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APPROACHES TO THE STUDY OF AGRICULTURAL INNOVATORS*

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Foreword

Economists are rarely interested in sociological knowledge for its own sake. This paper was written for a conference of agricultural economists, who apparently wanted to know what sociology could tell them about what they called the agricultural "innovator". It had been intended to make extensive revisions in the paper, to make it more suitable for presentation to a seminar within the Sociology Department; however, other commitments have made it necessary to give it in the original form, with only the addition of these few comments.

It seems that there were two implicit questions being posed by those who commissioned this paper:

(a) how can potential agricultural innovators be identified? Or alternatively, how can potentially innovative communities be identified?

(b) What conditions are conducive to innovativeness? At what point should there be intervention to modify conditions in order to increase receptivity to new ideas (in this case, to speed up adoption of improved agricultural technology)?

This paper provides no answers. However, in the search for available information from East Africa on these questions, I was struck once again by the extent to which the framing of the question influences its answer. Two different approaches to the study of agricultural "innovators" are reviewed here, and I have tried to pay some attention to the way in which the questions asked, and the conceptual apparatus brought to the efforts to provide answers, determine the criteria of relevance and influence the

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methodology. The very meaning of the term 'innovator' differs according to the theoretical orientation chosen, as we shall see below.

The two approaches chosen are the American diffusion/adoption school, and what might be inaccurately summarized as "new-style social anthropology". Many other theoretical approaches could contribute to a discussion of innovation, but in view of shortage of both time and space, the paper was limited to two which are directly influencing current research in East Africa. The paper concludes that the two questions listed above require different kinds of answers, and very different methodologies. Further, it is suggested that the whole approach of focussing on the "innovator" is unduly narrow and leads to omission of aspects essential to understanding the process of agricultural development. Thus there is a danger, for those who are involved in "applied" work, of defeating their own objectives by tackling the wrong questions.

Can the discipline(s) of sociology/anthropology provide answers to these kinds of questions from other disciplines? Or should the attempt not be made? I suggest in the paper that information to answer question one probably could be provided, relatively easily, given adequate research resources. These results would probably be useful to action people, but not very interesting for social scientists. Would the returns be worth the cost of the research involved? It is question two that raises the interesting problems. If our discipline(s) were in a position to provide answers on this point, we would be much further advanced in understanding of causal interconnections, and towards an adequate theory of social change, than I think we are at present.

It must be emphasised that this paper does not set out to deal with social change in general, or theories of social change. It starts with the humble question of what is known about agricultural "innovators" in East Africa, finds that the answer is "almost nothing", and explores some of the implications of two different approaches currently being used to find out about agricultural "innovation" in East Africa. Three empirical studies pertaining to this narrow topic are reviewed. However, it is suggested that some of the implications are of more general interest.

I was asked to give a theoretical review of the sociology of the innovator. Two questions arise: what theory and which innovators? The first question is the vital one, for on the theoretical orientation chosen will depend the delimitation of the subject of study, and the meaning or meanings to be attached to 'innovator'.

I wish to leave aside for the moment the question as to whether sociology offers anything that truly merits the term 'theory', and first define briefly what is meant here by 'innovation', then consider what social research has so far been able to tell us about agricultural innovation in East Africa.

'Innovation' has a very broad meaning. Barnett defines it as "any thought, behaviour or thing that is new because it is qualitatively different from existing forms. Strictly speaking, every innovation is an idea, or a constellation of ideas; but some innovations by their nature must remain mental organizations only, whereas others may be given overt and tangible expression". (Barnett, 1953: 7) La Piere's definition is somewhat more specific: "an innovation is an idea for accomplishing some recognized social end in a new way or for a means of accomplishing some new social end The idea or pattern of ideas may become manifest as a new kind of tool or mechanical device, as a new process or technical procedure, as a new material or substance, as a place or terrain previously unknown to man, as a mode of human action, or as a new concept or belief" (LaPiere, Social Change, p. 107, quoted in Jones, 1967:4). Throughout much of this paper, we shall be dealing with innovations of a much more restricted type-- those new ideas generated by application of scientific thought to a problem, in our case, problems of agricultural technology, and often for the specific purpose of proposing recommendations to a user or practitioner group. Nevertheless, it is important to recognize that agricultural innovations are not

confined to the products of the research stations; innovations can arise endogenously within the practitioner system—sometimes in the form of modifications in procedures consequent on other induced changes, originating outside the system, such as the demand for poll tax. Changes in the customary division of labour, as a reaction to labour migration, is one trite example.

It is immediately obvious that agricultural innovation is nothing new in East Africa. New crops have been diffusing through the area for several hundred years; cash crops, markets, new techniques spread at an accelerating rate during and since the colonial period. In recent decades, the amount of social research carried out in East Africa has also been accelerating, but it is astonishing how little the corpus of published work of the social scientists can tell us about the processes by which these changes have taken place in the ways by which the vast majority of East African peoples make their living.

I The social anthropological orientation

This neglect is a direct consequence of the theoretical orientation that has dominated social research in East Africa until very recently. Most research workers have been oriented towards British social anthropology, which has been concerned with very different problems. This school has produced numerous monographs giving meticulous studies of 'tribal' groups. In general, these have concentrated on establishing normative patterns, with little or no consideration of deviants such as innovators. Ironically, although anthropologists have prided themselves on taking a 'holistic' viewpoint, many have been adept at abstracting from the ongoing social system of the colonial regimes, only those aspects which they wished to study, usually what they regarded as 'indigenous' or 'native'. These partial aspects, the 'tribal' social system, was then presented as a self-contained entity. This is a sweeping generalization to which numerous exceptions ought to be made,

but it will do as a brief characterization.[1] It seems indisputable that the British social anthropological orientation focussed attention (when it looked at change at all) only on the targets of change, and not on the agents and agencies of change, and the interaction between them. Reinforcing this narrow focus, 'diffusion' has been rather a dirty word to followers of this school, for reasons stemming from the history of social anthropology which would be tedious to go into here. The process of technological change (which as we have seen is essentially ideational) has tended to be lumped together with the unfashionable subject of 'material culture', thought to be something best left to museum staff and Americans.[2]

Some anthropologists within the British tradition have given passing attention to the social consequences of changes in agricultural technology, and a few books on the periphery of our geographical area could be regarded as focussing primarily on such adjustments, e.g. Richards' classic, Land, Labour and Diet in Northern Rhodesia, and Watson's Tribal Cohesion in a Money Economy, which deals with responses to absence of male workers through labour migration. The main published work that attempts, rather inadequately, to deal with the administrators as well as the targets of an agricultural development programme is Reining's The Zande Scheme, which stems from the American rather than the British tradition of anthropology, as does Linton's [illegible] classic on the social consequences of the change to wet-rice farming in Madagascar (Linton and Kardiner, 1952).

But the British tradition of social anthropology should not be written off as useless. In recent years there has been evidence of a willingness to come to grips with the facts of change, both social and technological, and a number of studies are still underway, or have been only recently completed, that are of direct relevance to problems of agricultural development.

One of the few examples of this new trend to reach publication is Long's Social Change and the Individual, a study of emergent "progressive" farmers in Serenje District in Zambia. Mafeje's study of "big" farmers in Buganda, Robertson's work on farmers in Bugerere, Charsley's work on religion, attitudes to labour and economic performance among Kenyan settlers in Kigumba, Uganda and Sharman's study involving household decision-making in relation to nutritional status. are just a few examples of this trend. Characteristic of this new emphasis is a willingness to discard the theories of their elders if they prove restricting. broader criteria of relevance, and more use of quantitative methods than is commonly found in the older work. At the same time, this kind of work retains the anthropological focus on understanding the categories and concepts used by the subjects of study, instead of working only in terms of analytical categories imposed by the external observer. It may be that this kind of work will give us much more understanding of the agricultural innovator in his social setting, and the factors affecting his emergence. But at the moment, only two of these studies are available, and will be discussed below where relevant. Some of the work being done in East Africa by young American anthropologists also offers promise of useful material. One of the most significant trends in recent anthropology has been the tendency, after many years of parochial isolation, towards convergence of the theoretical approaches of the younger American and British scholars, together with increasing willingness to draw on the work of other disciplines, such as sociology and, to a lesser extent, economics (see Gluckman and Eggan in A.S.A., 1966).

Perhaps future students of the sociology of knowledge can delve more deeply into the reasons why previous social research on East Africa has provided so little of direct usefulness to those involved in bringing about agricultural development. The large number of published anthropological monographs

can give a change agent much information on kinship, traditional political systems, values, rituals and so on, which may or may not be helpful background material, but that is all From the point of view of the agricultural development planner, the social researchers of the past have been either asking the wrong questions or providing answers in an irrelevant framework.

II. The focus on diffusion and adoption of innovations

Since the newer trends in anthropology, although promising, have not yet provided us with a body of work giving insights into the social processes of agricultural change, what alternatives are available?

The main school of research on the diffusion of innovations has grown up in the United States, (with an outpost in Holland) from where it has in very recent years spread to other countries. This school has drawn on several traditions of research, perhaps the dominant one being that developed within rural sociology from a concern with evaluating the effectiveness of agricultural extension work. A strong secondary tradition has been the abiding interest of American cultural anthropologists (rather a different species to the British social anthropologists) in the diffusion of culture 'traits'. Work on innovations in medicine, education and industry have also contributed to this tradition. In addition, in post-war years in the U.S.A., research on 'communications' has mushroomed providing a focus for work by social psychologists, sociologists and electronics specialists (see Taylor's paper on 'The Administration of Innovations'). For many years this work was carried on in apparent ignorance of the related work by rural sociologists within the same country. In 1963 Katz, a leading worker in communications, wrote that communications research and rural sociology has only recently 'discovered' each other. At any rate, these various streams of research have now merged and produce a flood of empirical studies on the processes involved in the diffusion and adoption of innovations, mostly from the U.S.A., but some from Europe, Asia, Latin America, and Australia. The 468 - item bibliography provided by G.E. Jones (1967) gives an overview of the geographical dispersion, and highlights the lack of published work on Africa. Neither of the two African items listed, refers to East Africa. This research tradition has diffused to East Africa only very recently, not surprising in view of the cultural barriers erected by the previous dominance of the British social anthropological tradition. In East Africa,

several studies very recently completed or still underway have been influenced by the diffusion tradition, but as far as I know, only one has reached the stage of publication (Bowden and Moris 1969).[3]

In the next section of this paper, I shall very briefly examine the way in which this theoretical orientation delimits the field of study, and the kinds of studies that have been carried out. In the following section I shall discuss the potential usefulness and limitations of this approach for increasing our understanding of the processes of agricultural development in East Africa.

III. Studies of the adoption and diffusion of agricultural innovations.

Several summary reviews of the work done within this school are available, so it would be redundant to go into much detail. Lionberger (1960) gives an unpretentious summary intended primarily for the use of agricultural change agencies in the U.S.A. Rogers' 1962 book has both theoretical pretensions and some claims for universality of his generalizations (based primarily on American empirical data), although in later work (1964) he has admitted that some at least of those generalizations are probably culture-bound. The most recent is Jones' useful review article (1967).

Jones' very concise review runs to 21 pages, while the others are full-length books, so it is obviously impossible to give a potted version of the work of this school in a few paragraphs. It is important, however, to look at the way in which this theoretical orientation delimits the subject of study-- the criteria of relevance that are provided by this perspective.

Jones, in his section 'Delimiting the field', quotes approvingly Katz's summary: "Viewed sociologically, the process of diffusion may be characterized as (i) acceptance (ii) over time, (iii) of some specific item-- an idea or practice, (iv) by individuals, groups or other adopting units, linked (v) to specific channels of communication, (vi) to a social structure, and (vii) to a given system of values, or culture" (Katz et al 1963:237) Jones goes on to say:

> In terms of the sociological approach to a better appreciation of the processes involved in changing agricultural technology, the prime concern is with the relationships between:
>
> (1) an innovation or an improved practice, or a collection of such innovations, and

(2) the "adopter" (including within this term both an individual or some social group as the adopting unit, and also the potential adopter and rejector of the innovation(s) concerned), emphasizing not only the personal and situational traits which are associated with the adopter's decision, which include

(3) his perception of the characteristics of the innovation(s) but also

(4) the processes by which the innovation spreads through a given social system, and the ways in which the adopter affects, and is affected by, the other adopters within his social system, and

(5) the functions of various communication media and channels in the processes involved.

Two distinct processes have been conceptualized: (a) adoption - a mental process through which an individual passes from awareness to interest to evaluation, trial and adoption (with rejection a possibility at all stages after the first), (b) the social process by which an innovation is diffused through a social system.

Who, then, are the innovators? Rogers has proposed five "adopter categories" on the basis of relativetime of adoption of innovations. He uses mathematically determined divisions of a bell-shaped curve representing the diffusion process. He defines innovators as the earliest 2½ per cent, beyond two standard deviations from the mean time, to adopt. The other proposed categories are early adopters, early and late majority, and laggards. This categorization is based on the assumption of a behavioral trait of innovativeness, defined as the degree to which an individual is relatively earlier to adopt new ideas than other members of his social system (Rogers, 1962:159). The 'innovator' is conceived of as a separate role from the 'inventor' who creates new ideas, although in some instances the two roles may overlap. (Ibid, 195-6) This research tradition concerns itself only with the 'innovator' in the sense of those members of a social system who are the first to adopt an idea new to that system. Rogers rigid mathematical definition may be rejected, but this meaning of 'innovator' underlies most studies.

Numerous studies in the U.S.A. and recently in Asia have concentrated on isolating the personal and social characteristics of individuals in the different adopter categories, especially on the characteristics distinguishing innovators and early adopters from the later adopters. A recent example of this approach is Roy, et al 1968. The studies of the correlates of innovativeness, measured in terms of the adoption of recommended practices, either number adopted or relative earliness of adoption, are classified by Jones in

a table (p.14). He divides the studies by the categories of factors focussed on: situational and personal characteristics of the adopter: sociological characteristics: socio-psychological characteristics, and neighbourhood or community norms and institutional factors. The paucity of studies in the last category in comparison to the other three is striking. The mainstream work in this tradition has concentrated on the characteristics of the individual adopter (usually the male head of a farm household) while paying lip service to the influence of various social groups.

Nevertheless, a few studies in the U.S.A. have shown that community norms affect the rate of diffusion of new farming practices. (Jones, 1967:12 quoting Coughenour's study, also p. 15).

Other studies have concentrated on the effect of the characteristics of the innovation on the rate of diffusion. These have emphasized that it is not what might be termed the 'objective' characteristics which are of concern here, but the characteristics as perceived by the potential adopters. Rogers proposes five characteristics that affect the rate of diffusion: relative advantage of the innovation over the idea it supersedes: compatibility with existing values and past experiences: complexity: divisibility: communicability. He regards profitability as one dimension of relative advantage, and has engaged in a prolonged dispute with Griliches over the relative importance of "profitability" versus other characteristics in explaining rate of diffusion. He points out the possibility of over-adoption or irrational adoption of innovations either technically unsound or inappropriate to the scale of operations. However, very few studies in the U.S.A. have been carried out on these aspects, or on the category of 'rational rejectors'.

Another major line of study has been concerned with the channels of communication, including the mass media, personal contact with advisors such as extension agents, and interpersonal contact. Some of these studies have looked at the role of opinion leaders. This concern with the use of communications in agricultural diffusion has been influenced by the broad stream of communications studies in the U.S.A. dealt with in Saylor's paper.

The studies concerned with the influence of communications channels at different stages of the mental adoption process of the individual have produced some results of great

interest to practising change agents. Studies so far indicate that in the U.S.A., mass media can be effective in making people aware and interested in an agricultural innovation, but that at the evaluation and trial stage, influence from valued inter-personal sources is essential to push the farmer through to trial. However, research Rogers carried out in Colombia suggests that generalizations about the significance of the mass media at the awareness and interest stage may not hold true cross-culturally. (Rogers, 1964:74. See also Myren 1962)

In conclusion, mainstream work within this tradition can be assigned to certain broad categories:

a) those concerned with the adopter as decision-maker: the stages of the mental adoption process and the influences operative at each stage.

b) those concerned with the categorization of adopters and the characteristics of the members of the adopter categories, as perceived by the external observer/ research worker.

c) those focussing on the rate of diffusion of an innovation within a social system, and the factors affecting that rate.

Factors studied include:

i) characteristics of the innovation as perceived by members of the adopting social system.

ii) community norms and institutional factors.

iii) activities by change agents.

d) studies of the channels of communication (both mass media and inter-personal) and the role of opinion leaders.

Summarizing inevitably distorts the subject. In my concern for brevity, I have concentrated only on showing the [illegible] studies carried out, so making this work appear somewhat simple-minded, and failing to reflect the considerable theoretical and methodological sophistication that has already been achieved. Readers are referred to G.E. Jones' article (1967) for a review that does more justice to the subject.

IV. Implications of the diffusion approach for research in East Africa.

Can this research orientation be of any more use to agricultural development workers than the previously dominant orientations? I think perhaps it can, but I have a number of reservations. This approach does not seem to provide much hope for the general theory of social change that sociology badly needs: its usefulness in East Africa will lie in guiding studies on specific aspects of change-- the diffusion of new

ideas-- on the assumption that if this process is better understood, it can be speeded up. This is precisely the assumption that underlay many of the American studies in the rural sociological tradition, and also, it must be admitted, market research (Rogers, 1962:2). These studies have, however, had theoretical relevance through their contribution to an increasing body of generalizations or quasi-theory.[4]

Nevertheless, it seems to me highly dangerous to adopt this research orientation uncritically, without skeptical examination of its limitations and its relevance for the East African situation. A number of variables which can be assumed to be constant, or irrelevant to the situation under study, in the highly developed country where this school emerged, may have a profound effect on the local situation. I wish to look now at some of these limitations.

1. The delimitation of the field of study.

First, the mainstream studies delimit their field of study in a manner superficially different to the social anthropologists, but with results surprisingly similar. Take the definitions of field listed above; the "social system" (mentioned by Jones) or "the social structure" (mentioned by Katz) is usually defined as a local community group or region. External change agents will be considered insofar as they are involved in relations with members of this social system, or are a significant channel of communication to it. Since the "client system" or more frequently, the individual adopting units making up the system, are the primary focus of attention, only the front-line change agents-- those actually in contact with the client system-- fall within the ambit of the research workers' scrutiny. The generation of innovations, and the planning and administration of development programs above the level of the front-line change agent, are implicitly defined as irrelevant. In developed countries, these areas would be regarded as the preserve of the sociologists of science, and of students of formal organization. To this school, 'innovator' means simply, those members of a social system who are relatively earlier to <u>adopt</u> new ideas. There is no concern with the 'innovator' in the sense of one who <u>generates</u> new ideas. Jones explicitly states that the preliminary stages of invention and adaptation for the "recipient system (the market)" are outside the scope of the processes involved in the adoption and diffusion of innovations (1967:4).

This narrow focus on the 'client system' leaves the way open for the rationalizations of program failure in terms

of characteristics of this system-- its social values or whatever-- that Apthorpe complains of with regard to social anthropology. The frame of reference used by the diffusion school is a slight improvement in that it makes provision for consideration of the characteristics of the innovation as an independent variable affecting rate of diffusion, although in practice this line of study is underdeveloped.

Within this school, Coughenour seems to represent a minority view. In a stimulating paper, (1964) "Towards a theory of the diffusion of technology", he puts forward a model of the diffusion process involving innovative, linking and practitioner systems, the systems operating at different levels of institutional specificity. His innovative system refers to the system of scientists or others whose task is the production of new technology, which is then spread to the practitioner system through the linking system. This is a gross oversimplification of a rather complex model, but it will serve to emphasize the point that, pace Jones, to fully understand the process of technological change we need to start with the generation of innovations, not with their presentation to a recipient (or in Coughenour's terminology, a 'practitioner' system).

In the East African context, it seems preferable to regard the three systems delineated by Coughenour as subsystems of one overall system, to emphasize the importance of the inter-relations between the three.

A sociological study of the innovative system of agricultural technology in East Africa would be fascinating and probably illuminating with regard to understanding of the present state of technological knowledge. The men in white coats who man the research stations and the university laboratories are working in an institutional context rather different from that of agricultural scientists in the developed countries. We need to know much more about them, their values and attitudes, their decision-making processes, the social constraints that operate in their environment. Many of these scientists are working within an extreme form of bureaucracy-- a civil service-- and we know from the sociology of organization that a bureaucratic form of organization creates problems and conflicts for those who regard themselves as professionals. Furthermore, until recent years most of these scientists have been expatriates living in isolated enclaves with their own peculiar social pressures.

In a developed country, no matter how the research worker might bury himself in an ivory tower, he still shares

a common culture with the personnel of the linking and practitioner systems. Here, in East Africa the cultural gap between the research and the peasant farmer who is presumably the intended eventual consumer of much of his work, remains immense. The structural distance and, in pre-Independence years at least, the social pressures of the elite enclave, create difficulties for those who may want to "get to know the farmer" (Tanner,). This social and cultural isolation of the scientist/innovator must inevitably affect relations between the three sub-systems. Africanization might have less effect, sociologically, than one would expect. Much of the structural distance would remain, as would the gap between the scientific sub-culture of the research worker and that of the peasant farmer.

Illuminating as such a study of the innovative system might be, it seems unlikely to be carried out. The elites of East Africa have long held an attitude that social research is something applicable only to others-- to "the natives" or "the peasants" (Sofer and Sofer). It is regarded as quite permissible for a research worker to ask a peasant farmer how he allocates his time, and even to stand over him with a clock watch, to question him about factors influencing his decisions, and what his neighbours think of him. But the investigator who tried the same questions in a research laboratory would probably be given the boot for intolerable impertinance.

This tradition of study only of the 'practitioner' or recipient groups, has led to a one-sided and distorted picture of the problems of agricultural development in East Africa, or any other kind of development for that matter. Only in recent years has it been possible to mount some studies of the lower echelons of the extension service. The need for studies of the administrative or 'linking' systems is now widely recognized, although the execution of such studies remains problematical, at least in Uganda. It would be highly unfortunate if widespread adoption of the diffusion school's research orientation led once again to a narrow concentration on the characteristics of the practitioner/recipient system. One of the very great advantages of the A-B-X model, (discussed in Saylor's paper), is that it focusses attention on the relations between the administrative and recipient system.

2. The assumption of the desirability of the innovations

My second major reservation about the approach of the diffusion school is the possibility of distortions stemming from its value-laden scheme. This schema is perhaps superior to many in that its value-loading is so flagrant: "innovations and innovators are good; rejection and laggards are [illegible]

is a crude but perhaps not overly unjust characterization of the basic orientation, (Rogers, 1962:142). Rogers discusses a few American studies that have dealt with the categories of " irrational overadopters" and "rational rejectors", but in many cases the possibility of "rational rejection" is not provided for in the research design. These categories are difficult to deal with, especially for rural sociologists, in that they require some objective criterion of rationality against which the individual's perception of the desirability of the innovation for him can be compared. Such an objective criterion might be provided by farm management studies, if available. But usually the sociological research worker takes the easy way out and assumes that the practices recommended by the Agricultural Department are good for the farmers in the sample, thus eliminating the possibility of rational rejection. Such an assumption also avoids the difficult problem of dealing with practices that may be desirable for farmers with a certain level of available resources, but not for others.[5]

This assumption is made explicit in the recent study of Indian farmers by Roy, et al.,(1968:88). They say, "...we have limited the study to the diffusion of recommended practices. We have assumed that new seeds, fertilizer, or insecticides produce more crops and, therefore, we have not been so bold as to evaluate the feasibility of the recommended agricultural technology, or to carefully measure productivity".

Such an assumption can be highly dangerous for those studies which use adoption of recommended practices as an operational measure of a dependent variable variously conceptualized as "innovativeness" or "modernity", "receptivity to new ideas" etc. The danger can be averted, and to a considerable extent [illegible] in the study by Roy et al., by using this measure only as what it is-- a measure of adoption of recommended practices. But, given the implicit value orientation mentioned above, that adoption of innovation is good and rejection is not good, there is often the tendency to slip into regarding such a quantitative result as a measure of a mental trait or characteristic of the farmers in the sample. One very popular research design involves establishing the correlates of "innovativeness" or "receptivity" as measured in terms of adoption. The usefulness, practical or theoretical, of such studies will be considered below, but it is obvious that they are useless if the measure of the dependent variable . involves a significant number of items ill-adapted to the needs or resources of the subjects. This problem of constructing valid measures of the dependent

variable has been much discussed in work elsewhere,-particularly in the U.S.A. My contention is that construction of valid measures will be much more difficult here, where it is unwise simply to assume that the current recommended practices are desirable and advantageous for the farmers being studied.[c] Evaluation of the desirability of the innovations is highly complex, and as Collinson (1968) has shown, involves consideration of implications of the innovation for the whole farm operation. Such work is obviously beyond the competence of most sociologists.

Discarding the assumption that the recommended practices are invariably and inevitably good for all farmers, has several implications for research strategy. First, as pointed out above, it implies a much greater focus not only on the characteristics of the innovation, (including profitability) but also on its consequences. Collinson (ibid.) deals with possible short-term consequences of certain types of innovation for the farming system, but leaves aside the obvious consequences, e.g. of higher risk through restricted optimal planting times, for the way of the life of the household. Of course all the consequences of an innovation can never be foreseen either by the adopters or by observers, but this is no reason for the neglect of potential adopters' perceptions of possible consequences as a variable affecting rate of diffusion. The consequences of one recommended innovation are also significant, of course, in creating a favorable or unfavorable climate of opinion towards other innovations stemming from the same agency.) A focus on the consequences of an innovation has been characteristic of the American anthropological strand in diffusion studies, but is conspicuously lacking in the rural sociological tradition, perhaps because of the difficulties in dealing with this aspect through the quantitative, survey technique which has been favored by rural sociologists.

The second implication following on making the desirability of the recommended practices a question to be examined rather than an assumption, is that the category of rejector becomes equally as worthy of study as that of adopter. Research designs should recognize the real possibility of rational rejection, difficult as this concept may be to deal with.

Third, both the preceding points emphasize the necessity for collaborative research. Few rural sociologists will possess the expertise to decide on the objective rationality of given recommendations for various levels of resource endowment; the skills of the farm management economist are vital at this point of research design, and highly desirable throughout.

3. Some other problems of measurement

There are other aspects of the East African situation that could lead to distortion if the adoption of recommended practices were used as a measure of a trait or characteristic such as "receptivity to new ideas". Coughenour (1964:79) has pointed out that while diffusion of technology is usually thought of in terms of diffusion of ideas, it often involves material artifacts as well, which diffuse through different means and different channels. Most American studies pay little attention to this aspect; probably an efficient distributive system and transport facilities can be assumed. But in East Africa, diffusion of ideas and their material embodiment may be badly out of phase. Farmers could be quite receptive to the idea of using fertilizer, if it were available to them regularly and reasonably near at hand, rather than ten or twenty miles away, to be carried home on one's back. As Coughenour says, "...a general theory of technological diffusion must encompass both the process of diffusion of new ideas and the process of transmitting artifacts." The need for such studies of the distributive system will doubtless be obvious to economists; my concern is that sociologists may use in their measures of adoption, items whose material component is, in practice, often unavailable.

Also, just what does 'adoption' imply? The problem of using a superficially clear-cut concept such as this in a fluid situation is discussed in some detail by Mbithi (1968: 24-25). His data on use of Katumani maize shows that farmers in his sample areas in Eastern Kenya sometimes planted only a very small part of their total maize area to the new hybrid or followed only a few of the recommended practices or used them only on part of the crop. (See Mbithi 1967:15-16). Categorization of such farmers as 'adopters' affects the "realism and sensitivity" of the scale, and can give a very misleading picture of the extent of agricultural change. This problem is also discussed by Roy, et al. In their study they settled on 'adoption' in the sense of 'ever having used' the recommended practices.

These points may be regarded as mere methodological details, not as inherent defects in the theory. This is true to some extent, but these problems reflect the difficulties of trying to fit the rich variety of the many inter-related factors involved in a farming situation that is changing through time, into the model involving relatively few rigidly defined variables taken at one point in time.

In summary, equation of a high score on an index of adoption of recommended practices with "modernity" or "receptivity to new ideas", may be acceptable, although the intriguing logical possibility remains that the trait purportedly being measured might sometimes better be labelled "gullibility" or 'Willirgness to follow direction". But to equate a low score with "traditionalism" "resistence to new ideas", etc., is (as is done by Rogers. 1962, passim) illegitimate unless it can be demonstrated that the practices making up the index, or the majority of such practices, are in fact both desirable for and available to the farmers who have not adopted them.

4. "The characteristics of innovators"

Having discussed some of the problems of establishing a valid measure of a dependent variable of 'innovativeness', we turn to the rationale of the type of research design involving correlations of various social, economic and psychological traits of farmers with their score on such a measure. Rogers (1962:171-186) summarizes the results of these studies, based at that time primarily on American data. G.E. Jones' summary (1967:15) probably includes a higher proportion of studies in other cultures. These show that the larger the farm business, and the more specialized, the earlier the farmer tends to be in adoption of innovations. A relatively high level of education is generally positively related to innovativeness, as is urban experience. Religious affiliations usually show no correlation. (These points drawn mainly from Jones' summary.) Rogers makes generalizations such as, "earlier adopters are younger in age than later adopters", "earlier adopters have a more favorable financial position than later adopters". (To some extent, the last generalization may be built-in, if the measure of innovativeness involves practices requiring large holdings or capital, such as wire fencing, exotic cattle. Nevertheless, it seems intuitively true that the person with higher resources will be potentially more innovative because more able to withstand risk.) Also, early adopters are more cosmopolite-- have more contacts outside their own social system.

These specific generalizations and the others like them cannot be assumed to hold true in East Africa without testing, plausible though some of them seem. But even if these hypotheses were tested and found to hold true here, how much further ahead would we be? How much understanding of the process of diffusion and adoption can this type of research design really give us? Of what use are they?

The problem here is the trite one that correlations tell us nothing about the direction of causal relationships, hence can form a poor basis for drawing policy conclusions. Such studies could perhaps help in formulating criteria for selection of "progressive farmers" or settlers on a high unit-cost settlement scheme, but if we want to use them for understanding conditions that produce innovators, we need more than mere correlations.

For example, are innovators innovative because they listen to the radio and read newspapers more than later adopters? Or do they listen to the radio and read newspapers because they have a higher income and are able to buy a radio and newspapers? Or because their higher income gives them more leisure? Or because of their higher education (if innovators do under local conditions generally have higher education than later adopters)?

Another problem is the generalizability of such conclusions. One feels intuitively that generalizations such as, "earlier adopters have higher social status than later adopters" or "earlier adopters are younger in age than later adopters" can apply only in certain kinds of social systems. If we test such statements in Buganda, would the results hold true for Nyeri District or Rungwe? Studies that focus only on correlations of characteristics of individuals with some measure of adoption, without any systematic examination of the social structure of the system to which the individuals belong, provide us a poor basis for generalization of the results to other areas. We need to know more about why and how these social characteristics for example are related to innovativeness, in that kind of social system. One can imagine that in some East African social systems, Ankole, for example, agricultural innovativeness could be negatively correlated with high status in the traditional system. On age, I can think of parts of East Africa where I would expect successful, sustained adoption of innovations to be found most amongst middle-aged rather than young men, because of the nature of beliefs about proper behavior of the young who must not "over-step" their elders.

Turning to other aspects, studies of the effect of social structure and community norms on the diffusion of innovation seem of great importance here, even though there have been relatively few of these elsewhere (see table, Jones, 1967:14). We often hear change agents talk of the social sanctions brought to bear against the progressive farmer, or the way in which claims of kinsmen act as an impediment to the man striving to get ahead. But there is remarkably little actual

evidence about such "social constraints", much less detailed information about the dynamics of such forms of control or " levelling mechanisms".[7] We urgently need both a clearer conceptualization of these "social constraints" and much more data on their actual effects on agricultural development in different kinds of communities. We need to know what sanctions are brought to bear against the innovator, who is by definition deviant, and the effect that these sanctions have, not only on the decision-making of the innovator, but also on other potential adopters. To what extent and in what kind of communities do social sanctions explain discontinuance of innovation? Or are these sanctions a rationalization of change agents to explain their own failure? These kind of questions cannot be answered by survey techniques alone, but need detailed and patient community and case studies.

Such studies would have to take into account the effect of time. Studies carried out in the British social anthropological tradition have been accused (somewhat unfairly) of neglecting the time dimension, and change. The American school of diffusion research is, ironically, even more guilty of this neglect; although both the mental adoption process of individuals and the social process of diffusion through a social system are conceived of as taking place in time, the techniques of study used lead, at least in the case of the correlation studies discussed above, to a thoroughly static picture.[8] The characteristics of the subjects are established as for the date the survey enumerator visited; the subjects are then categorized as adopters, laggards or whatever, with rarely any consideration of the normative patterns prevalent in that society concerning appropriate behaviour at different stages of the life cycle. Yet Rogers himself quotes, in his 1962 review, (p. 189) two research studies in the U.S.A. showing that there is considerable shifting of individuals from one category to another over time (in one case, a re-test after ten years, in another case, a re-test after two years). But the implications of this shifiting are not explored. The anthropological emphasis on the life cycle of the individual, and the developmental cycle of the homestead[9] would seem a valuable supplement to the synchronic "snapshot" approach of the survey. Anthropological studies suggest that in East and Central African communities with strong norms about the propriety of certain types of behaviour for young men, innovation may be much easier once a certain stage in the life cycle of the individual and the developmental cycle of his homestead has been reached.

Many other points could be made about this theoretical orientation, some of them problems in adapting the methodology to local circumstances. But it is better to turn from the general and discursive to look at what little empirical data is provided by the few recent studies available so far.

Empirical studies

This section will review only those completed studies that are directly relevant to innovation, which are available to me, leaving aside works that cast some light on agricultural innovators incidental to another subject.

The main studies available are the Bowden and Moris (1969) paper, Mafeje's unpublished thesis on "big" farmers in Buganda and Long's work on "emergent farmers" in Zambia. As regards other known material, Robertson's report on "big" farmers in Bugerere has been submitted to the Ministry of Overseas Development in Great Britain, but as far as can be determined no copy has been sent to the country concerned. Although Mbithi's data collection was carried out more or less under the influence of the diffusion/adoption orientation, his thesis (1969) has been written completely within the framework of Young's theory of "differentiation", which, insofar as I can understand its implications, appears to be not directly relevant to our topic. The data presented are so thoroughly embedded in the concepts of this theory that it has proved impossible to discuss this data within any other framework.

1. Bowden and Moris on Buganda

None of these papers focusses directly on 'innovators in the strict sense of 'those members of a social system who are relatively earliest to adopt an innovation new to that system'. The first to be considered, Bowden and Moris (1969) talks of 'progressive farmers' and the questionnaire was designed to get at the history of farm improvements, not at recent innovations. Bowden explicitly states (footnote 1) that his definition of 'progressive' and 'modernization' is "operational rather than verbal, and is implicit in the list of traits comprising the Farm Modernization Index", which he uses as his measure of progressiveness. A look at some of the 17 farm practices in this Index shows the irrelevance of the 'innovator' concept to this particular study. Some of the traits, especially "uses permanent labourers", have been fully integrated into Kiganda culture for many years.[12] One wonders if the items in the questionnaire were intended to

be used for this sort of index. A number of the items are either ambiguous, appropriate only for farmers with certain types of enterprises or dependent on level of resources rather than innovativeness or progressiveness in the usual sense of these terms. Unfortunately, there is no attempt to test the validity of the Index by criteria external to the questionnaire itself. However, in the first part of the paper, Bowden does stick to his operational definition and uses 'progressive' and 'modern' only as a shorthand form for 'high score on the Index'. But in the discussion he slips into other connotations, and assumes that farmers with low scores on his index will be 'less successful' at farming, and that the 'modernized' farms of those scoring high will be more profitable. No information on relative size of the farm holdings for low and high scores, or on levels of income from the farm operations, is given in the paper to support these assumptions, although this information was easily available. But even to a non-agriculturalist like me, the relation between 'modernization' as measured by this Index, and farm profitability seems open to question.

We are then given the 'attitudes' and 'social characteristics' of those who score high on the Index. To quote from the abstract: "... the 'progressive' Baganda farmer is willing to experiment and try out new ideas: he visits the nearby town more frequently: tends to have lived in a town; to have worked for wages: to have some work-skill that he could practise instead of farming; and to have had some kind of work training. He has wider contacts with local administrative, government and farming officials; is more likely to visit farm institutes, research stations, cooperatives, etc., and has more contact with the outside world through radio and newspapers. But age, sex and education did not distinguish the 'progressive' farmer from others...."

Most of these characterisitcs are in the direction that one would expect on the basis of the generalization quoted above that 'early adopters' are more cosmopolite than later adopters. But the lack of relation between a high index score and age or education is not in accord either with the generalizations quoted above, or with the results of the Baseline Survey in Western Kenya, (Naylor and Ascroft 1966; see also Morris, 1967), which showed high correlations between education and numerous traits of agricultural 'progressiveness'. One immediately wonders what features of the Buganda institutional context might explain these differences, but the article gives us no clue. Some other anomalies in the results are given tentative explanations only in terms of imputed character traits of individuals. "Rather than being ambitious

strivers, planning for a prosperous future, the progressive farmers appear to be highly socially-oriented, compliant individuals ...," he says, apparently on the basis of certain inconsistencies in his questionnaire results.

One longs for some way in which these results can be tied in to what is known about Buganda. Are these farmers large- or small-scale? Bibanja tenants or landowners? What income range? How many were in employment-part-time farmers--at the time of interview? We are given no answers to these and many other highly relevant questions one would like to ask. He nowhere considers the structure of the social system, not even of that institution basic both to agriculture and Kiganda social life: the land tenure system. Most students of Buganda would want to know what effect if any land ownership or tenancy would have on 'farm modernization', but Bowden is apparently unaware of the existence the kibanja tenants, the largest single category of farmers in Buganda, for he refers only to "whether the farm was bought or inherited" (in Section c. of Results). Space for recording this vital information was provided on the questionnaire, although it was apparently ignored in the analysis.

Essentially this paper consists of the manipulation of numbers, and translation of these numbers back into words, totally without consideration of the context of an ongoing social system whose history has profoundly affected the course of agricultural development, and of which a great deal is known. The context of the farming system is also totally neglected. But the authors apparently take this game seriously, for they conclude with 'policy implications'.[13] These can speak for themselves.

It would be unfair to dismiss the diffusion/adoption approach on the basis of this one paper, although it highlights the danger, inherent in all quantitative approaches, of juggling with figures in a vacuum.

2. Mafeje on Ganda 'big farmers'

Of course some of the common complaints against the anthropological approach have been the often justified ones of too much context and not enough core, lack of quantitative data to support the conclusions, and lack of consideration of the representativeness of the sample. Mafeje's thesis, although it could perhaps be faulted on other grounds, does not display weaknesses.[14] In fact, this reader often wished for more context in terms of more details of the farming system and farm operations, the lack of which is presumably due to the "inter-disciplinary" nature of the

research project. The complementary report by the agricultural economist involved is not yet available.

Mafeje defines the main object of his enquiry as "(a) to trace the emergence of what are usually called 'big' or 'commercial' farmers; (b) to identify and define more clearly the category to which they belong; and (c) to investigate the process of economic and social differentiation among them, and between them and other members of the community. This will be done by considering the question of what category of people in Buganda made use of the new economic opportunities introduced by Europeans." (1968:1-2) He discusses, more frankly than is usual amongst anthropologists, the problems of fieldwork (which included a "revolution") and the difficulties of defining and locating the kind of farmers the project was to study. The research design had been worked out in England, and involved the assumption that 'progressiveness' among farmers correlated with the size of the unit of operation. which was found not to hold. Nor, in Buganda, are big landowners 'big farmers'; quite the contrary. In the end he had a sample of 110 famrers in four counties, of whom 44 had 20 or more acres under cultivation, and 30 had less than 10. However, his questionnaire data is based on only 90 satisfactory questionnaires. Those with less than 10 acres were "interesting cases" and ones who were 'big farmers' in the Kiganda conceptual scheme, even if not in terms of an 'objective' definition based on size of operation. One of the most interesting aspects of this thesis is the interplay in it between the observers' categories and the categories of the culture under study.

The thesis gives us no precise measure of adoption of specified innovations or recommended practices, but it seems permissible to regard these farmers as innovators/early adopters of a new farming pattern. Mafeje says,

> ... a few farmers have managed to increase their labour and capital inputs to a level far beyond that of the average producer. These men represent something new, the beginning of 'commercial farming' in Buganda.

But he queries whether this new pattern necessarily involves methods and techniques other than those used by small producers.[13]

Fortunately, Mafeje is able to draw on Wrigley's work on agrarian history, to show that these new commercial farmers could be found before 1939, but it was only in post-war decades that they really came to the fore, (ibid., p. 94). In this case, we are not dealing with innovation in the simple sense

of a new technique transmitted from outside to a passive recipient system. Instead, we are looking at the emergence of new patterns, forms of organization and allocation of resources slowly developed by the farmers themselves.[16] These new forms obviously are consequent on such introduced changes as cash crops and marketing facilities. But there is a very strong element of endogenous change here, as some individuals took advantage of the new economic opportunities to a greater extent than the majority.

Who are these new men: what motivated them? Mafeje found that of his questionnaire sample of 90, only 14 were descendents of these who received mailo allotments in 1900; 62 had bought their land or inherited purchased land, while the remaining 14 were tenants. The majority, he says, are "self-made" men. About 30 per cent started life as small-scale farmers on rented or borrowed land and then expanded into commercial farming without going into any other enterprise. Others used various forms of trade to build up capital, while others took up semi- or unskilled wage employment. The land tenure system made it possible, for those who had accumulated some capital, to get land, and a plentiful supply of immigrant labour has been available from the mid-1920's.[17]

Mafeje is skeptical about the usual hypotheses on the social characteristics of progressive farmers. His data show that the biggest farmers include a number who have never done anything else but farming, while two who had spent twenty years as clerks in Kampala were doing very badly. He suggests "that a potentially good or progressive farmer is not the man who passively imbibes commercial ideas in industry and commerce, but the man who, because of his motivation, actively seeks out such ideas wherever they are to be found.... Ambition and changed attitude among emergent farmers are more likely to be the cause for temporary migration to the cities /in search of capital/ than a result thereof.... If there were a casual relationship between contact with commercial ideas and emergence of progressive farmers, then we would have expected everyone in Buganda to be a progressive farmer by now" (ibid., pp. 180-181).

One surprising fact is the high average age of his "new men". More than half of the farmers in his sample are over 50 years of age. Partly, this can be explained in terms of motivation, "security against old age". But the emergence of farmers over time is striking. Mafeje says the majority started large-scale operations between 1938 and 1956, good years in the

economic history of Buganda, when it might have been relatively easy to obtain capital. In recent years, recruitment has slowed down considerably and fewer young men have become large-scale professional farmers.[18]

On education, he says "it cannot be assumed that education is a necessary condition for progressiveness among farmers. In our sample of 110, we found that nearly 50 per cent of the farmers had never been to school, and of those who had been, only six went beyond the primary level." Of these six, only one (an ex-agricultural officer) could be counted among the "best farmers",[19] while some of the "very best" were men who had never been to school (ibid., pp. 183-184). He discusses these facts in relation to the educational pattern, status system and job opportunities in Buganda. About 40 per cent of the farmers gave lack of education as one reason why they turned to commercial agriculture. Lack of education, by disqualifying ambiticus, striving individuals from well-paid town jobs, appears to pre-dispose them towards large-scale farming. But these same farmers place a very high value on education, and spend as much as one-third of their income on educating their children. Since the sons of these commercial farmers receive more than average education, they may be less likely to succeed their fathers as professional farmers.

Mafeje summarizes thus the "social characteristics" of this category labelled "big" or commercial farmers:

> The great majority are bakopi ('ordinary people' or 'peasants', not land-owning elite) by origin, who bought their land, while a few inherited purchased land, and a small minority inherited mailo land (which implies higher social origin). Virtually all of them lack education: only two have had any training in agriculture. The emergence of most of them coincided with the coffee boom period; "as a result they represent an outgoing generation". All of them work as individuals and do not belong to any economic corporation, traditional or modern.

But in what is perhaps the most important contribution of this work, Mafeje goes on to show that the statistical aggregate labelled "new commercial farmers" lumps together more than one kind of farmer. These sub-groups differ significantly in motivations and economic behaviour patterns. These differentiations were got at by analyzing the semantic categories used by the rural people themselves when speaking their own language. I am told that Luganda is especially rich in terms expressing fine shades of social differentiation, as one would expect from such a status-minded society. Mafeje deals with six terms used for different kinds of farmers, and exemplifies their meaning by a detailed analysis of case histories of a

sub-sample of 20 farmers in Busiro. (Most of his sample farmers were widely scattered, but in Busiro these twenty all lived in the same parish, sufficiently close that their social interactions with each other and the rest of the community could be studied.) The picture built up by this detailed analysis is complex and fascinating, but space forbids an adequate summary. For our purposes, the three most important categories are men of affairs, who mostly [illegible] heirs of large mailo land holdings, exert traditional forms of leadership in the community and tend towards more traditional attitudes. In contrast, the men of profit are the self-made men who bought their land, take little interest in politics or community affairs, and spend most of their time on their own farms. They have no tenants and do not play the role of 'omwami' as the men of affairs do. They are the extreme of the ambitious, striving farmer, whose existence Bowden attempts to deny. The smaller of the "commercial farmers" were referred to simply as 'farmers' (balimi). (Below these, of course, were the bakopi, also subdivided, and the immigrant labourers.)

Mafeje shows quite convincingly that patterns of recruitment of these new farmers and their behaviour, membership in voluntary associations, attitudes to labour and even wages paid to labourers, cannot be understood without reference to these social distinctions. The 'men of profit' tend to look on their farms as commercial enterprises capable of generating wealth continuously, while the 'men of affairs' tend to use their land only to maintain a certain standard of living, in effect, to keep their Mercedes' on the road. The patterns of consumption, the desire of the 'men of profit', the rich farmers of bakopi background, to 'live like a chief' cannot be understood without knowledge of the Kiganda ranking system. And as Mafeje says, "aspects of social differentiation can only become intelligible when viewed from a wider social angle." In other words, to understand the economic attitudes and behaviour of these farmers, we need to know a great deal about the social system and the categories members of that system use themselves. The extent and the applicability of such categories within a culture can be tested, and should be tested, by survey or quantitative techniques, but the discovery of the significant categories requires very different methods: the intensive methods, the concern for language and meaning, of the anthropologist. Once again, these two approaches emerge as complementary, not as exclusive alternatives.

The implications of Mafeje's study for cross-cultural generalizations are depressing. Rogers says (1962:311-313)

"innovativeness of individuals is related to a modern rather than a traditional orientation"-- the 'men of affairs' sub-category of these "new men" maintained strongly traditional interests. Rogers says, early adopters are younger in age and of higher social status. But in Buganda the commercial farmers are mainly old, and the majority are of low social origin, while their present status is ambivalent. G.E. Jones says (1967:15) there is usually a positive relationship between "innovativeness" and a relatively high level of education and also to urban experience in the individual's background. Yet the Ganda "new men" are overwhelmingly of no or low education, and a significant number had no urban experience. As for the generalizations about "cosmopolite" sources of information, Mafeje does state that these men listen to the radio, read newspapers and drive into town much more frequently than ordinary farmers, but he implies that to a great extent this is a consequence and not a cause of their relative affluence (p. 236).

Mafeje has explained these anomalous results by relating the characteristics of these innovators to their social system. We can see why, in Buganda, the ambitious man who wanted a high standard of living but had low education would turn to large-scale agriculture at a certain time period. What we still do not know is what creates this kind of ambitious, striving personality.

Even more depressing, from the point of view of accumulating a body of knowledge for immediate application elsewhere, is Mafeje's demonstration that the emergence of these men is a result of specific historical circumstances, and cannot be understood apart from the time factor: the high crop prices, the Kiganda political system and opportunities for mobility, the easy availability of land-- all these have changed. Recruitment to this category has slowed down; these men appear to be a dying phenomenon.[20] Their capacity for innovation appears to be spent since Mafeje reports that they were demoralized and unsure how to cope with the fall in Robusta coffee prices. Mafeje devotes many pages to the discussion of the likely sources for the future growth of agriculture in Buganda. Mafeje's "hunch" is that the small and medium farmers are more likely to be the future innovators. Whether he is right or not is immaterial to the point that the conditions have changed: we cannot hand his results to a credit officer as criteria for picking the future "progressive" farmers, the future innovators who should be backed, as one would back a race-ho We still have no form-sheet for picking innovators in any and all cultures, and we are unlikely

to get one. What this pioneering study does give us is much more insight and understanding into the conditions that have led to the emergence of one kind of innovators in one society and one historical period, than any other work I know of. It may be that generalizations of real cross-cultural applicability can only emerge after accumulation of many more such studies of the dynamics involved.

3. Long on emergent commercial farmers in Zambia.

Although the geographical focus of Long's book, Social Change and the Individual,[21] lies outside our political boundaries, the content and analytic method is of direct relevance. Long's primary aim is the study of "social responses to economic change" in one parish of Serenje District, Zambia, inhabited by the matrilineal Lala people.

Although Long sometimes speaks of innovation, his theoretical approach has no connection with the American diffusion/adoption school. In addition to the conceptual armoury built up by his predecessors of the Manchester school of social anthropology, he draws to some extent on Barth's work on entrepreneurs, and on Weber. Long says "emphasis will be given to the problem of how far economic innovation has brought about concomitant changes in the social organization and values of the people. It will be argued ... that the move from a subsistence-based axe agriculture to one where cash cropping and the use of the plough are becoming increasingly important, has led to certain changes in the organization of agricultural labour and in attitudes towards land holding, and has also stimulated the emergence of smaller, differently composed residential groupings," (p. 4). But in addition to studying consequences, Long also queries whether pre-existing changes might have facilitated acceptance of the new modes of socio-economic orientation. He concludes that one of these was ecological deterioration: the environment could no longer support the traditional ash-cultivation methods.[22]

The two outstanding agricultural innovations have been the plough, and Turkish tobacco as a cash crop. Plough cultivation was introduced about 1950, by the Agricultural Department, as part of a scheme to develop "peasant farmers". The registered "peasant farmers" were given instruction in the new techniques and granted implements and credit. They produce both food crops and tobacco for sale. (Tobacco is also grown by ordinary "subsistence" cultivators. Thus, unlike Buganda, the new form of agriculture has been presented to the practitioner as a 'package' originating outside the system.

(See pp. 16-20 for a discussion of these schemes).

Long found that the "peasant farmers", those who had adopted the package of innovations, were of two main types:

a) those who had recently returned from a considerable period in urban wage employment, and who had capital to set up as peasant farmers; and

b) those who were younger, with less urban experience, less capital and who had gradually moved into cash-crop farming after several years in some part-time trade. (Ibid., 237). These were predominantly members of the Jehovah's Witness religious sect.

The standard 'characteristics of innovators/early adopters" kind of study would stop at this point. But Long goes on to examine the different behaviour of these two kinds of farmers over time, as they manipulate their material and social resources to achieve their aim of a viable farm. He also explains the reasons why a disproportionate number of those who have taken advantage of the new economic opportunities have been Jehovah's Witnesses His argument is complex and cannot be briefly summarized. He suggests that the 'religious ethic' of the sect legitimates forms of behaviour that are deviant by customary standards, also the sect gives the economic innovator a range of non-kin as well as kin that he can draw on for support. But in addition, Long finds that there are certain social characteristics pre-disposing an individual to becoming a Witness (pp. 225-233). These are early and continuing contact with a number of active Jehovah's Witnesses, three or more years of formal schooling, low expectations for leadership of his matrilineal kin group; urban experience in some unskilled occupation but possessing some trade.

We see that included here are the familiar factors: education, urban experience, and alternative skills. But here these are related to a dependent variable of membership in a minority religious sect with an ethic that is "compatible" with the new forms of economic behaviour. This membership in turn is seen as a factor contributing to success in the new economic enterprises The causal interconnections here are obviously very complex.

My object in selecting this aspect, rather than the many other interesting and relevant things Long has to say, is to point to the probable relation between certain kinds of religious affiliation in certain kinds of situations, and innovative behaviour in agriculture, as well as other spheres of life. There is scattered evidence from elsewhere on this point. Long quotes Gluckman as saying that amongst the

Plateau Tonga of Zambia in the late 1940's, there was a 'perfect correlation' between membership of the commercial farmer category and membership of the Seventh-day Adventist Church. Konter, (1968:7) when describing his research on economic motivation in Rungwe District, Tanzania,[23] makes no specific reference to innovative behaviour, but comments that Christians, especially Moravians, are thriving economically more than pagans. He cites the usual reasons: the ban on polygamy, dancing and beer drinking, the closer social relations with fellow-Christians. But he also says "Many Christians are less afraid that the accumulation of wealth will be attacked by witchcraft." (Ibid.) It seems likely that these hard-working, abstemious farmers are also more prone to adopt agricultural innovations than their pagan neighbours. But if this assumption were to be confirmed by a precise "adoption of recommended practices" measure, we would still not have any simple causal connection. For Christianity in Rungwe is historically associated with education, and also, Konter implies, with a higher level of income that could be spent on innovations rather than beer or bridewealth.

It was suggested above that the relation between innovative behaviour and religious affiliation holds only under certain conditions. We could speculate about these conditions. First, some degree of sectariarism seems to be involved: the religious groups tend to form a minority in the larger community, to have their own network of social relations and their own means of social control. Thus their members are somewhat more independent of the prevailing forms of social control in the community. Second, the "religious ethic" usually seems to involve hard work, often monogamy, abstinence from beer and smoking, as a minimum component.

Although the examples I am familiar with are all Christian sects there is certainly no inherent correlation between Christianity as such, and innovative behaviour, especially not with Christianity as the religion of the majority. Moris' Buganda questionnaire did not enquire about religion. One would not expect any very great differences between Catholics and Protestants in Buganda on this kind of Farm Modernization Index.[24] But what about the Balokole? Stenning found that the Balokole amongst the pastoral Ankole of Uganda were more oriented to the cash economy than their fellow tribesmen, and also differed in attitudes to hygiene and patterns of consumptions (quoted in Long, 1968:24 -43). But the complex connections between social stratification, education and religion in Buganda may lead to a different pattern there.

There may be a time element at work here as well. Long

suggests that a correlation of religious affiliation with commercial farming found in the 1940's would not be so obvious today in Tongaland, given the general expansion of commercial farming. This gets some negative support from the lack of mention of significant social relations among sect members, in A.D. Jones' (1966) paper on social networks of commercial Tonga farmers. Is this factor most significant for innovators and early adopters, that is, for those who are deviating the most from the prevailing norms, and hence are most in need of a supportive reference group?

There is an extensive literature on the relation between religious ideology and socio-economic development, which I have not delved into at all. These few simple speculations based on examples familiar to me from East/Central Africa, are intended only to suggest that a study of innovators and innovation should not neglect the factor of religious affiliation, even if we are as yet unable to specify the kinds of affiliation or the kinds of social conditions which may be significant.

VI. Conclusions: Why "the sociology of the innovator"?

The gist of this lengthy paper can be expressed very simply. Two different meanings of 'agricultural innovator' have been identified as occuring in East Africa: the inventor or generator of innovations and the practitioner or adopter of innovations. We know very little about either kind. Some of the possible reasons for this ignorance have been explored, and two different research orientations that offer some promise of remedying this ignorance have been discussed. Doubtless there are other orientations within the broad field of sociology/anthropology that can be drawn on, but the paper is long enough as it is.[25] The review of available empirical studies shows, I think, that we have so far only nibbled at the edges of a very complex subject.

It should also be obvious that the title I was assigned is really rather odd. Like the man who aims at being happy, we are likely to miss our target if we aim too narrowly at the innovator.

The usual kind of measures used for isolating 'innovators' or 'progressive farmers' requires evaluation of the innovations if the measure is to be at all sensitive or realistic. Establishing the characteristics that correlate with 'innovativeness' does not get us very far unless we are able to relate these characteristics to the social system, and explain what social conditions lead to 'innovativeness'. If we wish to draw

policy implications, we need to know the direction of the causal connections, not merely what is correlated. And the inter-relations between, for example, education, communications behaviour, urban experience, access to capital, religion, alternative opportunities for mobility, will not be easy to disentangle. (The powerful analytic techniques of the statistically-minded sociologist will be needed here, but for some time to come in East Africa, the skills of the anthropologist will also be needed to help produce valid data for these techniques to be applied to).

My anthropological bias will have been obvious throughout this paper. Nevertheless, the fortuitous fact that two strong anthropological studies were available, and only one weak example of the diffusion/adoption type of approach, has unfairly loaded the argument. It needs emphasizing that the diffusion/adoption school has much to offer. For present research in East Africa, the value of the work of this school lies not so much in its results, but in the provision of a useful research orientation, a conceptual framework that suggests new kinds of questions, and a great deal of work already done on methodological problems. This approach has its limitations, some of which are discussed above. Although elsewhere it has been used mostly by sociologists (and social psychologists), under local circumstances the collaboration of agricultural economists and the use of some techniques from the anthropological tool kit seem essential for production of useful results.

Probably the most serious limitation of this approach, as usually practicdd, is the tendency to focus only on the practitioner system. The characteristics of the farmer as innovator may be relatively less significant for understanding agricultural development than the characteristics and the competence of those who plan, generate, and offer the innovations to the farmer, the relative advantage of the innovation to him, and the position of him and his fellow peasants in the larger socio-economic system of the nation.[26]

FOOTNOTES.

1. More detailed criticues of the orientation of social anthropology during the colonial period have been published by Apthorpe (1968) and Magubane (1968). Unfortunately the latter's many valid points are rather obscured by his intemperate polemic.

2 Hammond (1966, Introduction) discusses reasons for this neglect and also points out the lack of training of anthropologists in techniques for study of agricultural technology.

3. East African research projects influenced to a greater or lesser extent by this tradition include the work of Moris and Watts in Embu and Mbithi in Mbere and Machakos, Moris and Watts in Buganda, Bodenstedt in several areas of Kenya, the Uchendu-Anthony studies in Teso District; Uganda; Kisii; Kenya; Geita, Tanzania, and possibly Saylor's work in Tanzania. I have not been able to get any information on the theoretical orientation used by Nypan for her study in Arusha. This list is doubtless incomplete.

4. Although the phrase 'theory of diffusion and adoption' is often used, the present status of the work does not fit the strict meaning of 'theory' as "a set of hypotheses structured by the relation of implication or deducibility" (Galtung, 1967 :451).

5. A few studies of this sort have been done, e g., Goldstein (quoted in Rogers, 1962:44) and Eichhorn, 1961. Armed with a dictum from agricultural economists that four-row corn planters could only be used economically by farmers with 60 or more acres of corn, they found that of the adopters, 37 per cent could be classified as "rational", while 33 per cent were "rational rejectors"; the remaining were "irrational underadopters and "irrational overadopters"). It would be interesting to see the results of such a research design applied to the adoption of "Landmasters" in Buganda, an innovation recommended and subsidized by the Uganda Agricultural Dept., a few years back. A discussion of the use of this innovation in Buddu, Buganda, is found in Hunt (1966)

6. A similar point is made by Byrnes, 1956, based mainly on Southeast Asian material. He says while technical adequacy of extension staff and appropriateness of extension advice may be assumed to be at a satisfactory level in developed countries with well-established extension services, such satisfactory levels are extremely unlikely in most underdeveloped areas where the research and extension agencies also are developing.

7. The Dutch anthropologist, Van Velzen, has been studying some of these aspects in Rungwe District, Tanzania, specifically in relation to rural development.

8. Similar points are made in an unpublished manuscript by A.D. Jones on his study of agricultural innovation amongst the Plateau Tonga of Zambia, in which he emphasizes the importance of the life cycle for understanding innovative behaviour, and agricultural behavior in general. I am very grateful to Mr. Jones for permitting me to read this most stimulating manuscript.

9. By the 'developmental cycle of the homestead' is meant the cycle through time from foundation through a marriage, expansion (birth of children, addition of more wives, and in some areas establishment of sons within the compound), decline as sons move away, and fission (division of the remaining household property following the death of the homestead head and establishment of separate homesteads). The classic discussion of this concept is Fortes (1958). More immediately relevant is Gray and Gulliver. 1964, in which the interdependence between rights in land and livestock and the developmental cycle are exemplified in a number of case studies, mostly from East Africa. The developmental cycle is obviously much more important in understanding agricultural behaviour in Africa than in a country such as the U.S.A., where a very simple form of the cycle is predominant Nevertheless, some American studies of adoption of innovations have shown that the stage of the family in the developmental cycle was a significant variable. In this case the items involved were "homemaking" practices, not agricultural innovations. (Lionberger, 1960:74)

10. Unfortunately for those with a taste for such number games, the actual quantitative data is omitted.

11. This paper is based on data collectedby Moris in 1968, but analyzed by Bowden. Judging from the content of the paper, it would seem that Moris had very little hand in writing it, since it fails to reflect his knowledge of the area.

12. Mafeje (1968) summarizes the plentiful evidence on this point. By the mid-1920's, there was a regular flow of immigrant labour from outside Uganda, much of which was absorbed by Ganda farmers. In 1934, Mair wrote that employment of labourers had become a normal feature of Ganda life (quoted in ibid, p. 106). The employment of hired labour has been the norm amongst Ganda farmers for some time, even for those who have only 3-5 acres. Those who do not conform to this norm

usually explain they do not have enough money to pay for labour. There is ample evidence of over-employment of labour, for numerous reasons including prestige, and the traditional dislike of Ganda males for physical work on their farms, (ibid., p. 134-135). This subject has been thoroughly dealt with also in Richard's Economic development and tribal change. It is therefore difficult to understand how anyone who had looked even superficially at the literature on Buganda, could categorize this practice as a 'modern farm improvement' or as 'not traditional in the region studied'.

13. The discussion of this work has been written on the assumption that the paper itself would be available by the time of the conference, since it is, rather surprisingly, being published in the East African Journal of Rural Development, vol. 2, no. 1. But in case the journal appearance is delayed, here is the concluding paragraph:

> Some policy implications of these findings would seem to be, first, to provide a justification for the effort and cost of government agricultural activities; second, to suggest that an extended, carefully supervised loan scheme could be used to draw back onto the land some of the large number of individuals who flock to the city, overburdening its scarce employment resources, but who would appear from the present study to be potential progressive farmers, assuming they show enough initiative and ability to sustain at least partial employment for a year or two in the city; and, third, to suggest the planned and extensive use of press and radio to propagate modern ideas and attitudes about farming.

14. The thesis is based on field work in 1966-67 as part of a bigger multi-disciplinary project planned by the African Studies Centre and School of Agriculture, Cambridge. It must be emphasized that the population he is dealing with is very different from the population covered by the Bowden/Moris paper. According to Mafeje's economist colleague, the "large farmers" they studied represent only about 5 per cent of all farmers in Buganda (p. 24). The Bowden/Moris paper deals with a random sub-sample of the random sample used by Hall in two areas for his farm management study, with the difference that Bowden eliminated all non-Ganda. Therefore, the overwhelming majority of respondents in the Bowden/Moris paper will be small scale farmers.

15. He raises the point, but fails to answer it. Perhaps the answer will be found in the agricultural economist's report. Mafeje's questionnaire for farmers did include a simple "adoption of new techniques" section, but this data is not presented in the thesis.

16. One is struck by absence of information on guidance from outside received by these farmers, although the questionnaire did ask about courses in agriculture, etc. The only reference to the extension services is a disparaging one about the low quality of their advice. Since these farmers were primarily growing Robusta coffee, it may be that extension services have been an insignificant factor in their success.

17. Again, it must be emphasized that availability of hired labour is a pre-condition for large-scale farming, and use of it should not be part of the definition of "commercial farmers". See footnote 12. Mafeje deals with the problems of defining these farmers in pp. 141-148.

18 This statement applies to the four counties he studied in the heart of Buganda. He quotes Robertson as finding more young commercial farmers in Bugerere, a "frontier" area.

19. Unfortunately, Mafeje never gives us his criteria for distinguishing "best" or "very best" from other farmers.

20. But see footnote 18. It is impossible to discuss the implications in the absence of Robertson's report, but one wonders if the Bugerere farmers (mostly growing matoke?) are riding the crest of high crop prices for their main crop, as the Robusta growers did some years ago, and building up capital to finance expansion.

21. The book is based on 16 months field work in 1963-64. Since that time, the agricultural development programs have been changed considerably.

22. Long had the advantage of two pre-existing studies of the farming system and ecology of Serenje District, done in 1946 and 1958 by agriculturalists. This emphasizes that the factors relevant to any socio-economic change are so various that isolation and study of them all usually is beyond the competence of a member of any one discipline.

23. This research project provides a rare example of synthesis of diverse methods. Konter spent his first year in participant-observation in the villages, and learned the language. He then drew up his questionnaires in the local language and in terms of Nyakyusa culture, and collected schedules from some 1300 people.

24. Mafeje argues (pp. 219-220) that Catholics have responded to agricultural opportunities more than Protestants, for historical reasons, including the domination of Protestants in Buganda politics. But his numerical evidence in support of this argument is unconvincing.

25. I am especially conscious of the omission of the exciting perspectives opened by the work of the Norwegian anthropologist, Fredrik Barth.

26. My criticism of the narrow focus of this school may be only partially applicable to some of the most recent work. Since writing the above two new items have been received, the first a report of a speech by one of the leaders of this school, Prof. Everett M. Rogers of Michigan State University. In discussing his studies of factors which affect diffusion and adoption in agriculture in Nigeria, Brazil and India, he spoke of the separation of the elites from the masses and stated that this results in serious barriers to change.

The second is the report by Hursch, Roling and Kerr on the study of innovation in Eastern Nigeria, part of the larger project under Rogers' direction. A superficial reading of this report suggests that many of the criticisms made in this paper do not apply to it. It focusses on villages rather than individuals, treats change agents' characteristics as significant variables, gives a critical discussion of the innovations involved, and complains about the need for and lack of studies of higher echelons of change agencies. It is interesting that this research work involved anthropologists (especially in the early stages), sociologists, agriculturalists and agricultural economists.

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