

Comments

Explanations for High Levels of Infant Mortality in Pakistan – A Dissenting View

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Over the years, as social scientists and economists have become critical of simply using the GNP per capita statistic as the major indicator for the level of development, especially in the case of underdeveloped countries, other more sensitive indicators, which reflect a broader range than does the GNP per capita, have begun to be preferred. One of the most important indicators to replace or supplement the GNP per capita concept is the Infant Mortality Rate (IMR) statistic. The importance of this indicator as a reflector of the level of development is further enhanced when one considers the fact that it was one of the three indicators chosen by Morris (1979) for his monumental Physical Quality of Life Index. Thus, the careful observation over time of the IMR is an important, albeit still crude, sign for the way a country is progressing. And given this importance, it becomes one of the main targets which needs to be addressed by policy-makers. However, before one is able to implement measures that bring this rate down, i.e., pushing the 'policy package', it is necessary to study carefully the actual causes that result in the high rate. Seeking explanations for a high level of infant mortality, thus, becomes an important objective. The paper by Sathar (1987) is an attempt to do so.

Sathar queries "whether the roots of poor chances of survival of children in Pakistan lie in (1) poverty, (2) in child rearing and child bearing practices, (3) in the sparse distribution of health care or, (4) in the lack of individual attention and care given to children by parents as a result of widespread ignorance and illiteracy" (p. 56). Further, she says that she uses a "holistic approach . . . in the investigation of the critical elements sustaining high levels of infant mortality in Pakistan" (p. 56). Her data are drawn from the "Fertility Module of the Population, Labour Force and Migration (PLM) Survey carried out in 1979. This module . . . contains detailed reproductive histories for 9810 ever-married females selected from the

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11,000 households sampled in the PLM survey" (p. 56). The usual caveat about the quality of data is, of course, mentioned.

Sathar begins her analysis of the causes of infant mortality by looking at how (and if) poverty affects the IMR. Using the IMR for countries in the region with a low per capita income, she shows that "although Pakistan now ranks quite high amongst low-income level countries, and poverty levels in terms of per capita income have fallen, infant mortality remains high . . . *This fact goes against the hypothesis that elimination of poverty would also reduce infant mortality in Pakistan*" (p. 57, emphasis added). She goes on to look at the difference between mortality rates in the urban areas and the rural areas since "there are distinct differences between the economic and social circumstances of families" living in the two areas. Her conclusions: "infant mortality portrays the expected negative association with income in urban areas and a U-shaped association in rural areas" (p. 58). Since there is a possibility that income is inadequately reported in the rural areas, Sathar realizes that there is a need to study other measures of wealth as well. She looks at the size of farm, tenure status, occupation and tractor-ownership, and then looks at the IMR within each of the above groups. Her results show: (i) a 20 percent lower IMR amongst households which own tractors; (ii) a 40 percent higher infant mortality amongst farmers as compared to landlords; (iii) *kammis* and landless labourers experience the highest infant mortality; and (iv) owner-operators had lower mortality than sharecroppers and contract lessors. A similar exercise was carried out for the urban areas, and her conclusions were that "household heads who are employers are expected to have higher economic status and infants in their households have markedly lower mortality in urban areas . . . Amongst occupational groups, professional and clerical workers have lower levels of infant mortality than those in sales work or those who are skilled or unskilled labourers. *Thus higher income status, as measured by household head's being an employer or father's being in professional or clerical jobs, is most certainly associated with lower levels of infant mortality*" (p. 60, emphasis added).

Regarding child bearing and child rearing practices, the author says that since women in Pakistan have more than 6 births on average, child bearing patterns have a mortality-raising impact. She also finds a strong negative association between child-spacing and mortality, which is "one of the strongest demographic relationships found". So far as access to health care is concerned, Sathar finds an inverse relationship with infant mortality. Since there is a great contrast between the sophisticated health care available in the urban areas and the very primitive medical facilities provided in the rural areas, "[i]t is thus not surprising to find that infant mortality in urban areas is about twenty-five percent lower than in rural areas" (p. 65). Poor access to potable water and poor weaning habits, both of which cause illnesses related to the digestive system, are also found responsible for causing a greater

number of deaths in the rural areas. Maternal education is considered by the author to be "a good indicator of mother's greater awareness of seeking help at the right time from the right place and handling health problems of children more efficiently" (p. 66). Sathar's evidence and that of others cited by her indicate "that there is direct impact of maternal education on mortality, independently of health-service availability" (p. 66). Compared to wife's education, husband's education has less of an effect on infant mortality. A female-headed household is found to have lower infant mortality than a male-headed household.

Zeba Sathar's overall conclusions relate to poverty and to other factors. First, regarding poverty, after presenting the evidence shown above, she says: "the answer to the question whether widespread poverty is responsible for high levels of infant mortality in Pakistan is more negative than positive" (p. 61). And, further: "the economic differentials in infant mortality were not large enough to suggest that the elimination of widespread poverty would in itself reduce infant mortality" (p. 69). About the other indicators she says that "some conclusive evidence does emerge to support the importance of child bearing and child rearing and the ability of parents to take better care of children by availing themselves of more of the health services as explanations of high infant mortality in Pakistan" (p. 69). Her recommendations are "a strong maternal-child health approach to increase spacing by improving means of breast-feeding and other post-natal care, and restricting childbirth to ages 20-35 years" (p. 69).

One criticism of the paper is that the author has not only avoided carefully looking at her own data, but also at the numerous studies which do not support her conclusions regarding poverty. Furthermore, her explanations for the high infant mortality rate are analysed in a very compartmentalized fashion, and the inability to link all these factors with other *more determining* factors renders her efforts quite superficial. These are indeed very strong claims, which must be backed up by clear, scientific, and supportive arguments. It is the purpose of this paper to do precisely that.

One fails to understand how the author, after presenting a great deal of data which show how income, assets, and wealth affect the IMR, seems to reach conclusions which are completely contrary to those suggested by the evidence. After arguing that it is quite possible that income is an inadequate indicator in the household's wealth status, she quite rightly looks at other non-cash transfers, physical capital, and occupation. Her entire section on poverty *shows very conclusively* that poverty does have a very significant impact on the IMR. Her statements that "the direct relationship with household income is weak, particularly in rural areas" (p. 61), and that there "may be an inadequate reporting of income in rural areas" (p. 58) emphasise the need to evaluate other assets so as to be able to supplement the (pure) income figure. But the evidence does not support her conclusions.

The author shows the “infant mortality rates for neighbouring countries in the region with low per capita income”, and since Pakistan has both a high GNP per capita and a high IMR, Sathar says that “this fact goes against the hypothesis that elimination of poverty would also reduce infant mortality in Pakistan” (p. 57). Recent history, however, has shown repeatedly that as development or growth (measured on the basis of GNP per capita) takes place, the IMR does fall. Of course there are exceptions, and certainly more than just one or two that go contrary to expectations. But that does not throw out the theory. What is required of a scientist in this regard is to try to study the reasons why such exceptions persist, and not to denounce the logic and the facts (see, for example, the work by Sen (1980) for a possible explanation – although not related specifically to Pakistan). Similar evidence, which can be equally claimed as conclusive, can be cited to show precisely the opposite: Pakistan’s GNP per capita has risen substantially from, say, 1960 to 1988, and her IMR has also fallen over time. Thus, the fact that there is no exact and equal inverse relationship between the two should be quite clear.

That Sathar has conveniently decided to ignore numerous studies which show quite conclusively the relationship between income, poverty, and the IMR, goes against the credibility of her paper [see amongst numerous works: Yunes and Rochezel (1974); Shin (1975); Som (1977); Wood (1977); Carvalho and Wood (1978); Dysin (1978); Faruqee (1979); Hicks (1979); Isenman (1979); WHO (1979); Birdsall (1980); Cochrane *et al.* (1980); Merrick (1983); Vanzo, *et al.* (1985); Zaidi (1988)]. If her work is indeed an advance refuting previous work, the literature would be greatly improved if she were to offer a substantial critique of the evidence and the theories that go against her conclusions.

The fact that Sathar has compartmentalized her study further distorts her results and shows a further bias against the ‘income-affecting-the-IMR’ thesis. She fails to realize that “child-bearing and child-rearing practices... the sparse distribution of health care... [and] ...the lack of individual attention and care given to children by parents as a result of widespread ignorance and illiteracy” (p. 56) are all income- (and, thus, poverty-) related factors which affect the IMR [see, amongst many others: Birdsall (1980); Cochrane *et al.* (1980); Datta and Meerman (1980); Merrick (1983); Zacharia (1984); Vanzo. *et al.* (1985); Zaidi (1985); Zaidi (1988)].

Concerning child bearing practices, Sathar says that, on average, women in Pakistan have 6 births. One wonders what the socio-economic background of these women is. Does a consistent pattern amongst women across all income groups exist? Or does it vary? Theory and evidence seem to suggest that an increase in income eventually results in a decrease in the number of births, both among women, and more generally, across nations, i.e., in general, countries with much higher income (GNP per capita) have lower child bearing rates [see: Kocher (1973); Rich (1973), Freedman and Berelson (1976); Faruqee (1979); WHO (1979); Birdsall

(1980); Zacharia (1980)]. Sathar's evidence and conclusions are highly misleading for two reasons: she does not look at child bearing patterns across different social classes and thus she fails to see that child bearing practices are related, in essence, to income (thus poverty). Similarly, Sathar correctly shows that maternal education has a direct significant effect on the IMR. But maternal education is also directly related to income: as a family's income increases, it is more than likely that some interest and emphasis will be given to female literacy and education [see: Schultz (1979); WHO (1979); Merrick (1983); Zaidi (1988)]. This also appears true, barring exceptions resulting from, in particular, rigid social and religious codes concerning women, if one looks at different countries: countries with higher income (less poverty as shown by GNP per capita) have greater female literacy and maternal education and also a lower infant mortality rate. Maternal literacy is indeed a very important factor, and is likely to affect other variables, such as child bearing and child rearing practices.

The phrase which we find ourselves most at odds with is the one where the author says: "though important, the economic differentials in infant mortality were not large enough to suggest that the elimination of widespread poverty would *in itself* reduce infant mortality" (p. 69, emphasis added). This "in itself", is indeed quite strange. Income and wealth (and thus poverty) determine most other things, such as maternal education, housing, access to water and sanitation, nutrition, etc., and thus are the basic factors which determine the infant mortality rate, at least on an individual or a household basis [Zaidi (1988)]. Similarly, if we use the GNP per capita as a very crude indicator of income and wealth (and thus poverty) for nations, the evidence seems to suggest that as the GNP per capita rises (barring a few exceptions), the IMR falls. And here, at the national level, an increased national income may filter down to individuals, lowering their IMRs irrespective of distributional policies, and may consequently result in overall increased welfare. Furthermore, an increased national level of income needs to be seen with other indicators: in countries with high levels of GNP per capita (again, barring a few exceptions), there is greater literacy, better access and facilities, better nutrition, etc., and thus a better status of health for the people. Essentially, then, Sathar's *in itself* is quite meaningless, and is of importance to her only due to her compartmentalized approach, as opposed to the "holistic" one claimed by her.

There are two other points regarding her paper which need to be made. The first concerns the myth propagated by those who have failed to see the dynamic changes brought about by the changing social structure, by the growing impact of the media, and by a 'development' which, despite opposition, is indeed taking place in the rural areas. Such observers, like Sathar, believe that the people may not be very keen to avail themselves of modern health facilities: "A large proportion of Pakistanis still rely on conventional medical practitioners such as hakims and pirs

(spiritual leaders) for most medical ailments and do not avail themselves of the facility in discussion here [i.e., modern facilities]" (p. 65, emphasis added). She repeats this point when she says in conclusion, "it is important to remember that a majority of the populace may not have much faith in modern medicine and may prefer to continue using age-long remedies provided by indigenous health workers" (p. 69, emphasis added). What is the evidence to support this claim? What proportion is "a large proportion"? Do these poor, ignorant people only go to pirs, or to doctors as well? If both facilities were easily accessible, which would they prefer? It seems very easy to make such claims when unsupported by solid evidence. It is true that since modern facilities do not reach most people in Pakistan, they may be forced to find any relief for their ailment where such facilities do not exist or are not accessible; but to say that "the majority of the populace may not have much faith in modern medicine" is extremely suspect.

Another minor comment concerns the following observation by Sathar: "undoubtedly, improvements in educational facilities, nutrition, and health-care facilities will need to occur side by side, but in the absence of any radical socio-economic changes in Pakistan..." (p. 69, emphasis added). Sathar considers these very basic and minor changes as "radical". It is likely that this view is held on the basis of her starting from a very conservative position, as from no other position could the changes mentioned above be termed as "radical".

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Response to the Comments by Akbar Zaidi on “Seeking Explanations for High Levels of Infant Mortality in Pakistan”

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As clearly stated, the article seeks explanations for the relatively high levels of infant mortality in Pakistan as compared with other countries which have middle-level per capita incomes. Comparisons with other countries in the region are presented to demonstrate the same point. Pakistan has a per capita income of 380 US \$ and an infant mortality rate of 121 per 1000. A similar per capita income, US \$ 320 in Sri Lanka, for example, is accompanied by a much lower infant mortality, 32 per 1000, while a similar infant mortality, 133 in Bangladesh, for example, is accompanied by a much lower per capita income: US \$ 140. Clearly, infant mortality could be significantly reduced with no increase in per capita income. The question addressed was ‘How’? It was in this context that the search for “the roots of poor chances of survival of children in Pakistan” was carried out, a point that appears to have escaped the commentator. The idea was not to challenge the established inverse association between per capita income and infant mortality rates; but, on the contrary, to “seek explanations” for Pakistan’s deviation from the expected patterns.

Thus the questions posed in the paper are: (a) Why is the reduction in infant mortality in Pakistan so far short of what would have been expected on the basis of its per capita income? And (b) why are so many more babies dying in Pakistan than in countries with comparable per capita income, and what can be done to expedite a most speedy reduction of this rate.

Most of Zaidi’s criticism appears to be based on a misunderstanding of the paper’s intent as well as content. He claims that “at the national level, an increased national income may filter down to individuals, lowering their IMRs irrespective of distributional policies, and may consequently result in an overall increase in welfare”. The point is, it hasn’t. Instead of criticism of the conclusions derived from the empirical evidence as to how to realise a substantial reduction in infant mortality at current per capita levels, such as those achieved elsewhere, he insists on comparisons with countries with higher per capita incomes. He cites other indicators which tend to be associated with higher per capita income, such as fertility and female literacy. In these, too, Pakistan proves to be an exception, with a higher fertility, on the whole, and a lower literacy than in countries with comparable per capita incomes.

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In the search for explanations, factors were considered individually. Next, they were appraised relative to each other. The quantitative results were then transformed into policy implications. Far from being compartmentalized, as the commentator claims, it was a systematic approach which suggested how to effect the most speedy reduction in infant mortality.

The direct impact of the distribution of income, and hence of poverty, was indeed investigated first. An advantaged economic status was associated with lower infant mortality levels, and higher infant mortality was associated with the very poorest. This, however, accounted for a small part of the variation. The implied structural changes required to reduce substantially infant mortality on the basis of this direct relationship alone would be unrealistic in the envisaged time-scale. It is unlikely that a more precise measurement of income in the rural areas would alter this.

The commentator challenges the results on the basis of the expectation that "fertility patterns are found to be closely related to IMR and must be closely related to poverty also". My article, which perhaps has not been read carefully in its entirety, highlights the greater importance of such factors related to fertility as child spacing and mother's age at birth to IMR over and above that of household income when these factors are included simultaneously in a multivariate analysis. Though fertility is related to IMR, fertility patterns do not correspond directly with income levels as he assumes they must do. This finding would not have astounded Zaidi if he were more familiar with the demographic literature on Pakistan.

At no point is it denied that income, women's education, and the availability and accessibility of health services are inter-related. What is crucially important, however, is that a way forward has been identified which allows the possibility of reducing infant mortality in the short-term. The policy recommendations suggested are not a substitute for other changes. Obviously, any such policy would be speeded up with increasing female education, redistribution of income, etc. But, judging from the record of the last forty years, it is precisely these changes in female education and in redistribution of income which have been slow to come in Pakistan; any substantive improvement would be tantamount to a radical change.

Finally, it is absurd to interpret the article as an argument against the elimination of poverty. No one would argue that raising income levels and the elimination of poverty are not strongly desirable ends in themselves. To suggest that an education, accessible preventive health care, and having enough to eat are not radical in their effects on people's lives perhaps demonstrates the complacent, even blinkered, position of the commentator.