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# The Impact of Population Policies: Comment

JOHN BONGAARTS

IN THE ARTICLE "Desired fertility and the impact of population policies," Lant Pritchett (1994) concludes that changes in socioeconomic conditions are very important causes of fertility declines in developing countries. He also concludes that the effect of family planning programs on fertility is "quantitatively small." I agree with the former but not the latter conclusion. This note will deal only with the principal areas of disagreement.

Before turning to the details of Pritchett's analysis, I summarize briefly the two central arguments made by advocates of family planning programs regarding their impact on fertility: (1) there is substantial unwanted (and mistimed) fertility in most societies, and (2) well-designed family planning programs can effectively reduce unwanted childbearing.

Pritchett considers unwanted fertility ("excess fertility") to be a matter of little consequence. However, about one in four births in the developing world (excluding China) is unwanted—a total of some 25 million per year. In addition, approximately 25 million abortions are performed annually in developing countries, often under unsafe conditions. Aside from the hazards unwanted pregnancies pose to women, they contribute to population growth. If unwanted fertility could be eliminated entirely, fertility in the developing world would drop halfway from its current average of about 3.5 births per woman to the replacement level of just over two, and future population growth would be cut by nearly 2 billion (Bongaarts 1994). Helping women (and men) to implement their reproductive preferences is an obvious place to start if one wants to reduce fertility and future population growth.

How much difference can family planning programs make? Pritchett finds such programs to be largely ineffective. One key piece of evidence he cites in support of his conclusion is the lack of a relationship between unwanted fertility and overall fertility. While factually correct, this observation misses the point: the absence of such a relationship is not due to a lack

of program impact; it is observed precisely because program efforts are effective in preventing a rise in unwanted fertility as countries develop. Without family planning programs, one would expect unwanted fertility to increase as a society moves through the fertility transition. The reason is simply that the number of years in which women are exposed to the risk of unwanted childbearing rises over time as wanted fertility declines. Early in the transition, women want large families, and they bear wanted children during most of their reproductive lives, thus leaving little room for unwanted births. But near the end of the transition, when desired fertility has declined to a small number, most married women can complete their wanted childbearing in their 20s. They then face one or two decades of fecund married life during which they have to try to avoid getting pregnant. Given the various obstacles to contraceptive use and the limited effectiveness of many methods, it is not surprising that unwanted pregnancies then can occur in substantial numbers, unless effective programs are in place.

One would therefore expect empirical analyses to show that (1) in the absence of program efforts, unwanted fertility rises as wanted fertility declines, and (2) programs have an important negative effect on unwanted fertility. Because programs tend to be strongest in countries with low wanted fertility, the net effect of these two factors is offsetting, and unwanted fertility remains, on average, relatively invariant. To test the validity of these propositions, I performed a regression analysis in which unwanted fertility (UNWTFR) was the dependent variable, and wanted fertility (WTFR) and program effort (FPE) were the independent variables. The results were as follows (*t* statistics in parentheses):

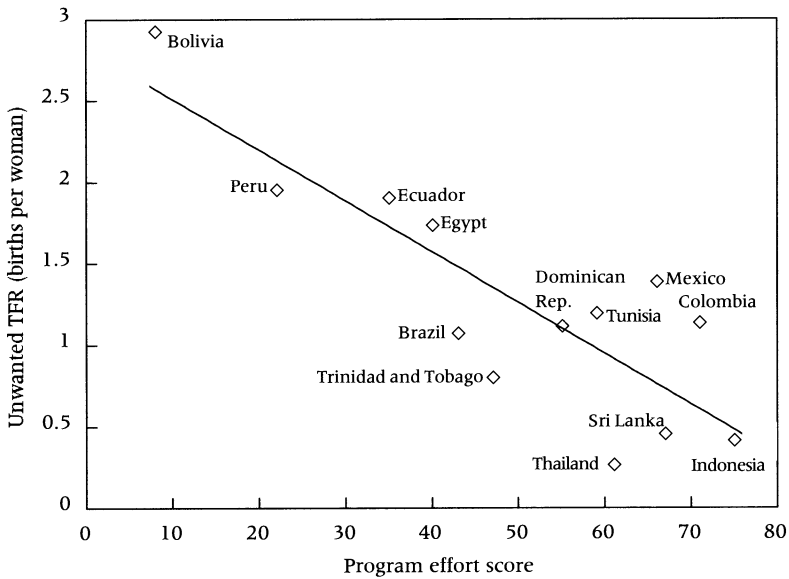
$$\text{UNWTFR} = 3.42 - 0.323 \times \text{WTFR} - 0.028 \times \text{FPE} \quad R^2 = 0.56 \quad N = 25$$

(4.5)                      (5.1)

My predictions are strongly confirmed.<sup>1</sup> On average, a 10-point increase in program effort leads to a decline in unwanted fertility of 0.28 births. Turning a weak program with a program effort score of 20 into a strong one with a score of 70 reduces unwanted fertility by 1.4 births per woman. Figure 1 plots the level of unwanted fertility by program effort for countries near the end of the transition in wanted fertility (WTFR < 3). The program effect is clear and highly significant.

Why did Pritchett's regressions show a weaker effect of programs? Aside from the fact that he did not examine the determinants of unwanted fertility separately, there is another reason. In his regression analyses, Pritchett combines data from the 1970s and 1980s. This leads to biased results because (1) program effort scores in 1972 were collected with different procedures from those used in 1982 and 1989, and they should therefore not

**FIGURE 1 Unwanted fertility by family planning program effort for countries near the end of the wanted fertility transition (WTFR <3)**



SOURCE: Bongaarts 1990; DHS data files; Ross, Mauldin, and Miller 1993.

be used in the same regression<sup>2</sup> (Mauldin 1994); and (2) the desired fertility rates (DTFRs) contain biases due to rationalization, and this effect is largest in the WFS surveys of the 1970s (Bongaarts 1990). These problems have been avoided in the above regression by limiting the analysis to data from the 1980s and by using WTFR rather than DTFR to measure fertility preferences.

The most egregious error in Pritchett's regression analyses is his use of incremental  $R^2$  to measure the relative importance of different explanatory variables. The reasons for this can be found in standard statistical textbooks and need not be repeated here. Instead, a brief counter-example will suffice. As Pritchett notes, contraceptive prevalence is highly correlated with fertility. In fact, prevalence alone explains 87 percent of variation in the total fertility rate. Pritchett claims (correctly, I believe) that female education/literacy is an important determinant of fertility. However, adding female literacy to the regression, together with contraceptive prevalence, increases the variance explained by less than one percent. Can we conclude from this analysis that female literacy is a trivial cause of fertility differences? Certainly not, and Pritchett would be the first to claim that such a

conclusion would be naive and wrong. The same conclusion applies to Pritchett's use of incremental  $R^2$  to assess the effects of family planning programs.

Much of the article attacks a straw man. It sees family planning programs as having only one role: providing access to low-cost contraceptive commodities. Not until his discussion of the Matlab experiment does Pritchett acknowledge the much broader and powerful influence programs can have by reducing noneconomic costs of contraceptive use, such as lack of knowledge, fear of side effects, and social and familial disapproval. Programs can indeed make "contracepting cheaper than free." Most programs are weaker than Matlab's, but they surely have a substantial impact beyond the role of easier physical access. These other effects may actually be predominant in explaining the role of programs evident in Figure 1.

Once one properly defines the cost of contracepting to include a wide range of obstacles to use, the discussion in the article's fourth section, "Supply and demand for contraception, child costs, and fertility," becomes pointless. Statements such as "The marginal cost of avoiding the birth of a child is generally trivial compared to the marginal cost of having a child," or "The decision to have another child is simply too important and too costly for contraceptive costs to play a major role" are simply false. The fact that unwanted births occur proves that, for the women having such births, the cost of avoiding them, rather than being trivial, *exceeds* the (net) cost of having them. Consequently, the elasticity of the demand for contraception with respect to the cost of contracepting is much greater than Pritchett suggests. This is again consistent with the results shown in Figure 1.

There are numerous problems with the cost calculations in the article. This is not the place to give details, but it should be noted that estimates of program costs per birth averted are by themselves not very helpful to policymakers. To decide whether investments in family planning programs are worthwhile, one should also have estimates of expenditures (for example, on education and health services) saved by averting unwanted births. In a recent review of population policy issues, Cassen (1994) cites a number of studies showing that family planning programs are highly effective in saving governments money: the costs in these studies refer to family planning programs, and the benefits are mainly savings in health and education budgets.

None of the above comments should be considered in any way to diminish the central, and often dominant, role of social, educational, cultural, and economic changes in reducing fertility in most developing countries. Nevertheless, family planning programs are much more important than Pritchett claims. Governments in the developing world would be well advised to increase investments in human resources *and* in family planning programs.

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## Notes

1 The regression equation is as similar as possible to the one used by Pritchett in Table 3. The only differences are that unwanted fertility is the dependent variable and data are restricted to the 1980s. Adding other social and economic indicators as explanatory variables does not significantly change the coef-

ficient for FPE. Adding an interaction term (WTFR  $\times$  FPE) improves the overall fit of the model (data not shown).

2 For example, the 1982 total program effort score was based on measures of 30 program dimensions, twice as many as in 1972.

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