Farm Size and Productivity
A New Look at the Old Debate Revisited

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On closer examination of the data, methodology and results of Manabendu Chattopadhyay and Atanu Sengupta, a number of critical points need to be raised which might cast some doubt on the legitimacy of the conclusions drawn from their analysis.

I
Introduction

IN a recent contribution to this journal, Manabendu Chattopadhyay and Atanu Sengupta (1997) purport to show that the inverse relationship between farm size and output per hectare seems to have been strengthened in the agriculturally developed regions of West Bengal compared to the relatively less developed regions. This, they claim, may be due to the impact of green revolution technologies on land productivity on the smaller sized farms. Given their use of disaggregated farm-level data for 1989-90, sampled from six agro-climatic zones across the state of West Bengal, this is at first sight a potentially significant finding which runs against much of the evidence from India and other countries which suggests a breakdown in the inverse relationship with higher levels of capitalist development in agriculture.

However, on closer inspection of the data, methodology and results produced by these authors, a number of critical points need to be raised which might cast some doubt on the legitimacy of the conclusions drawn from their analysis. I would like, firstly however, to address a number of potentially misleading statements made by the authors with respect to the current status of the inverse relationship debate.

II
Inverse Relationship Debate

I should first of all like to disabuse the authors of the notion that there were no published contributions on the subject of the inverse relationship after 1977, and prior to the publication of a joint statement by Rudra and Sen in this journal in 1980. The authors miss a very important contribution to the debate by G K Chadha (1978) which looks at farm-level data for three agro-climatic regions in the Punjab for the year 1969-70. Chadha found that the inverse relationship had ceased to hold in the more dynamic zones, particularly in the central zone dominated by tubewell irrigated maize cultivation. There is Ghose's 1979 re-examination of the FMS data which argues that an essential precondition for the existence of the inverse relationship phenomenon is technological backwardness. And then we have the compendious work by Berry and Cline (1979)[1] which surveys the inverse relationship evidence in a wide range of countries, including an appendix by Bhalla on the Indian evidence. Despite the claims of its authors, this latter work presented evidence for a great deal of heterogeneity and diversity with regard to the size-productivity relation. We also have an important study from the Indus basin of Pakistan by M Khan (1979) which again provides concrete evidence of a breakdown of the inverse relationship following the introduction of green revolution technology.

It was evidence such as this, along with a plethora of earlier studies, too numerous to mention here,[2] that surely convinced Sen of the illegitimacy of generalising a static and localised phenomenon of peasant agriculture to the level of Indian agriculture as a whole, and beyond to third world agriculture as less careful populist authors have done. This permitted the joint statement with Rudra to the effect that: "The general conclusion to emerge is the diversity of Indian agriculture, regarding the existence of the negative relation between size and productivity: 'the negative relation may hold in certain parts of the country at certain times but not everywhere and not at all times'. It also appears that even when the inverse relationship holds, it may hold in certain ranges but not in others, and in many cases it is particularly noticeable 'only for small size classes'. While counting the different regions, one would find that the inverse relation is more frequently confirmed than rejected, it would be a mistake to take it to be an empirical generalisation for Indian agriculture as a whole" [Rudra and Sen 1980: 693]. There is therefore no contradiction between Sen's summary in 1975 and his joint statement with Rudra in 1980, as Chattopadhyay and Sengupta maintain.

Indeed, Sen had questioned the statistical validity of the relationship as early as 1964, and repeated in his 1975 book, with regard to the analysis of the data aggregated over size-class: "the inverse relationship is not yet something that can be taken as a well established fact". In conjunction with Rudra's contention (1968a, b) that the analysis of farm-level data was being biased by aggregation across villages, considerable doubt was cast on the robustness of the aggregated FMS data. Sen, in his 1975 book, explicitly mentions the evidence presented by Bhattacharya and Saini (1972) as reinforcing that doubt. Nor should Chattopadhyay and Sengupta be perplexed over the use by Rudra and Sen of the 1976 paper by Chattopadhyay and Rudra. The latter explicitly state that they are considering aggregated data precisely in order to confront the FMS data on the same basis.

Since this joint statement by Sen and Rudra, there has appeared a long series of published contributions on the inverse relationship, some containing fresh empirical studies, some re-examining earlier studies, and each presenting a different theoretical explanation of the phenomenon and/or its breakdown.[3] I can find little evidence in this multitude of studies to support the notion that there "seems to be a general acceptance among agricultural economists and others that the debate on the so-called 'size and productivity relations in Indian agriculture' is settled and over with the publication" by Rudra and Sen of their common statement. Indeed, I find little evidence suggesting that the majority of contributors to the debate, particularly those who wish the inverse relation to be a generalised phenomenon, are even aware of the joint statement or the important issues it raises.

III
Data, Results and Methodology

Chattopadhyay and Sengupta use disaggregated farm-level data for West Bengal 1989-90. The data were collected by the Indian ministry of agriculture's comprehensive scheme for studying cost of cultivation survey for six agro-climatic zones based on cultivation practices, type of soil, irrigation facilities and rainfall. I shall have more to say about the categorisation of these zones below.[4]

The data seems to be somewhat compromised from the start by the exclusion of what the authors maintain are "minor crops": wheat, jute and potato. While aman, aus and boro padi dominate the cropping pattern in West Bengal, accounting for 73.7 per cent of gross cropped area in 1990-91 [Saha and Swaminathan 1994], wheat, jute, potato, mustard and other oilseeds can account for a significant part of the crop mix in gross value terms at the district level: for example, the potato crop accounts for 8 to 20 per cent of gross crop value in Bankura, Birbhum, Howrah, Midnapore and Burdwan districts and 45 per cent in Hoogly district. The jute crop is important in Coochbehar, Jalpaiguri, Murshidabad, North 24 Parganas and Nadia districts. Rapeseed accounts for between 5 and 8 per cent of total crop value in several districts.[5]

These differences in cropping pattern can be even more pronounced at the village level and across farm size categories themselves. Indeed, the exclusion of the potato crop, in particular, may have biased the analysis against finding an inverse size-productivity relationship in certain districts [Roy 1979: 76-77]. Similarly, it is not beyond the bounds of possibility that the exclusion of important commercial crops like wheat and rapeseed may bias the results in the opposite direction for certain areas. This is not to suggest that the inverse relationship is a crop specific phenomenon, but given that the evidence supporting its existence in certain regions is overwhelmingly related to the gross crop value per net cropped area, and is not generally associated with the physical yields of individual crops, then differential cropping patterns must be potentially important, along with cropping intensities [Bharadwaj 1974].

The results produced by the village level regression analysis, presented in table 1 of Chattopadhyay and Sengupta's article, provide scant confirmation of the inverse relationship at the village level. Only 12 of the 60 linear regressions at the village level produce statistically significant negative correlation coefficients (and half of these occur in zone V alone). As is to be expected, the log-linear regressions produce slightly better results, with 14 statistically significant negative correlation coefficients.[6] As Chattopadhyay and Sengupta themselves admit, out of 60 villages, a significant negative relation is valid for only nine villages where both the linear and log-linear regressions give significant results.[7] One can go further. If the authors are explicitly employing the log-linear regressions to reinforce and guarantee the linear results, then surely, the supplementary Kendall rank correlation coefficients should be used in the same way (otherwise this latter test is redundant). Applying the same reinforcement criteria to the latter results would suggest that only six out of the 60 village level regressions support a statistically significant inverse relationship between farm size and output per hectare. Only one of the six zones produces a significant negative relation at the zonal level for the linear regression (perhaps significantly, Zone II, ostensibly a relatively backward zone). None of the log-linear regression and rank correlation coefficients for the zonal levels is statistically significant. This is not an adequately strong statistical basis, therefore, on which to confirm the existence of such a relationship in West Bengal agriculture.

The authors claim that Bhattacharya and Saini (1972) achieved similar results. They *did not*, and this has significant implications for the next stage in the analysis. The preponderance of negative coefficients had suggested to the authors that perhaps overall the relationship was indeed negative. To get an overall view at the zonal level, the authors follow Bhattacharya and Saini in carrying out a joint likelihood test for combining the independent village level regressions.[8] Bhattacharya and Saini were perhaps justified in implementing such a test given their more robust results for at least one of their two districts. Their results for Muzaffarnagar in UP, for the years 1955-56, 1956-57, 1966-67 and 1967-68, but not for Ferozepur district in Punjab in roughly the same years, provide evidence for a statistically significant negative relationship at the district level for all four years above. They employ the joint likelihood test for overall significance in order to argue that the inverse relationship at the district level is not being generated by aggregation bias, but reflects the preponderance of negative correlation coefficients in the village level regressions.

However, Chattopadhyay and Sengupta have no such results to support. As we have seen, none of their zonal level regressions is statistically significant, i e, there seems to be no evidence for a close negative linear relationship between farm size and output per hectare at the zonal level. Now, of course, aggregation bias can cut both ways: it is possible that statistically significant negative correlation coefficients at the village level might be wiped out by aggregation bias at the zonal level. However, as we have seen, only six villages out of 60 produce strong evidence for such a relationship.

Furthermore, the overall chi-squared test itself may not be very robust. The overall chi-squared statistic is computed on the basis of the joint product of the p-values associated with the independent tests at the village level. With p (the area remaining in the left-hand tail of the t distribution) ranging from 0 to 1, it can be seen that one or two very small p-values will have a disproportionate effect on the joint product. In log terms, as p approaches zero, ln p approaches minus infinity exponentially. In other words, one or two highly significant results can generate a high chi-squared statistic. Hence Rudra's warning (1976) that it is necessary to have a proper appreciation of what a pooled test implies: if there are n different independent tests with null hypotheses $H_1, H_2 \ldots H_n$, each of which is found to be non-significant, whereas the result of a pooled test is significant, the implication is that *at least one* of the null hypotheses has to be rejected. In other words, a significant overall chi-squared statistic means only that at least some of the negative correlation coefficients are significant. It does not mean that the negative relationship is significant in all the cases. The misleading conclusions adduced by Chattopadhyay and Sengupta from these tests are made further apparent in their table 3 where they produce the average within-village correlation coefficients for each zone. These are, of course, entirely meaningless, the mean correlation coefficient being inflated by the skewed results.

There also seems to be some confusion surrounding the analysis of covariance tests which Bhattacharya and Saini employed to investigate possible aggregation bias in their district level regressions.[9] They found that the size-productivity relationship at the district level could not be specified by a simple model $y = a + bx$. Scatter diagrams suggested that $y = a_i + bx$ was a more appropriate model as y intercepts differed between villages due to factors such as soil fertility. However, they noted, correctly, that these standard analysis of covariance tests involve the assumption of equal variance in the village level regressions. As might be expected this seemed to be realistic only for the log-linear regressions. Here we have another example of Chattopadhyay and Sengupta blindly following the methodology employed by Bhattacharya and Saini without perhaps fully understanding the implications noted by the latter. While Chattopadhyay and Sengupta do indeed obtain similar results (presented in their table 4) to Bhattacharya and Saini, the meaning of these results must be open to question. While it is clear in both sets of results that the intercept parameters differ between village regressions, the discussion above might suggest that it is difficult to conclude that the hypothesis of equality of the regression coefficients can be accepted. And even if the latter hypothesis cannot be rejected, the regression results themselves might suggest that while villages show different overall levels of land productivity, there is no statistically discernible negative linear relationship between farm size and output per hectare in any individual village. Note, too, that Bhattacharya and Saini themselves admit that the intercept terms explain more of the variance than the regression coefficients.

A further issue that needs to be addressed, and one that was again pointed out by Rudra (1976 and 1977), is the question of the range of farm sizes over which an inverse relationship might hold. A negative regression or correlation coefficient (either significant or insignificant) between output per hectare and farm size may be generated even though such a relationship holds over a limited range of farm sizes. Indeed, the rather weak correlation coefficients presented by Chattopadhyay and Sengupta may suggest precisely this. It is unfortunate that the authors do not present any scatter diagrams which might reveal discontinuities in the distribution of observations (as Chattopadhyay and Rudra demonstrate in a 1977 addendum to their original article). If this be the case, then yet again, there seems little solid evidence on which to make such important generalisations concerning the inverse relationship.

IV
Claims Based on the Results

Chattopadhyay and Sengupta make a number of claims based on their results. The first is that the study carried out by Bhattacharya and Saini in 1972 had not ruled out the existence of the inverse relation between farm size and productivity, and that their (Chattopadhyay and Sengupta) study "vindicates their (Bhattacharya and Saini) conclusions and in fact, provides strong

support to it". This claim may be unfounded. As I have argued in the preceding section, the evidence presented by Bhattacharya and Saini is of a more robust nature than that presented by Chattopadhyay and Sengupta.

There is a further difference between the two studies that may be pertinent. Bhattacharya and Saini carry out two sets of regression exercises: one between the value of output per net cropped area and farm size, and the second between output per gross cropped area and the size of gross cropped area. These two sets of regressions provide us with more information concerning the pattern of land productivity and cropping intensity across farm size. The situation with Chattopadhyay and Sengupta is less clear, however. They seem to be employing a single hybrid specification in their regression equations: between "farm size (net cultivated area denoted by A) and value of output *per hectare of paddy* (V/A)" (emphasis added). This would appear to suggest that the authors are employing value of output per gross cropped hectare in their land productivity variable (and therefore the second A is not the same as the first A). It is ambiguous, and I stand to be corrected on this, but if, indeed, gross cropped area is being used in the productivity measure then the authors may be introducing a significant bias into their data which would tend to weaken any inverse relationship between farm size and land productivity. If the small farms are cropping land more intensively, and perhaps growing a range of other crops besides padi, then the abstraction from both cropping pattern and cropping intensity implied by the form of the data used may well be militating against finding an inverse relationship, particularly in the so-called "relatively less developed regions" (zones I, II and VI).

To say that the study by Bhattacharya and Saini did not rule out the existence of an inverse relationship is a rather weak interpretation of their results. Their results tell us much more about the farm size – land productivity relationship and its evolution over time. Their results with respect to net cropped area do indeed confirm an inverse relation for the Muzaffarnagar district for the years 1955-56, 1956-57, 1966-67, 1967-68, but provide little or no support for an inverse relation in Ferozepur (even in the mid-1950s prior to the green revolution). Their results with regard to gross cropped area amplify that difference, and further suggest a structural break between the pre- and post-green revolution periods. For Muzaffarnagar, the coefficients switch from being negative in the earlier years to positive (but insignificant) in 1968-69, while the coefficients for Ferozepur switch from being insignificant (both positive and negative in different years) to being positive and significant in 1968-69. As Bhattacharya and Saini state: "the correlation seems to have *become* positive in both the regions" (their emphasis).

The latter authors state explicitly that their investigation throws light on the changes in the size-productivity relationship brought about by the green revolution. This brings us to the second claim by Chattopadhyay and Sengupta. They make the claim that "the inverse relation between farm size and productivity becomes stronger in the agriculturally developed regions of West Bengal compared to the relatively less developed regions". They add that this is possibly due to the effects of the green revolution on the smaller-sized farms, in particular the improvement of the latter through assured all- year round irrigation water during the post-green revolution period. This is a most startling claim that requires very close attention.

It is all the more startling given the weight of accumulated evidence from a number of other studies which show a breakdown in the inverse relationship with the introduction of green revolution technology and the development of modern capitalist agriculture more generally. Thus, besides the evidence presented by Bhattacharya and Saini above, we have strong evidence of the disappearance of the inverse relationship in the Punjab areas of India and Pakistan presented by Chadha and Khan, respectively. Roy (1981), in a careful study of Punjab shows that it is precisely in those areas most affected by the green revolution, where the penetration of the new technology has been deepest, that the inverse relationship weakens and disappears. These findings are also supported by Patnaik's analysis of Bhalla's data for Haryana (1987). Dyer (1997) shows, in the case of Egyptian agriculture, that the introduction of green revolution technologies and the intensification of capitalist agriculture first increase cropping intensity and labour input intensity on the larger farms, thereby weakening the inverse relation by increasing output per hectare on the latter. Later, with capital intensification and mechanisation on the larger farms, significant scale economies result in the reversal of the size-productivity relation, the larger capitalist farms generating higher output per hectare than the smaller peasant farms.

The evidence presented by Chattopadhyay and Sengupta, if correct, might suggest that a process of catch-up is taking place in West Bengal agriculture, with small farms eventually gaining access to the new technologies, particularly tubewell irrigation, HYV seeds and chemical fertilisers, thereby re-establishing the inverse relation, as Berry and Cline, Bhalla and Lipton (1978) hoped. Or perhaps some other factors might be at work (see the discussion below). Unfortunately, whatever the case might be, the study presented by Chattopadhyay and Sengupta does not allow us to draw any firm conclusions on this matter. Besides the lack of robustness of the data described above, which must certainly weaken the claims of the authors, the organisation of the data employed actually precludes making such claims.

The main problem here is the appendance of rather vague captions to the six agro-climatic zones in which the data is organised. The authors tell us that these are based on cultivation practices, type of soil, irrigation facilities and rainfall. The authors inform us that zones III, IV and V are regarded as "prosperous zones" in terms of "soil fertility, irrigation facilities and other factors". But nowhere are we provided with any evidence to support these assertions. Agricultural development or progressivity cannot be gauged in terms of soil fertility. We are not told what the other factors might be, or their bearing on the level of agricultural development. The extent of irrigation facilities can be seen as one indicator of agricultural progressiveness, one of the *sine qua nons* of the green revolution, but the authors provide no evidence in terms of irrigation ratios in the sample farms/villages, and no indication of the qualitative nature of such facilities.[10]

In order to come to any sensible conclusion regarding the progressivity of these zones, or even better the sample villages, we would require data on the degree of technological development: the area sown to high yielding varieties, the use of chemical fertilisers and pesticides, the extent of both owned and rental machine inputs, for example. We would also require data on the extent to which peasant farming has been displaced by capitalist farming: possible indicators include the ratio of purchased inputs, particularly chemical fertilisers and pesticides, to total inputs, the ratio of hired labour to family labour, and marketed output ratios, for example. Indicators such as these, which relate to both the forces and relations of production, as well as exchange relations, can tell us the extent to which any particular village or region is agriculturally developed. Whether a region has red lateritic soils or coastal saline conditions tells us very little about differential productivity across farm size, either in the static or dynamic context.

The final claim on which I wish to comment is the authors' assertion that their findings suggest that "the small farmers in the agriculturally better-endowed regions are relatively more efficient compared to the larger ones". There is now an extensive literature on the causal factors behind the inverse relationship, some focusing on qualitative factor differences (such as soil quality and irrigation), others on differential factors intensities (especially labour input intensity). None of the contributions which emphasise the greater efficiency of small farmers are convincing. Firstly, the size-productivity debate concerns only land productivity, while labour productivity, and increasingly capital productivity, need to be taken into account to measure efficiency. Secondly, the inverse relation relates to total value of output per hectare of farm size, for which evidence exists for a number of case studies. However, there is little evidence to suggest an inverse relationship between the physical yields of individual crops and farm size, which one might expect if small farmers

were truly more efficient in terms of cultivation practices than large farmers.

The efficiency hypothesis cannot therefore be supported, and should certainly not be used as an argument for land redistribution to small farmers. This latter *non sequitur* has been pointed out by many contributors to the debate (Chattopadhyay and Rudra, Bharadwaj, Rudra and Sen, Patnaik and Dyer). This, of course, relates to the causal mechanisms behind the inverse relationship, a phenomenon characteristic of peasant agriculture, where small peasant farms and large peasant farms use essentially the same techniques of production. The small peasant farms are on the whole composed of poor and marginal peasant cultivators operating inadequate patches of land, and often heavily indebted. Their higher cropping intensity, and associated higher labour input intensity, must be seen as a problem of "forced intensification", a necessary survival strategy, rather than as efficient choice theoretic behaviour.[11]

One possible factor, however, to which Chattopadhyay and Sengupta do not refer, is state action by the government of West Bengal through the panchayat system and Operation Barga. It may be that intensive investment in green revolution technology, with assured access to such technology by small peasant farms, and, at the same time, a deliberate strategy of compressing peasant differentiation and limiting the spread of mechanisation on the larger farms, has produced an inverse relationship between farm size and land productivity. Dyer (1997) has suggested a similar, but not identical, process taking place in the most intensive land reform areas in the Egyptian countryside where differentiation has been compressed and the spread of technology constrained. Clearly, however, this is a very different process from that envisaged by writers like Bhalla and Lipton who posit an inherent superiority of small farmers over large.

V
Concluding Remarks

In conclusion, the study by Chattopadhyay and Sengupta can be seen to be defective on several counts: (1) it ignores a now copious literature, both theoretical and empirical, which provides ample evidence for the heterogeneity of experience with regard to the inverse relationship, and in particular its breakdown in the dynamic context; (2) no robust conclusions can be drawn from the problematic data and methodology employed; and (3) the classification of the degree of agricultural progressivity along agro-climatic lines has not been substantiated. To paraphrase Chattopadhyay and Sengupta, one therefore wonders as to what could possibly be the basis for Chattopadhyay to change his position from what he concluded in 1976 (along with Rudra).

One statement, however, with which I can agree wholeheartedly is the need for more disaggregated farm-level data analysis to be carried out, especially using larger sample sizes. The latter would obviate the need to resort to somewhat problematic tests of overall significance of aggregated data. Further, a wider range of data needs to be collected which relates centrally to peasant differentiation, technological dynamism, and the development of capitalist forms of agriculture.

Notes

[I am grateful to Terry Byres for his comments on an earlier draft of this paper.]

1 Much of the Berry and Cline study could be found in mimeograph form in 'Farm Size, Factor Productivity and Technical Change in Developing Countries', Brookings Institution, Washington, from 1976.

2 For a detailed summary and critique, see Dyer (1996 and 1997).

3 See references in note 2.

4 West Bengal is classified into six zones as follows: (i) Hill, (ii) Terai, (iii) Old Alluvial, (iv) New Alluvial, (v) Coastal Saline, and (vi) Red Laterite. Zones (iii), (iv) and (v) are regarded as being more agriculturally developed.

5 Figures taken from Districts Profile, Major Crops, West Bengal Government website at http://host.westbengal.com/wbidc/wb.

6 Logarithmic transformation compresses variance.

7 In fact, the results in table 1 [Chattopadhyay and Sengupta 1997] show 11 cases where both linear and log-linear coefficients are significant. The discrepancy is probably due to typographical errors.

8 This is the combination of probabilities test mentioned by Fisher (1990). An overall chi-squared statistic is computed from the internal product of the p-values associated with the independent tests.

9 Chattopadhyay and Sengupta write: "Bhattacharya and Saini...examined whether or not the size-productivity relationships for the different villages could be specified by a simple relation $y = a + bx$" (A-173). They then state in Note 5: "Scatter diagrams...indicate that a's are different for different villages but the b's are constant. Hence the model $y = a + bx$...seems to be realistic." This is further confused by their statement in the text that: "Bhattacharya and Saini considered this covariance test to be more realistic for the reason that the tests involve subtle assumption that the deviations of individual farm observations from the true linear regressions have the same variance for each sample village."

10 Indeed, Zone V (Coastal Saline), which is held to be a "prosperous zone", would seem to be rather poor in irrigation facilities. This zone must comprise areas of coastal Midnapur and South 24 Parganas, both of which have well below average irrigation ratios [Government of West Bengal].

11 See Dyer 1997.

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