUP, POOLED SAMPLE
(asymptotic standard errors in parentheses)

15-24	25-34		35-44		
All AFB	All AFB	AFB<25	All AFB	AFB<35	AFB<25
-.371 (.212)	-.096 (.096)		.061 (.078)		
		-.102 (.105)		.060 (.078)	
					.142 (.102)

women, the probability of working was smaller by .371 for those with twins on the first birth. This significant increase in home time early in the life-cycle associated with an "extra" birth, as shown in (19), would thus lead to a rejection of the model of life-cycle labor supply under the separability assumption, in which child quality, a time-intensive production activity, is not a choice variable. The twins first impact declines to -.102 for the 25-34 year olds and actually reverses in sign for the oldest women with twins, who have a .142 greater probability of participation. The <u>reduction</u> in home time as a consequence of an exogenous increase in births for older women is also consistent with the separable model which incorporates both serial dependence and time-intensive q-production. While the relatively large standard errors of these latter estimates should again draw the reader's attention to the small samples of twins first observations, the difference in the effects for the youngest and oldest age groups is statistically "significant" at conventional levels ($t = 2.17$).

The heightened participation of the oldest women who experienced twins births, while it is consistent with the general life-cycle model of Section II, casts doubt on the importance of human capital serial dependence. If the evident initial withdrawal from market activity induced by an extra child early in the life-cycle permanently reduced the optimal human capital investment or otherwise adversely affected future wage rates, such a phenomenon, if it exists, appears to be overwhelmed by the apparent substitutability of home time in the utility and q-functions over the life cycle. The results, therefore, appear to be more consistent with the notion of a permanent life-time market participation rate (Mincer 1962) in which current participation is adapted to transitory events such as a "timing" failure (a twin) and in which transitory labor-force withdrawal or entry has little impact on the permanent level.

We have so far interpreted the participation results based on the pooled sample in a life-cycle context even though we have not accounted for the possibility of a vintage-based explanation. As was stressed, the data are not well suited to such an endeavor, though we now report on two further analyses each based upon the separate surveys to provide limited evidence on the validity of the life-cycle interpretation. First, we disaggregated by survey year and performed the same logit analyses for the two life cycle stages that can directly be simulated: the 1965, 15-24 cohort is coupled with the 1973, 25-34, age at first birth less than 25 group and the 1965, 25-34 cohort is coupled with the 1973, 35-44 age at first birth less than 35 group. The results, presented in Table 5 below, confirm the existence of a declining twins first effect with age for the same cohort, although statistical significance at conventional levels is impossible to achieve given the very few twins first occurrences.

The second piece of confirmatory evidence is obtained only in the 1973 survey. Although that information was unfortunately tied to the specific fertility pattern, we were able to calculate the fraction of potential years since the birth of the first child spent by the mother in market work. Based on the same twins first regression scheme for the subsample of oldest women, those with a twins first birth actually worked a slightly greater fraction of their potential work experience, spending about 3/4 of a year more in the labor force out of a potential experience of over 19 years and an average actual work experience of over 6 years. Again, the initial increase in home time due to a twin birth that is indicated in the 15-24 samples, appears to have had no dominant permanent

TABLE 5

LIFE-CYCLE TWINS FIRST EFFECT ON PARTICIPATION[a]

Sample	15-24	25-34	35-44	Estimation Technique
1965	-.224 (.132)	-.229 (.168)	-	OLS[b]
1973 AFB<25	-	-.028 (.117)	-	ML LOGIT
1973 AFB<35	-	-	.043 (.100)	ML LOGIT

[a] From left to right there are 7, 7, 18, and 25 twins first observations respectively.

[b] These are OLS regressions since the TF=1, NOW=1 cell was empty. For comparison, the corresponding OLS estimates for Table 3 were -.226 (.114) and -.087 (.088).

influence on future home time.

3. Exogenous and Endogenous Fertility Effects on Participation: Estimates of the Bias

The twins first results in general confirm the existence of an important causal connection between fertility (the price of children) and labor supply that has ramifications throughout the life-cycle. As noted, however, most of the female labor supply literature has assumed actual fertility to be "exogenous". We now use the twins first results to test if the conditions hold under which the *ceteris paribus* relationship between *observed* fertility and labor supply provides unbiased estimates of price-effect information. This is done by estimating the difference between the truly exogenous effects on labor supply provided by TF and those effects implied by the relationships between CEB, CHILD, and participation; i.e., the bias in the fertility-participation relationship caused by heterogeneity in tastes.

It is a relatively simple matter to address the bias question once a labor supply equation is fully specified since the extent of the bias will, in general, depend upon what is relegated to the error term. We, therefore, experimented with several versions of the participation function and found the estimated conventional fertility measure (CEB and CHILD) relationships with participation to be robust to the inclusion of a variety of variables. For this reason we report on the simplest version only, namely logits which include solely CEB and CHILD. The method for calculating the bias is straightforward: estimate the following participation equation:

$$NOW = \gamma_1 CEB + \gamma_2 CHILD + \varepsilon_1$$

and compute

$$\frac{dNOW}{dTF} = \hat{\gamma}_1 \frac{\partial CEB}{\partial TF} + \hat{\gamma}_2 \frac{\partial CHILD}{\partial TF}$$

where $\frac{\partial CEB}{\partial TF}$ and $\frac{\partial CHILD}{\partial TF}$ are obtained from Table 2 and $\hat{\gamma}_1$ and $\hat{\gamma}_2$ are transformed logit coefficients. This biased estimate of the exogenous impact of twins first, denoted by $\left(\frac{dNOW}{dTF}\right)_B$, can then be compared to the unbiased estimate contained in Table 3, $\left(\frac{dNOW}{dTF}\right)_u$. These figures are summarized in Table 6.

It is evident that the fertility effect, i.e., the combined CEB and CHILD effect, is substantially dampened at all ages under the (erroneous) assumption that actual fertility does not reflect differences in preference orderings. While, the cause of this bias in the TF impact cannot be assigned to CEB or CHILD individually — one cannot tell whether the effect of younger children or that of older children is biased nor in which direction -- their combined effect is grossly underestimated in absolute value. The covariation between actual fertility and participation estimated from a cross-section of women therefore understates significantly the extent to which female labor supply responds to changes in the price of fertility control (p_n) relative to the responsiveness of fertility to alterations in contraceptive costs. Moreover, these results imply that estimates of conditional <u>own</u> price (wage) and income effects in labor supply equations which include a measure of actual fertility among the regressors will be inconsistent. While it is possible that these inconsistent estimates of "short run" price and income effects conditioned on the quantity of children will be fortuitously close to

TABLE 6

COMPARISONS OF EXOGENOUS AND ACTUAL FERTILITY "EFFECTS" ON PARTICIPATION, BY AGE GROUP, POOLED SAMPLE

	Age Group		
	15-24	25-34 (AFB<25)	35-44 (AFB<25)
$\left(\frac{dNOW}{dTF}\right)_B$	-.103	.024	.041
$\left(\frac{dNOW}{dTF}\right)_u$	-.371	-.102	+.142

consistent estimates of such effects forthcoming from the usual (reduced-form) demand functions (which provide the information appropriate to the Slutsky-type relations implied by theory), such fertility-conditioned estimates may stray far from those required to either test theory or draw policy conclusions.

V. Conclusions

In the first section of this paper we explored a one-period (lifetime) model which incorporated a production function for child quality and in which individuals differed in their preference orderings for leisure and/or children. The conditions under which, given lack of information on the price of children (e.g., the price of contraception) and preference orderings, labor supply equations that include children as exogenous regressors will not confound tastes and price heterogeneity were derived. It was demonstrated that if the price of children were known, the use of price as a regressor dominates the use of quantity, as simultaneous equations estimates were shown to provide conditional demand estimates which must rely on the existence of price information to exogenously alter quantities independently of tastes. Empirical models of the simultaneous determination of fertility and labor supply are thus no more informative than the usual set of consumer demand equation estimates (to which they must correspond) in terms of verifying theory.

In the following section of the paper, we examined the informational content of fertility effects and extended the model to a two-period context in which current human capital stocks were dependent on prior labor supply behavior. Within a rationing (of children) framework, it was shown that the sign of the compensated effect of a change in the price of children on labor supply provided by the impact of additional (exogenous) children on labor supply could provide information on the existence of a child quality production function and human capital depreciation only if restrictions on utility function parameters are imposed. Given strong separability in utility, a weak test for the existence of a quality dimension was derived based on a decomposition of cross price effects.

To simulate exogenous price variation we developed and implemented a methodology based on the natural event of a twins birth. In particular, we traced the impact of a twins occurrence on the first pregnancy on further fertility and on current and future female labor supply. These results were explicitly related to the life-cycle model discussed in previous sections. Two important conclusions emerged, qualified importantly by our small sample of twins first births. First, the use of actual fertility in participation equations greatly understates the impact of exogenous fertility on female work status. Second, total fertility and measures of lifetime labor supply appear to be unaffected by the transitory increase in children experienced by women with twins on the first birth. Initial withdrawal from market work, which was shown to be supportive of the time-intensive nature of child quality investment, is compensated almost exactly by the earlier re-entrance of women later in the life-cycle. Weaker evidence was obtained of the lesser ability of women to adjust their subsequent fertility to a multiple birth prior to the "contraceptive" revolution. The twins first labor supply effects obtained thus suggest that reductions in contraceptive costs play a causative role in increasing female participation.

Footnotes

1. A notable exception to these empirical labor supply traditions is Mincer (1963), in which the inappropriateness of including a fertility variable among the set of labor supply regressors is suggested. This paper is in the spirit of that seminal article, providing some possible theoretical rationales for such specifications and their interpretation. See also Schultz (1979).

2. These parameterizations of preferences are meant to be illustrative. Relegation of unobserved differences in preferences to additive error terms in linear demand or "structural" equations, as is common, implies as well a particular, although less easily interpretable, structure to "tastes" heterogeneity.

3. Given (2), the shadow price of child quality would be a function of both the wage (rH) and p_n.

4. Only compensated price effects are discussed, as the model provides no predictions about good-specific income effects.

5. Strong separability is a sufficient condition. In the one-period model it is only necessary that n and ℓ be substitutes in (1) to obtain the weak test. In the multi-period model described below, strong separability is required to obtain precise results.

6. Alternatively, the appreciation of human capital or, more generally, a threshold function in which home time above some value ℓ^* leads to depreciation and below ℓ^* induces a rise in the stock of human capital can be introduced into the multi-period model. The main points of the subsequent analysis would be unaffected.

7. Note that while this formulation implies that in a <u>behavioral</u> sense, optimal changes in human capital are related to past and future labor supply, all these variables are jointly determined out of the utility

maximizing process subject to the technological function (13) and the full (14) wealth constraint. It will be shown below that despite the cost of human capital depreciation being a positive function of future labor supply (from (16)), the model does not yield the prediction that an exogenous rise in future labor supply will reduce (increase) planned human capital decumulation (accumulation) in the current period.

8. Heckman (1978b) proposes a methodology for ascertaining the existence of 'state' dependence in terms of entrance into or exit from the labor force as a function of past labor-force status in the presence of population heterogeneity. In the continuous case of 'serial' dependence considered here, the difficulty related to obtaining a consistent estimate of $d\ell_2/d\ell_1$ is parallel to that relating to estimating $d\ell/dn$. Serial dependence, based on an assumption of some degree of participation (non-zero ℓ_i) is defined in terms of a human capital relationship. State dependence with respect to participation admits to both human capital and job (entry) costs interpretations.

9. Razin's model treats total fertility, intervals between births, as well as the total fertility span as control variables, with parental leisure fixed exogenously. The comparative statics of the model are not discussed, as they are not here, because of the difficulty of obtaining any verifiable predictions.

10. References to these concepts are made in Hill and Stafford (1978) and Schultz (1978) and are implicit in the notion of "short-run" labor supply effects, often used to describe fertility-conditioned labor supply estimates.

11. To correct for the rise in the probability of a multiple birth event with parity, the ratio of twins to the total number of pregnancies was proposed in

Rosenzweig and Wolpin (1980) as a variable capturing the random component of multiple births. The short-comings of that approach (necessitated by data limitations) detailed in that paper are eliminated in the methodology proposed here. The data discussed below reveal the following pattern of <u>per-pregnancy</u> probabilities of twinning for parities of one to six: .0070, .0110, .0144, .0168, .0178, .0206.

12. The rise in the probability of a multiple birth from the first pregnancy is approximately linear within each of the age intervals we consider in the subsequent empirical analysis with a slope coefficient of $.453\times10^{-4}$ over the age range 15-45 with a mean probability of .0076.

13. Let y_1, y_2 and y_3 represent deviations from means for the variables P, TF and AFB respectively, with ε' and u' the residual deviations in (27) and (28). Then

$$y_1 = ay_2 + by_3 + \varepsilon'$$

where $$y_2 = cy_3 + u' .$$

The least squares estimator of a, $\hat{a}$, is thus

$$\hat{a} = \left[(\Sigma y_1 y_2)(\Sigma y_3^2) - (\Sigma y_1 y_3)(\Sigma y_2 y_3)\right]\left[\Sigma y_2^2 \Sigma y_3^2 - (\Sigma y_2 y_3)^2\right]^{-1}$$

With the large sample assumptions $\Sigma\varepsilon u' = \Sigma y_3 u' = 0$ but with $\Sigma y_3 \varepsilon' \neq 0$, we wish to show that $\hat{a} = a$. Carrying out the multiplication,

$$\Sigma y_1 y_2 = ac^2 \Sigma y_3^2 + a\Sigma u'^2 + bc\Sigma y_3^2 + c\Sigma y_3 \varepsilon' + (b+2ac)\Sigma y_3 u' + \Sigma u'\varepsilon'$$

$$\Sigma y_1 y_3 = ac\Sigma y_3^2 + b\Sigma y_3^2 + \Sigma y_3 \varepsilon' + a\Sigma y_3 u'$$

$$\Sigma y_2 y_3 = c\Sigma y_3^2 + \Sigma y_3 u'$$

$$\Sigma y_2^2 = c^2 \Sigma y_3^2 + \Sigma u'^2 .$$

Given the covariance restrictions,

$$\hat{a} = a \frac{\Sigma u'^2 \Sigma y_3^2}{\Sigma u'^2 \Sigma y_3^2} = a .$$

14. It is possible that a multiple birth may not be totally unanticipated. Evidence exists that twinning is a heritable trait (Mittler), although a twins background is an extremely poor predictor. In the 1970 National Fertility Survey, described in Ryder and Westoff, 1976, of the 142 families in which either parent had a twin sibling (including one in which both the husband and wife were siblings of twins), none had experienced a multiple birth.

15. For an exogenous increase in any n_j to yield the price information discussed above, it may be necessary that planned fertility not be at biologically maximal fertility, since the occurrence of twins may augment welfare.

16. Table I is presented to enable interpretation of the relative size of twins first effects on fertility and labor supply. The age-profiles of participation and fertility are, however, directionally accurate. In particular, market participation, where participation denotes actual employment, increases even more dramatically with age controlling for vintage due to the increased participation at all ages between 1965 and 1973. On the other hand, since fertility has declined with newer cohorts (except for the oldest age group), the figures in Table I overestimate, though not greatly, the life-cycle increase in children ever born.

17. See Ryder and Westoff, 1976.

18. In using participation rather than a continuous measure of labor supply due to the exigencies of the data, it is necessary to assume that all the women in each age-group work some time in the relevant age-interval. For a fuller discussion of the relationship between Hicks-Slutsky price effects and participation estimates, see Heckman, 1978a).

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