

Communication

“Green Revolution and Redistribution of Rural Incomes: Pakistan’s Experience”— A Reply

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In view of the significance of technological break-through and the changing pattern of income distribution in the process of economic development, I recently published an article [1, pp. 173–205] which dealt with the redistributive impact of the Green Revolution technology on rural incomes in Pakistan. Basing my judgement on the available empirical evidence, I argued that the Green Revolution in Pakistan was accompanied by an improvement in rural income distribution. Of course, I also implied that there existed no room for the opposite, but generally prevalent, view that rural income inequalities had worsened with growing dependence on the Green Revolution technologies.

In a comment on my article [3, pp. 47–56] Professor Mahmood Hasan Khan (hereafter referred to as M. H. Khan for the sake of brevity) has alleged that most of the conclusions of my study are erroneous because (1) I have concentrated on evidence from a few districts of the Punjab but generalized it for the country, (2) I have been eclectic in the use of available evidence, and (3) I did not use even elementary statistical methods to test the representativeness of the averages arrived at in my study. Apart from these general comments, he also raised numerous small points questioning the validity of my arguments in each of the sections of my study.

Since M. H. Khan is an ardent supporter of the currently prevalent views about the adverse effects of the Green Revolution in Pakistan—views which were challenged and negated by me in my article—he, perhaps because of his preoccupation with his own opinions, overlooked the empirical evidence advanced by me¹ and in places even distorted my arguments. It is therefore, my professional duty to reply to his

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¹This is not the first time that M. H. Khan has been so overwhelmed by his convictions as to overlook empirical evidence. Furrugh Iqbal, in reviewing M. H. Khan’s book [4] for the *Pakistan Journal of Applied Economics*, Summer 1982, has pointed to a number of contradictions between M. H. Khan’s conclusions and the empirical evidence cited by him.

comments in full and to remove any misunderstanding even if it involves repetition of some of the conclusions of my original study. My replies to M. H. Khan's comments follow the order of his comments but are prefaced by a consideration of his general comments.

1. GENERAL COMMENTS

I find no valid grounds for the various allegations that have been made by M. H. Khan. For example, while my study provides ample empirical evidence substantiating my conclusions, it is surprising that M. H. Khan has produced no data at all in support of his claim that the conclusions of my study are erroneous.

It appears that M. H. Khan's claim rests on at least three points which seem to me to be all wrong. First he alleges that I have concentrated on evidence from a few districts of the Punjab. I think that this allegation is unfounded because, as my article shows, of the four analytical sections of my study only a small part of one section is based on data from the Punjab. Even there, however, it was shown that those data were consistent with the Pakistan averages and could profitably be used to substantiate the productivity trends implicit in the changing pattern of the use of inputs by small and large farmers. Secondly, he has accused me of being eclectic in the use of reference material. Now, this can not possibly be correct as in my article I weighed all important arguments adverse to my thesis. It may, however, be worth pointing out that in a small but analytical article like mine it is neither possible nor desirable to cite the entire literature, especially if the literature is as vast as that on the topic under discussion. In spite of this limitation, my article referred to as many as 15 works, all of the highest scholastic quality, which are directly opposed to the thesis developed in my article. Finally, M. H. Khan seems to be fascinated by statistical manipulations and levels of statistical significance. While I am fully aware of the usefulness and limitations of statistical manipulations, it is rather unfortunate that M. H. Khan should insist on performing statistical tests even for what is most obvious. It is my contention that there are statistically significant differences in the growth rates of incomes of the various income groups discussed in my article. One could, however, blur this distinction, as M. H. Khan did, by referring only to changes in the income shares of low-income groups. To do so would be undesirable as the income shares of the low-income groups or regions are very insensitive to changes in incomes relative to changes in the incomes of high-income groups or regions.

This brings me to M. H. Khan's comments on individual sections of my article.

2. INTER-FARM INCOME DISPARITIES

M. H. Khan begins his comment on inter-farm income disparities, brought out in my article, by turning my argument upside down. Although my discussion of

inter-farm income disparities begins from page 175 and ends at page 181, M. H. Khan begins his comment with page 181 and goes back to page 178, ignoring completely pages 175-177 of my article which contained a detailed discussion of changes in the input use by the two groups. This is unfair.

M. H. Khan's claim that I give no direct evidence is astonishing. My article is full of empirical evidence which shows that the rate of increase of farm inputs [1, pp. 175-177] and that of farm productivity (measured by gross and net incomes per acre) [1, pp. 178-179] have been higher for small farmers than for large farmers. His points (a), (b) and (c) in his comment on productivity differences are quite unnecessary as they have been taken care of in my discussion of input use [1, pp. 175-177]. I would like to add that my discussion on those pages clearly shows that although small farmers were not the early adopters of the Green Revolution technologies, they had almost caught up with the large farmers by the early Seventies. This, in other words, means that the rate of adoption of Green Revolution technologies was more rapid on small farms than on large farms during most of the period.

That the small farmers are more productive than large farmers and have remained so in the face of the Green Revolution is clearly borne out by my analysis of input use on small and large farms. It is no coincidence that the FAFB data support my views. It is my contention that these data, despite the limitations of the sample size, are among the best sources — and perhaps the sole source — for making intertemporal comparisons of farm productivities by farm size. The explanation, as included in my article, is simple as the FAFB surveys maintained the identity of the respondents year after year and used permanent and trained personnel for collection of income-accounts data as a year-round activity on the basis of the sample from the Punjab to represent conditions in Pakistan. M. H. Khan's points that technical efficiency of farm enterprises rather than gross and net incomes would be a more appropriate measure of farm productivity is well taken but is beside the point in dealing with income distribution changes. This is because here the emphasis is on relative incomes rather than on technical efficiency.

M. H. Khan's remaining comment on farm productivity differences has to do with alleged ambiguities in my definition of small and large farms and its consequences in terms of the number of respondents in each category as given in my paper. I admit these ambiguities. However, these have mainly arisen because of an important but inadvertent omission of a clarifying footnote in my article. Given the limitation of the FAFB sample size, the large farmers here were alternatively defined as those operating more than 25 acres rather than those customarily so defined.

Commenting on my section on changes in land distribution, M. H. Khan has alleged that I dealt only with changes in operational holdings and that too on the basis of 1960 and 1972 agricultural censuses which are not comparable without major adjustments in the 1960 census data. M. H. Khan, referring to S. Akmal

Hussain [2, Chapters 3 and 8], believes that had such adjustments been carried out, they would have resulted in more, not less, access to land area in Pakistan.

My dependence solely on operational holdings for studying land distribution changes was induced by two hard facts. Firstly, data on operational holdings are easily accessible and, secondly, operational holdings are a major source of income for agricultural families. By contrast, data on ownership holdings are not easily accessible and form no significant source of current incomes and remain irrelevant for studying year-to-year changes in income distribution. However, it is not true that I did not deal with the distribution of ownership holdings. My article [p. 180] does include a footnote that compares changes in the distribution of operational holdings with those in the distribution of ownership holdings. It is because of the identity of the changes in the two sets of data and the insignificance of ownership holdings in determining current incomes that I did not consider it necessary to separately discuss changes in ownership holdings.

As far as the incomparability of the two agricultural censuses is concerned, there may be some differences in methodologies. But such differences impinge only on absolute numbers leaving relative changes unaffected. As land distribution or concentration ratios are relative numbers, no ill-effects are likely to result from a change in methodology. The fact that the concentration ratios based on operational holdings of the two censuses are identical with those based on ownership holdings and reported in [4, p. 37] points strongly to the comparability of the data in the two censuses. The inevitable conclusion follows that land concentration ratios did fall between 1960 and 1972. This can also be supported by the data of the 1980 agricultural census [6, p. 2] as land concentration ratio fell further from 0.62 in 1960 to 0.54 in 1972 and to 0.53 in 1980. Although the fall in concentration ratio between 1972 and 1980 may be insignificant, there is no evidence that the ratio has risen.

It appears that M. H. Khan does not have much confidence in his own figures [4, p.37] when he refers me to Akmal Hussain's thesis [2] for adjustment of 1960 census data. It may interest M. H. Khan to know that Akmal Hussain's thesis and other work were actively criticised in a seminar for his adjustment of the 1960 census data with the information collected in 1978. As M. H. Khan may well realize on some reflection, such exercises carry inherent ambiguities. He should not, therefore, have relied on Akmal Hussain's data so profoundly.

3. CHANGES IN INCOMES OF LANDOWNERS AND LANDLESS TENANTS AND WORKERS

For the sake of clarification I would like to repeat what I said in my article that the existing literature includes all kinds of speculations for the falling tenant incomes with the onset of Green Revolution. Among them the most important are those that relate to falling shares in total output, reduced size of the land operated

by a tenant, and the weakening position of the tenant *vis-a-vis* the landlord. How these factors have affected tenant incomes in their totality with the passage of time or with the advancement of the Green Revolution is of direct relevance to this study. M. H. Khan's comments that either this or that factor might have affected either tenant's or landlord's income seem to me to be vague at best. M. H. Khan would be well advised to re-read the relevant parts of my article to note that growth differences between tenant- and owner-operated farms are quite marked and can in general be regarded as statistically significant.

M. H. Khan calls my estimates on changes in the area cropped, labour input per acre and cropping pattern [1, p. 184] dubious but he does not explain why he considers these estimates dubious, although he did point out that my estimates did not include interactive term. But had I included the interactive terms, it would have further raised my estimates of job opportunities which might not have been to his liking. Why I chose to use the U. N. estimates of the agricultural labour force has been adequately explained in my article.

There seems to be nothing new in M. H. Khan's paragraph 2 [3, p. 52] that has not been discussed in my article except that I would like to remind M. H. Khan that Green Revolution's indirect employment effects have been a major factor in increasing urban activities. While he says nothing about the growth of real rural wages discussed in my article, he does not seem to be happy with it either—for what reason, I do not know. I do not, as a rule, cite unauthentic sources of data. The ultimate source for my wages data was the United Nations' *Year Book of Labour Statistics* (various years). More recently, the *PIDE Econometric Model* (1982) under the authorship of Syed Nawab Haider Naqvi and his Associates used a similar series of data. Since I believe that the United Nations produce quite a reliable set of data, their use is fully warranted. I wonder why M. H. Khan found it necessary to cite the trend in industrial wages [3, p. 52] when my article dealt only with the trends in rural wages.

4. REGIONAL INCOME DIFFERENTIALS

On p. 189 of my article I had said: "It appears that the intertemporal comparisons of productivities of the two regions rather than those of incomes in this special case may be a more relevant measure of income distribution changes" [1, p. 189]. M. H. Khan quoted only a part of this sentence and then said: "More appropriate than what?" A careful study of the quoted sentence would help in answering his query. M. H. Khan doubts if I was dealing with the *barani* and irrigated areas of the Punjab alone. My article dealt with comparisons of productivities for both *barani* and irrigated areas on an all-Pakistan basis, as M. H. Khan may well find out if he reads the relevant part of my article once again. It is vague to claim again and again that the differences in the growth rates of incomes, as reflected by productivi-

ties of the two regions, were marked enough to be statistically significant and that irrespective of what factors determine the incomes of the two regions, changes in per acre incomes are relevant for studying income distribution changes. My preference for productivity per acre to total incomes of the *barani* and irrigated areas for measuring changes in regional distribution of incomes springs from the fact that the ever-expanding irrigation facilities in Pakistan tend to add to the size of the irrigated acreage with a concomitant decline in the *barani* land.

While many of the above clarifications also answer some of M. H. Khan's questions on the state of inter-provincial disparities, it may be added here that my treatment of the subject under discussion was based on the valuation of all agricultural commodities. It is rather naive to suggest, as M. H. Khan does, that the period of interest in the context of the Green Revolution should be between 1964-65 and 1974-75. The evidence in Pakistan suggests that factors such as tubewells and fertilizer use had already become significant before 1964-65 and that tubewells, tractors and fertilizer use continue to be significant factors underlying today's growth performance in agriculture. I do not think that I need to refer to Naseem's study [5, Chapter 10] as suggested by M. H. Khan when my own figures are clear enough to show significant growth-rate differences among the various provinces.

5. CHANGES IN INCOME SHARES OF RURAL HOUSEHOLDS

Much of this section involves no substantive comments by M. H. Khan. He has mainly repeated what I have already said in my article. I am fully aware of the qualifications and limitations of the measures of income distribution such as income shares and Gini coefficients and, indeed I have used them in my article to a certain degree. Accordingly, it was hardly necessary for him to restate them in his comment. M. H. Khan's reminder that rural incomes per person and per household declined during the 1963-1972 period seem to have no relevance to rural income distribution. Similarly, his reference to poverty studies, which are generally based on arbitrarily defined poverty lines, also seems to be of very little relevance in this context.

6. CONCLUSIONS

The clarifications provided by the note above should help to evaluate the relevance and validity of M. H. Khan's comments on my article. It would seem to me that his comments are based on misinterpretation and a not-too-careful study of my results.

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