DISCUSSION

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It is ten years or more since I called myself an agricultural economist, and in the meantime I have been designated a statistician. It is in that capacity, no doubt, that I have been asked to open this discussion, and I will concentrate on that side of Dr. Jarrett's paper. Even on the purely statistical side there is much that deserves comment that I have no time here to give. Suffice it to say that such remarks would, in the main, be congratulatory.

I have chosen, therefore, four main points for comment. The first relates to the problem of the mathematical specification of the model. The Cobb-Douglas form is one chosen in order to include "diminishing returns" in the model. This it does quite satisfactorily and is therefore a considerable improvement on a simple linear model. But it must not be forgotten that it assumes "diminishing return" in a particular form. It is true that, as long as the range of observations is relatively small, the linear logarithmic model may well be satisfactory, but if a really wide range is envisaged (e.g., as in livestock enterprises in Australia) it may not be appropriate. The point may often be quite irrelevant, but there are always dangers to be watched for in trying to twist the real world into a tractable mathematical form.

The second point I wish to make is to underline the dangers inherent in the aggregation process. Jarrett is obviously himself well aware of this. But it is wise to remember that the assumptions involve aggregation at three levels: of "factors of production", of "farms", and of "techniques". I will return to the "aggregation of techniques" problem in a moment. At this stage, the point I want to make is that it is not always clear to what exact unit the production function obtained precisely relates. Is it some concept of "average" farm, observed at the various production levels?

Thirdly I have some comments to make on the random term ε. Jarrett indicated that this mainly represents variations in the technical and managerial efficiency of the farmers. I feel there is a lot more contained in the term. It mops up not only errors in the form of the equation (e.g. non-linearities) and errors in the measurement of variables (errors of observation), but also other truly stochastic variables of which managerial efficiency may be one, but, in an agricultural context, weather variations are probably the most important.

Finally, a word on the problem of "continuity" which is raised by Dr. Jarrett in relation to the indivisibility of certain factors. Dr. Jarrett might have drawn a further moral at this point. The assumption of continuity is basically that it is possible to adjust the combination of "factors" however one wishes, and that movement from one productive technique to another is possible to infinitely small gradations. It is the possibility of "continuous" adjustment that makes the concept of continuously decreasing returns appear realistic. The real world situation appears, however, to be that factors can, in the main, only be applied in specific units that are not infinitely divisible. While one such combination is in use, constant returns are probably more realistic. Changes in the combination, by the addition, for example, of one more

unit of a certain factor, change production to a new "constant" level of return. Whereas the "production-function" approach, by aggregating techniques, smooths these all out into a continuous surface, the newer methods of linear programming endeavour to take account of the discontinuities.

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In commenting on this paper I do so as one who *is* prepared, to use Dr. Jarrett's phrase, "to make good tries to obtain good empirical approximations of marginal resource productivity." Despite such limitations as using an inter-firm function for intra-firm function purposes, a static analysis for a dynamic situation, assuming independence where co-linearity exists, assuming maximisation of profits where the motivating factors are in fact largely non-economic, the inevitable failure to select and use the right variables, ignoring factor indivisibility—or where these things are not ignored or assumed—assuming that they are randomly distributed—despite these and other limitations and those of a mathematical nature which I do not profess to be able to assess, marginal productivity estimates are, nevertheless, worthwhile making.

However wrong the estimates of marginal productivity may be I think their publication does at least give an opportunity to everyone who is concerned with the allocation of the particular resources studied to reconsider their position, and it forces them to do so within the framework of the marginal analysis. This is wholly good.

When I say everyone who is concerned with the allocation of resources I include not only farmers, extension officers, directors of land settlement schemes and so on, but also those agricultural scientists who are responsible for designing input-output type experiments. It is imperative that these scientists provide farmers and extension officers with fully determined empirical physical input-output functions which are a necessary basis for better adjustments of resources.

My second point is that competent agriculturalists may claim to know of and already do know of profitable opportunities for re-allocations of resources within farms, between farms, and between regions and so on, from common sense analyses, and without making such productivity estimates as you have before you. My answer to this is that I do not think that agriculturalists could do this so comprehensively in any other way. In Western Australia for instance, it is quite true that the Dairy Branch of the Department of Agriculture has long been telling whole-milk farmers that they over-feed with purchased feedstuffs. Mauldon and I seem to have confirmed this. On the other hand the suggestion that the same farmers may be under-fertilising seems to have occasioned some surprise and to have highlighted the paucity of experimental data with fertilisers in the area surveyd.

Finally, one of the objections to this type of analysis is that it is static whereas the inputs may be of a long term nature and have not at the time of analysis yet made their full contribution to output. Is it not possible to minimise this objection for some uses of the function by classifying inputs on a time basis as Antil, and Mauldon and I, for instance, attempted when we singled out feedstuffs and fertiliser inputs?