

## **Rejoinder**

### **The Onset of Fertility Transition in Pakistan**

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In a Comment published in the Autumn 2000 issue of this journal, Mr Ghulam Soomro<sup>1</sup> takes issue with our recent article in *Population and Development Review*.<sup>2</sup> Although Mr Soomro is highly critical of our article, we are pleased that he has read the article carefully and made the effort to write an extended comment. We are not prepared, however, to concede the major points in that Comment.

Two major points are made by him. First, that marital fertility decline is a small component of the recent fertility decline in Pakistan, which has been mainly due to postponement of entry to first marriage. Second, that the underlying motivation for fertility change in the 1990s has been economic distress, a consequence in part of the structural adjustment programmes instituted in the late 1980s. However, in the first point, Soomro interprets the demographic data from the past three decades *incorrectly* and, in the second point, he *misrepresents* our argument.

On the question of the extent and sources of fertility decline over the past four decades, we have asserted that the total fertility rate [TFR] declined only slightly from the 1960s until the early 1990s, with the slight decline primarily due to marriage postponement, followed by an accelerating decline in the 1990s due to increased practice of contraception within marriage. The demographic data presented by Soomro substantiate this argument. Looking at Table 2, it is clear that the main source of fertility decline between the 1975 PFS and the 1990-91 DHS was marriage change (compare the PFS and DHS columns), whereas the main source of fertility decline between the 1990-91 DHS and the 1996-97 PFFPS was increased use of contraception (compare the PDHS and PFFPS columns). The same data presented in a different fashion in Table 3 indicate that marriage change accounted for roughly a one birth decline in the TFR between the PFS-75 and the PFFPS-97 (3.471 – 2.421), whereas change in contraception accounted for over 1.5 births during the same period (2.185 – 0.468).

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<sup>1</sup>Ghulam Yasin Soomro. Comment: A Re-examination of Fertility Transition in Pakistan. *The Pakistan Development Review* 39:3, 247–261.

<sup>2</sup>Zeba A. Sathar and John B. Casterline. The Onset of Fertility Transition in Pakistan. *Population and Development Review* 24:4, 773–796.

The comment makes much of the lack of change in the total marital fertility rate [TMFR] from the PFS to the PFFPS (Table 4). But demographers have long-recognized that the TMFR is heavily determined by high rates at young ages (15–19 and 20–24), and that these rates can be misleading because they are based on brief post-marital exposure. This distortion is of special concern if the TMFR is used to monitor fertility change, because marriage postponement will often lead to a shortening of marital exposure at young ages and, therefore, a further inflating of age-specific marital fertility rates. This is just what we believe has occurred in Pakistan. The marriage postponement described both by Soomro and ourselves has produced the increase over time in marital fertility at ages 15–19 and 20–24, shown in Soomro’s Table 4. In contrast, the marital fertility rates at ages 30 and above are much less sensitive to the distorting effects of nuptiality change, and therefore are more valid indicators of marital fertility change. Consistent with the argument in our paper, Table 4 shows substantial declines from the PFS to the PFFPS in marital fertility rates at ages 30–34, 35–39, and 40–44.

We see nothing in the data presented by Soomro in Tables 1–5 that contradicts the basic demographic argument in our paper: fertility declined gradually prior to the 1990s; fertility declined more rapidly in the 1990s; prior to the 1990s, the main direct cause of fertility decline was marriage postponement; in the 1990s, the main direct cause of fertility decline was contraception within marriage.

Turning to the second major point in Soomro’s Comment—that economic distress has been the underlying motivation for fertility change in the 1990s—we are surprised that Soomro presents this as if it were at odds with our argument. Indeed, the fact that worsening economic circumstances led to a decline in the demand for children during the 1990s is a cornerstone of our argument (see pages 781 and 783). We subscribe to Soomro’s description of “Socioeconomic Conditions in the 1990s” (pages 254–258) and to how these have affected family life, including decision-making about marriage and childbearing. If there is a conflict, it would be Soomro’s desire to confine the effects of economic distress to marriage postponement. In contrast, we see no reason why growing poverty and perceptions of diminishing economic opportunity would not affect *both* the decision to marry *and* childbearing decisions within marriage. Curiously, on this point Soomro occasionally slips: he points to changes in “the cost of raising children” (page 255), changes in “the opportunity cost of women’s time for the bearing and rearing of children” (page 257), and “the rise in the cost of children” (page 257). Each of these statements describes an impact of economic distress on childbearing decisions. In short, on the question of the underlying cause of recent reproductive change in Pakistan, we perceive no fundamental contradiction between our views and Soomro’s.

Again, we thank Mr Soomro for his Comment on our paper, and we hope that this exchange would advance better understanding of the nature and causes of demographic change in Pakistan.