The Impact of Population Policies: Comment

JAMES C. KNOWLES
JOHN S. AKIN
DAVID K. GUILKEY

IN A RECENT ISSUE of this journal, Lant Pritchett (1994) argues, on the basis of empirical findings, that variations in fertility desires account for most of the variation in fertility rates across countries and that access to contraception contributes at best a significant but numerically small (i.e., 2–3 percent) independent effect. Pritchett states his conclusion colorfully (p. 52): "Of course, it is always true that changing fertility desires *and* increased contraceptive access cause fertility reductions in the same trivial sense that gin *and* tonic make you drunk" (italics in the original). We believe Pritchett's empirical analysis to be seriously flawed by his use of an essentially tautological statistical model. Whereas there are many other aspects of his article with which we disagree, we have chosen to focus our comment on the inappropriateness of the statistical model itself.

The first section of the article presents cross-national regressions of the total fertility rate (TFR) on three alternative measures of fertility preferences: the "average ideal number of children" (AINC); Lightbourne's and Westoff's "desired" total fertility rate (DTFR); and Bongaarts's "wanted" total fertility rate (WTFR). Pritchett estimates regression models such as the following:

$$TFR_i = \alpha + \beta DTFR_i + u_i \tag{1}$$

where TFR and DTFR refer respectively to the total fertility rate and the "desired" total fertility rate, α and β are fixed parameters to be estimated, u is a random disturbance term, and the subscript i refers to a particular country observation (drawn from a variety of years). The curious thing about this model is that DTFR is basically TFR with minor adjustments (in most countries)—that is, the same births are used to compute TFR and DTFR, except that births are excluded from DTFR if they exceed the woman's reported desired family size. In other words, TFR is always greater than or equal to DTFR, implying that the random disturbance term (u) has a truncated distribution (that TFR–DTFR must always be positive is a statistical problem in itself). The main problem, however, is that the model is essentially trying to explain variations in one variable (TFR) by variations in a slight mutation of itself (DTFR). It is akin to trying to explain variations in annual precipitation using variations in annual rainfall. Even though the former differs from the latter according to the amount of sleet and snowfall in a given area (just as TFR and DTFR differ by the amount of "excess fertility"), finding a strong relationship between the two variables is not surprising (especially in warmer climates) nor does it advance the science of meteorology very much.

To see the problem more clearly, we can rewrite equation (1), using the author's definition of "excess fertility" (EXFERT) as the difference between TFR and DTFR: EXFERT = TFR–DTFR (Table 2, p. 14):

$$\mathrm{TFR}_i = \alpha + \beta(\mathrm{TFR}_i - \mathrm{EXFERT}_i) + u_i \tag{2}$$

The above model, it can be seen, effectively constrains the coefficients of both TFR and EXFERT to equal the constant β (in unconstrained form, one would of course get a perfect fit!). To get an unconstrained estimate of β without having the dependent variable on the righthand side, one could estimate the following equation, which is obtained by rearranging the terms in equation (2):

$$\begin{aligned}\mathrm{TFR}_i &= [\alpha/(1-\beta)] - [(\beta/(1-\beta)]\mathrm{EXFERT}_i + [1/(1-\beta)]u \\ &= \alpha' - \beta'\mathrm{EXFERT}_i + u'_i \end{aligned} \tag{3}$$

where: $\alpha' = \alpha/(1-\beta)$; $\beta' = \beta/(1-\beta)$; and $u'_i = [1/(1-\beta)]u_i$. However, applying ordinary least squares (OLS) to equation (3), using the data provided in the article's appendix, one obtains a statistically insignificant estimate of β' (0.180; $t = 0.49$), with an R^2 of only 0.004. The importance of this result cannot be overstated; total fertility is *not* significantly related to excess fertility in the data.[1]

The preceding results are what one would expect if EXFERT were simply a random measurement error (i.e., "random noise") rather than a true measure of "excess fertility." In other words, the underlying model Pritchett attempts to estimate might be written (ignoring the random error) as:

$$\mathrm{TFR}_i = \mathrm{TFR}_i \tag{4}$$

But instead of estimating equation (4) directly, Pritchett estimates the informationally equivalent equation (1) above, with

$$\mathrm{DTFR}_i = \mathrm{TFR}_i - v_i \tag{5}$$

where v is a random measurement error with a truncated distribution (i.e., $v \geq 0$). From elementary econometrics (e.g., Johnston 1972: 282), we know that the effect of random measurement error in an independent variable in a bivariate regression is to bias downward (in large samples) its estimated coefficient. If, for example, EXFERT is indeed a random measurement error (and truncation is ignored), then regressing TFR on DTFR would produce an estimate of the coefficient β that would approach (as a probability limit):

$$\beta \,/\, [\, 1 + \sigma^2_{\mathrm{EXFERT}}/\sigma^2_{\mathrm{TFR}} \,]$$

where $\sigma^2_{\mathrm{EXFERT}}$ and σ^2_{TFR} are respectively the variances of EXFERT and TFR. Assuming that the model in equations (4) and (5) applies, the true value of β is one. For the present data set, an estimate (based on the sample variances of EXFERT and TFR) of the probability limit of the OLS estimator of β would be:

$$1 \,/[\, 1 + (0.27 \div 2.27)\,] = 0.89$$

This is identical with the estimate of β reported in the article (0.89), and is also quite close to what we obtained with the data provided in the article's appendix (0.88).[2]

We also performed a simple Monte Carlo experiment in which we generated a normally distributed (but truncated) random error with mean and standard deviation equal to the sample mean and sample standard deviation of EXFERT, subtracted this "measurement error" from TFR, and regressed TFR on the resulting "randomized" total fertility rate (RTFR).[3] We obtained a slope estimate of 0.87, a t-statistic of 24.3, and an R^2 of 0.89 (again, very similar to the results reported in the article). In other words, the author's results are equally consistent with a simple errors-in-variable model, with the estimate of "excess fertility" behaving exactly as a random measurement error.[4]

Summarizing, the dramatic result obtained by the author, namely that two measures of desired fertility explain about 90 percent of the variation in the total fertility rate, is an artifact of the model, stemming from the inclusion of a slight transformation of the dependent variable itself as the explanatory variable. Moreover, the results obtained are equally consistent with those one would obtain if the measures of "excess fertility" used were simply random noise. If the correct model is in fact that described by equations (4) and (5), it is not surprising that instrumental variable estimation raises estimates of β toward a value of one;[5] nor is it surprising that other explanatory variables (e.g., contraceptive prevalence, family planning program effort, unmet need), when added to a regression of TFR on DTFR, tend to be insignificant. After all, when a variable is essentially regressed on itself, little variation remains to be explained by other factors.

Pritchett's strong conclusions in this lengthy article are not at all merited, relying as they do on his seriously flawed empirical analysis. As we have all been taught with respect to empirical research, no set of results ever proves or disproves a theory (e.g., that family planning programs have, or do not have, a significant impact on fertility). The evidence is simply consistent or inconsistent, at some level of probability, with the hypothesis being tested. It is extremely rare to find all of the evidence in the literature on a topic to be consistent with a particular theory of how the world actually works. The Pritchett article is being referenced in policy discussions as showing that family planning programs have little impact on fertility. It must be emphatically stated that the article provides no such evidence.

To the extent that the main message of the article is that the desire to reduce fertility is the most important factor leading to fertility reduction, we completely agree. It is not that message we would question, but rather the oversimple interpretation that family planning programs (and affiliated efforts at information, education, and communication about family planning) are not significant factors affecting those desires, and that having accessible family planning supplies and programs is not important. If those latter two points are to be interpreted as the message of the article, we strongly disagree. The evidence presented simply does not prove these assertions.

Notes

1 Actually, Pritchett (1994: 4–5) is aware of the lack of any relationship between the TFR and excess fertility, but he interprets this as further evidence in support of his thesis that "fertility rates reflect almost entirely desired fertility."

2 We could not replicate the author's results reported in Figure 1 (pp. 6–7). For example, in regressing TFR on DTFR, we obtained a slope estimate of 0.88 and R^2 of 0.90, as compared to the author's reported slope of 0.89 and R^2 of 0.91. In regressing TFR on WTFR, we obtained a slope estimate of 0.90 and R^2 of only 0.81 (compared to the author's reported slope estimate of 0.91 and R^2 of 0.85). We believe that there are errors in the TFR values reported in the article's appendix. In the case of Mali, for example, the values

given for the TFR and DTFR are 7.0 and 7.1 respectively (which is definitionally impossible).

3 Whereas the DTFR removes births from the TFR that exceed women's stated desired number of children, RTFR may be thought of as randomly removing approximately the same number of births from the TFR.

4 In contrast, the other measure of fertility desires (AINC) is not derived from the TFR. Not surprisingly, it exhibits a much weaker correlation with the TFR (i.e., an estimated slope of 0.72 and an R^2 of 0.58, using the data provided in the article).

5 A reasonable argument could be made that Pritchett's choice of instrument, the proportion of women who want no more children, is also endogenous and affected by the same set of unobserved variables that determine TFR.

References

Johnston, J. 1972. *Econometric Methods*. New York: McGraw-Hill.

Pritchett, Lant H. 1994. "Desired fertility and the impact of population policies," *Population and Development Review* 20, no. 1: 1–55.