REPLY

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Daniel A. Seiver raises two questions concerning my paper (Hicks, 1974). First, did age-specific fertility in Mexico really fall between 1960 and 1970 as data for the total fertility rate indicate; and second, does my model accurately reflect the effect of various "independent" variables on fertility? I shall consider these two questions in order.

There are many different measures of fertility; they do not necessarily move together and different measures are appropriate for different purposes. The number of children ever born to women in a given cohort who have completed their fertility is the theoretically appropriate measure of fertility in the traditional economic framework for explaining behavior over the life cycle. However, short-run changes in fertility may be obscured by measures of completed fertility while they may be discernible in period fertility rates (i.e., the total fertility rate or age-specific fertility rates). "If one is not to wait decades until today's younger parents . . . have completed their childbearing years in order to estimate the effects of changing environmental constraints on their completed fertility, research must in the interim analyze current birth rates for evidence of the contribution of specific dimensions of economic change and population policies" (Schultz, 1973, p. 244). Period rates (i.e., the total fertility rate) can be used to show changes in levels of fertility across time. The total fertility rate in Mexico corrected for underregistration and increased delayed registration increased from 6.231 in 1951–1953 to 6.514 in 1959–1961 and declined to 6.128 in 1969–1971 (Pillet, 1974).

Seiver has used cohort rates when he should have used period rates; he uses the number of children ever born (the stock) to measure changes in fertility between 1960 and 1970. Seiver presents evidence based on children ever born to women 20–24, 25–29, and 30–34 to show that "fertility" increased between 1960 and 1970. As indicated in my paper (Hicks, 1974, p. 416), I had refrained from making comparisons between 1950, 1960, and 1970 for the number of children ever born because of an apparent underenumeration of mothers in the 1950 and 1960 population censuses. A fertility survey taken in Mexico City in 1964 which was coordinated by CELADE shows that the 1960 Population Census underestimated the number of children ever born in Mexico City in the 20–24 age group by 11 percent, the 25–29 age group by 18 percent, and the 30–34 age group by 11 percent (Benitez-Zenteno, 1970). If the number of children ever born in Mexico in 1960 were underreported by the Population Census as this survey suggests (and if underreporting was reduced in 1970), this might explain why Seiver's Table 1 shows a sharp increase in children ever born in Mexico for the 20–24, 25–29, and 30–34 age groups between 1960 and 1970.

Turning to the second question, the dependent variable that Seiver uses to analyze the effects of various "independent" variables on the levels of fertility among the 32 states of Mexico is the child-woman ratio. "The general fertility (child-woman) ratio may be viewed as . . . a substitute for the general fertility rate" (Shryock et al., 1971, p. 501). However, the child-woman ratio is inferior to the total fertility rate because

it does not control for differences in the distribution of females between 15 and 49 years.

The reasons for choosing the independent variables in Seiver's model generally are not given nor does he provide a theoretical or empirical rationale for using the log-linear functional form. Seiver uses the percentage of the population 15–29 with no education as an alternative to the proxy for education that I used—the percentage of women 6 years and over who were literate. However, when I replaced the percent of population literate with the percentage of *women* with no education the results were very similar to those I got using literacy.

The most appropriate income concept is lifetime income (or wealth). While measured per capita income at a point in time, which is the variable I used, is a poor proxy for the desired measure, it appears to be superior to the percent of the population reporting income over 1,500 pesos per month, which is the measure used by Seiver. This latter variable appears to measure more nearly the distribution of income rather than the level of lifetime income (or wealth). No justification for including the percent of the labor force in white collar occupations is given other than that it is "commonly considered to influence fertility in the demographic transition" (Seiver, 1976, p. 150). At least one of the variables Seiver introduces would appear to be redundant. The percent of the labor force in agriculture and the percent of the population living in areas of more than 2,500 would appear to measure the same phenomena and should have a high negative correlation. The independent effects of these two variables are unclear and the cutoff for urban places should be 15,000+ rather than 2,500+ according to knowledgeable Mexican demographers (Centro de Estudios Economicos y Demograficos, 1970, p. 117). The implications of female labor force participation on fertility in Mexico may be different than in more developed countries and it is not surprising that the coefficients on this variable in Seiver's analysis were not significant. The participation rate for the female population 12 years and over in Mexico in 1970 was 16 percent and 37 percent of those employed were unpaid or employed in agriculture or domestic services. The sex ratio is the only variable in Seiver's model that is even close to being statistically significant (at the 5 percent level) in 1970 although it is not significant in 1960. This variable might have been better specified as the ratio of males 25–34 to females 20–29 since women tend to mate with men roughly five years their senior. Seiver's failure to include a measure of mortality and the indigenous population is unfortunate. There is both theoretical and empirical justification for including mortality in an analysis of the kind in question (see Hicks, 1974, pp. 410, 417). The exclusion of mortality from the analysis may have biased Seiver's results (Ramsey, 1974). Similar comments apply to the exclusion of the indigenous population, a variable which has proven to be one of the more robust variables affecting fertility in Mexico. This variable gave particularly interesting results in an analysis of age-specific fertility rates (Del Rio et al., 1975). The indigenous population percentage was positively associated with fertility in the 10–14 and 15–19 age groups. The relationship changed in sign with the 20–24 age group and became increasingly negative for older groups. Thus, it would appear that there is a pronounced change in fertility rates of indigenous, vis-à-vis mestizo, populations across age groups. This changing relationship between fertility and indigenous population can be explained by two factors. First, as age increases there may be a decline in fecundity within the indigenous population. This could result from low incomes, poor nutrition, undesirable living conditions, and other factors affecting health. Second,

the health conditions may give rise to the premature breaking of marriages, thus tending to lower birth rates among older age groups (Arriaga, 1967). As living conditions of the indigenous population improve, some tendency may exist for fertility to rise among the indigenous population, unless it is offset by other factors.

Seiver is concerned with multicollinearity. While it is clear that the variables in my model are correlated, the independent variation (relative to the variance of the error term) was adequate to make most variables statistically significant. Multicollinearity seems to be a greater problem in Seiver's model especially for the percentage of the labor force in agriculture and the percentage of the population in "urban areas" as noted earlier.

Siever found that he "obtained radically different results from those of Hicks (mine)" (p. 149). He might have also indicated that the results he obtained for 1960 and 1970 using his own model were radically different. He apparently made no attempt to test for changes in the structural relationships in his model between 1960 and 1970, but casual inspection suggests that there were important structural changes. In the case of my model, a test of the equality of the coefficients on the independent variables showed no statistically significant differences in the value of the coefficients on the independent variables in 1950, 1960, and 1970 (Fisher, 1970).

In summary, it is not surprising that Seiver's results differ from mine. I have shown that there are good and obvious reasons for these differences. Redundant and omitted independent variables and a dependent variable that is not standardized for differences in age composition constitute serious problems for Seiver's model. A common limitation of both Seiver's and my model is that they are based on cross-sectional (or pooled cross-sectional) data. The coefficients from cross sections represent long-run responses to changes in independent variables after there has been an opportunity for the effects of these changes to work their way through the childbearing population. As such they may be useful in making long-run projections (i.e., to the year 1990 or 2000) of the effect of economic development on fertility; but we have not estimated coefficients which show the short-run dynamic response of fertility to changes in economic conditions.

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