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LARGE SCALE AFRICAN COMMERCIAL FARMING IN SELECTED AREAS OF UGANDA AND BUSOGA - AN OUTLINE OF PROPOSED RESEARCH. by D.A.Hougham.

Any series of seminars devoted to the consideration of planned research projects cannot but end up in questions concerning research objectives generally. Thus the author of this paper will presume, only two weeks after his arrival in East Africa, to raise certain fundamental questions, especially in regard to the role of "small scale" research in the field of economics - or, for that matter, in any quantitative field of research.

In part these considerations were prompted by the apparent preference on the part of the governments of developing countries for the large scale research project, as often organised and executed by an international agency. Since at the same time universities and other smaller bodies, both in the country itself and abroad, will continue to direct less ambitious projects, it is necessary to attempt to define the fields in which the respective types of research may be most relevant.

Since the project with which the author is concerned is of the smaller university sponsored type, it is upon this scale of research that he will concentrate, hoping to use his own project to illustrate his points.

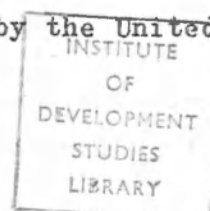
The broad nature of the project.

The broadest definition of the project, as outlined at its conception, was that it was to be a study of the emergence and nature of larger scale (25 acre plus) African commercial farming. It was decided to undertake this study in Buganda and Busoga, partly in the light of their contrasting land tenure patterns. The directors of the project are Dr.A.I.Richards of the African Studies Centre, University of Cambridge, and Professor Sir J. Hutchinson and Mr. Forde Sturrock of the Department of Agriculture of the same university.

Professor R. Apthorpe and Mr.D.Belshaw have been appointed to supervise the project from Makerere University College.

The main research will be undertaken by two research officers, Mr. A. Mafeje, a social anthropologist, and Mr. D.A. Hougham, an agro-cultural economist. They will be joined for three months by Mr. Robertson, at present undertaking research in Egerere.

The project is being financed by the United Kingdom Ministry of Overseas Development.



THE FUTURE OF SMALL SCALE QUANTITATIVE RESEARCH.

Having drawn up a provisional outline of more direct research objectives than those in the initial planning, the author might have been on safer ground simply to have attempted to justify these. Instead he attempted to define the objectives and possibilities of "small scale" research, only, of course, to find that his own objectives went beyond the possibilities implied in his own definitions of this type of research.

It has already been suggested that at the end of the colonial era many former colonial countries became attractive to the undertaking of large scale, often internationally organised, research. Yet at the same time a world wide expansion of both universities and centres of regional specialisation did not encourage a reduction in smaller scale research.

If "small scale" research is not to become a series of isolated, unconnected projects, and if the trend towards larger projects continues, then it seems imperative that the objectives of the less ambitious projects should bear relevance to the larger projects - upon which governments tend to rely for development data.

It might even help us to define the role of "small scale" research if we attempt to understand certain apparent faults in the larger scale research. One particular point is felt by the author as a result of recent papers delivered by Casley, on the agricultural census of Uganda, and by Morris on the Embu project. This is that there is a danger that the greater resources available to these types of research are being used less to increase accuracy than to extend the range of the area studied, or the independent variables controlled. Further even schemes of their size appear to lack the resources to undertake a full investigation of techniques employed, and even, sometimes, to incorporate a satisfactory preliminary survey.

One might suggest therefore that "small scale" research should link itself directly to these larger projects - but most people would acknowledge the administrative and "quasi-political" obstacles to this. In fact it appears often that the expansion of research projects in many areas or subjects is multiplying faster than the ability even to document their occurrence.

I would not, therefore, suggest that "small scale" research should involve itself in undertaking preliminary surveys for as yet unplanned larger projects. I would, however, suggest it can perform too useful, yet independent roles.

1. It can investigate the validity of established techniques, discover new techniques, and even study the extent to which techniques used in one environment may be transferred to another environment.

2. It may investigate the nature of certain specific variables, in particular those which are common to all larger projects in certain defined fields.

Both these objectives neither force the "small scale" project to be dependent upon its larger neighbour, nor prevent the "small scale" research from lacking conclusive results of its own.

However, at present much "small scale" research is being conducted outside these limits, of which the present project is an example. In such fields as agricultural economics, such projects, if they desire to draw conclusions from collected statistical data, increasingly face greater obstacles - if only because of the greater complexity of definitions of statistical validity.

It seems that all economic research involving field work has certain common controlling factors.

1. Labour available - Jon Morris indicated well how the complexity of the administrative and writing up sections of statistical field studies enforce the use of enumerators. This obviously becomes more apparent with increases in the valid size of sample required, and the number of independent variables controlled.

2. Time available - In a society such as East Africa, where the individual (such as the farmer) is unlikely to keep his own records, data on continuously operative activities, such as farming, can only be obtained by a series of visits over the period of the activity. Further the measurement of certain resources, such as labour and land, is a lengthy process, and could easily occupy over 50% of the total research time.

3. Finance available - The ultimate control, since it determines the amount of time and labour available.

In a few lucky projects, one may be able to define objectives and methods, and, after estimating the time and labour needed, gain the requisite finance. More normally, as in the project being discussed, the broad objectives determine the finance, and the finance determines the final selection of objectives and methods. Where research is organised in one country, and executed in another, it seems easy for the limitations imposed by the available finance to considerably invalidate the initial objectives.

Thus one has proposed projects which are not simply investigations of techniques or variables, but attempt more ambitious surveys, only to find that finance which might have been adequate in theory, becomes very limiting in reality. Thus the present project, on the economics side, received finance that would have been adequate for input-output analyses in the United Kingdom,

but precluded such studies in an African context.

In a long term project such a discovery could well have been solved by radically changing objectives. However, where time available is strictly limited, such changes may be impossible.

It has therefore been the author's conclusion that his labour available (himself and one assistant) and time available (one year) preclude, in the field of agricultural economics, any attempts at either input-output studies, or such studies based on case histories during the year (the "setting up" of farms would leave less than a year for their study). Further he felt that he could not eliminate variables by close definition. This was mainly because,

(a) The elimination of variables makes the occurrence of suitable subjects less common, and therefore excessively enlarges the time spent on drawing up a sampling frame.

(b) In such a project as the one discussed, there is a great problem that the variables eliminated by the economist may be quite irrelevant, or even destructive, to the work of the sociologist.

If variables could not be eliminated, they certainly could not be controlled - there is just not the time available for the study of a relevantly large sample.

FINAL SELECTED OBJECTIVES

Where variables cannot be controlled or eliminated satisfactorily, and where the initial objectives of the project are outside the limits for "small scale" research which were earlier defined, one begins to find oneself faced by the ogre (or is it really?) of generalisations. However, since generalisations are the excuse for lack of validity in the conclusions, one probably should avoid them.

The solution decided upon was to make the project a case history one - acknowledging that one then had to severely discipline oneself not to use the results in any of the ways previously thought impossible.

Although the economist might be able to choose individual farms as his objects of study, the sociologist needed to relate the farmers selected to a wider community. Further as the scale of farming studied increases, so the community in which the farmer is involved is enlarged. For this reason it had earlier been decided to undertake the research in certain sub-counties in Kyagwe, Buddu and Bugerere counties of Uganda, and in Butembe-Bunya county of Busoga. From the sociological point of view, this has the advantage that the study of each farmer does not require a completely new study of the

local society. It might therefore be noted that even if the control of variables had been possible, and even if input-output analyses could have been undertaken, there would have been considerable conflict in the sample selection between economist and sociologist. Although I have not fairly faced this problem in the paper (mainly because it is less restrictive in case history work, and this approach was dictated as much by economic considerations as those of inter-disciplinary demands), there do seem to the author to be several very debateable points.

1. Can economists and sociologists usefully, certainly at the smaller scales of research, draw up a mutually satisfactory sample, and thereby indicate a satisfactory agreement on the control of variables. (i.e. religion might be important comparatively to the sociologist, but much less so to the economist)?

2. In any joint study, with the aim of fairly wide conclusions, can the project be completed by both disciplines in the same time?

3. If these cannot be achieved, does one aspect of the dual study become subservient to the other?

Having chosen the case history approach - or rather having had little alternative - one had to decide what aspects of agriculture to concentrate upon in the light of the inability to undertake input-output studies. Was one able still to find indices of agricultural achievement which might be related to the social position of the farmer? It might certainly be argued that economic indices, such as gross enterprise margins in respect to the input of the limiting resource, are less accurate an indication of agricultural achievement than the adoption of certain farming methods or crop patterns - except that these latter ought, if valid - to be reflected in an input - output analysis. Further certain "improved" methods might be aimed at satisfying the agricultural officer, whilst less obvious inefficiencies negated any gain from these "improvements". Again, indices drawn from farming methods need a standard measurement of their indication of achievement - which means accepting that the agriculture department's advice is gospel. This was previously questioned in the use of certain crops as indices of "innovativeness" in the Embu scheme.

It has, therefore, been decided to concentrate not upon the statistics of production, but upon the methods, without their use as indices. Conflicts between crop demands, the extent of receipt of agricultural advice, the use by the farmer of his position to enter local crop marketing, his own system of marketing, his use of machinery, etc. will be investigated. Further the economist may tempt fate and venture partially onto the sociologists terrain, in that the reasons for the farmers adoption or rejection of certain enterprises may be investigated. This is not new to agricultural economists - it is studied when defining the minimum and maximum limits for any enterprise in farm planning.

It seems that this type of study of the methods, and more especially of the institutions, utilized by the farmer has recently received less emphasis than in the past. Whilst lying within, possibly, the field of economics, sociology and geography, it yet seems to occupy no central position in the work of any of these disciplines. This is not to deny that work has been done on individual aspects of the production organisation, such as mechanisation or marketing. Even less work has been attempted upon economic motivation amongst farmers in Africa, though many hypotheses have been forwarded.

Yet the author can think of at least two schemes in Nigeria - the Moka and Shendam - which have virtually failed, largely, it seems, due to a failure to understand the strength of the existing social frameworks and economic systems in the areas. Not only does such ignorance place more purely agricultural advice in a void, but further means that the institutions through which changes may best be encouraged are not properly identified.

THE EMERGENCE OF LARGER SCALE COMMERCIAL FARMING

I have tended to accent that part of the project related to the present nature of the farming, and not to that section dealing with the emergence of the larger farms. In that this is mainly an historical study, ones main obstacle is to judge the historical depth to which one may usefully penetrate, this being mainly controlled by the length and accuracy of the informants' memories.

This part of the project divides fairly easily into two sections.

1. The social emergence of the farmer, and the factors influential in causing him to become a commercial farmer.

2. The economic emergence of the commercial farmer, mainly through the study of the accumulation of resources.

There is certainly less conflict in this section between the two disciplines, since the sociologist will be primarily concerned with the farmer, the economist with the farm.

Two main resources may be isolated as important factors - those of land and capital.

The accumulation of capital

Capital will have played an important role if either the farm was purchased, or large farm improvement schemes undertaken. This capital must have been either inherited or earned. In, however, investigating its origin one will face the many problems that face any interviewer asking

financial questions of a personal nature. It may be possible to observe, though again not conclude, as to the extent to which the ownership of larger farms is in a sense "hereditary" (i.e. a self-perpetuating group), or whether it is the ultimate goal and achievement of those who have accumulated capital through economic activity in other fields.

Accumulation of land

Most of the case studies in Buganda will most certainly be of Mailo land. On this land it appears that two forces militate against the formation of large farms.

1. The extreme limitations imposed upon the land-owner in regard to the removal of tenants.

2. The continued sub-division of land through inheritance.

Yet the large farms do exist, and the project may, in its selected areas, be able to discover whether these larger farms are the remnants of even larger units, or the product of land accumulation. If the latter, it would be very useful to know how this was achieved.

METHODOLOGY

It has already been indicated that the chief means of gaining the data will be through interviews. The list of relevant farmers in the sub-counties is very dependent on help received - mainly from gonbolola and muluku chiefs, and from local agricultural officers. Obviously farmers themselves may be able to add to the list. A further indication of farm size as opposed to area of land owned may be gained from the tax lists. Yet all these sources are only suggestive, and each will have to be checked.

In regard to the accumulation of land, the interview technique will be backed by use of the Mailo Land Registers and through wills. The author has already had a long talk with Mr. H. West, and is, he hopes, fully aware of the deficiencies of the Land Registers. They will probably be most useful as an approximate check on data from the interviews.

Because of the lack of a preliminary survey, an advantage has been seen in the use of two research officers. In each sub-county the farms may be divided into two groups, with the two officers changing groups at "half-time". Thus questions thought up during the first half of each sub-county study may be incorporated over the whole range of farmers in the second half. Of course, this does mean each officer asking questions concerned with the other officer's subject!

Whilst the sociologist is planning to integrate himself into local society by becoming resident in the sub-county (I don't know whether in a large farm, bibanja or shop); the economist is risking a compromise, being resident in the sub-counties only when they are distant from Kampala, and then only for the working week. He even hesitantly justifies this on the grounds that, since he will be concerned almost entirely with the larger farmers, his identification with local society might not be conducive to respect!

The directors have planned for the second year of the project to be spent writing it up in Cambridge.

CONCLUSIONS

It is the author's hope that this paper has raised questions as to the importance, and objectives, of "small scale" research in countries in which the macro-structure is only itself very partially known. This I suspect may be of greater relevance to the economist than the sociologist. It certainly seems that the projects of the former are becoming more extravagant in terms of use of resources than those of the latter. There may, of course, be reasons other than the necessity of accuracy in this trend.

It does however seem that, for the economist, the breadth of objectives studied by the researcher is closely limited to the knowledge of the economic macro-structure. Where, as in many developing countries, this is only limitedly known, then the researcher must be much less ambitious. For the "small scale" project I hope that I have, by discussing the practical problems faced, indicated some possible broad lines of usefulness for this type of research. However, whilst these problems may well be appreciated in the developing countries, they have received, I believe, little attention in the developed countries, from which much of this "small scale" research is sponsored and, to some degree, controlled.

Although it may be felt that the solutions must be administrative rather than academic, there are definite lines of self-discipline the academic can follow.

Much of what has been said is discussed primarily in the context of statistical research - but I believe it is relevant in any research involving the control of variables and the selection of samples. I do not, however, presume to suggest the problems of non-statistical research, even though, in the project that has been discussed, the problems posed by the statistical demands have largely forced an abandonment of the statistical approach!

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